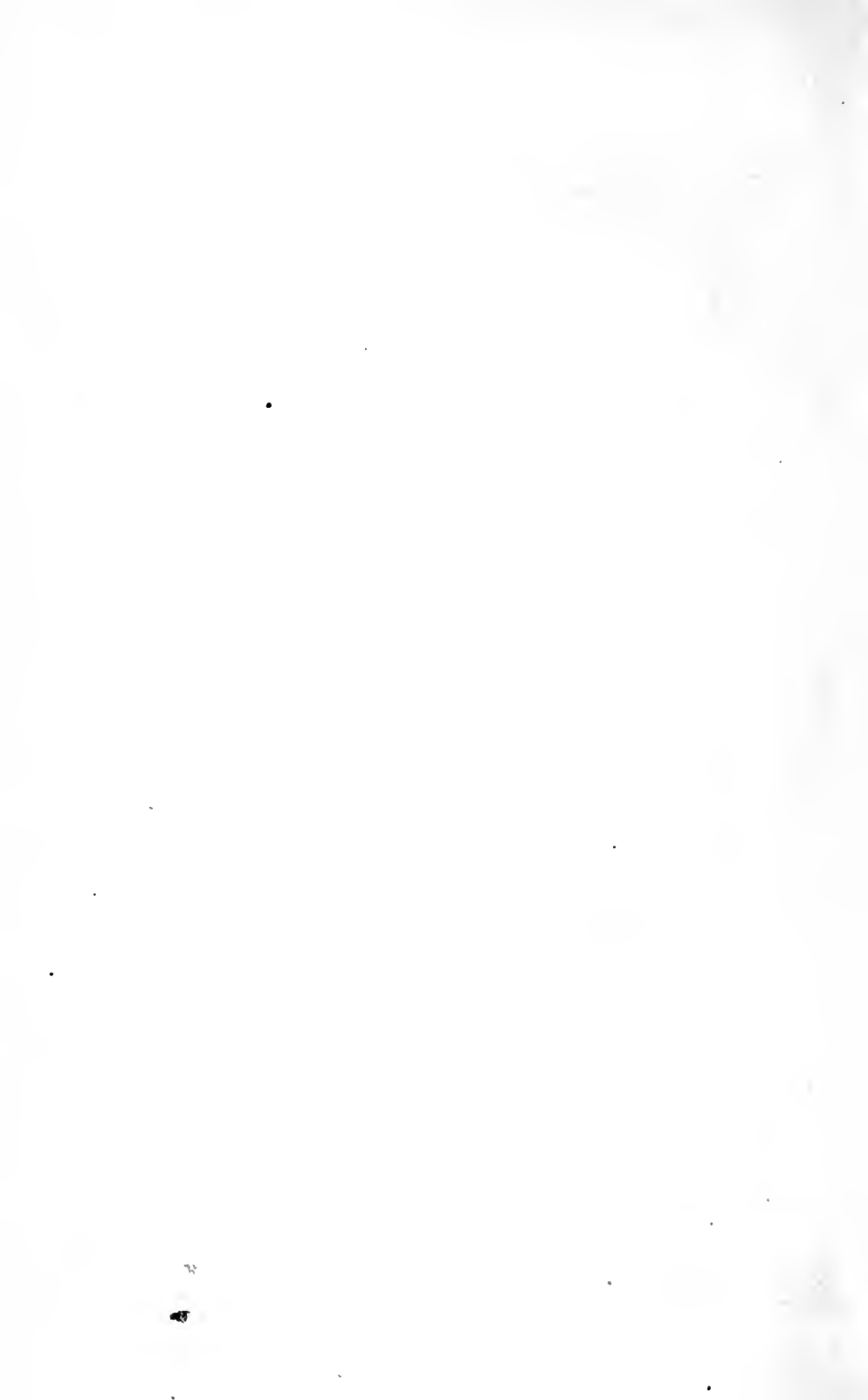INTERNATIONAL CONGRESS OF ARTS & SCIENCE

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The most important services of Archimedes were rendered to pure Geometry, but his popular fame rests chiefly on his application of mathematical theory to mechanics. He invented the water-screw and discovered the principle of the lever. Conerning the latter the famous saying is attributed to him: "Give me where I may stand and I will move the world." He first established the truth that a body plunged in a fluid loses as much of its weight as is equal to the weight of an equal volume of fluid. This is known as the "Principle of Archimedes," and is one of the most important discoveries in the science of Hydrostatics. It was by this law that he determined how much alloy the goldsmith, whom King Hiero had commissioned to make a crown of pure gold, had fraudulently mixed with the metal. The solution of the problem suggested itself to Archimedes as he was entering the bath, and he is reported to have been so overjoyed that he ran through the streets without waiting to dress, exclaiming, "Eureka! Eureka!" (I have found it!). He was killed at the age of seventy-five, during the capture of Syracuse by Marcellus in 287 B.C. The original painting of Archimedes by Niccolo Barabino is in the Orsini palace, Genoa.

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VOLUME II

AESTHETICS AND MATHEMATICS

COMPRISING

Lectures on the Relation of Aesthetics to Psychology
and Philosophy, Problems of Ethics, History of
Mathematics in the Nineteenth Century,
Algebra, Analysis, Geometry, and
Mathematical Physics.

ILLUSTRATIONS

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MENTAL EDUCATION OF A GREEK YOUTH

Photogravure from the Painting by Otto Knille

Greek youths were carefully trained by educators who gave equal attention to the physical and the mental needs of their charges, severity of ordeal being characteristic of both. The picture opposite is a reproduction of a section of a frieze painted by Knille for the library of the Berlin University, in which by a series of four pictures the artist very admirably depicted the prime features that distinguished the process of Greek education.

SECTION E — ETHICS

SECTION E — ETHICS

(Hall 6, September 23, 10 a. m.)

CHAIRMAN: PROFESSOR GEORGE H. PALMER, Harvard University.
SPEAKERS: PROFESSOR WILLIAM R. SORLEY, University of Cambridge.
PROFESSOR PAUL HENSEL, University of Erlangen.
SECRETARY: PROFESSOR F. C. SHARP, University of Wisconsin.

THE RELATIONS OF ETHICS

BY WILLIAM RITCHIE SORLEY

[William Ritchie Sorley, Knightbridge Professor of Moral Philosophy in the University of Cambridge; Fellow of the British Academy. b. Selkirk, Scotland, 1855. M.A. Edinburgh; Litt.D. Cambridge; Hon. LL.D. Edinburgh. Post-Graduate, Shaw Fellow, Edinburgh University, 1878; Fellow, Trinity College, Cambridge, 1883; Lecturer, Local Lectures Syndicate and for the Moral Science Board, Cambridge, 1882–86; Deputy for the Professor of Philosophy, University College, London, 1886–87; Professor of Philosophy, University College, Cardiff, 1888–94; Regius Professor Moral Philosophy, Aberdeen, 1894–1900. Author of *Ethics of Naturalism*, 1885 (new ed. 1904); *Mining Royalties*, 1889; *Recent Tendencies in Ethics*, 1904; Edition of *Adamson Development of Modern Philosophy*, 1903.]

THERE are many departments of inquiry whose scope is so well defined by the consensus of experts that one may proceed, almost without preliminary, to mark off the boundaries of one science from other departments, to investigate the relations in which it stands to them, and to exhibit the place which each occupies in the whole scheme of human knowledge. In other departments opinion differs not only regarding special problems and results, but concerning the whole nature of the science and its relation to connected subjects. The study of ethics still belongs to this latter group. In it there is no consensus of experts. Competent scholars hold diametrically opposed views as to its scope. They differ not merely in the answers they give to ethical questions, but in their views as to what the fundamental question of ethics is. And this opposition of opinion as to its nature is connected with a difference of view regarding the relation of ethics to the sciences. By many investigators it is set in line with the sciences of biology, psychology, and sociology; and its problems are formulated and discussed by the application of the same historical method as those sciences employ. On the other hand, it is maintained that ethics implies and requires a concept so different from the concepts used by the historical and natural sciences as to give its problem an altogether distinct character and to indicate

for it a far more significant position in the whole scheme of human thought.

The question of the relation of ethics to the sciences implies a view of the nature of ethics itself and, in particular, of the fundamental concept used in ethical judgments. If the nature of this concept and its relation to the concepts employed in other branches of inquiry can be determined, the relations of ethics will become clear of themselves. The problem of this paper will receive its most adequate solution — so far as the time at my disposal permits — by an independent inquiry into the nature of the ethical concept in relation to the concepts used in other sciences.

The immediate judgments of experience fall into two broadly contrasted classes, which may be described in brief as judgments of fact and judgments of worth. The former are the foundations on which the whole edifice of science (as the term is commonly used) is built. Science has no other object than to understand the relations of facts as exhibited in historical sequence, in causal interconnection, or in the logical interdependence which may be discovered amongst their various aspects. In its beginnings it may have arisen as an aid to the attainment of practical purposes: it is still everywhere yoked to the chariot of man's desires and aims. But it has for long vindicated an independent position for itself. It may be turned to what uses you will; but its essential spirit stands aloof from these uses. It has one interest only, — to know what happens and how. Otherwise it is indifferent to all purposes alike. It studies with equal mind the slow growth of a plant or the swift destruction wrought by the torpedo, the reign of a Caligula or of a Victoria; it takes no side, but observes and describes all "just as if the question were of lines, planes, and solids." Mathematical method does not limit its range, but it typifies its attitude of indifference to every interest save one, — that of knowing the what and how of things.

We can conceive an intelligence of this nature, a pure intelligence, or mere intelligence, to whose understanding all the relations of things are evident, with the prophetic power of the Laplacian Demon and the gift of tongues to make its knowledge clear, and yet unable to distinguish between good and evil or to see beauty or ugliness in nature. We can conceive such an intelligence; but it is an unreality, a mere abstraction from the scientific aspect of human intelligence. Pure intelligence of this sort does not exist in man, and we have no grounds for asserting its existence anywhere. In the experience which forms the basis of mental life, judgments of reality are everywhere combined with and colored by judgments of worth. And the latter are as insistent as the former, and make up as large a part of our experience. If we go back to the original judgments of experience, we find that they are not only of the form "it is here or there,"

"it is of this nature or that," "it has such and such effects;" just as a large part of our experience is of another order which may be expressed in judgments of the form "it is good or evil," "it is fair or foul."

Nor does the way in which scientific judgments are elaborated give any rationale of the distinction between good and evil. If we ask of science "What is good?" it can give no relevant answer to the question. Strictly speaking, it does not understand the meaning of the question at all. The ball has gone out of bounds; and science cannot touch it until it has been thrown back into the field. It can say what is, and what will happen, and it can describe the methods or laws by which things come to pass; that is all; it has only one law for the just and the unjust.

But science is very resourceful, and is able to deal with judgments of worth from its own point of view. For these judgments also are facts of individual experience: they are formed by human minds under certain conditions, betray certain relations to the judgments of fact with which they are associated, and are connected with an environment of social institutions and physical conditions of life: they have a history therefore. And in these respects they become part of the material for science: and a description of them can be given by psychological and historical methods.

The general nature and results of the application of these methods to ethics are too well known to need further comment, too well established to require defense. But these results may be exaggerated and have been exaggerated. When all has been said and done that the historical method can say and do, the question "What is good?" is found to remain exactly where it was. We may have learned much as to the way in which certain kinds of conduct in certain circumstances promote certain ends, and as to the gradual changes which men's ideas about good and evil, virtue and vice, have passed through; but we have not touched the fundamental question which ethics has to face — the question of the nature of worth or goodness or duty.

And yet it is this question only which gives significance to the problems on which historical evolution has been able to throw light. Moral ideas and moral institutions have all along been effective factors in human development, as well as the subject of development themselves. And the secret of their power has lain in this that men have believed in those ideas as expressing a moral imperative or a moral end, and that they have looked upon moral institutions as embodiments of something which has worth for man or a moral claim upon his devotion. These ideas and institutions would have had no power apart from this belief in their validity.

But was this belief true? Were the ideas or institutions valid? This question the man of science, as sociologist or historian, does not

answer and has no means of answering. He can show their adaptation or want of adaptation to certain ends, but he can say nothing about the validity of these ends themselves. It is implied in their efficiency that these ends were conceived as having moral value or moral authority. But to what ends does this moral value or authority truly belong? and what is its significance? — these are questions which the positive sciences (such as psychology and sociology) cannot touch and which must be answered by other methods than those which they employ.

The moral concept is expressed in various ways and by a variety of terms, — right, duty, merit, virtue, goodness, worth. And these different terms indicate different aspects opened up by a single new point of view. Thus "right" seems to imply correspondence with a standard or rule, which standard or rule is some moral law or ideal of goodness; and "merit" indicates performance of the right, perhaps in victory over some conflicting desire; and "virtue" means a trait of character in which performance of this sort has become habitual. The term "worth" has conveniences which have led to its having considerable vogue in ethical treatises since the time of Herbart; it lends itself easily to psychological manipulation; but it does not seem to refer to a concept fundamentally distinct from goodness. But between "goodness" and "duty" there seems to be this difference at any rate, that the latter term refers definitely to something to be done by a voluntary agent, whereas, in calling something "good," we may have no thought of action at all, but only see and name a quality.

There lies here therefore a difference which is not a mere difference of expression.

On the one hand it may be held that good is a quality which belongs to certain things and has no special and immediate reference to volition: that we say this or that is good as we say that something else is heavy or green or positively electrified. No relation to human life at all may be implied in the one form of judgment any more than in the other. That relation will only follow by way of application to circumstances. Just as a piece of lead may serve as a letter-weight because it is heavy, so certain actions may come to be our duty because they lead to the realization of something which is objectively good in quality.

According to the other view goodness has reference in its primary meaning to free self-conscious agency. The good is that which ought to be brought into existence: goodness is a quality of things, but only in a derivative regard because these things are produced by a good will. It is objective, too, inasmuch as it unites the individual will with a law or ideal which has a claim upon the will; but it does not in its primary meaning indicate something out of relation to the

will: if there were no will there would be no law; apart from conscious agency good and evil would disappear.

The question thus raised is one of real and fundamental importance. "Ethics" by its very name may seem to have primary reference to conduct; and that is the view which most moralists have, in one way or another, adopted. But the other view which gives to the concept "good" an independence of all relation to volition is not always definitely excluded, even by these moralists; by others it has been definitely maintained: it seems implied in Plato's idealism, at one stage of its development; and quite recently a doctrine of the principles of ethics has been worked out which is based on its explicit recognition.[1]

If we would attempt to decide between these two conflicting views of the ethical concept, we must, in the first place, imitate the procedure of science and examine the facts on which the concept is based. To get to the meaning of such scientific concepts as "mass," "energy," or the like, we begin by a consideration of the facts which the concepts are introduced to describe. These facts are in the last resort the objects of sense perception. No examination of these sense percepts will, as we have seen, yield the content of the ethical concept; good and evil are not given in sense perception — they are themselves an estimate of, or way of regarding, the immediate material of experience. Moral experience is thus in a manner reflex, as so many of the English moralists have called it. Its attitude to things is not merely receptive; and the concepts to which it gives rise have not mere understanding in view. Objects are perceived as they occur; and experience of them is the groundwork of science. There is also, at the same time, an attitude of approbation or disapprobation; this attitude is the special characteristic of moral experience; and from moral experience the ethical concept is formed.

This reflex experience, or reflex attitude to experience, is exhibited in different ways. There is, to begin with, the appreciation of beauty in its various kinds and degrees and the corresponding depreciation of ugliness or deformity. These give rise to the concepts and judgments of æsthetics. They are closely related to moral approbation and disapprobation, so closely that there has always been a tendency amongst a school of moralists to strain the facts by identifying them. A certain looseness in our use of terms favors this tendency. For we do often use good of a work of art or even scene in nature when we mean beautiful. But if we reflect on and compare our mental attitudes in commending, say, a sunset and self-sacrifice, it seems to me that there can be no doubt that the two attitudes are different. Both objects may be admired; but both are not, in the same sense, approved. It is hard to express this difference otherwise than by

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[1] *Principia Ethica*, by G. E. Moore (1903).

saying that the moral attitude is present in the one and absent in the other. But the difference is brought out by the fact that our æsthetical and moral attitudes towards the same experience may diverge from one another. We may admire the beauty of that which we condemn as immoral. De Quincey saw a fine art in certain cases of murder; the finish and perfection of wickedness may often stir a certain artistic admiration, especially if we lull the moral sense to sleep. And, on the other hand, moral approval is often tempered by a certain æsthetic depreciation of those noble characters who do good awkwardly, without the ease and grace of a gentleman. John Knox and Mary Queen of Scots (if I may assume for the moment an historical judgment which may need qualification) will each have his or her admirers according as the moral or æsthetic attitude preponderates — the harsh tones of the one appealing to the law of truth and goodness, the other an embodiment of the beauty and gaiety of life, "without a moral sense, however feeble."

Nor is æsthetic appreciation the only other reflex attitude which has a place in our experience side by side with the moral. Judgments about matters of fact and relations of ideas are discriminated as true or false; an ideal of truth is formed; and conditions of its realization are laid down. Here again we have a concept and class of judgments analogous to our æsthetical and ethical concepts and judgments, but not the same as them, and not likely to be confused with them.

Beside these may be put a whole class of judgments of worth which may be described as judgments of utility. We estimate and approve or disapprove various facts of experience according to their tendency to promote or interfere with certain ends or objects of desire. That moral judgments are to be identified with a special class of these judgments of utility is a thesis too well known to require discussion here, and too important to admit of discussion in a few words. But it may be pointed out that it is only in a very special and restricted sense of the term "utility" that judgments of utility have ever been identified with moral judgments. The "jimmy" is useful to the burglar, as his instruments are useful to the surgeon; and they are in both cases appreciated by the same kind of reflective judgment. Judgments of utility are all of them, properly speaking, judgments about means to ends; and the ends may and do differ; while it is only by a forced interpretation that all these ends are sometimes and somehow made to resolve themselves into pleasure.

It is enough, however, for my present purpose to recognize the *prima facie* distinction of moral judgments or judgments of goodness from other judgments of worth, such as those of utility, of beauty, and of truth (in the sense in which these last also are judgments of

worth). Had the question of the origin and history of the moral judgment been before us, a great deal more might have been necessary. For our present purpose what has been already said may be sufficient: it was required in order to enable us to approach the consideration of the question already raised concerning the application and meaning of the moral concept.

The question is, Does our moral experience support the assignment of the predicate "good" or "bad" to things regarded as quite independent of volition or consciousness? At first sight it may seem easy to answer the question in the affirmative. We do talk of sunshine and gentle rain and fertile land as good, and of tornadoes and disease and death as bad. But I think that when we do so, in nine cases out of ten, our "good" or "bad" is not a moral good or bad; they are predicates of utility or sometimes æsthetic predicates, not moral predicates; and we recognize this in recognizing their relativity: the fertile land is called good because its fertility makes it useful to man's primary needs; but the barren and rocky mountain may be better in the eyes of the tourist, though the farmer would call it bad land. There is an appreciation, a judgment of worth in the most general sense, in such experiences; but they are in most cases without the special feature of moral approbation or disapprobation.

There remains, however, the tenth case in which the moral predicate does seem to be applied to the unconscious. One may instance J. S. Mill's passionate impeachment of the course of nature, in which "habitual injustice" and "nearly all the things which men are hanged or imprisoned for doing to one another" are spoken of as "nature's every-day performances;"[1] and a similar indictment was brought by Professor Huxley, twenty years after the publication of Mill's essay, against the cosmic process for its encouragement of selfishness and ferocity.[2] These are only examples. Literature is full of similar reflections on the indiscriminate slaughter wrought by the earthquake or the hurricane, and on the sight of the wicked flourishing or of the righteous begging his bread; and these reflections find an echo in the experience of most men.

But the nature of this experience calls for remark.

In the first place, if we look more closely at the arguments of Mill or Huxley, we see that both are cases of criticism of a philosophical theory. Mill was refuting a view which he held (and rightly held) to have influence still on popular thought, though it might have ceased to be a living ethical theory — the doctrine that the standard of right and wrong was to be found in nature; it was in keeping with his purpose, therefore, to speak of the operations of nature as

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[1] J. S. Mill, *Three Essays on Religion*, pp. 35, 38.
[2] T. H. Huxley, *Evolution and Ethics* (Romanes Lecture).

if they were properly the subject of moral praise or blame. In the same way, when Huxley wrote, the old doctrine which Mill regarded as philosophically extinct and only surviving as a popular error had been revived by the impetus which the theory of evolution had given to every branch of study; and Huxley was criticising the evolutionist ethics of Spencer and others who looked for moral guidance to the course of evolution. He, therefore, was led to speak of the cosmic process as a possible subject of moral predicates, not necessarily because he thought that application appropriate, but in order to demonstrate the hollowness of the ethics of evolution by showing that if the moral predicate could be applied at all, then the appropriate adjective would be not "good" but "bad."

Perhaps there is more than this in Huxley; and Mill's expressions often betray a direct and genuine moral condemnation of the methods of nature as methods of wickedness; and, still more clearly, this immediate moral disapproval may be found in expressions of common experience as yet uncolored by philosophy. But if we examine these we find that, while there is no reference to philosophical theories about nature, the things approved or condemned are yet looked upon as implying consciousness. In the lower stages of development this implication is simply animistic; at a later period it becomes theological. But throughout experience moral judgments upon nature are not passed upon mere nature. Its forces are regarded as expressing a purpose or mind; and it is this that is condemned or approved. The primitive man and the child do not merely condemn the misdoings of inanimate objects; they wreak their vengeance upon them or punish them: and this is a consequence of their animistic interpretation of natural forces. Gradually, in the mental growth of the child, this animistic interpretation of things gives place to an understanding of the natural laws of their working; and at the same time and by the same degrees, the child ceases to inflict punishment upon the chair that has fallen on him or to condemn its misdemeanor. Here the moral judgment is displaced by the causal judgment; and the reason of its displacement is the disappearance of mind or purpose from amongst the phenomena. When the child comes to understand that the chair falls by "laws of nature" which are not the expression of will, like the acts done by himself or his companions, he ceases to disapprove or to resent, though he does not cease to feel pain or to improve the circumstances by setting the chair firmly on the floor. The recognition of natural causation as all that there is in the case leaves no room for the moral attitude. So true is this that the same result is sometimes thought to be a consequence of the scientific understanding even of what is called moral causation, "tout comprendre c'est tout pardonner" — as if knowledge of motive and circumstances were sufficient to dispense with praise or blame.

Moral judgments of a more mature kind on the constitution and course of nature form the material for optimistic and pessimistic views of the world — at least, when these views rise above the assertion of a preponderance of pleasure or of pain in life. But, so far as I can see, in such moral judgments nature is never looked upon as consisting of dead mechanical sequences. It is because it is looked upon as the expression of a living will or as in some way — perhaps very vaguely conceived — animated by purpose or consciousness, that we regard it as morally good or evil. Apart from some such theological conception, it does not seem to me that the nature of things calls out the attitude of moral approval or disapproval. Things are estimated as useful for this or that end, they are seen and appreciated as beautiful or the reverse, without any reference to them as due to an inspiring or originating mind; and in one or other of these references the terms "good" or "bad" may be used. But when we use the term good in its specifically moral signification, we do not apply it to the inanimate, except in a derivate way, on account of the relation in which these inanimate things stand to the moral ends and character of conscious beings.

So far, therefore, as the evidence of moral experience goes, it does not support the view that the "good" is a quality which belongs to things out of relation to self-conscious activity. And, in so far, the peculiarity of the moral experience would seem to be better brought out by the conception "ought" than by the conception "good."

But here a difficulty arises at once. For how can we say that anything ought to be done or to be except on the assumption that it is antecedently good? Is not such antecedent and independent goodness necessary in order to justify the assertion that any one ought to produce it?

The question undoubtedly points to a difficulty; and if that difficulty can be solved it may help to bring out the true significance of the moral concept. The judgment which assigns the duty of an individual — according to which I or any one ought to adopt a certain course of action — involves a special application of the moral concept. It binds the individual to a certain objective rule or end. The individual's desires as mere facts of experience may point in an altogether different direction; the purpose or volition contemplated and approved by the moral judgment has in view the union of individual striving with an end which is objective and, as objective, universal. This union involves an adaptation of two things which may fall asunder, and which in every case of evil volition do fall asunder. And the adaptation may be regarded from either side: on the side of the individual, application to his individuality is implied; the duty of one man is not just the same as the duty of any other; he

has his own special place and calling. But he is connected with a larger purpose which in his consciousness becomes both an ideal and a law, while its application is not limited to his individuality or his circumstances.

All this is implied in the moral judgment. It is not limited to one individual consciousness or volition. But it does not follow that the predicate "good," in the ethical meaning of the term, is or can be applied out of relation to consciousness altogether. At the earliest stages of moral development we find it applied unhesitatingly wherever conscious activity is supposed to be present — to anything that is regarded as the embodiment of spirit; and it is applied to the universe as a whole when the universe is thought of as the product of mind. "Good" is not even limited to an actual existent; it neither implies nor denies actual existence. "Such and such, if it existed, would be a good" is as legitimate though not so primitive an expression of the moral judgment as "this existent is good." But it does imply a relation to existence. It does not even seem possible to distinguish except verbally between "good" and "ought to be." And this "ought" seems to imply a reference to a purpose through which the idea is to be realized.

This conception "ought to be" is not the same as the concept "ought to be done by me." The latter is an application of the more general concept to a special individual in special circumstances; and this is the common meaning of the concept duty. The former is the more general concept of "goodness." It may be called objective, because it does not refer to any individual state of mind; it is universal because independent of the judgments and desires of the individual; and when the goodness is not due to its tendency towards some further end, it may also be called absolute.

The point of the whole argument can thus be made clear if we bear in mind the familiar distinction between "good in itself" and "good for me now." That the latter has always a relation to consciousness is obvious: it is something to be done or experienced by me. But there must be some ground why anything is to be or ought to be done or experienced by me at any time. Present individual activity must rest upon or be connected with some wider or objective basis. What is good for me points to and depends upon something which is not merely relatively good, but good in itself or absolutely. Yet it does not follow that this good in itself is necessarily absolute in the sense of having significance apart altogether from consciousness. Its absoluteness consists in independence of individual consciousness or feeling, not in independence of consciousness altogether. It is objective rather than absolute in the literal sense of the term. The good in itself, like the relative good, is one aspect which can only belong to a consciousness — to purpose. The moral judgment on

things — either on the universe as a whole, or on anything in the universe which is not regarded as due to the will of man — is only justified if we regard these things as in some way expressing consciousness; either as directly due to it, or as aiding it, or as in conflict with it. From any other point of view, to speak of things as good or evil (unless in some non-ethical sense of these terms) seems out of place, and is unsupported by the mode of application which belongs to the immediate judgments of the moral consciousness. If the moral concept has significance beyond the range of the feelings and desires of men, it is because the objects to which it applies are the expression of mind.

This is not put forward as a vindication of a spiritual idealism. It is only a small contribution towards the meaning of "good." A comprehensive idealism may not be the only view of reality with which the conclusions reached so far will harmonize. But it is the view with which they harmonize most simply. The conception of a purpose to which all the events of the world are related is a form in which the essential feature of idealism may be expressed; the view of this purpose as good makes the idealism at the same time a moral interpretation of reality, and allows of our classing each distinguishable event as good or evil according as it tends to the furtherance or hindrance of that purpose.

This doctrine of the significance and application of the ethical concept would enable us to reach a definite view of the nature of ethics and of the way in which it is related to the sciences and to metaphysics. The ethical concept is based upon the primary facts of the moral consciousness, just as scientific concepts have as their basis the facts of direct experience. The primary facts of the moral consciousness are themselves of the nature of judgment — they are approbations or disapprobations. But all facts of experience involve judgments, though these judgments may be only of the form "it is here" or "it is of this or that nature." Again, the primary ethical facts or judgments cannot be assumed to be of unquestionable validity: we may approve what is not worthy of approval, or disapprove what ought to have been approved. Our moral judgments claim validity; and their claim is of the nature of an assertion, not that one simply feels in such and such a way, but that something ought or ought not to be. They imply an objective standard. But the objective standard, when more clearly understood, may modify or even reverse them. Our primary ethical judgments — all our ethical judgments, indeed — stand in need of revision and criticism; and they receive this revision and criticism in the course of the elaboration of the ethical concept and of its application to the worlds of fact and possibility. In the same way it may be contended that the direct judgments of experience upon which science is based

need criticism and correction; though their variation may be less in amount than the variation of moral judgments. The color-blind man identifies red with green, and his judgment on this point has to be reversed; the hypersensitive subject often confuses images with percepts; exact observation needs a highly trained capacity. The correction and criticism which is needed come from objective standards; and these are the result of the comparison of many experiences and the work of many minds.

It is no otherwise in the case of ethics. Criticism brings to light inconsistencies in the primary judgments of approbation and disapprobation as well as in the later developments of the moral judgment. And these inconsistencies must be dealt with in a way similar to that in which we deal with inconsistencies in the judgments of perception and of science. The objective standard is not itself given once for all; it has to be formed by accumulation and comparison of moral experiences. Like the experiences on which science is based, these have to be made as far as possible harmonious, and analysis has to be employed to bring out the element of identity which often lurks behind apparent contradiction. They have also to be made as comprehensive as possible, so that they may be capable of application to all relevant facts, and that the scattered details of the moral consciousness may be welded into an harmonious system. In these general respects the criticism of ethical concepts proceeds upon the same lines as the criticism of scientific concepts. The difference lies in the concepts themselves, for ethics involves a point of view to which science must always remain a stranger.

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The relations of ethics are discussed in almost every ethical treatise; special reference may be made to the writers who have worked out the theory of worth or value, especially von Ehrenfels, *System der Wert-theorie* (1897); Meinong, *Psychologisch-Ethische Untersuchungen zur Werth-theorie* (1894), and an article in *Archiv für Syst. Phil.* 1895; Krueger, *Begriff der Absolut Wertvollen* (1898); also to articles by Standinger and by Natorp in *Archiv für Syst. Phil.* (1896); by Wentscher, *Archiv für Syst. Phil.* 1899; by Westermarck, *Mind*, 1900; and by Belot, *Revue de Métaphysique et de Morale*, 1905. — W. R. S.

PROBLEMS OF ETHICS

BY PAUL HENSEL

(*Translated from the German by Professor J. H. Woods, Harvard University*)

[**Paul Hensel,** Professor of Systematic Philosophy, University of Erlangen, since 1902. b. May 17, 1860, Great Barten, East Prussia. Ph.D. Freiburg, Baden, 1885. Privat-docent, Strassburg, 1888–95; Special Professor, Strassburg, 1895–98. **Author of** *The Ethical Basis and Ethical Transactions; Carlyle; The Principal Problems of Ethics.*]

SINCE the appearance of the three chief works of Kant a certain rhythm in the treatment of philosophical problems, first of all in Germany, but also, in less degree, in other civilized countries, is unmistakable. After an intense occupation with theoretical problems a flood of ethical discussion usually follows; and this then is usually resolved into a renewed revision of æsthetical problems. If I am not deceived, we are now at the period of transition from the second to the third epoch; so much the more favorable is the time to review the present condition of ethical problems. In the first place, then, it seems rather remarkable that recent ethical discussion, so intensely carried on, has resulted in a definite victory for neither one school nor the other. One thing alone, however, may with some accuracy be said, that the school of utilitarianism of the older interpretation by Bentham, which earlier prevailed almost alone in England with a fairly strong representation in France and Germany, seems to be withdrawn from the field. Not as if there were no men to-day who in other times would have sworn by Bentham's flag, rather we are here facing a fact that a theory which formerly appeared in independence, now may be deemed a special case of a more inclusive theory, which with the help of its wider horizon can remove a whole series of difficulties, which apparently raised insolvable problems for the special theory. Utilitarianism, since it had started with the examination of the individual, could not, even in the master-hand of Bentham, transfer itself without remainder into the greatest happiness of the greatest number; the interest paid on the sacrifice offered to fellow men, again and again seemed dubitable and probable; again and again the best calculation seemed to consist in egoism pure and simple. The impossibility of an exact calculation of consequences in pleasure and in pain was likewise repeatedly emphasized by opponents; the suggestion that we do not count the shrewd calculator so good as the man who acts impulsively was also not lacking: all these were difficulties, which, on the ground of the older utilitarianism, could be evaded but not quite entirely put out of the world.

It is then easily understood that the further combinations into which evolution was able to advance ethical questions have resulted in the cessation of utilitarianism as an independent system. Around the huge system of thought of Herbert Spencer one of the great camps of ethical workers is collected. It is not correct to count Herbert Spencer as systematizer of Darwin's thoughts; his main thoughts were finished, before a line of Darwin had appeared. But it is correct that the wonderful inductions of Darwin were precisely that which Spencer's system needed in order to begin its triumphal march through the civilized world. Here the case is the reverse of that of Copernicus and Giordano Bruno: the systematizer precedes the man of special research. It is superfluous on American soil to give a description of Spencer's thoughts; they have become parts of the general consciousness. So it may suffice to emphasize a few characteristic features, to which my remarks shall be attached, since, otherwise, in view of the richness of the system, there might easily be other sides of it in the mind of my hearers than those to which I have here to attach importance.

The characteristic feature of the system of Spencer is its unity and compactness. Just as every picture has a definite point from which it should be seen, so also the system of Spencer is a view of the world from a quite definite point of view, — that of evolution. Systems of evolution had already occurred in philosophy, — I mention the vast performance of Hegel only, — but that which gives Spencer's system its characteristic significance is that here evolution is conceived not as logical, but as biological; while in the case of Hegel nature is the vestibule of the realm of purpose, and therein alone has its significance, Spencer takes nature as his point of departure, and the realm of human activity represents itself to him merely as the finest conformation of natural events. Here the whole evolution from the nebula in world-space to the most delicate relations between man and man are comprehended in one grand conception. The same amount of force which then existed in world-space exists still to-day, only in infinitely more differentiated form. The new which is produced is nothing else than the transformed old, but transformed in an essential relation, in the direction towards constantly increasing complexity of relations in which single things and centres of force stand to each other.

If it be asked what this principle is which is the ground for this differentiation, a glance at the behavior of organisms informs us. In them we can most clearly recognize effects which result, with the necessity of laws of nature, from increasing differentiation. The undifferentiated individual is powerless in the presence of every change of his environment. Banished to its accidental place, the plant must wait for what happens to it. Only within a narrow limit

can it maintain its existence. Better equipped we find the animal, especially when it has gathered into social groups, either for protection against carnivora or for the breeding of progeny in common. The young steer has an infinitely better prospect to maintain itself, to grow up, than the single egg in the spawn of the sturgeon.

So it is, before all else, the fact of social combination which attracts to itself the attention of the revolutionary ethicist. His ethics is social ethics. The analysis of the historical development of mankind forms the standard, in which the social combinations have resulted, and in which greater and world-inclusive formations have replaced those earlier, smaller, and smallest, usually engaged in war with each other. It is a long way from the time when *hospes* was equivalent to *hostis* to international expositions, and the single stages of this way reflect themselves in the moral behavior of the individuals. The old question, which in so many ways agitated the English ethics of the seventeenth and eighteenth centuries, the question, whether man should be regarded as an originally egoistic being, or whether equally original, benevolent instincts must be ascribed to him, is transferred by evolutionists of to-day beyond the realm of man to that of his animal ancestors and, in this case, in favor of the originality of egoism. But long before man appeared as an independent species the effects of the life of the horde must have shown themselves in him, since those communities only in which the single members were bound to each other by sympathy had any prospect of survival. It is therefore possible to speak of animal ethics. The interesting attempts which Darwin had made in this field were taken by Spencer, as a whole, into his system. It must, however, be conceded that we must observe the full development of this process, first of all, in man, and the tendency then consists in a constant decrease of egoistic, as compared with altruistic, actions. How it was possible that the individual was ever willing to renounce the amounts of pleasure, which he could obtain, in favor of others, Spencer skillfully tried to explain by the introduction of the egoistic-altruistic feelings. These give the impulse to actions which are useful to the community, but which give to the doer honor and distinction, and thus, from egoistic motives, make actions which promote the welfare of the community commendable. But those actions which damage the community are visited with punishment of all kinds. The theory of sanctions in Bentham and Mill here passes over into the more extensive system of evolution. For modern theory of evolution, by the broader biological foundation of its system, succeeds in explaining why even, in the case of those who cannot overlook the consequences of such actions as are injurious to their own person, these consequences are still ignored. The fact of the conscience, for the consistent Benthamite a negligible quantity, forms

the keystone of Spencer's ethics, and affords the chance of making
the theory of heredity applicable in a new field of ethical speculation.

It is, as a matter of fact, impossible for the single individual to
calculate, by Bentham's receipt, all the consequences of pleasure or
of pain which result from the actions for his own welfare. The
individual need not, however, undertake this calculation at all. He
does not begin at the beginning of making his experiences in this
world; he enjoys the heaped-up treasure of experiences which, before
him, long-forgotten generations of ancestors had made; and the
sum of these experiences he calls his conscience. This voice of the
conscience restrains the individual from anti-social actions, which,
in accordance with experience, must lead to an injury to his own
person; in accordance, of course, with the experience not of single
ancestors but of the whole line. Here, again, a selective process in the
struggle for existence is being completed. Men with no conscience at
all or with an only imperfectly developed conscience have to contend
with disadvantages similar to those in whom the corporal adjustment
to the modern conditions of civilization have proved defective; they
are exterminated by seclusion in prison or by execution, as the others
by diseases which their bodies cannot resist. The criminal of to-day
might perhaps have been, in primitive times, a respected member of
his horde, perhaps, even a great chief. To-day he can be regarded
only as an atavistic survivor, who fits into our conditions as little
as a living ichthyosaurus into this lecture-hall. Again, it is to be
hoped, it is even definitely to be predicted, that many who to-day
are quite irreproachable in moral respects, in later times will no
longer succeed in satisfying the requirements in the form of their
grandson or great-grandson. For the progress is a biological necessity;
and he who cannot attach himself to its ways is submerged.

It is small disparagement for this vast construction of the connec-
tion between the moral life of the individual and the total evolution
of the associations of men, of organisms, of the whole, that, now espe-
cially in English ethics, a bitter strife has broken forth, which we may
regard as the one-sided elaboration of the individualistic parts of
Spencer's ethics on the one side, of the social on the other side.
While the orthodox disciples of Spencer insist that such progress
only can be kept in aim which must assure to the individual, to the
fit the most unrestricted possible amount of free movement, while
the whole rigor of the process of selection must fall upon the unad-
justed and the unfit, the socialist tendencies of our time tend to
advocate a reversal of this harsh result and to advocate both the
united struggle of human society, by suppressing over-energetic
individuals, and the preservation of the economically weak. Though
it would be interesting to trace this division to its final grounds, I
must limit myself to note the fact that the socialist movement

seems here also to be in advance, — at least, so far as European movements of thought are concerned; and that they are in the condition to compensate for their departure from the teachings of the master by an appeal to the main thoughts of his system, concerns me just here. Doubtless socialistic thought is on the whole in advance when compared with liberal and individualistic thought. And, under these circumstances, the inference for every disciple of Darwin's theory of evolution is simple; that here again is a case of survival of the fittest; that socialistic ideals represent a higher form of adjustment; that just by the fact of their victory the necessity and justification of this victory is placed beyond doubt. It helped little that the venerable thinker himself in the last years of his rich and active life descended into the arena of the contest and warned his beloved England against the dangers of this socialistic tendency. It was inconsistent that he tried to brand these thoughts as a retrograde movement, as a step backward, since his own system with its powerful optimism affords no possibility for victorious retrograde movements. Even imperfection and evil has for Spencer only the significance of an imperfect progress; and the thought that imperfection could even win the victory over the perfect, that must be warned against it, could only be nonsense in connection with his system. For him, as for Hegel, the final formula, obtained it is true by a very different way, is the thesis: The actual is rational.

But just this reference to Hegel's system makes clear to us the opposition which Herbert Spencer's system found in Germany, first of all, but also in wide circles in England and in America. If it could be objected against Hegel that the activity of the individual, in contrast to the might of the developing process of the logical idea, is reduced to insignificance, this consideration returns with doubled force in contrast to the concept of the thought of development, which is found in the modern theory of evolution of Spencer. For here it is not teleological necessities which prevail, but causal. To have proved evolution by the laws of nature is precisely his system's title to fame. The question must then be raised whether an obligation to any definite practical action can be deduced from the proof of the necessity of any event. If the development is necessary, it will be completed whether I coöperate with it or not. If it needs my coöperation, it need not be regarded as a law of nature. It is exactly the same difficulty which beset the Stoics, when they tried to harmonize the determinism of world events with the demands which their ethics put upon the moral resolves of the individual. It is absurd to will any necessary event of the laws of nature; I can suspend my action so that I count upon the occurrence of such an incident, but I cannot make this incident the object of my will. I can decide that

I will observe an eclipse of the moon, but I cannot will the occurrence of this eclipse of the moon, or not will it.

If we reduce the difficulty to the simplest formula, it would be as follows: the theory of evolution did not distinguish between two completely different kinds of attitudes on the part of human mental activity; between the knowledge of the necessity of what exists and its judgment by standards of value. But it is precisely with the latter that ethics has to do. It is, like logic and æsthetics, a science of values; the interest in the question how something has come to be, is quite different from the interest in determining its value. Everything has come to be, the valueless as well as the valued, with the same necessity; that is a self-evident presupposition of all explanatory science. The bungling drawing of a school-boy and the Sistine Madonna, the hallucinations of a lunatic and the thought of a Herbert Spencer, a demonic crime and a deed of the purest ethical fulfillment of duty, are, in the same sense, necessary; but with the knowledge of this necessity we have not come a single step nearer to the task of their valuation.

The difference between these two kinds of attitudes has perhaps never been more clearly sketched than in Fichte's book *On the Calling of Man.* If we assume that I have a fully adequate scientific knowledge of the course of nature, I might discern that this grain of sand which the storm has set in motion could not drift a hair's breadth farther, unless the whole previous course of nature had been quite different; what then would be gained for my own moral action? The answer must be: Nothing. More than that, if this point of view were the only possible for man, then this action would have no longer, as a moral action, any significance, and could have none; since as a part of the world event alike in value to all other parts it would remain like in value, and it would be meaningless to select and emphasize out of this continuum of facts and environments, alike in value, single elements as especially valuable and significant. The man who could not resign himself to this knowledge, who could not be satisfied to continue, in cool content, at the point of view of the silent contemplation of causes, must fall into conflicts similar to those which Carlyle so vividly described in *Sartor Resartus.* We must then, in order to an understanding for this new problem, provisionally disregard, above all else, whatever the theory of evolution has accomplished by way of scientific explanation, and reserve for a later investigation the ethical valuation of this sequence of development. The question which is now to occupy us is directed, first of all, to the subject of our moral valuation. What do we call good or bad?

This is the main question of all normative ethics in general, and its answer by Kant will always remain a brilliant feat in this field. He

proved, in the first place, that this predicate can be properly applied to no action whatever, that we can speak of a good action in figurative language only, when we believe that we can make from this action an inference with regard to something else, — the disposition of the actor; and that the same action which we do not hesitate to describe as good, on the supposition of the correctness of this inference, loses directly this character as soon as doubt of the correctness of the inference arises. This disposition, which we distinguish in this way, which forms the substrate of our moral valuation, we call the good will, and the Magna Charta of the Kantian ethics consists in the celebrated thesis: Nothing can possibly be good except a good will. This reasoning appears to be as self-evident as its result is important.

The whole ethical process is removed within the soul. While the theory of evolution and, still more, utilitarianism could still hope to obtain, with the character of the work, at the same time an expression with regard to the ethical value of the action; while, in this combination of ideas, the ethical goodness of the disposition could be judged by the usefulness or value to civilization of the performance done, so that both these systems would have essentially the character of an ethics of results, we have in Kant and his successors, most decidedly, an ethics of dispositions. It has rightly been pointed out that this ethics could grow only upon Protestant soil, that here the same contradiction prevails which Luther once summed up in the words: "Good works do not make the good man, but the good man creates good works." All the excellences, but all the weaknesses also, of Protestantism, cling to Kant's ethics.

First, let us follow the further stages of Kant's thought. How must a good will be constituted, so that we may count it as ethically good? All our acts happen in order to fulfill a purpose. The character of the action depends upon the character of the purpose, which the actor proposes for himself, which he affirms with his will, which he makes his own. But if the purpose be no longer willed, then all the actions cease, which hitherto had had to be accomplished for its fulfillment. All those purposes, which under the circumstances cannot be willed, cannot therefore produce that lasting constitution of the will which we understand under the term the good will. But among the different motivations of the will, there are some which for the observer become separated. They have not a character such that they could, under any circumstances, cease to motivate the will; they are necessary and universal determinations of the will. The imperative which they contain and with which they demand action has not the hypothetical form: "If thou wilt obtain this or that, you must;" but the absolute: "Thou shalt." It is a categorical imperative, to which the will is here subordinated, which determines

my actions; and such a categorical imperative we term duty. Only
the dutiful will is good. It is clear that this determination shows
an exact analogy to the other norms of judgment in the logical and
the æsthetical field. The principle of contradiction states nothing
at all with regard to the single thoughts, it only asserts that our think-
ing can then alone make a claim upon a logical valuation while it
fills the condition which the principle of contradiction states. Like-
wise, the impulses of our wills can be morally valued only when they
refer to an absolute "Thou shalt;" if this is not the case, they are
excluded from the range of valuation, just as the play of our fancy,
which does not recognize the principle of contradiction, is excluded
from the realm of the norm of scientific thinking.

Here again the normal action of ethics is represented as a selective
process. While the evolutionist ethicist can estimate every single
content of human consciousness with reference to the point whether
it is preservative of the species or not, and thus give it ethical value,
the realm of the Kantian ethics is much more confined. Only those
impulses of the will occur with conscious subordination under the
command of duty, or in conscious opposition to it fall within the
realm of moral valuation. All others — and their name is legion —
must be termed unmoral. Not as if they become thereby actually
valueless; they may stand as high as you please in the intellectual,
æsthetic, or religious scale of values. But to bring them under just
the moral norms of judgment would be an attempt at an unappli-
able object. This is the point, perhaps, where the Kantian ethics
gives the hardest shock to the healthy human understanding. It
will always seem a paradox that we have a moral act when a man
with strong desires for theft, after a severe inner struggle, does not
put a silver spoon into his pocket, while the man who omits all this
quite as a matter of course may have no claim upon moral desert.
And yet each one of us would feel it as an insult, if he should be
praised for such omission. The solution of this difficulty lies in the
distinction of the value of the single resolve and that of the whole
moral personality. The man who is still led into temptation by silver
spoons stands morally upon the same plane upon which the scholar
stands who struggles with extreme mental effort to calculate a simple
example in multiplication. In the case of the more advanced person
our moral approval is not aroused because he no longer needs, in
this simple case, to appeal to the law of duty, but because we be-
lieve that we may conclude that his moral personality is attacking
other more difficult problems with full force, and that he is here in
himself feeling the full weight of the contest. If we were deceived
in this, if it prove true that he, content with what had been attained,
had withdrawn to the position of the ethical capitalist, our ethical
interest in him would likewise cease, just as our intellectual interest

ceases in the scholar for whom there are no more problems in his science. From this point of view the result is necessary that the category of duties, to speak with Hegel, is absolutely infinite; and in this perhaps lies the considerable difference between modern and ancient ethics. For ancient ethics the ideal of the wise man was a distinctly finitely determined amount. However difficult it might be to fulfill the conditions for it, it could still be fulfilled in a human life; and a further advance beyond this fulfilled ideal would have been to the Greeks an absurdity: it is the "nothing too much" transferred to the ethical point of view. It is otherwise in modern ethics, and with this is connected the change in that the concept of the infinite has become a concept of value. It is as Carlyle says: "Fulfill the next duty which presents itself to thee, and when thou hast fulfilled it, wait for ten, twenty, a hundred to be fulfilled." But we recognize the degree of ethical development which a man has attained by noting that it is no longer duty to him.

If the limits of the moral valuation have been much restricted by the introduction of the concept of unmoral actions, it has been extended in the other direction by the insight that now every action which happens in fulfillment of a command of duty is to be valued as the result of a moral disposition. We come thus to the problem which, since the time of the ancient sophists, has not ceased to occupy minds, and which may most simply be termed the anthropological problem. What in the world is there that is not by individuals and by people deemed to be moral! With what strange contents the formal "Thou shalt" of morality is filled! In face of these contradictions, is there any sense at all in speaking of ethical commands? All skeptical attacks upon ethics find in such considerations their strongest support; and here again the answer is easy when we reflect upon the analogy with science, art, and religion. Aristotle and Democritus, Hegel and Hobbes, have taught very differently, and yet all have been busy with science. Raphael and Menzel are surely to be valued as artists; Mahomet and Buddha were both religious geniuses of the first magnitude. Why should it be different in the field of ethics? What other men have held to be moral, how they have acted, this can be valuable to me, in order for me to become clear with regard to my own moral determination, just as the artist sees the works of other masters, just as the scientific man must know the theorems of others. But all this cannot be the standard for the formation of my own life. I am, once for all, placed in this world, to be active there; I am responsible to myself for what I wish to accomplish with this life. And so it can, it is true, be an encouragement to me that other men have felt in themselves the same motive to moral activity; I can give them my hand as striving for the same with me through the separating centuries and across the estranging seas. But their way

of solving the great problems of life cannot be the standard for me save in the sense that I receive them into my will, recognize them as valid for my own life.

So, then, the whole weight of the distinction, the whole moral process, is transferred to the individual. He is the point of departure and the goal of the struggle for a content in life. Is this now egoism? This much-discussed question also suffers, as I believe, by a defect in the statement of the problem. If it is intended that that action is meant by egoism, the motive for which is one's own welfare or happiness, by altruism, however, the action which aims at the happiness of others, it is quite clear that these two contrasts have as little meaning for the ethics of disposition as the complementary contrast of beautiful and ugly. Moral action is completely indifferent with regard to these contrasts. Moral actions can be characterized as altruistic as well as egoistic, and the same is the case for unmoral or bad actions. By knowing that distinct advantages have resulted to the doer from an action, or that "the greatest happiness of the greatest number" has resulted from it, I have not gained one step for the moral valuation of this action. I should surely act immorally if I omitted an action acknowledged as moral by me because it would involve pain for others and thus would have an anti-altruistic character. Whence this confusion of the altruistic with the moral arose is easy to see. Long before the child could himself act morally, it must be accustomed to feel that its beloved self cannot be the sole standard for its action; and to the end that it keep peace and content with its brothers and playmates, it is properly accustomed to regard in its action the welfare of the human beings about it. That is a preparatory step to moral action; but, strictly speaking, it can be counted as moral by those only who are determined not to recognize the limits between psychological motivation and normative determination.

It would be an interesting task to trace the relations into which the autonomous moral individual enters with the great moral institutions which dominate the community and have combined in usage, society, and state, and which Hegel described in a happy expression as "objective morality." Here it is no longer the regard for the weal or woe of fellow men which strives to gain influence over my action; here the ethical will of past generations of my own ancestors accosts and asks me whether I can bring my action into harmony with that which they willed and for which they strove. It is a slight disadvantage to the ethically directed man that, in order to protect these moral institutions from injury, an arsenal of punishments, of social influences, of boycotts, and of whatever finer or coarser means of compulsion there may be, are set up. This arsenal is necessary to sustain the social structure which alone

affords the chance for moral action; and he who calculates with pleasure and pain, who tries to arrange his life as happily as possible, will be restrained by shrewd calculation from injuring the prevailing moral institutions. The moral man has nothing to do with such considerations. When he affirms the objective morality, he does so because he recognizes his moral will as identical with that of previous generations which have made these forms. But the time can come when he discovers that a moral life within these forms is no longer possible for him, when with deep regret he sees the bond of continuity break which knit him in affection with the past, when he must resolve to enter new untrodden paths, just as Copernicus was forced to resolve to substitute a new knowledge for those which had satisfied centuries. Such a man will endure calmly and patiently the consequences which result from such a course; he will not expect to be justified, through the purity of his intentions, in the eyes of his fellows, if he undertakes to lay hands on the institutions which the moral consciousness of his contemporaries recognizes as valid. But he will also know that these same institutions owe what sacredness they possess to the blood of previous martyrs, that these shadows of a past can only then speak to a living generation when they have tasted the sacred blood of sacrifice.

So then we see two great movements in our time struggling about the ethical questions. The one has on its side the whole apparatus of scientific conceptions, the presupposition of necessary events without exceptions, the knowledge that the single individual is an infinitely small element in a necessary sequence of development. It can explain everything, deduce everything from its conditions. At one point only its power breaks down: it cannot make the individual comprehend why he should raise a finger to keep in motion this machine which goes of itself.

And, opposed to this, is the other movement, which rests upon the one fact that the point of view of its opponent, the scientific, is also a relation of reality to values, and that man alone introduces these values into reality, measures and tests it by these values. According to this movement, every new human life has the question put to it, what it can accomplish with these values, whether it is capable of making something out of reality, out of itself, which has in itself a value such as to raise it above the flux of appearances as the bearer of these values. Everything previous as well as everything subsequent vanishes before these thoughts that it is now day, that the night is soon coming when no man can work, that at the day's end the day's work must be done. But what each recognizes as his day's work, he must himself find within himself. This decision is his destiny.

I cannot better close than with the words of the man whose life had little joy, but who grappled with these questions in the solitude

of Craigenputtock, in the supreme solitude of the human wilderness of London, with a seriousness which still to-day proves to be soul-wooing and soul-winning: "Centuries have passed that thou mightest be born, and centuries are waiting in dumb expectation of what thou wilt accomplish with this life, now that it has begun." And what this life can offer Carlyle, by combining the thoughts of Fichte and of Goethe, has united in the call:

"Work and despair not."

SECTION F — ÆSTHETICS

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THE RELATION OF ÆSTHETICS TO PSYCHOLOGY AND PHILOSOPHY

BY HENRY RUTGERS MARSHALL

[**Henry Rutgers Marshall,** Practicing Architect, President of the New York
Chapter, American Institute of Architects, Member of Art Commission,
City of New York. **b.** July 22, 1852, New York City. B.A. Columbia University, 1873; M.A. *ibid.* 1875; L.H.D. Rutgers College, 1903. Member
American Psychological Association, Society of American Naturalists, Fellow American Institute of Architects, Honorary Member National Society of
Mural Painters, Member American Philosophical Association. **Author of**
Pain, Pleasure, and Æsthetics; Æsthetic Principles; Instinct and Reason.]

IF conventional divisions of time are to serve as means by which we
may mark the movement of thought as it develops, we may well
say that the nineteenth century saw a real awakening in relation
to æsthetics among those who concern themselves with accurate
thinking, — a coming to consciousness, as it were, of the importance
to the philosophy of life of the existence of beauty in the world, and
of the sense of beauty in man.

And with this awakening came a marked breadth of inquiry; an
attempt to throw the light given by psychological analysis upon the
broad field of æsthetics, and an effort to grasp the relations within
the realm in which beauty holds sway to philosophy as a whole.

That the questions thus presented to us have been answered, I
imagine few, if any, would claim; rather may we say that the nineteenth century set the problems which it concerns the æsthetician
of the twentieth century to solve; and this without underestimating
the value of the work of the masters in æsthetics who lived and
wrote in the century so lately closed, some of whom are fortunately
with us still.

Of these present problems M. Dessoir will treat in his address to
follow mine; in the regretted absence of Professor Lipps the privilege
has been granted to me to consider with you briefly the relations
of æsthetics to psychology, and to philosophy, which must in the

end determine the nature of the problems to be studied by the æsthetician, and the import of the solutions of these problems which they present for our consideration.

I. *The Relation of Æsthetics to Psychology*

We live in what may well be called the era of psychological development, an era marked by the recognition of the truth that no philosophical view of life can be adequate which does not take full account of the experience of the individual human spirit which interprets this life. And so quite naturally for ourselves, and in all probability quite in accord with the habit of thought of the immediate future, we begin our study by the consideration of the relation of æsthetics to psychology.

In turning for light to psychology, the æsthetician finds himself of course asking what is the nature of the states of mind related to his inquiry; and here at once he finds himself confronted with a distinction which must be made if a correct æsthetic doctrine is to become established. He notes that there is a sharp difference between (1) the mental attitude of an artist who produces works of beauty; and (2) the mental attitude of a man at the moment when he appreciates beauty in his experience.[1] The failure to note this distinction has in my view led to much confusion of thought among the æstheticians of the past, and to the defense of dogmas which otherwise would not have been maintained.

That this distinction is an important one becomes clear in the fact that the sense of beauty is aroused in us by objects in nature which bear no relation to what men call fine art. The mental state of the appreciator of beauty has therefore a breadth which does not belong to the mental state which accompanies, or leads to, the production of works of beauty by the artist.

And yet it should not surprise us that this distinction has so often been overlooked; for the theorists first follow the trend of thought of the uncritical man, and this uncritical man does not naturally make the distinction referred to.

For, on the one hand, even the least talented of men has some little tendency to give part of his strength to artistic creation in one form or another; the creative artist is guided by what is a truly racial instinct, which under favorable conditions will appear in any man who is not defective: each of us thus in the appreciation of beauty throws himself to some degree into the attitude of the creative artist.

And, on the other hand, the artist, when not in creative mood, falls back into the ranks of men who keenly appreciate beauty but who

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[1] Cf. my *Æsthetic Principles*, chap. I, "The Observer's Standpoint," and chap. III, "The Artist's Standpoint."

are not productive artists; he thus alternately creates and appreciates, and with difficulty separates his diverse moods.

We may well consider these two distinguishable mental attitudes separately.

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In asking what is the nature of the experience which we call the sense of beauty, we are stating what may well be held to be the most important problem in æsthetics that is presented to the psychologist.

Man is practical before he deals with theory, and his first theoretical questionings are aroused by practical demands in connection with his failures to reach the goal toward which he strives. The development of modern æsthetic theory has in the main quite naïvely followed this course, and we may properly consider first the psychological inquiries which seem to have the most direct bearing upon practical questions.

The artist asks why his efforts so often fail, and he is led to inquire what are the qualities in his work which he so often misses, but now and again gains with the resulting attainment of beauty.

It is thus that we naturally find the æsthetician appealing to the psychologist, asking him what special types of impression yield beauty, what special characteristics of our mental states involve the fullest æsthetic experience.

The psychologist is naturally first led to consider certain striking relations found within the beautiful object which impresses us, and to inquire into the nature of the psychic functioning which is involved with the impressions thus given. He thus comes to consider the relations of the lineal parts of pleasing plane-surface figures; and the study of these relations has given to us such investigations as the notable ones of Fechner in respect to the "Golden Section," which have been supplemented by the more rigid tests of Dr. Witmer and Doctors Haines and Davies in our own day. In similar manner the basis of the beauty found in symmetry and in order, and the problems related to rhythm, have been closely studied, especially in late years by Lipps; and the fundamental principles of tonal relation, and of melodic succession, by Helmholtz, Stumpf, and later writers.

But all these studies of the striking characteristics found in the object are for the psychologist necessarily involved with the study of the distinctly subjective accompaniments in the sense of beauty aroused by the objective forms thus brought to our attention, and he is led to dwell upon the active part the mind takes in connection with æsthetic appreciation. We see this tendency in Berenson's emphasis, and perhaps on the whole over-emphasis, of the importance of the interpretation of works of art, in the group of what I would call the arts of sight, in terms of the tactile sensibilities. But

we see it much more markedly in the important studies of Lipps, who shows us how far our appreciation of beauty in nature, and in artistic products, is due to the sympathetic introjection of ourselves as it were into the object, — to what he calls *Einfühlung.*

But, broad as he shows the applicability of this principle to be, it is clear that we have not in it the solution of the fundamental æsthetic problem with which the psychologist must deal when appealed to by the æsthetician. For no one would claim that all of this sympathetic introjection — this *Einfühlung* — is æsthetic: the æsthetic *Einfühlung* is of a special type. Nor to my mind does it seem clearly shown that there are no sources of beauty which do not involve this introjection, as would be the case if we had reached in this principle the solution of the fundamental æsthetico-psychologic problem. For instance, the sense of beauty experienced when I look at some one bright star in the deep blue of the heaven seems to me to be inexplicable in terms of such introjection.

All this work, however, brings help to the practical artist and to the critic. They do not acknowledge it fully to-day, but year by year, more and more will the influence of the results of these studies be felt as they gain the attention of thinking men.

Nevertheless, we cannot but face the fact that the practical benefit to be gained from them is of a negative sort. There is no royal road to the attainment of beauty; but the psychologist is able to point out, by the methods here considered, the inner nature of certain sources of beauty; thus teaching the artist how he may avoid ugliness, and even indicating to him the main direction in which he may best travel toward the attainment of his goal.

But, after all, the relations thus discovered in the beautiful object, and the related special analyses of mental functioning which are involved with our appreciation of beauty, tell us of but relatively isolated bits of the broad realm of beauty. The objects which arouse within us the sense of beauty are most diverse, and equally diverse are the modes of mental functioning connected with the appreciation of their beauty.[1]

And this has led to the formulation of such principles as that of the "unity of manifoldness" of which Fechner makes so much, and that of the *monarchischen Unterordnung* which Lipps has more lately enunciated.

It is indeed of great interest to inquire why it is that the processes which lead to the recognition of these principles are so clearly defined in many cases where the sense of beauty is aroused. But very evidently these general principles, important though they be in them-

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[1] Nothing has shown this more clearly than the investigations of Haines and Davies in reference to the Golden Section of which we have spoken above. See *Psychological Review,* xi, 415.

selves, are not ones upon which we can afford to rest: for clearly they apply in very many cases where beauty does not claim sway.

Our whole mental life exemplifies the unification of the manifold, and monarchic subordination, whether the processes be æsthetic or not. It does not suffice us to show, what is thus shown, that the æsthetic states conform with conditions of our mental life that have a broad significance, although it is of great importance to demonstrate the fact: for our mental functioning in the appreciation of beauty appears thus as in truth an important type, but for all that but a special and peculiar type of the functioning which we thus bring into prominence.

The problem then remains, what is the special nature of this functioning which yields to us the sense of beauty?

And here in my view we have the problem which is of prime importance to æsthetics to-day, and which psychology alone can answer; namely, what is the characteristic that differentiates the sense of beauty from all other of our mental states? Until this question is answered, all else must seem of secondary importance from the standpoint of theoretical psychology, however important other forms of inquiry may be from a practical point of view.

When the psychologist turns his attention to this problem, he at once perceives that he is unable to limit his inquiry to the experience of the technically trained artist, or even to that of the man of culture who gives close attention to æsthetic appreciation.

Beauty is experienced by all men. But beauty is very clearly of varied types, and the sense of beauty is evidently called out by impressions of most varied nature; but the fields of what is considered beautiful by different people so far overlap that we can rest assured that we all refer to an experience of the same characteristic mental state when we proclaim the existence of beauty; for when we by general agreement use a special term as descriptive of an objective impression, we do so because this impression excites in us certain more or less specific mental states; and when different people use the same term in reference to objects of diverse nature, we are wont to assume, and are in general correct in assuming, that these objects affect these different people in approximately the same way.

It seems probable, therefore, that if the child, who has learned how to apply words from his elders, speaks of having a beautiful time at his birthday party; and if the grown man speaks of a beautiful day; and if the pathologist speaks of his preparation of morbid tissue as beautiful; and if the artist or connoisseur speaks of the beauty of a picture, a statue, a work of architecture, a poem, a symphony; then the word beauty must be used to describe a certain special mental state which is aroused in different people by very diverse objective impressions.

This view is strengthened when we consider that the application of the term by individuals changes as they develop naturally or by processes of education; and that the standards of beauty alter in like manner in a race from generation to generation as it advances in its development.

We must then look for the essence of beauty in some quality of our mental states which is called up by different objective impressions in different people, and under diverse conditions by different objects at different times in the same individual.

Search for such a quality has led not a few psychologists to look to pleasure as the quality of our mental states which is most likely to meet our demand. It is true that the consideration of pleasure as of the essence of the sense of beauty has not often been seriously carried out; apparently because so many of what we speak of as our most vivid pleasures appear as non-æsthetic; and because pleasure is recognized to be markedly evanescent, while beauty is thought of as at least relatively permanent.

It is true, also, that there is a hesitancy in using the word pleasure in this connection; many writers preferring the less definite word "feeling" in English, and "gefühl" in German. But by a large number of psychologists the words pleasure and feeling are used as synonyms; and those who, with me, agree that what we loosely call feeling is broader than mere pleasure, must note that it is the pleasurable aspect alone of what is called "feeling" that is essentially related to our experience of the sense of beauty.

All of us agree, in any event, that the sense of beauty is highly pleasant; and, in fact, most of our æstheticians have come to assume tacitly in their writings that the field of æsthetics must be treated as a field of pleasure-getting; and this whether or not they attempt to indicate the relation of pleasure-getting to the sense of beauty.

The suggestion that pleasure of a certain type is of the essence of beauty seems the more likely to prove to be satisfactory when we consider that pleasure is universally acknowledged to be the contradictory opposite of pain; and that we have in ugliness, which is always unpleasant, a contradictory opposite of beauty.[1]   ·

Clearly then it behooves the psychologist to give to the æsthetician an account of the nature of pleasure which shall be compatible with the pleasurable nature of the sense of beauty; and which shall either explain the nature of this sense of beauty in terms of pleasure, or explain the nature of pleasure in a manner which shall throw light upon the nature of this sense of beauty to which pleasure is so indissolubly attached.

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[1] It is of course agreed that beauty and ugliness may be held together in a complex impression: but in such cases the beauty and the ugliness are inherent in diverse elements of the complex.

The æsthetician thus demands urgently of the psychologist an analysis of the nature of pleasure; and an analysis of this so-called "feeling," which shall show the relation between the two experiences.

Concerning the latter problem I hope some day to have something to say.

Those of you who happen to be familiar with my published works will realize that my efforts in this field in the past have been given largely to the study of the former problem. My own view may be succinctly stated thus.

While all æsthetic experiences are pleasant, very evidently much that we *call* pleasant is not æsthetic. We must look then for some special differentiation of æsthetic pleasure, and this I find in its relative permanency.

This view is led up to by a preliminary study of the psychological nature of pleasure.

Pleasure I find to be one phase of a general quality — Pleasure-Pain — which, under proper conditions, may inhere in any emphasis within the field of attention, or, to use more common language, may belong to any element of attention.

Now pleasure, as we have said, is notably evanescent, but this does not preclude the existence of pleasurable states of attention which are relatively permanent. This permanency may be given by the shifting of attention from one pleasurable element to another; by the summation of very moderate pleasures, etc., etc.

Any pleasant psychic element may become an element of an æsthetic complex: and any psychic complex which displays a relative permanency of pleasure is in that fact æsthetic. Our æsthetic states are those in which many pleasant elements are combined to produce a relative permanency of pleasure.

Our "non-æsthetic pleasures," so called, are those states which have been experienced in the past as vividly pleasant, and to which the name pleasure has become indissolubly attached: but they are states which do not produce a relatively permanent pleasure in revival; and correctly speaking, are not pleasures at the moment when they are described as such, and at the same time as non-æsthetic.

I am glad to feel that this view of mine is not discrepant from that of Dr. Santayana, as given in quite different terms in his book entitled *The Sense of Beauty*. For what is relatively permanent has the quality which I call realness; and that in experience which has realness we tend to objectify. Hence it is quite natural to find Dr. Santayana defining beauty as objectified pleasure.

You will not blame me I believe for thinking that my own definition cuts down closer to the root of the matter than Dr. Santayana's.

But if this theory of mine is found wanting, the æsthetician will not cease to call upon the psychologist for some other which shall meet the demands of introspection; and which shall accord with our experiences of the sense of beauty, which in all their wealth of impression the æsthetician offers to the psychologist as data for the laborious study asked of him.

Before leaving this subject I may perhaps be allowed to call attention to the fact that the theoretical view, which places the essence of the sense of beauty in pleasure-getting, if it prove to be true, is not without such practical applications as are so properly demanded in our time. For if this view is correct, it teaches to the critic a lesson of sympathetic tolerance; for he learns from it that the sources from which the sense of beauty are derived differ very markedly in people of diverse types: and it warns him also against the danger of an artificial limitation of his own æsthetic sense, which will surely result unless he carefully avoids the narrowing of his interests.

It teaches further that there is no validity in the distinction between fine art and æsthetics on the one hand, and beauty on the other, on the ground, commonly accepted by the highly trained artist and connoisseur, that a work of art may deal with what is not beautiful.

For it appears that while the sense of beauty is the same for each of us, the objects which call it out are in some measure different for each.

Now it happens naturally that the objects which arouse the sense of beauty in a large proportion of men of culture get the word beauty firmly attached to them in common speech.

But under the view here maintained, it must be that the highly trained artist or critic will pass beyond these commoner men, and find his sense of beauty aroused by objects and objective relations quite different from those which arouse the sense of beauty in the commoner man; so that often he may deal with the beauty of elements in connection with which beauty is unknown to the commoner man, and even with elements which arouse a sense of ugliness in the commoner man; while on the other hand the objects which the commoner man signalizes as most beautiful, and which are currently so called, may not arouse in the trained artist or critic the sense of beauty which is now aroused in him by effects of broader nature, and of less common experience.

The critic and the skilled artist thus often find their æsthetic sense aroused no longer by the objects to which the word beauty has by common consent come to be attached; although with the commoner man he still uses the word beauty as descriptive of the object which arouses the æsthetic thrill in the mass of normally educated men. He may

even find his æsthetic sense aroused by what the common man calls ugly; although it is for himself really beautiful. And he comes thus quite improperly to think of the highest art as in a measure independent of what he calls "mere beauty." What he has a right to say, however, is merely this, that the highest art deals with sources of beauty which are not appreciated by even the generally well-cultivated man.

<center>*b*</center>

I have dwelt, perhaps, too long on the psychological problems presented when the psychologist attempts to describe to the æsthetician the nature of the experience of one who appreciates beauty; and have left perhaps too little time for the consideration of the problems presented when he is asked to consider the nature of the
· experience of the artist who creates.

The man who finds strongly developed within him the creative tendency, is wont, when he turns to theory, to lay emphasis upon *expression* as of the essence of beauty.

It is, of course, to be granted that the process of *Einfühlung*, — of introjection, — above referred to, leads us to find a source of beauty in the vague imagination of ourselves as doing what others have done; and we may take great æsthetic delight in reading, through his work, the mind of the man who has created the object of beauty for us. But evidently, when we lay stress upon this introjection, we are dealing with the appreciation of beauty, and not with the force which leads to its production.

Just as clearly is it impossible to hold that expression is of the essence of the *making* of beauty. For expressiveness is involved in all of man's creative activity, much of which has no relation whatever to the æsthetic. The expression of the character of the genius of the inventor of a cotton loom, or of the successful leader of an army in a bloody battle, excites our interest and wonder; but the expression of his character as read in the result accomplished does not constitute it a work of beauty.

I speak of this point at this length because in my opinion views of the nature of that here objected to could not have been upheld by such men as Bosanquet and Véron had they kept clear the distinction referred to above between the experience of one who appreciates beauty, and the experience of the creative artist; and especially because the teaching of the doctrine thus combated is wont to lead the artist whose cry is "Art for Art's sake" to excessive self-satisfaction, and to lack of restraint which leads to failure.[1]

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[1] In order to avoid misunderstanding, I may say here that notwithstanding these remarks I am in full sympathy with the artist who thus expresses himself, as will presently appear clear.

The strong hold which this theory has in many minds has its value, however, in the emphasis of the fact that æsthetic creation is due to impulses which are born of innate instincts expressing themselves in the production of works of beauty. And if this be so, we see how true it must be that each of us must have in him some measure of this instinct; and that the appearance of its appropriate impulses should not mislead us, and induce us to devote our lives to the worship of the Muses, unless we become convinced that no other work can adequately express the best that is in us.

But the true artist is not troubled by such questionings. He finds himself carried away by what is a true passion; by what is instinctive and not ratiocinative.

The fact that the artist is thus impelled by what may well be called the "art instinct" is one he could only have learned from the psychologist, or when in introspective mood he became a psychologist himself; and it carries with it corollaries of great value, which the psychologist alone can elucidate.

It teaches the artist, for instance, that his success must be determined by the measure of this instinct that is developed within him; that he must allow himself to be led by this instinct; that his best work will be his "spontaneous" work. This, of course, is very far from saying that he cannot gain by training; but it does mean that he must learn to treat this training as his tool; that he must not trust overmuch to his ratiocinative work, the result of which must be assimilated by, and become part of, his impulsive nature, if he is to be a master.

An artist is one in whom is highly developed the instinct which leads him to create objects that arouse the sense of beauty. The expression of this instinct marks his appropriate functioning. He may incidentally do many useful things, and produce results apart from his special aptitude; but as an artist his work is solely and completely bound up in the production of works of beauty.

We naturally ask here what may be the function in life of the expressions of such an instinct as we have been studying, and this leads us to consider a point of more than psychological interest, and turns our thought to our second division.

II. *The Relation of Æsthetics to Philosophy*

For while the science of psychology must guide, it can never dominate the thought of the philosopher who strives to gain a broad view of the world of experience; and, as will appear below, the æsthetician calls upon the philosopher for aid which the psychologist as such cannot give.

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In approaching this subject we may take at the start what we may call the broadly philosophical view, and may consider the question raised immediately above, where we ask what may·be the function in life of the art instinct, and what the significance of the æsthetic production to which its expression leads.

We, in our day, are still strongly influenced by the awakening of interest in the problems of organic development with which Darwin's name is identified, and thus naturally look upon this problem from a genetic point of view; from which, to my mind, artistic expression appears, as I have elsewhere argued at length, as one of nature's means to enforce social consolidation. But it is possible that we are led, by the present-day interest above spoken of, to over-emphasize the importance of the processes of the unfolding of our capacities, and it is not improbable that those who follow us, less blinded by the brilliancy of the achievement of the evolutionists, may be able to look deeper than we can into the essence of the teleological problem thus raised.

That art is worthy for art's sake is the conviction of a large body of artists, who labor in their chosen work often with a truly martyr-like self-abnegation; and as an artist I find myself heartily in sympathy with this attitude. But æsthetics looks to philosophy for some account of this artistic τέλος, which shall harmonize the artist's effort with that of mankind in general, from whom the artist all too often feels himself cut off by an impassable gulf.

The study of æsthetics by the philosopher from the genetic standpoint has, however, already brought to our attention some facts which are both significant and helpful.

It has shown us how slow and hesitant have been the steps in the development of æsthetic accomplishment and appreciation in the past, and how dependent these steps have been upon economic conditions. This on the one hand arouses in us a demand for a fuller study of the relations of the artistic to the other activities of men; and on the other hand is a source of encouragement to critic and artist alike, each of whom in every age is apt to over-emphasize the artistic failures of his time, and to minimize the importance of its artistic accomplishment.

This genetic study has a further value in the guidance of our critical judgment, in that it shows us that the artistic tendencies of our time are but steps in what is a continuous process of development. It shows us arts which have differentiated in the past, and teaches us to look for further artistic differentiations of the arts in the future; thus leading us to critical conclusions of no little importance. This consideration seems to me to be of sufficient interest to warrant our dwelling upon it a little at length.

The arts of greatest importance in our time may well be divided into the arts of hearing (that is, literature, poetry, music), and the arts of sight (that is, architecture, sculpture, painting, and the graphic arts).

These diverse groups of arts were differentiated long before any age of which we have a shadow of record. But many animals display what seem to be rudimentary art instincts, in which rhythmical movement (which is to be classed as an art of sight) and tonal accompaniment are invariably combined — as they are also in the dance and song of the savage; and this fact would seem to indicate that in the earliest times of man's rise from savagery the differentiation between the arts of sight and the arts of hearing was at least very incomplete.

But leaving such surmises, we may consider the arts of sight and the arts of hearing in themselves. We see them still in a measure bound together; for many an artist, for instance, devotes his life to the making of paintings which "tell a story," and many a poet to the production of "word-pictures."

In general, however, it may be said that the arts of hearing and the arts of sight express themselves in totally different languages, so to speak, and they have thus differentiated because each can give a special form of æsthetic delight.

Turning to the consideration of each great group, we note that the arts of sight have become clearly differentiated on lines which enable us to group them broadly as the graphic arts, painting, sculpture, and architecture. Each of these latter has become important in itself, and has separated itself from the others, just so far as it has shown that it can arouse the sense of beauty in a manner which its kindred arts of sight cannot approach. It is true that all the arts of sight hold together more closely than do the arts of sight, as such, with the arts of hearing, as such. But it is equally clear that the bond between the several arts of sight was closer in earlier times than it is to-day, in the fact that modeled painting, and colored sculpture, were common media of artistic expression among the ancients, the latter being still conventional even so late as in the times of the greatest development of art among the Greeks.

But the modern has learned that in painting and graphics the artist can gain a special source of beauty of color and line which he is able to gain with less distinctness when he models the surface upon which he works: and the experience of the ages has gradually taught the sculptor once for all that he in his own special medium is able to gain a special source of beauty of pure form which no other arts can reach, and that this special type of beauty cannot be brought into as great emphasis when he colors his modeled forms.

In my view we may well state, as a valid critical principle, that, other things being equal, in any art the artist does best who presents in his chosen medium a source of beauty which cannot be as well presented by any other art.   That this principle is appreciated and widely accepted (although implicitly rather than explicitly) is indicated by the unrationalized objection of the cultivated critic in our day to colored sculpture or to modeled painting, and in a more special direction to the use of body-color in *aquarelle* work.   The objection in all cases is apparently to the fact that the artist fails to bring into prominence that type of beauty which his medium can present as no other medium can.

Personally I have no objection to raise to a recombination of the arts of sight, provided a fuller sense of beauty can thereby be reached.   But it is clear that this recombination becomes more and more difficult as the ages of development pass; and I believe the principle of critical judgment above enunciated is valid, based as it is upon the inner sense of cultivated men.

Better than attempts to recombine the already differentiated arts of sight are attempts to use them in conjunction, so that our shiftings of attention from one type of beauty to another may carry with them more permanent and fuller æsthetic effects ; and such attempts we see common to-day in the conjunction of architecture and of sculpture and of painting, in our private and public galleries, in which are collected together works of the arts of sight.

Now if we turn to the consideration of the arts of hearing, we find a correspondence which leads to certain suggestions of no little importance to the critical analyst in our day.

The arts of hearing have become differentiated on lines which enable us to group them broadly as rhetoric, poetry and literature, and music. Each has become important in itself, and has gradually separated itself from the others; — and this just so far as it has shown that it can arouse in men, in a special and peculiar manner, the sense of beauty.

It is true, as with the arts of sight, that the special arts of hearing still hold well together.

But in relatively very modern times music, having discovered a written language of its own, has differentiated very distinctly from the other arts of hearing. Men have discovered that *pure music* can arouse in a special manner the sense of beauty, and can bring to us a form of æsthetic delight which no other art can as well give.

Poetry has long been written which is not to be sung; and it has gained much in freedom of development in that fact.

Music in our modern times is composed by the greatest masters for its own intrinsic worth, and not as of old as a mere accompani-

ment of the spoken word of the poet; the existence of the works of Bach, to mention no others, tells of the value of this differentiation.

And here I think we may apply with justice the principle of criticism above presented. The poet and the musician each do their best work, other things being equal, when they emphasize the forms of beauty which their several arts alone can give. We have here in my view a rational ground for the repulsion many of us feel for the so-called "programme music" of our day.

Music and literature of the highest types nowadays present sources of beauty of very diverse character, and any effort to make one subsidiary to the other is likely to lessen the æsthetic worth of each, and of the combination.

Here again I may say that I have no objection to raise to a recombination of the arts of hearing, provided a fuller sense of beauty can thereby be reached. But this recombination becomes year by year more difficult as the several arts become more clearly differentiated, and must in my view soon reach its limit.

The opera of to-day attempts such a recombination, but does so either to the detriment of the musical or of the literary constituent; that is clear when we consider the musical ineptitude of such operas as deal with a finely developed drama, and the literary crudeness of the plot-interest in Wagner's very best works. Such a consideration makes very clear to us how much each of the great divisions of the arts of hearing has gained by their differentiation, and by their independent development.

Here as with the arts of sight we may, in my view, hope for better æsthetic results from the development of each of the differentiated arts in conjunction; rather from the persistent attempt to recombine them, with the almost certain result that the æsthetic value of each will be reduced.

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But æsthetics demands more of philosophy than an account of the genesis of art, with all the valuable lessons that this involves. It demands, rightly, that it be given a place of honor in any system which claims to give us a rationalized scheme of the universe of experience.

The æsthetician tells the philosopher that he cannot but ask himself what significance æsthetic facts have for his pluralism, or for his monism. He claims that this question is too often overlooked entirely or too lightly considered; but that it must be satisfactorily answered if the system-maker is to find acceptance of his view. And in the attempt to answer this and kindred questions, the æsthetician is not without hope that no inconsiderable light may be thrown by the philosopher upon the solution of the problems of æsthetics itself.

Nor are the problems of æsthetics without relation to pure meta-physic. The existence of æsthetic standards must be considered by the metaphysician, and these standards, with those of logic and ethics, must be treated by him as data for the study of ontological problems.

But beyond this, æsthetics cries out for special aid from the ontologist. What, he asks, is the significance of our standards of æsthetic appreciation? What the inner nature of that which we call the real of beauty? What its relation with the real of goodness and the real of truth?

From a practical standpoint this last-mentioned question is of special import at this time. For the world of art has for centuries been torn asunder by the contention of the æsthetic realists and their opponents.

That, in its real essence, beauty is truth, and truth beauty, is a claim which has often been, and is still heard; and it is a claim which must finally be adjudicated by the metaphysician who deals with the nature of the real.

The practical importance of the solution of this problem is brought home forcibly to those who, like myself, seem to see marked æsthetic deterioration in the work of those artists who have been led to listen to the claims of æsthetic realism; who learn to strive for the expression of truth, thinking thus certainly to gain beauty.

That many great artists have announced themselves as æsthetic realists shows how powerfully the claims of the doctrine appeal to them. But one who studies the artistic work of Leonardo, for instance, cannot but believe that he was a great artist *notwithstanding* his theoretical belief, and cannot but believe that all others of his way of thinking, so far as they are artists, are such because in them genius has overridden their dogmatic thought.

It is clearly not without significance that the world of values is by common consent held to be covered by the categories of the True, the Good, and the Beautiful. This common consent seems surely to imply that each of the three is independent of the other two, although all are bound together in one group. And if this is true, then the claim of the æsthetic realist can surely not be correct.

But this claim will not be overthrown by any reference to such a generalization as that above mentioned. The claim of the æsthetic realist is based upon what he feels to be clear evidence founded upon experience; and he cannot be answered unless we are able to show him what is the basis for his ready conviction that truth and beauty are one and identical; and what is the true relation between the True, the Good, and the Beautiful. And these problems, which are in our day of vital importance to the artist, the philosopher alone can answer.

In my view some aid in the solution of this problem may be gained from the examination of the meaning of our terms. From this study I feel convinced that we must hold that when we speak of the True, and the Good, and the Beautiful, as mutually exclusive as above, we use the term "true" in a narrow sense. On the other hand, the True is often used in a broader sense, as equivalent to the Real.

This being so we may say

That the Beautiful is the Real as discovered in the world of impression; the relatively permanent pleasure which gives us the sense of beauty being the most stable characteristic of those parts of the field of impression which interest us we may also assent

That the Good is the Real as discovered in the world of expression, that is, of impulse, which is due to the inhibited capacity for expression, and the reaction of the self in its efforts to break down the inhibition. And in the same way we may conclude

That the True (using the term in the narrow sense) is the Real as discovered in the realm of experience exclusive of impression or expression.

$$
\text{THE REAL} \atop \begin{array}{c} \text{or} \\ \text{THE TRUE} \\ \text{(in the broad sense} \\ \text{of the term)} \end{array}
\left\{
\begin{array}{l}
\alpha. \text{ The Real of Impression — The Beautiful} \\[1em]
\beta. \text{ The Real of Expression — The Good} \\[1em]
\gamma. \text{ The Real in realms} \quad \text{— The True} \\
\quad \text{ exclusive of } \alpha \text{ and } \beta \quad \text{ (in the narrower} \\
\qquad\qquad\qquad\qquad\qquad\quad \text{ sense of the term)}
\end{array}
\right.
$$

That the Beautiful is part of the REAL, that is, is always the TRUE, *using the term true in the broader sense,* is not questioned: and that, in my view, is the theoretical truth recognized by the æsthetic realists. But in practice the æsthetic realist maintains that the beautiful is always the true, *using the term true in the narrow sense,* and in this, in my view, lies his error.

And if the relation of the beautiful to the true demands the attention of the philosopher, equally so does the relation of the beautiful to the good. As I look upon it, all of the true (using the term as above explained in the narrow sense) and all of the good, so far as either involve relatively permanent pleasure of impression, are possible elements of beauty. But, on the other hand, it seems clear that neither the true (still using the term in the narrower sense), nor the good, is necessarily pleasing, but may be unpleasant, and therefore either of them may be an element of ugliness, and as such must lose all possibility of becoming an element in the beautiful.

One further word, in closing, upon the closely allied question as to the nature of worth-values. There is a worth-value involved in the Good, and a worth-value involved in the True, and a worth-

value involved in the Beautiful: and each of these worth-values in itself seems to be involved with pleasure-getting. Now if this is the case, then, under the theory I uphold, any worth-value should be a possible æsthetic element, and this I think it will be granted is true. But the distinctions between these worth-values are on different planes, as it were. In the case of the worth-value of the Good, we appreciate the worth-pleasure within the realm of the Real of Expression, that is, of impulse. In the case of the worth-value of the True (in the narrow sense), we appreciate the worth-pleasure within the realm of the Real in other fields than that of expression or that of impression. In the case of the worth-value of the Beautiful, we appreciate the worth-pleasure within the realm of the Real of Impression; that is, we appreciate, with pleasure, the significance for life of the existence of relatively permanent pleasure in and for itself.

THE FUNDAMENTAL QUESTIONS OF CONTEMPORARY ÆSTHETICS

BY MAX DESSOIR

(Translated from the German by Miss Ethel D. Puffer, Cambridge, Mass.)

[**Max Dessoir**, Professor of Philosophy, University of Berlin, since 1897. b. 1867, Berlin, Germany. Ph.D. Berlin, 1889; M.D. Würzburg, 1892. Privat-docent, University of Berlin, 1892–97. Member German Psychological Society, Society for Psychical Research, London. **Author of** *The Double Ego; History of the New German Psychology; Philosophical Reader; Æsthetik u. Allgemeine Kunstwissenschaft;* and many other works and papers on philosophy.]

I

In the development which our science has undergone, from its inception up to the present day, one thought has held a central place, — that æsthetic enjoyment and production, beauty and art, are inseparably allied. The subject-matter of this science is held to be, though varied, of a unitary character. Art is considered as the representation of the beautiful, which comes to pass out of an æsthetic state or condition, and is experienced in a similar attitude; the science which deals with these two psychical states, with the beautiful and its modifications, and with art in its varieties, is, inasmuch as it constitutes a unity, designated by the single name of æsthetics.

The critical thought of the present day is, however, beginning to question whether the beautiful, the æsthetic, and art stand to one another in a relation that can be termed almost an identity. The undivided sway of the beautiful has already been assailed. Since art includes the tragic and the comic, the graceful and the sublime, and even the ugly, and since æsthetic pleasure can attach itself to all these categories, it is clear that by "the beautiful" something narrower must be meant than the artistically and æsthetically valuable. Yet beauty might still constitute the end and aim and central point of art, and it might be that the other categories but denote the way to beauty — beauty in a state of becoming, as it were.

But even this view, which sees in beauty the real content of art, and the central object of æsthetic experiences, is open to serious question. It is confronted with the fact, above all, that the beauty enjoyed in life and that enjoyed in art are not the same. The artist's copy of the beauty of nature takes on a quite new character. Solid objects in space become in painting flat pictures, the existent is in poetry transformed into matter of speech; and in every realm is a

like metamorphosis. The subjective impression might indeed be sup-
posed to remain the same, in spite of objective differentiations. But
even that is not the case. Living human beauty — an acknowledged
passport for its possessor — speaks to all our senses; it often stirs
sex-feeling in however delicate and scarce conscious a way; it
involuntarily influences our actions. On the other hand, there hangs
about the marble statue of a naked human being an atmosphere
of coolness in which we do not consider whether we are looking
upon man or woman: even the most beauteous body is enjoyed as
sexless shape, like the beauty of a landscape or a melody. To
the æsthetic impression of the forest belongs its aromatic fragrance,
to the impression of tropical vegetation its glowing heat, while
from the enjoyment of art the sensations of the lower senses are
barred. In return for what is lost, as it were, art-enjoyment involves
pleasure in the personality of the artist, and in his power to over-
come difficulties, and in the same way many other elements of pleas-
ure, which are never produced by natural beauty. Accordingly,
what we call beautiful in art must be distinguished from what goes
by that name in life, both as regards the object and the subjective
impression.

Another point, too, appears from our examples. Assuming that
we may call the pure, pleasurable contemplation of actual things
and events æsthetic, — and what reason against it could be adduced
from common usage ? — it is thus clear that the circle of the æsthetic
is wider than the field of art. Our admiring and adoring self-abandon-
ment to nature-beauties bears all the marks of the æsthetic attitude,
and needs for all that no connection with art. Further: in all in-
tellectual and social spheres a part of the productive energy expresses
itself in æsthetic forms; these products, which are not works of art,
are yet æsthetically enjoyed. As numberless facts of daily experience
show us that taste can develop and become effective independently
of art, we must then concede to the sphere of the æsthetic a wider
circumference than that of art.

This is not to maintain that the circle of art is a narrow section of
a large field. On the contrary, the æsthetic principle does not by
any means exhaust the content and purpose of that realm of human
production which taken together we call "art." Every true work of
art is extraordinarily complex in its motives and its effects; it arises
not alone from the free play of æsthetic impulse, and aims at more
than pure beauty — at more than æsthetic pleasure. The desires
and energies in which art is grounded are in no way fulfilled by
the serene satisfaction which is the traditional criterion of the æs-
thetic impression, as of the æsthetic object. In reality the arts
have a function in intellectual and social life, through which they are
closely bound up with all our knowing and willing.

It is, therefore, the duty of a general science of art to take account of the broad facts of art in all its relations. Æsthetics is not capable of this task, if it is to have a determined, self-complete, and clearly bounded content. We may no longer obliterate the differences between the two disciplines, but must rather so sharply separate them by ever finer distinctions that the really existent connections become clear. The first step thereto has been taken by Hugo Spitzer. The relation of earlier to current views is comparable to that between materialism and positivism. While materialism ventured on a pretty crude resolution of the spiritual into the corporeal, positivism set up a hierarchy of forces of nature, whose order was determined by the relation of dependence. Thus mechanical forces, physico-chemical processes, the biological and the social-historical groups of facts, are not traced back each to the preceding by an inner connection, but are so linked that the higher orders appear as dependent on the lower. In the same way is it now sought to link art methodologically with the æsthetic. Perhaps even more closely, indeed, since already æsthetics and the science of art often play into each other's hands, like the tunnel-workers who pierce a mountain from opposite points, to meet at its centre.

Often it so happens, but not invariably. In many cases research is carried to an end, quite irrespectively of what is going on in other quarters. The field is too great, and the interests are too various. Artists recount their experiences in the process of creation, connoisseurs enlighten us as to the technique of the special arts; sociologists investigate the social function, ethnologists the origin, of art; psychologists explore the æsthetic impression, partly by experiment, partly through conceptual analysis; philosophers expound æsthetic methods and principles; the historians of literature, music, and pictorial art have collected a vast deal of material — and the sum total of these scientific inquiries constitutes the most substantial though not the greatest part of the published discussions, which, written from every possible point of view, abound in newspapers and magazines. "There is left, then, for the serious student, naught but to resolve to fix a central point somewhere, and thence to find out a way to deal with all the rest as outlying territory" (Goethe).

Only by the mutual setting of bounds can a united effect be possible from the busy whirl of efforts. Contradictory and heterogeneous facts are still very numerous. He who should undertake to construct thereof a clear intelligible unity of concepts, would destroy the energy which now proves itself in the encounters, crossing of swords, and lively controversies of scholars, and would mutilate the fullness of experience which now expresses itself in the manifold special researches. System and method signify for us: to be free from *one* system and *one* method.

II

If we are to consider how we answer to-day the questions put for scientific consideration as to the facts of æsthetic life and of art, first of all we must examine the now prevailing theories of æsthetics. They fall in general into æsthetic objectivism and subjectivism. By the first collective name we denote the aggregate of all theories which find the characteristic of their field of inquiry essentially in the quality and conformation of the object, not in the attitude of the enjoying subject. This quality of the æsthetically valuable is most easily characterized by setting it off against reality. Of such theories, which explain "the beautiful" and art from their relation to what is given in nature, naturalism stands for the identity of reality and art, while the various types of idealism set forth art as more than reality, and *vice versa*, formalism, illusionism, sensualism make it less than reality.

Inasmuch as naturalism is still defended only by a handful of artists who write, it would appear almost superfluous to consider it. But the refutations of it which are still appearing indicate that it must have some life. And in fact it still exists, partly as a present-day phenomenon in literature and art, partly as the permanent conviction of many artists. The naturalistic style testifies to revolt against forms and notions which are dying out; it therefore only attains a pure æsthetic interest through the theoretic ground which is furnished to it. And this rests above all on the testimony of the artists, who are never weary of assuring us that they immediately reproduce what is given in perception. Some philosophical conceptions also play therein a certain rôle. The adherents of the doctrine that only the sense-world is real come as a matter of course to the demand that art shall hold itself strictly to the given. And what optimist, who explains the real world as the best of all possible worlds, can, without a logical weakening, admit a play of imagination different from the reality.

Æsthetic idealism, too, is borne on general philosophical premises. However various these are, in this they all agree, that the world is not exhausted by appearances, but has an ideal content and import, which finds in the æsthetic and in the field of art its expression to sense. Even H. Taine sets to art the task of showing the "dominant character" of things. The beautiful is therefore something higher than the chance reality, — the typical as over against the anomalous natural objects or events. It can then be objectively determined with reference to its typical and generic quality and in its various kinds.

Somewhat different is the case of formalism, which to-day scarcely anywhere sets up to be a complete system of æsthetics, but points

the way for many special investigations. It seeks the æsthetically effective in the form, that is, in the relation of parts, which has in principle nothing to do with the content of the object. Every clearly perceptible unity in manifoldness is pleasing. As this arrangement is independent of the material, the æsthetic represents only a part of reality.

In contrast thereto, illusionism sets the world of art as a whole over against the whole of reality. Art, we are taught, presents neither a new aspect of the given nor hidden truth, nor pure form; it is, on the contrary, a world of appearance only, and is to be enjoyed without regard to connections in life or any consequences. While we otherwise consider objects as to how they serve our interests and as to their place in the actual connection of all things, in the æsthetic experience this twofold relation is disregarded. Neither what things do for us, nor what they do for each other, comes in question. Their reality disappears, and the beautiful semblance comes to its own. Konrad Lange has given to this theory — especially in the line of a subjective side, to be later mentioned — its modern form.

Of the nearly-related sensualism, the connoisseur Fiedler and the sculptor Hildebrand are the recent exponents; Rutgers Marshall and certain French scholars also lean that way. It is the arts which fix the transitory element of the sense-image, hold fast the fleeting, make immortal the perishable, and lend stability and permanence to all pleasure that is bound up with perception. What does painting accomplish? Arisen, as it has, out of the demands of the eye, its sole task is to gain for the undefined form- and color-impressions of reality a complete and stable existence. The same thing is true of the other arts, for their respective sense-impressions.

To sum up: If the transformation of reality is acknowledged as a fundamental principle of art, it is also to be granted that this takes place in two directions: — art is something at once more and less than nature. Inasmuch as art pushes on to the *vraie vérité*, and at the same time disregards all that is not of the nature of semblance or image, we take from it ideas whose quality enthralls and stimulates us quite independently of their meaning. Art shows us the hidden essence of the world and of life and at the same time the outsides of things created for our pleasure; that is, the objects' pure psychical value in the field of sense. It involves a lifting above nature, and at the same time the rounding out and fulfillment of sense. Through making of the object an image, it frees us from our surrounding, yet leaves us at rest in it.

We turn now to æsthetic subjectivism. Under this name we comprehend the essence of those theories which seek to solve the riddle of the beautiful by a general characterization of the æsthetic attitude. Many of these are near akin to the objectivistic theories; some,

however, like the *Einfühlung*-theory, take an independent place. For the former, therefore, a mere indication will suffice. The principle of "semblance" or illusion, for instance, takes very easily a subjectivistic turn. The question then runs: Wherein consists the peculiarity of the conscious processes which are set up by the semblance? The answer as given by Meinong and Witasek starts from the fact that the totality of psychical processes falls into two divisions. Every process in one division has its counterpart in the other. To perception corresponds imagination, to judgment assumption, to real emotion ideal emotion, to earnest desire fancied desire. The æsthetic emotions attached to assumptions, the semblance-emotions, that is, are held to be scarcely distinguished, so far as feeling goes, from other emotions, at most, perhaps, by less intensity. The chief difference lies rather in the premise or basis of emotion; and this is but a mere assumption or fiction.

A critical treatment of the foregoing cannot be given here; nor of that view which explains the psychical condition in receiving an æsthetic impression as a conscious self-deception, a continued and intentional confusion of reality and semblance. The æsthetic pleasure, according to this, is a free and conscious hovering between reality and unreality; or, otherwise expressed, the never successful seeking for fusion of original and copy. The enjoyment of a good graphic representation of a globe would then depend on the spectator's now thinking he sees a real globe, now being sure he views a flat drawing.

While this theory has found but small acceptance, comparatively many modern æstheticians admit the doctrine of *Einfühlung*. Its leading exponent, Theodor Lipps, sees the decisive characteristic of æsthetic enjoyment in the fusion of an alien experience with one's own: as soon as something objectively given furnishes us the possibility of freely living ourselves into it, we feel æsthetic pleasure. In the example of the Doric column, rearing itself and gathering itself up to our view, Lipps has sought to show how given space-forms are interpreted first dynamically, then anthropomorphically. We read into the geometrical figure not only the expression of energy, but also free purposiveness. In so far as we look at it in the light of our own activity, and sympathize with it accordingly, in so far do we feel it as beautiful.

Could we enter upon a critical discussion at this point, it would appear that the *Einfühlung*-theory, like its fellows, is open to well-founded objections. The belief in an all-explaining formula is a delusion. In truth, every one of the enumerated principles is relatively justified. And as they all have points of similarity with one another, it is not hard for the past-master of terminology and the technique of concepts to epitomize the common element in a single

phrase or thesis. Still, nothing is gained by such a general formula in presence of the richness of the reality; and just as little — as an exhaustive treatment would have to prove — by the concise exposition of a single method for our science.

The specially approved method of procedure at the present day is that of psychological description and explanation. It seems, indeed, very natural to see in psychical processes the real subject-matter of æsthetics, and in psychology, accordingly, the science to which it is subordinate. Some philosophers, however, — among whom I may instance Jonas Cohn, — wish to make of æsthetics a science of values, and demand that on the basis of this pretension the mutually contradictory judgments of taste and types of art be tried and tested. They will have no mere descriptive and explanatory æsthetics, but a normative, precept-giving science. In truth, the opposition of the schools is complete at every point; in the writings of Volkelt and Groos we have the proof of it.

III

The special research in the narrower field of æsthetics is at present almost entirely of the psychological type. Our survey can touch upon only the salient points.

The aim of the extended and highly detailed study consists in fixating by means of psychological analysis the course of development, the effective elements, and the various sub-species of the æsthetic experience. Certain philosophers seek a point of departure for this undertaking in the æsthetic object. Thus Volkelt's system of æsthetics finds, for the chief elements of the æsthetic enjoyment, corresponding features in the object; in the special field of poetry Dilthey has undertaken an analysis along the same lines. For the most part, however, such dissection is limited to the subjective side. In Wundt's psychology, for instance, the æsthetic state of mind is shown to be built up of sense-feelings, feelings from perceptions, intellectual and emotional excitements; the most important, that is to say, the pivotal feelings, which are bound up with space- and time-relations, become in turn the condition and support of the higher emotions, because they lead over from the field of sense to that of the logical and emotional.

If we limit ourselves to the psychological, we must first ask in what order the elements of the æsthetic impression are wont to follow each other. The phases of this development, however, are as yet not completely studied, although they are of great significance for the differences in enjoyment. The second problem concerns the constitution (taken as timeless) of the experience. All formulas which attempt to fix in two words the totality of the impression fail com-

pletely, — so extraordinarily various and manifold are the factors which enter here. What these are and how they are bound together is the question which is for the moment occupying the scholars with a leaning toward psychology.

The æsthetic impression is an emotion. According to the well-known sensualistic theory of the emotions, it must therefore, in so far as it is more than mere perception or idea, be composed of organic sensations. G. Sergi and Karl Lange see, in fact, the peculiar mark of the æsthetic experience in the general sensations which appear with changes in the circulation, breathing, etc. Unprejudiced observation must satisfy every one that much in all this is true. On the other hand, it is to be recalled that we do not reckon the organic sensations to the objective qualities of æsthetic things, and that we cannot explain in this way every artistic enjoyment. — In regard to the sensations of taste, smell, and touch, it is generally granted that they play a certain rôle, even if but as reproduced ideas and only corresponding to natural beauty. Among the most important are the attitudes and imitative movements, finely investigated by Karl Groos. — To this must be added the sensuous pleasantness of visual and auditory perceptions. Yet attempts to construct the æsthetic enjoyment in its entirety out of such pleasure-factors have so far failed. The undertaking is already wrecked by the fact that elements displeasing to sense are demonstrably present, not only as negligible admixtures, but also as necessary factors. The relations of similarity between the contents of a sense-field, and the spatial and temporal connections between them, are in any case incomparably more important; we devote to them, therefore, a closer consideration. Finally, alongside all these ideas and the emotions immediately attaching to them, there must be arrayed the great multitude of associated ideas and connecting judgments. While scientific interest in the associations is now greatly diminished, explanations of the part played by the element of really active thought are many. A universally satisfactory theory is still to appear, for the reason, above all, that here the higher principles referred to in the second section enter into the problems.

Elementary æsthetics, therefore, willingly turns aside from the shore of the very complex emotions, of association, *Einfühlung* and illusion in æsthetic experience, in order to become independent of general philosophical fundamental conceptions. Its own field lies in the general province of the perception-feelings determined immediately by the object: more exactly, of the feelings which are induced partly by the relations of similarity, partly by the outer connections of the content, partly by the linking of inner and outer reference. The qualitative relation of tones and colors arouses the so-called feelings of harmony; the arrangement in space and time

awakes the so-called proportion-feelings; and from the coöperation of these two arise the so-called æsthetic complication-feelings.

As to the pleasurable tone- and color-combinations, the first are better known than the second, but even their theoretical interpretation is not well settled. More diligent and successful at the present time is the research into the proportion-feelings. So far as these bear upon space-relations, they attach either to the outlines or to the structure of the forms. The bounding-lines are then pleasing, one theory holds, when they correspond to the easiest eye-movements, and in general meet our desire for easy, effortless orientation. Another doctrine, already referred to, explains their æsthetic value from a coöperation of general bodily feelings, especially sensations of breathing and equilibrium. Accurate experiments have not succeeded in finding a real conformity to law in either the first or the second direction. In the matter of the structure of forms, symmetry in the horizontal position, and the proportion of the golden section in the vertical position, receive especial attention. All those space-shapes may be called symmetrical, whose halves are of equal value æsthetically. How these must be constituted, has been studied from the simplest examples by Münsterberg and his pupils. The explanation of the pleasing quality rests on the fact that the spectator feels the contents of the two halves — lines or colors — as light or heavy, according to the energy expended in the necessary eye-movements. In the vertical position a proportion pleases (as does also equality) which is only approximately that of the golden section. The numerical proportion is, therefore, not the ground of pleasure, for otherwise those forms which are thus divided would have to be the absolutely beautiful ones, and the more a division varies from the exact fraction, the more would it sacrifice in beauty. The ground of pleasure is rather descried in the fact that in the case of the pleasing divisions the two parts stand out as distinct and clearly characterized, while yet unified effect is secured through the larger division.

The temporal ordering of an æsthetic character is that of rhythm. Concerning the æsthetic object as such — that is, concerning the metrical forms in music and poetry, the views are still widely at variance; this is true to a startling degree of poetry, because here the element, that is to say, the word, is made up of accented and unaccented syllables, and because the tendency of the logical connections of the content to create unities cannot be done away with. This state of confusion is so much the more to be regretted as it is just to the art-forms that the most vivid rhythmical feelings attach. The psychological investigations of Neumann, Bolton, and others have nevertheless much advanced our scientific understanding of this subject. A new point of view has taken its rise from Souriau and Bücher: the connection of the art-rhythm with work and other

aspects of life. But the collections of data do not yet render it pos-
sible to settle the question in what manner the rhythm of work,
which runs on automatically, and is controlled by the idea of an end,
goes over into æsthetic rhythm.

The æsthetic complication-feelings are bound up with the products
of the fusion of rhythm and harmony, form and color, rhythm and
form (in the dance). So long as all elements of association are
neglected, three characteristics remain to be noted: an increasing
valuation of the absolute quantity, the building-up of definite
form-qualities (*Gestaltqualitäten*), and a reconciliation or harmony
of differences, wherein the quantitative element is wont to be the
unifying, the qualitative element the separating factor. I need not,
however, go any further into investigations so subtle, and even now
merely in their beginnings.

This entire fabric of experience, from which but a few threads
have been drawn out to view, can now take on various shadings.
These we refer to as the æsthetic moods, or by a less psychological
name, as the æsthetic categories. The ideally beautiful and the
sublime, the tragic and the ugly, the comic and the graceful, are
the best known among them. Modern science has shown most
interest in the study of the comic and the tragic. According to Lipps
the specific emotion of the comic arises in the disappointing of a
psychical preparation for a strong impression, by the appearance of
a weak one. The pleasurable character of the experience would be
explained by the fact that the surplus of psychical impulse, like every
excess of inner energy, is felt as agreeable. The tragic mood is under-
stood no longer as arising in fear and pity, but in pathos and wonder.
Its objective correlate should not be forced to the standard of a nar-
row ethics. The demand for guilt and expiation is being given up
by progressive thinkers in æsthetics; but the constituents of tragedy
remain fast bound to the realm of harshness, cruelty, and dissonance.

IV

From a period more or less remote there have existed poetics,
musical theory, and the science of art. To examine the presupposi-
tious methods and aims of these disciplines from the epistemological
point of view, and to sum up and compare their most important results,
is the task of a general science of art; this has besides, in the pro-
blems of artistic creation and the origin of art, and of the classification
of the arts and their social function, certain fields of inquiry that
would otherwise have no definite place. They are worked, indeed,
with remarkable diligence and productiveness. Most to be regretted,
on the other hand, is that so little energy is applied to laying the
epistemological foundation.

The theory of the development of art deals with it both in its

individual and its generally human aspect. Concerning the genesis of
the child's understanding of art and impulse to produce it, we learn
most from the studies of his drawings at an early age. Here are to be
noted well-established results of observation, even though as yet
they are few in number. On the other hand, the unfolding of primi-
tive feeling (and of the æsthetic sensibility in general) during the
historical period can be only approximately reconstructed. The
case is somewhat more favorable for our information in regard to
the beginnings of art, especially since it has been systematically
assembled by Ernst Grosse and Yrjö Hirn. If the conditions of the
most primitive of the races now living in a state of nature can be
taken as identical with those at the beginnings of civilization, the
entire vast material of ethnology can be made use of. We gather
therefrom how close-linked with the useful and the necessary beauty
is, and see clearly that primitive art is thoroughly penetrated by
the purpose of a common enjoyment, and is effective in a social
way; but beyond such general principles one can go only with
hesitation, inasmuch as it seems scarcely possible to us, creatures of
civilization, to fix the boundaries of what is really art there.

There are three conjectures as to objective origin of art. It may
be that the separate arts have developed through variation from one
embryonic state. Or the main arts may have been separate from the
very first, having arisen independently of each other. Finally, there
are middle views, like that of Spencer, according to which poetry,
music, and the dance on the one hand, and writing, painting, and
sculpture on the other, have a common root ; Möbius recognizes
three primitive arts, to which the others are to be traced back. The
solution of this question would be especially important, could one
hope to find Darwin's maxim for all ætiological investigations valid
for our field also — that is, the dictum: What is of like origin is of
like character.

As psychological conditions, from which the artistic activity is
likely first to have arisen, the following functions have been suggested
and maintained, — the play-instinct, imitation, the need for expres-
sion and communication, the sense for order and arrangement, the
impulse to attract others and the opposed impulse to startle others.
Each of these theories of conditions must clearly connect itself with
one or the other of the just-named three theories of art's origin; for
had music, taken in our sense and independently, existed as the orig-
inal art, one could hardly regard imitation as the psychological root
of art. All in all, art and the play-instinct seem most closely linked;
that is also true, moreover, of its development with the child.

I come now to the fundamental problems of artistic creation. It
is they which present the most obstinate difficulties to a thorough
and exact investigation, for experiment and the questionnaire —

which aims at least at objectivity — are but crude means to the end in view. At the present day, as earlier, there is no lack of very refined, penetrating, — nay, brilliant analyses. They have a very superior value; but this has no special significance for the present status of the science of æsthetics, and for this reason our survey may omit much which yet has an interest for individuals.

The influence of heredity and environment on the artist's talent offers rich material for research. It is conceded, though, that how the most material and the most spiritual of influences, inherited disposition and fortune, the chances of descent and of intercourse with one's fellows, — how all this is fused into a unified personality, can be established only in individual cases by the biographer. A second very productive source of material in this field has appeared in Lombroso's teaching. The days of the most violent controversies lie behind us. It is the general view that genius and madness are near allied in their expression, that greatness often breaks forth in questionable forms; yet the majority perceive an essential difference ; the genius points onward, the mind diseased harks back; the one has purposive significance, the other not. After these more introductory inquiries, the real work begins. It has to show in what points every gift for art coincides with generally disseminated abilities, and just where the specific power sets in, which the inartistic person lacks. Take, for example, the memory. We retain this or that fact without, in principle, any selection; the remembrance of the artist, on the contrary, is dissociative — it favors what is needful for its own ends. The memory of the painter battens on forms and colors, the consciousness of the musician is filled with melodies, the fancy of the poet lives in verbal images. Also there is, especially with the poet, a peculiar understanding for human experience. In truth, the fanciful products of the imagination are but the starting-point for the soul-knowledge of the poet. Without going into details we may say that by such penetrating and delimiting analyses the superficial theory of inspiration is refuted. Out of date, too, is the notion that the artist creates by putting things together; on the contrary his fancy has the whole before the parts, it gives to the world an organism, within which the members gradually emerge. Finally, the old theory is no longer held, according to which the work of art is already complete in the inner man, and afterwards merely brought to light. More definite explanation is given by the doctrine of the way in which the artistic creation runs its course, which Eduard v. Hartmann has skillfully portrayed.

The distinction, differentiation, and comparison of the special arts offers opportunity and material for numberless special studies. Music is here the least fully represented, since it is only exceptionally that art-philosophers feel a drawing to it. So much the more, how-

ever, are they inclined to the study of poetry. They are even beginning to make use, for poetics, of the studies in the modern psychology of language, since it is acknowledged that language is the essential element, and thus more than the mere form of expression, of the poetic art. Th. A. Meyer has thrown an apple of discord into the question whether the poet's words must, in order to arouse pleasure, also awake an image. As a matter of fact, the æsthetic value does not depend on the chance-aroused sense-images, but on the language itself and the images which belong to it alone; for the most part the understanding of the words alone is enough to give the reader pleasure in the poetic treatment. In the general theory of the visually representative arts there are two opposed doctrines. The one emphasizes the common element, and believes to have found it in the so-called *Fernbild*, or distant image; the other seeks salvation in complete separations — as, for instance, of the so-called *Griffel-kunst*, or graphic art, from painting. Only the future can decide between them.

The existence of the total field of art as an essential factor of human endeavors involves difficulties which must be removed partly in the philosophical consideration, partly in law and governmental practice. The last factor must also be taken account of in theory; for so long as we do not live in an ideal world, the state will claim regulation of all activities expressing themselves in it, and so also of art. In first line it is concerned for art's relation to morality. Secondly, the social problems arise: does art bind men together, or part them? does it reconcile or intensify oppositions? is it democratic or aristocratic? is it a necessity or a luxury? does it further or reject patriotic, ethical, pedagogical ends? The artistic education of youth and the race has become a burning question. Ruskin and Morris have developed from art-critics to critics of the social order, and Tolstoï has contracted the democratic point of view to the most extreme degree. With the desire to transform art from the privilege of the few to the possession of all is, finally, bound up the wish that art shall emerge from another seclusion — that it shall not be throned in museums and libraries, in theatres and concert-halls, but shall mingle with our daily domestic life, and direct and color every act of the scholar as of the peasant.

A satisfactory decision can be reached only by him who keeps in view that art presents something extremely complex, and by no means mere æsthetic form; that, on the other hand, the æsthetic life is not banished to the sacred circle of the independent arts. With this conclusion we return to the first words of our reflections herein presented.

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SHORT PAPERS

A short paper was contributed by Professor A. D. F. Hamlin, of Columbia University, on the "Sources of Savage Conventional Patterns." The speaker said that, in the exhibit of the Department of the Interior, two glass cases displayed side by side the handiwork of the American Indian of one hundred years ago and of to-day. In the Fine Arts palace the blankets and basketry of the Navahoes were shown beside the leather work and other handicrafts of white Americans. In both instances the contrast between the savage and the civilized work emphasizes the fact that civilization tends to stifle or destroy the decorative instinct. The savage art is spontaneous, instructive, unpremeditated. The work of the civilized artist is thoughtful, carefully elaborated, intellectual. Among these peoples both the crafts and the patterns are traditional, and there is little or no ambition to innovate. The forms and combinations we admire in their work are the result of long-continued processes of evolution and elimination in which, as in the world of living organisms, the fittest have survived. The structure of savage patterns is almost always extremely simple. There are three theories advanced to account for them: that they were invented out of hand; that they were evolved out of the technical processes, tools, and materials of primitive industry; that they are descended from fetish or animistic representations of natural forms. The first is the common view of laymen; the second was first expressed (though chiefly with reference to civilized art) by Semper; and the third is widely entertained by anthropologists.

The savage instinct for decoration has probably developed from primitive animism — from that fear of the powers of nature, and that confounding of the animate and inanimate world which is universally recognized as a primitive trait. But once awakened in even the slightest degree, it has found exercise in the operations of primitive industry, and given existence to a long series of repetitive forms produced in weaving, basketry, string-lashing, and carving. The two classes of patterns thus originated — those derived from the imitation of nature under fetish ideas, and those derived from technical processes — have invariably converged, overlapping at last in many forms of decorative art, so that the real origin of a given pattern may be dual. Myths have invariably arisen to explain the origin of the technical patterns, which have received magic significance and names, in accordance with savage tendency to assign magical powers to all visible or at least to all valued objects: all savage art is talismanic. One ought to be cautious about dogmatizing as to origins in dealing with savage art, because both the phenomenon of what I call convergence in ornament evolution, and that of the myths, poetic faculty, and habit among savages, tend to confuse and obscure the real origin of the patterns with which they deal. And finally, for the artist as distinguished from the archæologist and the theorist, the real lesson of savage art is not in its origins, but in its products; in the strength, simplicity, admirable distribution, and high decorative effects of poor and despised peoples. Savage all-over patterns and Greek carved ornament and decorative sculpture represent the opposed poles of decorative design, and both are of fundamental value as objects of study for the designer.

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CHARLES EMILE PICARD, LL.D.,

Professor of Algebra and Analysis, University of Paris (Sorbonne).

HEINRICH MASCHKE, Ph.D.,

Associate Professor of Mathematics, University of Chicago.

E. H. MOORE, Ph.D., LL.D.,

Professor of Mathematics, University of Chicago.

DEPARTMENT II — MATHEMATICS

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CHAIRMAN: PROFESSOR HENRY S. WHITE, Northwestern University.
SPEAKERS: PROFESSOR MAXIME BÔCHER, Harvard University.
PROFESSOR JAMES P. PIERPONT, Yale University.

THE Chairman of the Department of Mathematics was Professor Henry S. White, of Northwestern University. In opening the proceedings Professor White said:

"Influenced by patriotism and by pride in material progress, cities and whole nations meet and celebrate the building of bridges, the opening of long railways, the tunneling of difficult mountain passes, the acquisition of new territories, or commemorate with festivity the discovery of a continent. These things are real and significant to us all.

"In the realm of ideas also there are events of no less moment, discoveries and conquests that greatly enlarge the empire of human reason. In the lapse of a century there may be many such notable achievements, even in the domain of a single science.

"Mathematics is a science continually expanding; and its growth, unlike some political and industrial events, is attended by universal acclamation. We are wont to-day, as devotees of this noble and useful science, to pass in review the newest phases of her expansion, — I say *newest*, for in retrospect a century is but brief, — and to rejoice in the deeds of the past. At the same time, also, we turn an eye of aspiration and resolution towards the mountains, rivers, deserts, and the obstructing seas that are to test the mathematicians of the future."

THE FUNDAMENTAL CONCEPTIONS AND METHODS OF MATHEMATICS

BY PROFESSOR MAXIME BÔCHER

[**Maxime Bôcher,** Professor of Mathematics, Harvard University. b. August 28, 1867, Boston, Mass. A.B. Harvard, 1888; Ph.D. Göttingen, 1891. Instructor, Assistant Professor and Professor, Harvard University, 1891–. Fellow of the American Academy. **Author of** *Ueber die Reihenentwickelungen der Potentialtheorie;* and various papers on mathematics.]

I. *Old and New Definitions of Mathematics*

I AM going to ask you to spend a few minutes with me in considering the question: what is mathematics? In doing this I do not propose to lay down dogmatically a precise definition; but rather, after having pointed out the inadequacy of traditional views, to determine what characteristics are common to the most varied parts of mathematics but are not shared by other sciences, and to show how this opens the way to two or three definitions of mathematics, any one of which is fairly satisfactory. Although this is, after all, merely a discussion of the meaning to be attached to a name, I do not think that it is unfruitful, since its aim is to bring unity into the fundamental conceptions of the science with which we are concerned. If any of you, however, should regard such a discussion of the meaning of words as devoid of any deeper significance, I will ask you to regard this question as merely a bond by means of which I have found it convenient to unite what I have to say on the fundamental conceptions and methods of what, with or without definition, we all of us agree to call mathematics.

The old idea that mathematics is the science of quantity, or that it is the science of space and number, or indeed that it can be characterized by any enumeration of several more or less heterogeneous objects of study, has pretty well passed away among those mathematicians who have given any thought to the question of what mathematics really is. Such definitions, which might have been intelligently defended at the beginning of the nineteenth century, became obviously inadequate as subjects like projective geometry, the algebra of logic, and the theory of abstract groups were developed; for none of these has any necessary relation to quantity (at least in any ordinary understanding of that word), and the last two have no relation to space. It is true that such examples have had little effect on the more or less orthodox followers of Kant, who regard mathematics as concerned with those conceptions which

are obtained by direct intuition of time and space without the aid of empirical observation. This view seems to have been held by such eminent mathematicians as Hamilton and DeMorgan; and it is a very difficult position to refute, resting as it does on a purely metaphysical foundation which regards it as certain that we can evolve out of our inner consciousness the properties of time and space. According to this view the idea of quantity is to be deduced from these intuitions; but one of the facts most vividly brought home to pure mathematicians during the last half-century is the fatal weakness of intuition when taken as the logical source of our knowledge of number and quantity.[1]

The objects of mathematical study, even when we confine our attention to what is ordinarily regarded as *pure* mathematics are, then, of the most varied description; so that, in order to reach a satisfactory conclusion as to what really characterizes mathematics, one of two methods is open to us. On the one hand we may seek some hidden resemblance in the various objects of mathematical investigation, and having found an aspect common to them all we may fix on this as the one true object of mathematical study. Or, on the other hand, we may abandon the attempt to characterize mathematics by means of its *objects of study*, and seek in its *methods* its distinguishing characteristic. Finally, there is the possibility of our combining these two points of view. The first of these methods is that of Kempe, the second will lead us to the definition of Benjamin Peirce, while the third has recently been elaborated at great length by Russell. Other mathematicians have naturally followed out more or less consistently the same ideas, but I shall nevertheless take the liberty of using the names Kempe, Peirce, and Russell as convenient designations for these three points of view. These different methods of approaching the question lead finally to results which, without being identical, still stand in the most intimate relation to one another, as we shall now see. Let us begin with the second method.

II. *Peirce's Definition*

More than a third of a century ago Benjamin Peirce wrote:[2] *Mathematics is the science which draws necessary conclusions.* According to this view there is a mathematical element involved in every inquiry in which exact reasoning is used. Thus, for instance,[3] a jury listening to the attempt of the counsel for the prisoner to prove an alibi in a criminal case might reason as follows: " If the witnesses

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[1] I refer here to such facts as that there exist continuous functions without derivatives, whereas the direct untutored intuition of space would lead any one to believe that every continuous curve has tangents.

[2] *Linear Associative Algebra.* Lithographed 1870. Reprinted in the *American Journal of Mathematics*, vol. IV.

[3] This illustration was suggested by the remarks by J. Richard, *Sur la philosophie des mathmèatiques.* Paris, Gauthier-Villars, 1903, p. 50.

are telling the truth when they say that the prisoner was in St. Louis at the moment the crime was committed in Chicago, and if it is true that a person cannot be in two places at the same time, it follows that the prisoner was not in Chicago when the crime was committed." This, according to Peirce, is a bit of mathematics; while the further reasoning by which the jury would decide whether or not to believe the witnesses, and the reasoning (if they thought any necessary) by which they would satisfy themselves that a person cannot be in two places at once, would be inductive reasoning, which can give merely a high degree of probability to the conclusion, but never certainty. This mathematical element may be, as the example just given shows, so slight as not to be worth noticing from a practical point of view. This is almost always the case in the transactions of daily life and in the observational sciences. If, however, we turn to such subjects as chemistry and mineralogy, we find the mathematical element of considerable importance, though still subordinate. In physics and astronomy its importance is much greater. Finally in geometry, to mention only one other science, the mathematical element predominates to such an extent that this science has been commonly rated a branch of pure mathematics, whereas, according to Peirce, it is as much a branch of applied mathematics as is, for instance, mathematical physics.

It is clear from what has just been said that, from Peirce's point of view, mathematics does not necessarily concern itself with quantitative relations, and that any subject becomes capable of mathematical treatment as soon as it has secured data from which important consequences can be drawn by exact reasoning. Thus, for example, even though psychologists be right when they assure us that sensations and the other objects with which they have to deal cannot be measured, we need still not necessarily despair of one day seeing a mathematical psychology, just as we already have a mathematical logic.

I have said enough, I think, to show what relation Peirce's conception of mathematics has to the applications. Let us then turn to the definition itself and examine it a little more closely. You have doubtless already noticed that the phrase, "the science which draws necessary conclusions," contains a word which is very much in need of elucidation. What is a *necessary* conclusion? Some of you will perhaps think that the conception here involved is one about which, in a concrete case at least, there can be no practical diversity of opinion among men with well-trained minds; and in fact when I spoke a few minutes ago about the reasoning of the jurymen when listening to the lawyer trying to prove an alibi, I assumed tacitly that this is so. If this really were the case, no further discussion would be necessary, for it is not my purpose to enter into

any purely philosophical speculations. But unfortunately we cannot dismiss the matter in this way; for it has happened not infrequently that the most eminent men, including mathematicians, have differed as to whether a given piece of reasoning was exact or not; and, what is worse, modes of reasoning which seem absolutely conclusive to one generation no longer satisfy the next, as is shown by the way in which the greatest mathematicians of the eighteenth century used geometric intuition as a means of drawing what they regarded as necessary conclusions.[1]

I do not wish here to raise the question whether there is such a thing as absolute logical rigor, or whether this whole conception of logical rigor is a purely psychological one bound to change with changes in the human mind. I content myself with expressing the belief, which I will try to justify a little more fully in a moment, that as we never have found an immutable standard of logical rigor in the past, so we are not likely to find it in the future. However this may be, so much we can say with tolerable confidence, as past experience shows, that no reasoning which claims to be exact can make any use of intuition, but that it must proceed from definitely and completely stated premises according to certain principles of formal logic. It is right here that modern mathematicians break sharply with the tradition of *a priori* synthetic judgments (that is, conclusions drawn from intuition) which, according to Kant, form an essential part of mathematical reasoning.

If then we agree that "necessary conclusions" must, in the present state of human knowledge, mean conclusions drawn according to certain logical principles from definitely and completely stated premises, we must face the question as to what these principles shall be. Here, fortunately, the mathematical logicians from Boole down to C. S. Peirce, Schröder, and Peano have prepared the field so well that of late years Peano and his followers [2] have been able to make a rather short list of logical conceptions and principles upon which it would seem that all exact reasoning depends.[3] We must remember, however, when we are tempted to put implicit confidence in certain fundamental logical principles, that, owing to their extreme generality and abstractness, no very great weight can be attached to the mere fact that these principles appeal to us as obviously

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[1] All writers on elementary geometry from Euclid down almost to the close of the nineteenth century use intuition freely, though usually unconsciously, in obtaining results which they are unable to deduce from their axioms. The first few demonstrations of Euclid are criticised from this point of view by Russell in his *Principles of Mathematics*, vol. I, 404–407. Gauss's first proof (1799) that every algebraic equation has a root gives a striking example of the use of intuition in what was intended as an absolutely rigorous proof by one of the greatest and at the same time most critical mathematical minds the world has ever seen.

[2] And, independently, Frege.

[3] It is not intended to assert that a single list has been fixed upon. Different writers naturally use different lists.

true; for, as I have said, other modes of reasoning which are now universally recognized as faulty have appealed in just this way to the greatest minds of the past. Such confidence as we feel must, I think, come from the fact that those modes of reasoning which we trust have withstood the test of use in an immense number of cases and in very many fields. This is the severest test to which any theory can be put, and if it does not break down under it we may feel the greatest confidence that, at least in cognate fields, it will prove serviceable. But we can never be sure. The accepted modes of exact reasoning may any day lead to a contradiction which would show that what we regard as universally applicable principles are in reality applicable only under certain restrictions.[1]

To show that the danger which I here point out is not a purely fanciful one, it is sufficient to refer to a very recent example. Independently of one another, Frege and Russell have built up the theory of arithmetic from its logical foundations. Each starts with a definite list of apparently self-evident logical principles, and builds up a seemingly flawless theory. Then Russell discovers that his logical principles when applied to a very general kind of logical *class* lead to an absurdity; and both Frege and Russell have to admit that something is wrong with the foundations which looked so secure. Now there is no doubt that these logical foundations will be somehow recast to meet this difficulty, and that they will then be stronger than ever before.[2] But who shall say that the same thing will not happen again?

It is commonly considered that mathematics owes its certainty to its reliance on the immutable principles of formal logic. This, as we have seen, is only half the truth imperfectly expressed. The other half would be that the principles of formal logic owe such degree of permanence as they have largely to the fact that they have been tempered by long and varied use by mathematicians. "A vicious circle!" you will perhaps say. I should rather describe it as an example of the process known to mathematicians as the method of successive approximations. Let us hope that in this case it is really a convergent process, as it has every appearance of being.

But to return to Peirce's definition. From what are these neces-

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[1] If the view which I here maintain is correct, it follows that if the term "absolute logical rigor" has a meaning, and if we should some time arrive at this absolute standard, the only indication we should ever have of the fact would be that for a long period, several thousand years let us say, the logical principles in question had stood the test of use. But this state of affairs might equally well mean that during that time the human mind had degenerated, at least with regard to some of its functions. Consider, for instance, the twenty centuries following Euclid when, without doubt, the high tide of exact thinking attained during Euclid's generation had receded.

[2] Cf. Poincaré's view in *La Science et l'Hypothèse*, p. 179, according to which a theory never renders a greater service to science than when it breaks down.

sary conclusions to be drawn? The answer clearly implied is, from any premises sufficiently precise to make it possible to draw necessary conclusions from them. In geometry, for instance, we have a large number of intuitions and fixed beliefs concerning the nature of space: it is homogeneous and isotropic, infinite in extent in every direction, etc.; but none of these ideas, however clearly defined they may at first sight seem to be, gives any hold for exact reasoning. This was clearly perceived by Euclid, who therefore proceeded to lay down a list of axioms and postulates, that is, specific facts which he assumes to be true, and from which it was his object to deduce all geometric propositions. That his success here was not complete is now well known, for he frequently assumes unconsciously further data which he derives from intuition; but his attempt was a monumental one.

III. *The Abstract Nature of Mathematics*

Now a further self-evident point, but one to which attention seems to have been drawn only during the last few years, is this: since we are to make no use of intuition, but only of a certain number of explicitly stated premises, it is not necessary that we should have any idea what the nature of the objects and relations involved in these premises is.[1] I will try to make this clear by a simple example. In plane geometry we have to consider, among other things, points and straight lines. A point may have a peculiar relation to a straight line which we express by the words, the point lies on the line. Now one of the fundamental facts of plane geometry is that two points determine a line, that is, if two points are given, there exists one and only one line on which both points lie. All the facts that I have just stated correspond to clear intuitions. Let us, however, eliminate our intuition of what is meant by a point, a line, a point lying on a line. A slight change of language will make it easy for us to do this. Instead of points and lines, let us speak of two different kinds of objects, say A-objects and B-objects; and instead of saying that a point lies on a line we will simply say that an A-object bears a certain relation R to a B-object. Then the fact that two points determine a line will be expressed by saying: If any two A-objects are given, there exists one and only one B-object to which they both bear the relation R. This statement, while it does not force on us any specific intuitions, will serve as a basis for mathematical reasoning [2] just as well as the more familiar statement where the terms *points* and *lines*

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[1] This was essentially Kempe's point of view in the papers to be referred to presently. In the geometric example which follows it was clearly brought out by H. Wiener: *Jahresbericht d. deutschen Mathematiker-Vereinigung*, vol. I (1891), p. 45.

[2] In conjunction, of course, with further postulates with which we need not here concern ourselves.

are used. But more than this. Our *A*-objects, our *B*-objects, and our relation *R* may be given an interpretation, if we choose, very different from that we had at first intended.

We may, for instance, regard the *A*-objects as the straight lines in a plane, the *B*-objects as the points in the same plane (either finite or at infinity), and when an *A*-object stands in the relation *R* to a *B*-object, this may be taken to mean that the line passes through the point. Our statement would then become: Any two lines being given, there exists one and only one point through which they both pass. Or we may regard the *A*-objects as the men in a certain community, the *B*-objects as the women, and the relation of an *A*-object to a *B*-object as friendship. Then our statement would be: In this community any two men have one, and only one, woman friend in common.

These examples are, I think, sufficient to show what is meant when I say that we are not concerned in mathematics with the nature of the objects and relations involved in our premises, except in so far as their nature is exhibited in the premises themselves. Accordingly mathematicians of a critical turn of mind, during the last few years, have adopted more and more a purely nominalistic attitude towards the objects and relations involved in mathematical investigation. This is, of course, not the crude mixture of nominalism and empiricism of the philosopher Hobbes, whose claim to mathematical fame, it may be said in passing, is that of a circle-squarer.[1] The nominalism of the present-day mathematician consists in treating the objects of his investigation and the relations between them as mere symbols. He then states his propositions, in effect, in the following form: If there exist any objects in the physical or mental world with relations among themselves which satisfy the conditions which I have laid down for my symbols, then such and such facts will be true concerning them.

It will be seen that, according to Peirce's view, the mathematician *as such* is in no wise concerned with the source of his premises or with their harmony or lack of harmony with any part of the external world. He does not even assert that any objects really exist which correspond to his symbols. Mathematics may therefore be truly said to be the most abstract of all sciences, since it does not deal directly with reality.[2]

This, then, is Peirce's definition of mathematics. Its advantages in the direction of unifying our conception of mathematics and of assigning to it a definite place among the other sciences are clear.

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[1] Hobbes practically obtains as the ratio of a circumference to its diameter the value $\sqrt{10}$. Cf. for instance Molesworth's edition of Hobbes's English Works, vol. VII, p. 431.

[2] Cf. the very interesting remarks along this line of C. S. Peirce in *The Monist*, vol. VII, pp. 23–24.

What are its disadvantages? I can see only two. First that, as has been already remarked, the idea of drawing necessary conclusions is a slightly vague and shifting one. Secondly, that it lays exclusive stress on the rigorous logical element in mathematics and ignores the intuitional and other non-rigorous tendencies which form an important element in the great bulk of mathematical work concerning which I shall speak in greater detail later.

IV. *Geometry an Experimental Science*

Some of you will also regard it as an objection that there are subjects which have almost universally been regarded as branches of mathematics but are excluded by this definition. A striking example of this is geometry, I mean the science of the actual space we live in; for though geometry is, according to Peirce's definition, preëminently a mathematical science, it is not exclusively so. Until a system of axioms is established mathematics cannot begin its work. Moreover, the actual perception of spatial relations, not merely in simple cases but in the appreciation of complicated theorems, is an essential element in geometry which has no relation to mathematics as Peirce understands the term. The same is true, to a considerable extent, of such subjects as mechanical drawing and model-making, which involve, besides small amounts of physics and mathematics, mainly non-mathematical geometry. Moreover, although the mathematical method is the traditional one for arriving at the truth concerning geometric facts, it is not the only one. Direct appeal to the intuition is often a short and fairly safe cut to geometric results; and on the other hand experiments may be used in geometry, just as they are used every day in physics, to test the truth of a proposition or to determine the value of some geometric magnitude.[1]

We must, then, admit, if we hold to Peirce's view, that there is an independent science of geometry just as there is an independent science of physics, and that either of these *may* be treated by mathematical methods. Thus geometry becomes the simplest of the natural sciences, and its axioms are of the nature of physical laws, to be tested by experience and to be regarded as true only within the limits of error of observation. This view, while it has not yet gained universal recognition, should, I believe, prevail, and geometry be recognized as a science independent of mathematics, just as psychology is gradually being recognized as an independent science and not as a branch of philosophy.

The view here set forth, according to which geometry is an experimental science like physics or chemistry, has been held ever

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[1] I am thinking of measurements and observations made on accurately constructed drawings and models. A famous example is Galileo's determination of the area of a cycloid by cutting out a cycloid from a metallic sheet and weighing it.

since Gauss's time by almost all the leading mathematicians who have been conversant with non-Euclidean geometry.[1] Recently, however, Poincaré has thrown the weight of his great authority against this view,[2] claiming that the experiments by which it is sought to test the truth of geometric axioms are really not geometrical experiments at all but physical ones, and that any failure of these experiments to agree with the ordinary geometrical axioms could be explained by the inaccuracy of the *physical* laws ordinarily assumed. There is undoubtedly an important element of truth here. Every experiment depends for its results not merely on the law we wish to test, but also on other laws which for the moment we assume to be true. But, if we prefer, we may, in many cases, assume as true the law we were before testing and our experiment will then serve to test some of the remaining laws. If, then, we choose to stick to the ordinary Euclidean axioms of geometry in spite of what any future experiments may possibly show, we can do so, but at the cost, perhaps, of our present simple physical laws, not merely in one branch of physics but in several. Poincaré's view[3] is that it will always be expedient to preserve simple geometric laws at all costs, an opinion for which I fail to see sufficient reason.

V. *Kempe's Definition*

Let us now turn from Peirce's method of defining mathematics to Kempe's, which, however, I shall present to you in a somewhat modified form.[4] The point of view adopted here is to try to define mathematics, as other sciences are defined, by describing the objects with which it deals. The diversity of the objects with which mathematics is ordinarily supposed to deal being so great, the first step must be to divest them of what is unessential for the mathematical treatment, and to try in this way to discover their common and characteristic element.

The first point on which Kempe insists is that the objects of mathematical discussion, whether they be the points and lines of geometry, the numbers real or complex of algebra or analysis, the elements of groups or anything else, are always individuals, infinite in number perhaps, but still distinct individuals. In a particular mathematical investigation we may, and usually do, have several different kinds of individuals; as for instance, in elementary plane geometry, points, straight lines, and circles. Furthermore, we have to deal with certain relations of these objects to one another. For instance, in the example

[1] Gauss, Riemann, Helmholtz are the names which will carry perhaps the greatest weight.
[2] Cf. *La Science et l'Hypothèse.* Paris, 1903.
[3] L. c., chapter v. In particular, p. 93.
[4] Kempe has set forth his ideas in rather popular form in the *Proceedings of the London Mathematical Society,* vol. xxvi (1894), p. 5; and in *Nature,* vol. xliii (1890), p. 156, where references to his more technical writings will be found.

just cited, a given point may or may not lie on a given line; a given line may or may not touch a given circle; three or more points may or may not be collinear, etc. This example shows how in a single mathematical problem a large number of relations may be involved, relations some of which connect two objects, others three, etc. Moreover these relations may connect like or they may connect unlike objects; and finally the order in which the objects are taken is not by any means immaterial in general, as is shown by the relation between three points which states that the third is collinear with and lies between the first two.

But even this is not all; for, besides these objects and relations of various kinds, we often have *operations* by which objects can be combined to yield another object, as, for instance, addition or multiplication of numbers. Here the objects combined and the resulting object are all of the same kind, but this is by no means necessary. We may, for instance, consider the operation of combining two points and getting the perpendicular bisector of the line connecting them; or we may combine a point and a line and get the perpendicular dropped from the point on the line.

These few examples show how diverse the relations and operations, as well as the objects of mathematics, seem at first sight to be. Out of this apparent diversity it is not difficult to obtain a very great uniformity by simply restating the facts in a little different language. We shall find it convenient to indicate that the objects $a, b, c, \ldots$, taken in the order named, satisfy a relation R by simply writing $R(a, b, c, \ldots)$, where it should be understood that among the objects $a, b, c, \ldots$ the same object may occur a number of times. On the other hand, if two objects a and b are combined to yield a third object c, we may write $a \circ b = c$,[1] where the symbol $\circ$ is characteristic of the special operation with which we are concerned.

Let us first notice that the equation $a \circ b = c$ denotes merely that the three objects a, b, c bear a certain relation to one another, say $R(a, b, c)$. In other words the idea of an operation or law of combination between the objects we deal with, however convenient and useful it may be as a matter of notation, is essentially merely a way of expressing the fact that the objects combined bear a certain relation to the object resulting from their combination. Accordingly, in a purely abstract discussion like the present, where questions of practical convenience are not involved, we need not consider such rules of combination.[2]

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[1] I speak here merely of dyadic operations, — i. e., of operations by which two objects are combined to yield a third, — these being by far the most important as well as the simplest. What is said, however, obviously applies to operations by which any number of objects are combined.

[2] Even from the point of view of the technical mathematician it may sometimes be desirable to adopt the point of view of a relation rather than that of an operation. This is seen, for instance, in laying down a system of postulates for the

Furthermore, it is easy to see that when we speak of objects of different *kinds*, as, for instance, the points and lines of geometry, we are introducing a notion which can very readily be expressed in our relational notation. For this purpose we need merely to introduce a further relation which is satisfied by two or more objects when and only when they are of the same "kind."

Let us turn finally to the relations themselves. It is customary to distinguish here between dyadic relations, triadic relations, etc., according as the relation in question connects two objects, three objects, etc. There are, however, relations which may connect any number of objects, as, for instance, the relation of collinearity which may hold between any number of points. Any relation holds for certain ordered groups of objects but not for others, and it is in no way *necessary* for us to fix our attention on the fact, if it be true, that the number of objects in all the groups for which a particular relation holds is the same. This is the point of view we shall adopt, and we shall relegate the property that a relation is dyadic, triadic, etc., to the background along with the various other properties relations may have,[1] all of which must be taken account of in the proper place.

We are thus concerned in any mathematical investigation, from our present point of view, with just two conceptions: first a set, or as the logicians say, a *class* of objects $a, b, c, \ldots$; and secondly a class of relations $R, S, T, \ldots$. We may suppose these objects divested of any qualitative, quantitative, spatial, or other attributes which they may have had, and regard them merely as satisfying or not satisfying the relations in question, where, again, we are wholly indifferent to the nature which these relations originally had. And now we are in a position to state what I conceive to be really the essential point in Kempe's definition of mathematics, although I have omitted one of the points on which he insists most strongly,[2] by saying:

If we have a certain class of objects and a certain class of relations, and if the only questions which we investigate are whether ordered groups of these objects do or do not satisfy the relations, the results of the investigation are called mathematics.

theory of abstract groups (cf., for example, Huntington, *Bulletin of the American Mathematical Society*, June, 1902), where the postulate:
    If a and b belong to the class, $a \circ b$ belongs to the class,
which in this form looks indecomposable, immediately breaks up, when stated in the relational form, into the following two:
    1. If a and b belong to the class, there exists an element c of the class such that $R(a, b, c)$.
    2. If a, b, c, d belong to the class, and if $R(a, b, c)$ and $R(a, b, d)$, then $c = d$.
    [1] For instance, the property of symmetry. A relation is said to be symmetrical if it holds or fails to hold independently of the *order* in which the objects are taken.
    [2] Namely, that the only relation that need be considered is that of being "indistinguishable," *i. e.*, a symmetrical and transitive relation between two groups of objects.

It is convenient to have a term to designate a class of objects associated with a class of relations between these objects. Such an aggregate we will speak of as a *mathematical system*. If now we have two different mathematical systems, and if a one-to-one correspondence can be set up between the two classes of objects, and also between the two classes of relations in such a way that whenever a certain ordered set of objects of the first system satisfies a relation of that system, the set consisting of the corresponding objects of the second system satisfies the corresponding relation of that system, and *vice versa*, then it is clear that the two systems are, from our present point of view, mathematically equivalent, however different the nature of the objects and relations may be in the two cases.[1] To use a technical term, the two systems are *simply isomorphic*.[2]

It will be noticed that in the definition of mathematics just given nothing is said as to the method by which we are to ascertain whether or not a given relation holds between the objects of a given set. The method used may be a purely empirical one, or it may be partly or wholly deductive. Thus, to take a very simple case, suppose our class of objects to consist of a large number of points in a plane and suppose the only relation between them with which we are concerned is that of collinearity. Then, if the points are given us by being marked in ink on a piece of white paper, we can begin by taking three pins, sticking them into the paper at three of the points; then, by sighting along them, we can determine whether or not these points are collinear. We can do the same with other groups of three points, then with all groups of four points, etc. The same result can be obtained with much less labor if we make use of certain simple properties which the relation of collinearity satisfies, properties which are expressed by such propositions as:

$R(a, b, c)$ implies $R(b, a, c)$,

$R(a, b, c, d)$ implies $R(a, b, c)$,

$R(a, b, c)$ and $R(a, b, d)$ together imply $R(a, b, c, d)$, etc.

By means of a small number of propositions of this sort it is easy to show that no empirical observations as to the collinearity of groups of more than three points need be made, and that it may not be necessary to examine even all groups of three points. Having

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[1] The point of view here brought out, including the term isomorphism, was first developed in a special case, — the theory of groups.

[2] Inasmuch as the relations in a mathematical system are themselves objects, we may, if we choose, take our class of objects so as to include these relations as well as what we called objects before, some of which, we may remark in passing, may themselves be relations. Looked at from this point of view, we need one additional relation which is now the only one which we explicitly call a relation. If we denote this relation by inclosing the objects which satisfy it in parentheses, then if the relation denoted before by $R(a, b)$ is satisfied, we should now write (R, a, b), whereas we should *not* have (a, R, b) (S, R, a, b), etc. Thus we see that any mathematical system may be regarded as consisting of a class of objects and a *single* relation between them.

made this relatively small number of observations, the remaining results would be obtained deductively. Finally, we may suppose the points given by their coördinates, in which case the complete answer to our question may be obtained by the purely deductive method of analytic geometry.

According to the modified form of Kempe's definition which I have just stated, mathematics is not necessarily a deductive science. This view, while not in accord with the prevailing ideas of mathematicians, undoubtedly has its advantages as well as its dangers. The non-deductive processes, of which I shall have more to say presently, play too important a part in the life of mathematics to be ignored, and the definition just given has the merit of not excluding them. It would seem, however, that the definition in the form just given is too broad. It would include, for instance, the determination by experimental methods of what pairs of chemical compounds of the known elements react on one another when mixed under given conditions.

VI. *Axioms and Postulates. Existence Theorems*

If, however, we restrict ourselves to exact or deductive mathematics, it will be seen that Kempe's definition becomes coextensive with Peirce's. Here, in order to have a starting-point for deductive reasoning, we must assume a certain number of facts or *primitive propositions* concerning any mathematical system we wish to study, of which all other propositions will be necessary consequences.[1] We touch here on a subject whose origin goes back to Euclid and which has of late years received great development, primarily at the hands of Italian mathematicians.[2]

It is important for us to notice at this point that not merely these primitive propositions but all the propositions of mathematics may be divided into two great classes. On the one hand, we have propositions which state that certain specified objects satisfy certain specified relations. On the other hand are the *existence theorems*, which state that there exist objects satisfying, along with certain specified objects, certain specified relations.[3] These two classes of propositions are well known to logicians and are designated by them

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[1] These primitive propositions may be spoken of as *axioms* or *postulates*, according to the point of view we wish to take concerning their source, the word axiom, which has been much misused of late, indicating an intuitional or empirical source.

[2] Peano, Pieri, Padoa, Burali-Forti. We may mention here also Hilbert, who, apparently without knowing of the important work of his Italian predecessors, has also done valuable work along these lines.

[3] Or we might conceivably have existence theorems which state that there exist relations which are satisfied by certain specified objects; or these two kinds of existence theorems might be combined. If we take the point of view explained in the second footnote on p. 467, all existence theorems will be of the type mentioned in the text.

universal and *particular* propositions respectively.[1] It is only during
the last fifty years or so that mathematicians have become conscious
of the fundamental importance in their science of existence theorems,
which until then they had frequently assumed tacitly as they needed
them, without always being conscious of what they were doing.

It is sometimes held by non-mathematicians that if mathematics
were really a purely deductive science, it could not have gained
anything like the extent which it has without losing itself in trivial-
ities and becoming, as Poincaré puts it, a vast tautology.[2] This
view would doubtless be correct if all primitive propositions were
universal propositions. One of the most characteristic features of
mathematical reasoning, however, is the use which it makes of aux-
iliary elements. I refer to the auxiliary points and lines in proofs
by elementary geometry, the quantities formed by combining in
various ways the numbers which enter into the theorems to be
proved in algebra, etc. Without the use of such auxiliary elements
mathematicians would be incapable of advancing a step; and
whenever we make use of such an element in a proof, we are in reality
using an existence theorem.[3] These existence theorems need not,
to be sure, be among the primitive propositions; but if not, they must
be deduced from primitive propositions some of which are existence
theorems, for it is clear that an existence theorem cannot be deduced
from universal propositions alone.[4] Thus it may fairly be said that
existence theorems form the vital principle of mathematics, but these
in turn, it must be remembered, would be impotent without the
material basis of universal propositions to work upon.

VII. *Russell's Definition*

We have so far arrived at the view that exact mathematics is
the study by deductive methods of what we have called a mathe-
matical system, that is, a class of objects and a class of relations
between them. If we elaborate this position in two directions we
shall reach the standpoint of Russell.[5]

In the first place Russell makes precise the term *deductive method*

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[1] "All men are mortals" is a standard example of a universal proposition;
while as an illustration of a particular proposition is often given: "Some men are
Greeks." That this is really an existence theorem is seen more clearly when we
state it in the form: "There exists at least one man who is a Greek."

[2] Cf. *La Science et l'Hypothèse*, p. 10.

[3] Even when in algebra we consider the sum of two numbers $a + b$, we are using
the existence theorem which says that, any two numbers a and b being given,
there exists a number c which stands to them in the relation which we indicate in
ordinary language by saying that c is the sum of a and b.

[4] The power which resides in the method of mathematical induction, so called,
comes from the fact that this method depends on an existence theorem. It is,
however, not the only fertile principle in mathematics as Poincaré would have
us believe (cf. *La Science et l'Hypothèse*). In fact there are great branches of
mathematics, like elementary geometry, in which it takes little or no part.

[5] *The Principles of Mathematics*, Cambridge, England, 1903.

by laying down explicitly a list of logical conceptions and principles which alone are to be used; and, secondly, he insists,[1] on the contrary, that no mathematical system, to use again the technical term introduced above, be studied in pure mathematics whose existence cannot be established solely from the logical principles on which all mathematics is based. Inasmuch as the development of mathematics during the last fifty years has shown that the existence of most, if not all the mathematical systems which have proved to be important can be deduced when once the existence of positive integers is granted, the point about which interest must centre here is the proof, which Russell attempts, of the existence of this latter system.[2] This proof will necessarily require that, among the logical principles assumed, existence theorems be found. Such theorems do not seem to be explicitly stated by Russell, the existence theorems which make their appearance further on being evolved out of somewhat vague philosophical reasoning. There are also other reasons, into which I cannot enter here, why I am not able to regard the attempt made in this direction by Russell as completely successful.[3] Nevertheless, in view of the fact that the system of finite positive integers is necessary in almost all branches of mathematics (we cannot speak of a triangle or a hexagon without having the numbers three and six at our disposal), it seems extremely desirable that the system of logical principles which we lay at the foundation of all mathematics be assumed, if possible, broad enough so that the existence of positive integers — at least finite integers — follows from it; and there seems little doubt that this can be done in a satisfactory manner. When this has been done we shall perhaps be able to regard, with Russell, pure mathematics as consisting exclusively of deductions "by logical principles from logical principles."

VIII. *The Non-Deductive Elements in Mathematics*

I fear that many of you will think that what I have been saying is of an extremely one-sided character, for I have insisted merely on the rigidly deductive form of reasoning used and the purely abstract character of the objects considered in mathematics. These, to the great majority of mathematicians, are only the dry bones of the science. Or, to change the simile, it may perhaps be said that instead of inviting you to a feast I have merely shown you the empty dishes

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[1] In the formal definition of mathematics at the beginning of the book this is not stated or in any way implied; and yet it comes out so clearly throughout the book that this is a point of view which the author regards as essential, that I have not hesitated to include it as a part of his definition.

[2] Cf. also Burali-Forti, *Congrès internationale de philosophie.* Paris, vol. III, p. 289.

[3] Russell's unequivocal repudiation of nominalism in mathematics seems to me a serious if not an insurmountable barrier to progress.

and explained how the feast would be served if only the dishes were filled.[1] I fully agree with this opinion, and can only plead in excuse that my subject was the *fundamental* conceptions and methods of mathematics, not the infinite variety of detail and application which give our science its real vitality. In fact I should like to subscribe most heartily to the view that in mathematics, as elsewhere, the discussion of such fundamental matters derives its interest mainly from the importance of the theory of which they are the so-called foundations.[2] I like to look at mathematics almost more as an art than as a science; for the activity of the mathematician, constantly creating as he is, guided though not controlled by the external world of the senses, bears a resemblance, not fanciful I believe but real, to the activity of an artist, of a painter let us say. Rigorous deductive reasoning on the part of the mathematician may be likened here to technical skill in drawing on the part of the painter. Just as no one can become a good painter without a certain amount of this skill, so no one can become a mathematician without the power to reason accurately up to a certain point. Yet these qualities, fundamental though they are, do not make a painter or a mathematician worthy of the name, nor indeed are they the most important factors in the case. Other qualities of a far more subtle sort, chief among which in both cases is imagination, go to the making of the good artist or good mathematician. I must content myself merely by recalling to you this somewhat vague and difficult though interesting field of speculation which arises when we attempt to attach *value* to mathematical work, a field which is familiar enough to us all in the analogous case of artistic or literary criticism.

We are in the habit of speaking of logical rigor and the consideration of axioms and postulates as the foundations on which the superb structure of modern mathematics rests; and it is often a matter of wonder how such a great edifice can rest securely on such a small foundation. Moreover, these foundations have not always seemed so secure as they do at present. During the first half of the nineteenth century certain mathematicians of a critical turn of mind — Cauchy, Abel, Weierstrass, to mention the greatest of them — perceived to their dismay that these foundations were not sound, and some of the best efforts of their lives were devoted to strengthening and improving them. And yet I doubt whether the great results of mathematics

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[1] Notice that just as the empty dishes could be filled by a great variety of viands, so the empty symbols of mathematics can be given meanings of the most varied sorts.

[2] Cf. the following remark by Study, *Jahresbericht der deutschen Mathematiker-Vereinigung*, vol. xi (1902), p. 313:

"So wertvoll auch Untersuchungen über die systematische Stellung der mathematischen Grundbegriffe sind . . . *wertvoller* ist doch noch der materielle Inhalt der einzelnen Disciplinen, um dessentwillen allein ja derartige Untersuchungen überhaupt Zweck haben. . . ."

seemed less certain to any of them because of the weakness they perceived in the foundations on which these results are built up. The fact is that what we call mathematical rigor is merely one of the foundation stones of the science; an important and essential one surely, yet not the only thing upon which we can rely. A science which has developed along such broad lines as mathematics, with such numerous relations of its parts both to one another and to other sciences, could not long contain serious error without detection. This explains how, again and again, it has come about, that the most important mathematical developments have taken place by methods which cannot be wholly justified by our present canons of mathematical rigor, the logical "foundation" having been supplied only long after the superstructure had been raised. A discussion and analysis of the non-deductive methods which the creative mathematician really uses would be both interesting and instructive. Here I must content myself with the enumeration of a few of them.

First and foremost there is the use of intuition, whether geometrical, mechanical, or physical. The great service which this method has rendered and is still rendering to mathematics both pure and applied is so well known that a mere mention is sufficient.

Then there is the method of experiment; not merely the physical experiments of the laboratory or the geometrical experiments I had occasion to speak of a few minutes ago, but also arithmetical experiments, numerous examples of which are found in the theory of numbers and in analysis. The mathematicians of the past frequently used this method in their printed works. That this is now seldom done must not be taken to indicate that the method itself is not used as much as ever.

Closely allied to this method of experiment is the method of analogy, which assumes that something true of a considerable number of cases will probably be true in analogous cases. This is, of course, nothing but the ordinary method of induction. But in mathematics induction may be employed not merely in connection with the experimental method, but also to extend results won by deductive methods to other analogous cases. This use of induction has often been unconscious and sometimes overbold, as, for instance, when the operations of ordinary algebra were extended without scruple to infinite series.

Finally there is what may perhaps be called the method of optimism, which leads us either willfully or instinctively to shut our eyes to the possibility of evil. Thus the optimist who treats a problem in algebra or analytic geometry will say, if he stops to reflect on what he is doing: "I know that I have no right to divide by zero; but there are so many other values which the expression by which I am dividing might have that I will assume that the Evil One has not

thrown a zero in my denominator this time." This method, if a proceeding often unconscious can be called a method, has been of great service in the rapid development of many branches of mathematics, though it may well be doubted whether in a subject as highly developed as is ordinary algebra it has not now survived its usefulness.[1]

While no one of these methods can in any way compare with that of rigorous deductive reasoning as a method upon which to base mathematical results, it would be merely shutting one's eyes to the facts to deny them their place in the life of the mathematical world, not merely of the past but of to-day. There is now, and there always will be room in the world for good mathematicians of every grade of logical precision. It is almost equally important that the small band whose chief interest lies in accuracy and rigor should not make the mistake of despising the broader though less accurate work of the great mass of their colleagues; as that the latter should not attempt to shake themselves wholly free from the restraint the former would put upon them. The union of these two tendencies in the same individuals, as it was found, for instance, in Gauss and Cauchy, seems the only sure way of avoiding complete estrangement between mathematicians of these two types.

[1] Cf. the very suggestive remarks by Study, *Jahresbericht d. Deutschen Mathematiker-Vereinigung*, vol. xi (1902), p. 100, footnote, in which it is pointed out how rigor, in cases of this sort, may not merely serve to increase the correctness of the result, but actually to suggest new fields for mathematical investigation.

THE HISTORY OF MATHEMATICS IN THE NINETEENTH CENTURY

BY PROFESSOR JAMES P. PIERPONT OF YALE UNIVERSITY

The extraordinary development of mathematics in the last century is quite unparalleled in the long history of this most ancient of sciences. Not only have those branches of mathematics which were taken over from the eighteenth century steadily grown, but entirely new ones have sprung up in almost bewildering profusion, and many of these have promptly assumed proportions of vast extent.

As it is obviously impossible to trace in the short time allotted to me the history of mathematics in the nineteenth century even in merest outline, I shall restrict myself to the consideration of some of its leading theories.

Theory of Functions of a Complex Variable

Without doubt one of the most characteristic features of mathematics in the last century is the systematic and universal use of the complex variable. Most of its great theories received invaluable aid from it, and many owe their very existence to it. What would the theory of differential equations or elliptic functions be to-day without it, and is it probable that Poncelet, Steiner, Chasles, and von Staudt would have developed synthetic geometry with such elegance and perfection without its powerful stimulus?

The necessities of elementary algebra kept complex numbers persistently before the eyes of every mathematician. In the eighteenth century the more daring, as Euler and Lagrange, used them sparingly; in general one avoided them when possible. Three events, however, early in the nineteenth century changed the attitude of mathematicians toward this mysterious guest. In 1813 Argand published his geometric interpretation of complex numbers. In 1824 came the discovery by Abel of the imaginary period of the elliptic function. Finally Gauss in his second memoir on biquadratic residues (1832) proclaims them a legitimate and necessary element of analysis.

The theory of function of a complex variable may be said to have had its birth when Cauchy discovered his integral theorem

$$\int f(x)dx = 0$$

published in 1825. In a long series of publications beginning with the *Cours d'Analyse* (1821), Cauchy gradually developed his theory of functions and applied it to problems of the most diverse nature;

for example, existence theorems for implicit functions and the solutions of certain differential equations, the development of functions in infinite series and products, and the periods of integrals of one and many valued functions.

Meanwhile Germany is not idle; Weierstrass and Riemann develop Cauchy's theory along two distinct and original paths. Weierstrass starts with an explicit analytical expression, a power series, and defines his function as the totality of its analytical continuations. No appeal is made to geometric intuition, his entire theory is strictly arithmetical. Riemann growing up under Gauss and Dirichlet not only relies largely on geometric intuition, but he also does not hesitate to impress mathematical physics into his service. Two noteworthy features of his theory are the many leaved surfaces named after him, and the extensive use of conformal representation.

The history of functions as first developed is largely a theory of algebraic functions and their integrals. A general theory of functions is only slowly evolved. For a long time the methods of Cauchy, Riemann, and Weierstrass were cultivated along distinct lines by their respective pupils. The schools of Cauchy and Riemann were the first to coalesce. The entire rigor which has recently been imparted to their methods has removed all reason for founding, as Weierstrass and his school have urged, the theory of functions on a single algorithm, namely, the power series. We may therefore say that at the close of the century there is only one theory of functions in which the ideas of its three great creators are harmoniously united.

Let us note briefly some of its lines of advance. Weierstrass early observed that an analytic expression might represent different analytic functions in different regions. Associated with this is the phenomenon of natural boundaries. The question therefore arose, What is the most general domain of definition of an analytic function? Runge has shown that any connected region may serve this purpose. An important line of investigation relates to the analytic expression of a function by means of infinite series, products, and fractions. Here may be mentioned Weierstrass's discovery of prime factors; the theorems of Mittag-Leffler and Hilbert; Poincaré's uniformization of algebraic and analytic functions by means of a third variable, and the work of Stieljes, Padé, and Van Vleck on infinite fractions. Since an analytic function is determined by a single power series, which in general has a finite circle of convergence, two problems present themselves: determine, first, the singular points of the analytic function so defined, and, second, an analytic expression valid for its whole domain of definition. The celebrated memoir of Hadamard inaugurated a long series of investigations on the first problem; while Mittag-Leffler's star theorem is the most important result yet obtained relating to the second.

Another line of investigation relates to the work of Poincaré, Borel, Padé, *et al.*, on divergent series. It is, indeed, a strange vicissitude of our science that these series which early in the century were supposed to be banished once and for all from rigorous mathematics should at its close be knocking at the door for readmission.

Let us finally note an important series of memoirs on integral transcendental functions, beginning with Weierstrass, Laguerre, and Poincaré.

Algebraic Functions and their Integrals

A branch of the theory of functions has been developed to such an extent that it may be regarded as an independent theory; we mean the theory of algebraic functions and their integrals. The brilliant discoveries of Abel and Jacobi in the elliptic functions from 1824 to 1829 prepared the way for a similar treatment of the hyperelliptic case. Here a difficulty of gravest nature was met. The corresponding integrals have $2p$ linearly independent periods; but as Jacobi had shown, a one valued function having more than two periods admits a period as small as we choose. It therefore looked as if the elliptic functions admitted no further generalization. Guided by Abel's theorem, Jacobi at last discovered the solution to the difficulty (1832); to get functions analogous to the elliptic functions we must consider functions not of one but of p independent variables, namely, the p independent integrals of the first species. The great problem now before mathematicians, known as Jacobi's Problem of Inversion, was to extend this *aperçu* to the case of any algebraic configuration and develop the consequences. The first to take up this immense task were Weierstrass and Riemann, whose results belong to the most brilliant achievements of the century. Among the important notions hereby introduced we note the following: the birational transformation, rank of an algebraic configuration, class invariants, prime functions, the theta and multiply periodic functions in several variables. Of great importance is Riemann's method of proving existence theorems, as also his representation of algebraic functions by means of integrals of the second species.

A new direction was given to research in this field by Clebsch, who considered the fundamental algebraic configuration as defining a curve. His aim was to bring about a union of Riemann's ideas and the theory of algebraic curves for their mutual benefit. Clebsch's labors were continued by Brill and Nöther; in their work the transcendental methods of Riemann are placed quite in the background. More recently Klein and his school have sought to unite the transcendental methods of Riemann with the geometric direction begun by Clebsch, making systematic use of homogeneous coördinates and

the invariant theory. Noteworthy, also, is his use of normal curves in $(p-1)$ way space. to represent the given algebraic configuration. Dedekind and Weber, Hensel and Landsberg, have made use of the ideal theory with marked success. Many of the difficulties of the older theory, *e. g.*, the resolution of singularities of the algebraic configuration, are treated with a truly remarkable ease and generality.

In the theory of multiply periodic functions and the general θ functions we mention, besides Weierstrass, the researches of Prym, Krazer, Frobenius, Poincaré, and Wirtinger.

Automorphic Functions

Closely connected with the elliptic functions is a class of functions which has come into great prominence in the last quarter of a century, namely, the elliptic modular and automorphic functions. Let us consider first the modular functions of which the modulus κ and the absolute invariant J are the simplest types.

The transformation theory of Jacobi gave algebraic relations between such functions in endless number. Hermite, Fuchs, Dedekind, and Schwarz are forerunners, but the theory of modular functions as it stands to-day is principally due to Klein and his school. Its goal is briefly stated thus: Determine all sub-groups of the linear group

$$(1) \qquad\qquad x^1 = \frac{\alpha x + \beta}{\gamma x + \delta}$$

where α, β, γ, δ are integers and $\alpha\delta - \beta\gamma = 1$; determine for each such group associate modular functions and investigate their relation to one another and especially to J. Important features in this theory are the congruence groups of (1); the fundamental polygon belonging to a given sub-group, and its use as substitute for a Riemann surface; the principle of reflection over a circle, the modular forms.

The theory of automorphic functions is due to Klein and Poincaré. It is a generalization of the modular functions; the coefficients in (1) being any real or imaginary numbers, with non-vanishing determinant, such that the group is discontinuous. Both authors have recourse to non-Euclidean geometry to interpret the substitutions (1). Their manner of showing the existence of functions belonging to a given group is quite different. Poincaré by a brilliant stroke of genius actually writes down their arithmetic expressions in terms of his celebrated θ series. Klein employs the existence methods of Riemann. The relation of automorphic functions to differential equations is studied by Poincaré in detail. In particular, he shows that both variables of a linear differential equation with algebraic coefficients can be expressed uniformly by their means.

Differential Equations

Let us turn now to another great field of mathematical activity, the theory of differential equations. The introduction of the theory of functions has completely revolutionized this subject. At the beginning of the nineteenth century many important results had indeed been established, particularly by Euler and Lagrange; but the methods employed were artificial, and broad comprehensive principles were lacking. By various devices one tried to express the solution in terms of the elementary functions and quadratures — a vain attempt; for as we know now, the goal they strove so laboriously to reach was in general unattainable.

A new epoch began with Cauchy, who by means of his new theory of functions first rigorously established the existence of the solution of certain classes of equations in the vicinity of regular points. He also showed that many of the properties of the elliptic functions might be deduced directly from their differential equations. Ere long, the problem of integrating a differential equation changed its base. Instead of seeking to express its solution in terms of the elementary functions and quadratures, one asked what is the nature of the functions defined by a given equation. To answer this question we must first know what are the singular points of the integral function and how does it behave in their vicinity. The number of memoirs on this fundamental and often difficult question is enormous; but this is not strange if we consider the great variety of interesting and important classes of equations which have to be studied.

One of the first to open up this new path was Fuchs, whose classic memoirs (1866-68) gave the theory of linear differential equations its birth. These equations enjoy a property which renders them particularly accessible, namely, the absence of movable singular points. They may, however, possess points of indetermination, to use Fuchs's terminology, and little progress has been made in this case. Noteworthy in this connection is the introduction by v. Koch of infinite determinants, first considered by our distinguished countryman Hill; also the use of divergent series — that invention of the Devil, as Abel called them — by Poincaré. A particular class of linear differential equations of great importance is the hypergeometric equation; the results obtained by Gauss, Kummer, Riemann, and Schwarz relating to this equation have had the greatest influence on the development of the general theory. The vast extent and importance of the theory of linear differential equations may be estimated when we recall that within its borders it embraces not only almost all the elementary functions, but also the modular and automorphic functions.

Too important to pass over in silence is the subject of algebraic

differential equations with uniform solutions. The brilliant researches of Poinlevé deserve especial mention.

Another field of great importance, especially in mathematical physics, relates to the determination of the solution of differential equations with assigned boundary conditions. The literature of this subject is enormous; we may therefore be pardoned if mention is made only of the investigation of our countrymen Bôcher, Van Vleck, and Porter.

Since 1870 the theory of differential equations has been greatly advanced by Lie's theory of groups. Assuming that an equation or a system of equations admits one or more infinitesimal transformations, Lie has shown how they may be employed to simplify the problem of integration. In many cases they give us exact information how to conduct the solution and upon what system of auxiliary equations the solution depends. One of the most striking illustrations of this is the theory of ordinary linear differential equations which Picard and Vessiot have developed, analogous to Galois's theory for algebraic equations. An interesting result of this theory is a criterion for the solution of such equations by quadratures. As an application, we find that Ricatti's equation cannot be solved by quadratures. The attempts to effect such a solution of this celebrated equation in the century before were therefore necessarily in vain.

A characteristic feature of Lie's theories is the prominence given to the geometrical aspects of the questions involved. Lie thinks in geometrical images, the analytical formulation comes afterwards. Already Morge had shown how much might be gained in geometrizing the problem of integration. Lie has gone much farther in this direction. Besides employing all the geometrical notions of his predecessors extended to n-way space, he has introduced a variety of new conceptions, chief of which are his surface element and contact transformations.

He has also used with great effect Plücker's line geometry, and his own sphere geometry in the study of certain types of partial differential equations of the first and second orders which are of great geometrical interest, for example, equations whose characteristic curves are lines of curvature, geodesics, etc. Let us close by remarking that Lie's theories not only afford new and valuable points of view for attacking old problems, but also give rise to a host of new ones of great interest and importance.

Groups

We turn now to the second dominant idea of the century, the group concept.

Groups first became objects of study in algebra when Lagrange (1770), Ruffini (1799), and Abel (1826) employed substitution groups

with great advantage in their work on the quintic. The enormous importance of groups in algebra was, however, first made clear by Galois, whose theory of the solution of algebraic equations is one of the great achievements of the century. Its influence has stretched far beyond the narrow bounds of algebra.

With an arbitrary but fixed domain of rationality, Galois observed that every algebraic equation has attached to it a certain group of substitutions. The nature of the auxiliary equations required to solve the given equation is completely revealed by an inspection of this group.

Galois's theory showed the importance of determining the subgroups of a given substitution group, and this problem was studied by Cauchy, Serret, Matthieu, Kirkmann, and others. The publication of Jordan's great treatise in 1870 is a noteworthy event. It collects and unifies the results of his predecessors and contains an immense amount of new matter.

A new direction was given to the theory of groups by the introduction by Cayley of abstract groups (1854, 1878). The work of Sylow, Hölder and Frobenius, Burnside and Miller, deserve especial notice.

Another line of research relates to the determination of the finite groups in the linear group of any number of variables. These groups are important in the theory of linear differential equations with algebraic solutions, in the study of certain geometrical problems as the points of inflection of a cubic, the twenty-seven lines on a surface of the third order, in crystallography, etc. They also enter prominently into Klein's Formen-problem. An especially important class of finite linear groups are the congruence groups first considered by Galois. Among the laborers in the field of linear groups, we note Jordan, Klein, Moore, Maschke, Dickson, Frobenius, and Wiman.

Up to the present we have considered only groups of finite order. About 1870 entirely new ideas coming from geometry and differential equations give the theory of groups an unexpected development. Foremost in this field are Lie and Klein.

Lie discovers and gradually perfects his theory of continuous transformation groups and shows their relations to many different branches of mathematics. In 1872 Klein publishes his *Erlanger Programme* and in 1877 begins his investigations on elliptic modular functions, in which infinite *discontinuous* groups are of primary importance, as we have already seen. In the now famous *Programme*, Klein asks what is the principle which underlies and unifies the heterogeneous geometrical methods then in vogue, as, for example, the geometry of the ancients, whose figures are rigid and invariable; the modern projective geometry, whose figures are in ceaseless flux passing from one form to another; the geometries of Plücker and Lie, in which the elements of space are no longer points, but line

spheres, or other configurations at pleasure, the geometry of birational transformation, the analysis situs, etc., etc. Klein finds this answer: In each geometry we have a system of objects and a group which transforms these objects one into another. We seek the invariants of this group. In each case it is the abstract group and not the concrete objects which is essential. The fundamental rôle of a group in geometrical research is thus made obvious. Its importance is the solution of algebraic equation, in the theory of differential equations in the automorphic functions we have already seen. The immense theory of algebraic invariants developed by Cayley and Sylvester, Aronhold, Clebsch, Gordan, Hermite, Brioschi, and a host of zealous workers in the middle of the century, also finds its place in the far more general invariant theory of Lie's theory of groups. The same is true of the theory of surfaces, so far as it rests on the theory of differential forms. In the theory of numbers, groups have many important applications, for example, in the composition of quadratic forms and the cyclotomic bodies. Finally, let us note the relation between hypercomplex numbers and continuous groups discovered by Poincaré.

In résumé, we may thus say that the group concept, hardly noticeable at the beginning of the century, has at its close become one of the fundamental and most fruitful notions in the whole range of our science.

Infinite Aggregates

Leaving the subject of groups, we consider now briefly another fundamental concept, namely, infinite aggregates. In the most diverse mathematical investigations we are confronted with such aggregates. In geometry the conceptions of curves, surface, region, frontier, etc., when examined carefully, lead us to a rich variety of aggregates. In analysis they also appear, for example, the domain of definition of an analytic function, the points where a function of a real variable ceases to be continuous or to have a differential coefficient, the points where a series of functions ceases to be uniformly convergent, etc.

To say an aggregate (not necessarily a point aggregate) is infinite is often an important step; but often again only the first step. To penetrate farther into the problem may require us to state *how* infinite. This requires us to make distinctions in infinite aggregates, to discover fruitful principles of classification, and to investigate the properties of such classes.

The honor of having done this belongs to George Cantor. The theory of aggregates is for the most part his creation; it has enriched mathematical science with fundamental and far-reaching notions and results.

The theory falls into two parts; a theory of aggregates in general,

and a theory of point aggregates. In the theory of point aggregates the notion of limiting points gives rise to important classes of aggregates as discrete, dense, everywhere dense, complete, perfect, connected, etc., which are so important in the function theory.

In the general theory two notions are especially important, namely, the one to one correspondence of the elements of two aggregates, and well-ordered aggregates. The first leads to cardinal numbers and the idea of enumerable aggregates, the second to transfinite or ordinal numbers.

Two striking results of Cantor's theory are these: the algebraic and therefore the rational numbers, although everywhere dense, are enumerable; and secondly, one-way and n-way space have the same cardinal number.

Cantor's theory has already found many applications, especially in the function theory, where it is to-day an indispensable instrument of research.

Functions of Real Variables — The Critical Movement

One of the most conspicuous and distinctive features of mathematical thought in the nineteenth century is its critical spirit. Beginning with the calculus, it soon permeates all analysis, and toward the close of the century it overhauls and recasts the foundation of geometry and aspires to further conquests in mechanics and in the immense domains of mathematical physics.

Ushered in with Lagrange and Gauss just at the close of the eighteenth century, the critical movement receives its first decisive impulse from the teachings of Cauchy, who in particular introduces our modern definition of limit and makes it the foundation of the calculus. We must also mention in this connection Abel, Bolzano, and Dirichlet. Especially Abel adopted the reform ideas of Cauchy with enthusiasm, and made important contributions in infinite series.

The figure, however, which towers above all others in this movement, whose name has become an epithet of rigor, is Weierstrass. Beginning at the very foundations, he creates an arithmetic of real and complex numbers, assuming the theory of positive integers to be given. The necessity of this is manifest when we recall that until then the simplest properties of radicals and logarithms were utterly devoid of a rigorous foundation; so, for example,

$$\sqrt{2}\ \sqrt{5} = \sqrt{10} \qquad\qquad \log 2 + \log 5 = \log 10$$

Characteristic of the pre-Weierstrassean era is the loose way in which geometrical and other intuitional ideas were employed in the demonstration of analytical theorems. Even Gauss is open to this criticism. The mathematical world received a great shock when Weierstrass showed them an example of a continuous function

without a derivative, and Hankel and Cantor, by means of their principle of condensation of singularities, could construct analytic expressions for functions having in any interval however small an infinity of points of oscillation, an infinity of points in which the differential coefficient is altogether indeterminate, or an infinity of points of discontinuity. Another rude surprise was Cantor's discovery of the one to one correspondence between the points of a unit segment and a unit square, followed up by Peano's example of a space-filling curve.

These examples and many others made it very clear that the ideas of a curve, a surface region, motion, etc., instead of being clear and simple, were extremely vague and complex. Until these notions had been cleared up, their admission in the demonstration of an analytical theorem was therefore not to be tolerated. On a purely arithmetical basis, with no appeal to our intuition, Weierstrass develops his stately theory of functions which culminates in the theory of Abelian and multiply periodic functions.

But the notion of rigor is relative and depends on what we are willing to admit either tacitly or explicitly. As we observed, Gauss, whose rigor was the admiration of his contemporaries, freely admitted geometrical notions. This Weierstrass would criticise. On the other hand, Weierstrass has made a grave oversight: he nowhere shows that his definitions relative to the number he introduces do not involve mutual contradictions. If he replied that such contradictions would involve contradictions in the theory of positive integers, one might ask what assurance have we that such contradictions may not actually exist. A flourishing young school of mathematical logic has recently grown up under the influence of Peano. They have investigated with marked success the foundations of analysis and geometry, and in particular have attempted to show the non-contradictoriness of the axioms of our number-system by making them depend on the axioms of logic, which axioms we must admit, to reason at all.

The critical spirit, which in the first half of the century was to be found in the writings of only a few of the foremost mathematicians, has in the last quarter of the century become almost universal, at least in analysis. A searching examination of the foundation of arithmetic and the calculus has brought to light the insufficiency of much of the reasoning formerly considered as conclusive. It became necessary to build up these subjects anew. The theory of irrational numbers invented by Weierstrass has been supplanted by the more flexible theories of Dedekind and Cantor. Stolz has given us a systematic and rigorous treatment of arithmetic. The calculus has been completely overhauled and arithmetized by Thomae, Harnack, Peano, Stolz, Jordan, and Vallée-Poussin.

Leaving the calculus, let us notice briefly the theory of functions of real variables. The line of demarcation between these two subjects is extremely arbitrary. We might properly place in the latter all those finer and deeper questions relating to the number-system; the study of our curve, surface, and other geometrical notions, the peculiarities that functions present with reference to discontinuity, oscillation, differentiation, and integration; as well as a very extensive class of investigations whose object is the greatest possible extension of the processes, concepts, and results of the calculus. Among the many not yet mentioned who have made important contributions to this subject we note: Fourier, Riemann, Stokes, Dini, Tannery, Pringsheim, Arzelà, Osgood, Broden, Ascoli, Borel, Baire, Kopke, Hölder, Volterra, and Lebesgue.

Closely related with the differential calculus is the calculus of variations; in the former the variables are given infinitesimal variations, in the latter the functions. Developed in a purely formal manner by Jacobi, Hamilton, Clebsch, and others in the first part of the century, a new epoch began with Weierstrass, who, having subjected the labors of his predecessors to an annihilating criticism, placed the theory on a new and secure foundation and so opened the path for further research by Schwarz, A. Mayer, Scheffers, v. Escherich, Kneser, Osgood, Bolza, Kobb, Zermelo, and others. At the very close of the century Hilbert has given the theory a fresh impulse by the introduction of new and powerful methods, which enable us in certain cases to neglect the second variation and simplifies the consideration of the first. As application he gives the first direct and yet simple demonstration of Dirichlet's celebrated Principle.

Theory of Numbers — Algebraic Bodies

The theory of numbers as left by Fermat, Euler, and Legendre was for the most part concerned with the solution of Diophantine equations, that is, given an equation $f(x, y, z, \ldots) = 0$ whose coefficients are integers, find all rational, and especially all integral solutions. In this problem Lagrange had shown the importance of considering the theory of forms. A new era begins with the appearance of Gauss's *Disquisitiones arithmeticae* in 1801. This great work is remarkable for three things: (1) The notion of divisibility in the form of congruences is shown to be an instrument of wonderful power; (2) the Diophantine problem is thrown in the background and the theory of forms is given a dominant rôle; (3) the introduction of algebraic numbers, namely, the roots of unity.

The theory of forms has been further developed along the lines of the *Disquisitiones* by Dirichlet, Eisenstein, Hermite, H. Smith, and Minkowski.

Another part of the theory of numbers also goes back to Gauss, namely, algebraic numerical bodies. The Law of Reciprocity of Quadratic Residues, one of the gems of the higher arithmetic, was first rigorously proved by Gauss. His attempts to extend this theorem to cubic and biquadratic residues showed that the elegant simplicity which prevailed in quadratic residues was altogether missing in these higher residues, until one passed from the domain of real integers to the domain formed of the third and fourth roots of unity. In these domains, as Gauss remarked, algebraic integers have essentially the same properties as ordinary integers. Further exploration in this new and promising field by Jacobi, Eisenstein, and others soon brought to light the fact that already in the domain formed of the twenty-third roots of unity the laws of divisibility were altogether different from those of ordinary integers; in particular, a number could be expressed as the product of prime factors in more than one way. Further progress in this direction was therefore apparently impossible.

It is Kummer's immortal achievement to make further progress possible by the invention of his ideals. These he applied to Fermat's celebrated Last Theorem and the Law of Reciprocity of Higher Residues.

The next step in this direction was taken by Dedekind and Kronecker, who developed the ideal theory for any algebraic domain. So arose the theory of algebraic numerical bodies, which has come into such prominence in the last decades of the century through the researches of Hensel, Hurwitz, Minkowski, Weber, and, above all, Hilbert.

Kronecker has gone farther, and in his classic *Grundzüge* he has shown that similar ideas and methods enable us to develop a theory of algebraic bodies in any number of variables. The notion of divisibility so important in the preceding theories is generalized by Kronecker still farther in the shape of his system of moduli.

Another noteworthy field of research opened up by Kronecker is the relation between quadratic forms with negative determinant and complex multiplication of elliptic functions. H. Smith, Gierster, Hurwitz, and especially Weber have made important contributions.

A method of great power in certain investigations has been created by Minkowski, which he called the *Geometrie der Zahlen*. Introducing a generalization of the distance function, he is led to the conception of a fundamental body (*Aichkörper*). Minkowski shows that every fundamental body is nowhere concave, and conversely to each such body belongs a distance function. A theorem of great importance is now the following: The minimum value which each distance function has at the lattice points is not greater than a certain number depending on the function chosen.

We wish finally to mention a line of investigation which makes use of the infinitesimal calculus and even the theory of functions. Here belong the brilliant researches of Dirichlet relating to the number of classes of binary forms for a given determinant, the number of primes in a given arithmetic progression; and Riemann's remarkable memoir on the number of primes in a given interval.

In this analytical side of the theory of numbers we notice also the researches of Mertens, Weber, and Hadamard.

Projective Geometry

The tendencies of the eighteenth century were predominantly analytical. Mathematicians were absorbed for the most part in developing the wonderful instrument of the calculus with its countless applications. Geometry made relatively little progress. A new era begins with Monge. His numerous and valuable contributions to analytical descriptive and differential geometry, and especially his brilliant and inspiring lectures at the Ecole Polytechnique (1795, 1809), put fresh life into geometry and prepared it for a new and glorious development in the nineteenth century.

When one passes in review the great achievements which have made the nineteenth century memorable in the annals of our science, certainly projective geometry will occupy a foremost place. Pascal, De la Hire, Monge, and Carnot are forerunners, but Poncelet, a pupil of Monge, is its real creator. The appearance of his *Traité des propriétés projectives des figures*, in 1822, gives modern geometry its birth. In it we find the line at infinity, the introduction of imaginaries, the circular points at infinity, polar reciprocation, a discussion of homology, the systematic use of projection, section, and anharmonic ratio.

While the countrymen of Poncelet, especially Chasles, do not fail to make numerous and valuable contributions to the new geometry, the next great steps in advance are made on German soil. In 1827 Möbius publishes the *Barycentrische Calcul;* Plücker's *Analytisch-geometrische Entwickelungen* appears in 1828–31 and Steiner's *Entwickelung der Abhängigkeit geometrischer Gestalten von einander* in 1832. In the ten years which embrace the publication of these immortal works of Poncelet, Plücker, and Steiner, geometry has made more real progress than in the two thousand years which had elapsed since the time of Appolonius. The ideas which had been slowly taking shape since the time of Descartes suddenly crystallized and almost overwhelmed geometry with an abundance of new ideas and principles.

To Möbius we owe the introduction of homogeneous coördinates, and the far-reaching conception of geometric transformation, including collineation and duality as special cases. To Plücker we owe the

use of the abbreviate notation which permits us to study the properties of geometric figures without the intervention of the coördinates, the introduction of line and plane coördinates, and the notion of generalized space elements. Steiner, who has been called the greatest geometer since Appolonius, besides enriching geometry in countless ways, was the first to employ systematically the method of generating geometrical figures by means of projective pencils.

Other noteworthy works belonging to this period are Plücker's *System der analytischen Geometrie* (1835), and Chasles's classic *Aperçu* (1837).

Already at this stage we notice a bifurcation in geometrical methods. Steiner and Chasles become eloquent champions of the synthetic school of geometry, while Plücker, and later Hesse and Cayley, are leaders in the analytical movement. The astonishing fruitfulness and beauty of synthetic methods threatened for a short time to drive the analytic school out of existence. The tendency of the synthetic school was to banish more and more metrical methods. In effecting this the anharmonic ratio became constantly more prominent. To define this fundamental ratio without reference to measurement, and so free projective geometry from the galling bondage of metric relations, was thus a problem of fundamental importance. The glory of this achievement, which has, as we shall see, a far wider significance, belongs to v. Staudt. Another equally important contribution of v. Staudt to synthetic geometry is his theory of imaginaries. Poncelet, Steiner, Chasles operate with imaginary elements as if they were real. Their only justification is recourse to the so-called principles of continuity or to some other equally vague principle. V. Staudt gives this theory a rigorous foundation, defining the imaginary points, lines, and planes by means of involutions without ordinal elements.

The next great advance made is the advent of the theory of algebraic invariants. Since projective geometry is the study of those properties of geometric figures which remain unaltered by projective transformations, and since the theory of invariants is the study of those forms which remain unaltered (except possibly for a numerical factor) by the group of linear substitutions, these two subjects are inseparably related and in many respects only different aspects of the same thing. It is no wonder, then, that geometers speedily applied the new theory of invariants to geometrical problems. Among the pioneers in this direction were Cayley, Salmon, Aronhold, Hesse, and especially Clebsch.

Finally we must mention the introduction of the line as a space element. Forerunners are Grassmann (1844) and Cayley (1859), but Plücker in his memoirs of 1865, and his work *Neue Geometrie des Raumes* (1868–69), was the first to show its great value by studying

complexes of the first and second order and calling attention to their application to mechanics and optics.

The most important advance over Plücker has been made by Klein, who takes as coördinates six-line complexes in involution. Klein also observed that line geometry may be regarded as a point geometry on a quadric in five-way space. Other laborers in this field are Clebsch, Reye, Segre, Sturm, and Königs.

Differential Geometry

During the first quarter of the century this important branch of geometry was cultivated chiefly by the French. Monge and his school study with great success the generation of surfaces in various ways, the properties of envelopes, evolutes, lines of curvature, asymptotic lines, skew curves, orthogonal systems, and especially the relation between the surface theory and partial differential equations.

The appearance of Gauss's *Disquisitiones generales circa superficies curvas*, in 1828, marks a new epoch. Its wealth of new ideas has furnished material for countless memoirs, and given geometry a new direction. We find here the parametric representation of a surface, the introduction of curvilinear coördinates, the notion of spherical image, the Gaussian measure of curvature, and a study of geodesics. But by far the most important contributions that Gauss makes in this work is the consideration of a surface as a flexible, inextensible film or membrane, and the importance given quadratic differential forms.

We consider now some of the lines along which differential geometry has advanced. The most important is perhaps the theory of differential quadratic forms with their associate invariants and parameters. We mention here Lamé, Beltrami, Menardi, Codazzi, Christoffel, and Weingarten.

An especially beautiful application of this theory is the immense subject of applicability and deformation of surfaces, in which Minding, Bauer, Beltrami, Weingarten, and Voss have made important contributions.

Intimately related with the theory of applicability of two surfaces is the theory of surfaces of constant curvature which play so important a part in non-Euclidean geometry. We mention here the work of Minding, Beltrami, Dini, Backlund, and Lie.

The theory of rectilinear congruences has also been the subject of important researches from the standpoint of differential geometry. First studied by Monge as a system of normals to a surface and then in connection with optics by Malus, Dupin, and Hamilton, the general theory has since been developed by Kummer, Ribaucour, Guichard, Darboux, Voss, and Weingarten. An important application of this theory is the infinitesimal deformation of a surface.

Minimum surfaces have been studied by Monge, Bonnet, and Enneper. The subject owes its present extensive development principally to Weierstrass, Riemann, Schwarz, and Lie. In it we find harmoniously united the theory of surfaces, the theory of functions, the calculus of variations, the theory of groups, and mathematical physics.

Another extensive division of differential geometry is the theory of orthogonal systems, of such importance in physics. We note especially the investigations of Dupin, Jacobi, Darboux, Combescure, and Bianchi.

Other Branches of Geometry

Under this head we group a number of subjects too important to pass over in silence, yet which cannot be considered at length for lack of time.

In the first place is the immense subject of algebraic curves and surfaces. To develop adequately all the important and elegant properties of curves and surfaces of the second order alone would require a bulky volume. In this line of ideas would follow curves and surfaces of higher order and class. Their theory is far less complete, but this lack it amply makes good by offering an almost bewildering variety of configurations to classify and explore. No single geometer has contributed more to this subject than Cayley.

A theory of great importance is the geometry on a curve or surface inaugurated by Clebsch in 1863.

Expressing the coördinates of a plane cubic by means of elliptic functions and employing their addition theorems, he deduced with hardly any calculation Steiner's theorem relating to the inscribed polygons and various theorems concerning conics touching the curve. Encouraged by such successes, Clebsch proposed to make use of Riemann's theory of Abelian functions in the study of algebraic curves of any order. The most important result was a new classification of such curves. Instead of the linear transformation, Clebsch in harmony with Riemann's ideas employs the birational transformation as a principle of classification. From this standpoint we ask what are the properties of algebraic curves which remain invariant for such transformation.

Brill and Nöther follow Clebsch. Their method is, however, algebraical, and rests on their celebrated Residual theorem which in their hands takes the place of Abel's theorem. We mention further the investigation of Castelnuovo, Weber, Krause, and Segre. An important division of this subject is the theory of correspondences. First studied by Chasles for curves of deficiency 0 in 1864, Cayley, and, immediately after, Brill extended the theory to the case of any p. The most important advance made in later years has been made

by Hurwitz, who considers the totality of possible correspondences on an algebraic curve, making use of the corresponding integrals of the first species.

Alongside the geometry on a curve is the vastly more difficult and complicated geometry on a surface, or more generally, on any algebraic spread in n-way space. Starting from a remark of Clebsch (1868), Nöther made the first great step in his famous memoir of 1868–74. Further progress has been due to the French and Italian mathematicians. Picard, Poincaré, and Humbert make use of transcendental methods, in which figure prominently double integrals which remain finite on the surface and single integrals of total differentials. On the other hand, Enriques and Castelnuovo have attacked the subject from a more algebraic-geometric standpoint by means of linear systems of algebraic curves on the surface.

The first invariants of a surface were discovered by Clebsch and Nöther; still others have been found by Castelnuovo and Enriques in connection with irregular surfaces.

Leaving this subject, let us consider briefly the geometry of n dimensions. A characteristic of nineteenth-century mathematics is the generality of its methods and results. When such has been impossible with the elements in hand, fresh ones have been invented; witness the introduction of imaginary numbers in algebra and the function theory, the ideals of Kummer in the theory of numbers, the line and plane at infinity in projective geometry. The benefit that analysis derived from geometry was too great not to tempt mathematicians to free the latter from the narrow limits of three dimensions, and so give it the generality that the former has long enjoyed. The first pioneer in this abstract field was Grassmann (1844); we must, however, consider Cayley as the real founder of n-dimensional geometry (1869). Notable contributions have been made by the Italian school, Veronese. Segre, etc.

Non-Euclidean Geometry

Each century takes over as a heritage from its predecessor a number of problems whose solution previous generations of mathematicians have arduously but vainly sought. It is a signal achievement of the nineteenth century to have triumphed over some of the most celebrated of these problems.

The most ancient of them is the Quadrature of the Circle, which already appears in our oldest mathematical document, the *Papyrus Rhind*, B.C. 2000. Its impossibility was finally shown by Lindemann (1882).

Another famous problem relates to the solution of the quintic, which had engaged the attention of mathematicians since the middle of the sixteenth century. The impossibility of expressing its roots by

radicals was finally shown by the youthful Abel (1824), while Hermite and Kroneker (1858) showed how they might be expressed by the elliptic modular functions, and Klein (1875) by means of the icosahedral irrationality.

But of all problems which have come down from the past, by far the most celebrated and important relates to Euclid's parallel axiom. Its solution has profoundly affected our views of space, and given rise to questions even deeper and more far-reaching which embrace the entire foundation of geometry and our space conception. Let us pass in rapid review the principal events of this great movement. Wallis in the seventeenth, Seccheri, Lambert, and Legendre in the eighteenth, are the first to make any noteworthy progress before the nineteenth century. The really profound investigations of Seccheri and Lambert, strangely enough, were entirely overlooked by later writers and have only recently come to light.

In the nineteenth century non-Euclidean geometry develops along four directions, which roughly follow each other chronologically. Let us consider them in order.

The naïve-synthetic direction. —The methods employed are similar to those of Euclid. His axioms are assumed with the exception of the parallel axiom; the resulting geometry is what is now called hyperbolic or Lobatschewski's geometry. Its principal properties are deduced, in particular its trigonometry, which is shown to be that of a sphere with imaginary radius as Lambert had divined. As a specific result of these investigations the long-debated question relating to the independence of the parallel axiom was finally settled. The great names in this group are Lobatschewski, Bolyai, and Gauss. The first publications of Lobatschewski are his *Exposition succinct des principes de la géométrie* (1829), and the *Geometrische Untersuchungen*, in 1840. Bolyai's *Appendix* was published in 1832. As to the extent of Gauss's investigations, we can only judge from scattered remarks in private letters and his reviews of books relating to the parallel axioms. His dread of the *Geschrei der Boötier*, that is, the followers of Kant, prevented him from publishing his extensive speculations.

The metric-differential direction. —This is inaugurated by three great memoirs by Riemann, Helmholtz, and Beltrami, all published in the same year, 1868.

Beltrami, making use of results of Gauss and Minding relating to the applicability of two surfaces, shows that the hyperbolic geometry of a plane may be interpreted on a surface of constant negative curvature, the pseudosphere. By means of this discovery the purely logical and hypothetical system of Lobatschewski and Bolyai takes on a form as concrete and tangible as the geometry of a plane.

The work of Riemann is as original as profound. He considers space as an *n*-dimensional continuous numerical multiplicity, which

is distinguished from the infinity of other such multiplicities by certain well-defined characters. Chief of them are (1) the quadratic differential expression which defines the length of an elementary arc, and (2) a property relative to the displacements of this multiplicity about a point. There are an infinity of space multiplicities which satisfy Riemann's axioms. By extending Gauss's definition of a curvature k, of a surface at a point to curvature of *space* at a point, by considering the geodesic surfaces passing through that point, Riemann finds that all these spaces fall into three classes according as k is equal to, greater, or less than 0. For $n = 3$ and $k = 0$ we have Euclidean space; when $k < 0$ we have the space found by Gauss, Lobatschewski, and Bolyai; when $k > 0$ we have the space first considered in the long-forgotten writings of Seccheri and Lambert, in which the right line is finite.

Helmholtz, like Riemann, considers space as a numerical multiplicity. To characterize it further, Helmholtz makes use of the notions of rigid bodies and free mobility. His work has been revised and materially extended by Lie from the standpoint of the theory of groups.

In the present category also belong important papers by Newcomb and Killing.

The projective direction. — We have already noticed the efforts of the synthetic school to express metric properties by means of projective relations. In this the circular points at infinity were especially serviceable. An immense step in this direction was taken by Laguerre, who showed, in 1853, that all angles might be expressed as an anharmonic ratio with reference to these points, that is, with reference to a certain fixed conic. The next advance is made by Cayley in his famous sixth memoir on quantics, in 1859. Taking any fixed conic (or quadric, for space) which he calls the absolute, Cayley introduces two expressions depending on the anharmonic ratio with reference to the absolute. When this degenerates into the circular points at infinity, these expressions go over into the ordinary expressions for the distance between two points and the angle between two lines. Thus all metric relations may be considered as projective relations with respect to the absolute. Cayley does not seem to be aware of the relation of his work to non-Euclidean geometry. This was discovered by Klein, in 1871. In fact, according to the nature of the absolute, three geometries are possible; these are precisely the three already mentioned. Klein has made many important contributions to non-Euclidean geometry. We mention his modification of v. Staudt's definition of anharmonic ratio so as to be independent of the parallel axiom, his discovery of the two forms of Riemann's space, and finally his contributions to a class of geometries first noticed by Clifford and which are characterized by the fact that only certain of its motions affect space as a whole.

As a result of all these investigations, both in the projective as also in the metric differential direction, we are led irresistibly to the same conclusion, namely: The facts of experience can be explained by all three geometries when the constant k is taken small enough. It is, therefore, merely a question of convenience whether we adopt the parabolic, hyperbolic, or elliptic geometry.

The critical synthetic direction represents a return to the old synthetic methods of Euclid, Lobatschewski, and Bolyai, with the added feature of a refined and exacting logic. Its principal object is no longer a study of non-Euclidean but of Euclidean geometry. Its aim is to establish a system of axioms for our ordinary space which is complete, compatible, and irreducible. The fundamental terms point, line, plane, between, congruent, etc., are introduced as abstract marks whose properties are determined by inter-relations in the form of axioms. Geometric intuition has no place in this order of ideas which regards geometry as a mere division of pure logic. The efforts of this school have already been crowned with eminent success, and much may be expected from it in the future. Its leaders are Peano, Veronese, Pieri, Padoa, Burali-Forti, and Levi-Civittà, in Italy, Pasch and Hilbert in Germany, and Moore in America.

Closing at this point our hasty and imperfect survey of mathematics in the last century, let us endeavor to sum up its main characteristics. What strikes us at once is its colossal proportions and rapid growth in nearly all directions, the great variety of its branches, the generality and complexity of its methods; an inexhaustible creative imagination, the fearless introduction and employment of ideal elements, and an appreciation for a refined and logical development of all its parts.

We who stand on the threshold of a new century can look back on an era of unparalleled progress. Looking into the future, an equally bright prospect greets our eyes; on all sides fruitful fields of research invite our labor and promise easy and rich returns.

Surely this is the golden age of mathematics.

GERMAN UNIVERSITY STUDENTS

Photogravure from the Painting by Carl Heyden.

SECTION A—ALGEBRA AND ANALYSIS

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(*Hall* 9, *September* 22, 10 *a. m.*)

CHAIRMAN: PROFESSOR E. H. MOORE, University of Chicago.
SPEAKERS: PROFESSOR CHARLES EMILE PICARD, The Sorbonne; Member of the
Institute of France.
PROFESSOR HEINRICH MASCHKE, University of Chicago.
SECRETARY: PROFESSOR A. G. BLISS, University of Chicago.

ON THE DEVELOPMENT OF MATHEMATICAL ANALYSIS AND ITS RELATIONS TO SOME OTHER SCIENCES

BY CHARLES EMILE PICARD

(Translated from the French by Professor George Bruce Halsted, Kenyon College)

[Charles **Emile Picard**, Professor of Higher Algebra and Higher Analysis, University of Paris; also Professor of General Mechanics, l'Ecole Centrale des Arts et Manufactures, Paris. b. Paris, France, July 24, 1856. LL.D. Clark University, Glasgow University, University of Christiania. Member of Institute of France; Academy of Science, Berlin, St. Petersburg, Bologna, Boston, Turin, Copenhagen, Washington, and many others; Mathematical Society of London. Former President of Mathematical Society of France, Mathematical Societies of London and Kharkow, and many other mathematical societies. **Author and editor of** *Memoirs, Traits and Discussions of Mathematics; Theory of Algebraic Functions of Two Variables.*]

IT is one of the objects of a congress such as this which now brings us together, to show the bonds between the diverse parts of science taken in its most extended acceptation. So the organizers of this meeting have insisted that the relations between different sections should be put in evidence.

To undertake a study of this sort, somewhat indeterminate in character, it is necessary to forget that all is in all; in what concerns algebra and analysis, a Pythagorean would be dismayed at the extent of his task, remembering the celebrated formula of the school: " Things are numbers." From this point of view my subject would be inexhaustible.

But I, for the best of reasons, will make no such pretensions.

In casting merely a glance over the development of our science through the ages, and particularly in the last century, I hope to be able to characterize sufficiently the rôle of mathematical analysis in its relations to certain other sciences.

I

It would appear natural to commence by speaking of the concept itself of whole number; but this subject is not alone of logical order,

it is also of order historic and psychologic, and would draw us away into too many discussions.

Since the concept of number has been sifted, in it have been found unfathomable depths; thus, it is a question still pending to know, between the two forms, the cardinal number and the ordinal number, under which the idea of number presents itself, which of the two is anterior to the other, that is to say, whether the idea of number properly so called is anterior to that of order, or if it is the inverse.

It seems that the geometer-logician neglects too much in these questions psychology and the lessons uncivilized races give us; it would seem to result from these studies that the priority is with the cardinal number.

It may also be there is no general response to the question, the response varying according to races and according to mentalities.

I have sometimes thought, on this subject, of the distinction between auditives and visuals, auditives favoring the ordinal theory, visuals the cardinal.

But I will not linger on this ground full of snares; I fear that our modern school of logicians with difficulty comes to agreement with the ethnologists and biologists; these latter in questions of origin are always dominated by the evolution idea, and, for more than one of them, logic is only the résumé of ancestral experience. Mathematicians are even reproached with postulating in principle that there is a human mind in some way exterior to things, and that it has its logic. We must, however, submit to this, on pain of constructing nothing. We need this point of departure, and certainly, supposing it to have evolved during the course of prehistoric time, this logic of the human mind was perfectly fixed at the time of the oldest geometric schools, those of Greece; their works appear to have been its first code, as is expressed by the story of Plato writing over the door of his school, "Let no one not a geometer enter here."

Long before the bizarre word algebra was derived from the Arabic, expressing, it would seem, the operation by which equalities are reduced to a certain canonic form, the Greeks had made algebra without knowing it; relations more intimate could not be imagined than those binding together their algebra and their geometry, or rather, one would be embarrassed to classify, if there were occasion, their geometric algebra, in which they reason not on numbers but on magnitudes.

Among the Greeks also we find a geometric arithmetic, and one of the most interesting phases of its development is the conflict which, among the Pythagoreans, arose in this subject between number and magnitude, apropos of irrationals.

Though the Greeks cultivated the abstract study of numbers, called

by them arithmetic, their speculative spirit showed little taste for practical calculation, which they called logistic.

In remote antiquity, the Egyptians and the Chaldeans, and later the Hindus and the Arabs, carried far the science of calculation.

They were led on by practical needs; logistic preceded arithmetic, as land-surveying and geodesy opened the way to geometry; in the same way trigonometry developed under the influence of the increasing needs of astronomy.

The history of science at its beginnings shows a close relation between pure and applied mathematics; this we shall meet again constantly in the course of this study.

We have remained up to this point in the domain which ordinary language calls elementary algebra and arithmetic.

In fact, from the time that the incommensurability of certain magnitudes had been recognized, the infinite had made its appearance, and, from the time of the sophisms of Zeno on the impossibility of motion, the summation of geometric progressions must have been considered.

The procedures of exhaustion which are found in Eudoxus and in Euclid appertain already to the integral calculus, and Archimedes calculates definite integrals.

Mechanics also appeared in his treatise on the quadrature of the parabola, since he first finds the surface of the segment bounded by an arc of a parabola and its chord with the help of the theorem of moments; this is the first example of the relations between mechanics and analysis, which since have not ceased developing.,

The infinitesimal method of the Greek geometers for the measure of volumes raised questions whose interest is even to-day not exhausted.

In plane geometry, two polygons of the same area are either equivalent or equivalent-by-completion, that is to say, can be decomposed into a finite number of triangles congruent in pairs, or may be regarded as differences of polygons susceptible of such a partition.

It is not the same for the geometry of space, and we have lately learned that stereometry cannot, like planimetry, get on without recourse to procedures of exhaustion or of limit, which require the axiom of continuity or the Archimedes assumption.

Without insisting further, this hasty glance at antiquity shows how completely then were amalgamated algebra, arithmetic, geometry, and the first endeavors at integral calculus and mechanics, to the point of its being impossible to recall separately their history.

In the Middle Ages and the Renaissance, the geometric algebra of the ancients separated from geometry. Little by little algebra properly so called arrived at independence, with its symbolism and

its notation more and more perfected; thus was created this language so admirably clear, which brings about for thought a veritable economy and renders further progress possible.

This is also the moment when distinct divisions are organized.

Trigonometry, which, in antiquity, had been only an auxiliary of astronomy, is developed independently; toward the same time the logarithm appears, and essential elements are thus put in evidence.

II

In the seventeenth century, the analytic geometry of Descartes, distinct from what I have just called the geometric algebra of the Greeks by the general and systematic ideas which are at its base, and the new-born dynamic were the origin of the greatest progress of analysis.

When Galileo, starting from the hypothesis that the velocity of heavy bodies in their fall is proportional to the time, from this deduced the law of the distances passed over, to verify it afterward by experiment, he took up again the road upon which Archimedes had formerly entered and on which would follow after him Cavalieri, Fermat, and others still, even to Newton and Leibnitz. The integral calculus of the Greek geometers was born again in the kinematic of the great Florentine physicist.

As to the calculus of derivatives or of differentials, it was founded with precision apropos of the drawing of tangents.

In reality, the origin of the notion of derivative is in the confused sense of the mobility of things and of the rapidity more or less great with which phenomena happen; this is well expressed by the words *fluents* and *fluxions*, which Newton used, and which one might suppose borrowed from old Heraclitus.

The points of view taken by the founders of the science of motion, Galileo, Huygens, and Newton, had an enormous influence on the orientation of mathematical analysis.

It was with Galileo an intuition of genius to discover that, in natural phenomena, the determining circumstances of the motion produce accelerations: this must have conducted to the statement of the principle that the rapidity with which the dynamic state of a system changes depends in a determinate manner on its static state alone. In a more general way we reach the postulate that the infinitesimal changes, of whatever nature they may be, occurring in a system of bodies, depend uniquely on the actual state of this system.

In what degree are the exceptions apparent or real? This is a question which was raised only later and which I put aside for the moment.

From the principles enunciated becomes clear a point of capital

importance for the analyst: Phenomena are ruled by differential equations which can be formed when observation and experiment have made known for each category of phenomena certain physical laws.

We understand the unlimited hopes conceived from these results. As Bertrand says in the preface of his treatise, "The early successes were at first such that one might suppose all the difficulties of science surmounted in advance, and believe that the geometers, without being longer distracted by the elaboration of pure mathematics, could turn their meditations exclusively toward the study of the natural laws."

This was to admit gratuitously that the problems of analysis, to which one was led, would not present very grave difficulties.

Despite the disillusions the future was to bring, this capital point remained, that the problems had taken a precise form, and that a classification could be established in the difficulties to be surmounted.

There was, therefore, an immense advance, one of the greatest ever made by the human mind. We understand also why the theory of differential equations acquired a considerable importance.

I have anticipated somewhat, in presenting things under a form so analytic. Geometry was intermingled in all this progress. Huygens, for example, followed always by preference the ancients, and his *Horologium oscillatorium* rests at the same time on infinitesimal geometry and mechanics; in the same way, in the *Principia* of Newton, the methods followed are synthetic.

It is, above all, with Leibnitz that science takes the paths which were to lead to what we call mathematical analysis; it is he who, for the first time, in the latter years of the seventeenth century, pronounces the word function.

By his systematic spirit, by the numerous problems he treated, even as his disciples James and John Bernoulli, he established in a final way the power of the doctrines to the edification of which had successively contributed a long series of thinkers from the distant times of Eudoxus and of Archimedes.

The eighteenth century showed the extreme fecundity of the new methods. That was a strange time, the era of mathematical duels where geometers hurled defiance, combats not always without acrimony, when Leibnitzians and Newtonians encountered in the lists.

From the purely analytic point of view, the classification and study of simple functions is particularly interesting; the function idea, on which analysis rests, is thus developed little by little.

The celebrated works of Euler hold then a considerable place. However, the numerous problems which present themselves to the mathematicians leave no time for a scrutiny of principles; the

foundations themselves of the doctrine are elucidated slowly, and the *mot* attributed to d'Alembert, "Allez en avant et la foi vous viendra," is very characteristic of this epoch.

Of all the problems started at the end of the seventeenth century or during the first half of the eighteenth, it will suffice for me to recall those isoperimetric problems which gave birth to the calculus of variations.

I prefer to insist on the interpenetration still more intimate between analysis and mechanics when, after the inductive period of the first age of dynamics, the deductive period was reached where one strove to give a final form to the principles. The mathematical and formal development played then the essential rôle, and the analytic language was indispensable to the greatest extension of these principles.

There are moments in the history of the sciences and, perhaps, of society, when the spirit is sustained and carried forward by the words and the symbols it has created, and when generalizations present themselves with the least effort. Such was particularly the rôle of analysis in the formal development of mechanics.

Allow me a remark just here. It is often said an equation contains only what one has put into it. It is easy to answer, first, that the new form under which one finds the things constitutes often of itself an important discovery.

But sometimes there is more; analysis, by the simple play of its symbols, may suggest generalizations far surpassing the primitive outline. Is it not so with the principle of virtual velocities, of which the first idea comes from the simplest mechanisms; the analytic form which translates it will suggest extensions leading far from the point of departure.

In the same sense, it is not just to say analysis has created nothing, since these more general conceptions are its work. Still another example is furnished us by Lagrange's system of equations; here calculus transformations have given the type of differential equations to which one tends to carry back to-day the notion of mechanical explanation.

There are in science few examples comparable to this, of the importance of the form of an analytic relation and of the power of generalization of which it may be capable.

It is very clear that, in each case, the generalizations suggested should be made precise by an appeal to observation and experiment, then it is still the calculus which searches out distant consequences for checks, but this is an order of ideas which I need not broach here.

Under the impulse of the problems set by geometry, mechanics, and physics, we see develop or take birth almost all the great divisions of analysis. First were met equations with a single independent vari-

able. Soon appear partial differential equations, with vibrating cords, the mechanics of fluids and the infinitesimal geometry of surfaces.

This was a wholly new analytic world; the origin itself of the problems treated was an aid which from the first steps permits no wandering, and in the hands of Monge geometry rendered useful services to the new-born theories.

But of all the applications of analysis, none had then more renown than the problems of celestial mechanics set by the knowledge of the law of gravitation and to which the greatest geometers gave their names.

Theory never had a more beautiful triumph; perhaps one might add that it was too complete, because it was at this moment above all that were conceived for natural philosophy the hopes at least premature of which I spoke above.

In all this period, especially in the second half of the eighteenth century, what strikes us with admiration and is also somewhat confusing, is the extreme importance of the applications realized, while the pure theory appeared still so ill assured. One perceives it when certain questions are raised like the degree of arbitrariness in the integral of vibrating cords, which gives place to an interminable and inconclusive discussion.

Lagrange appreciated these insufficiencies when he published his theory of analytic functions, where he strove to give a precise foundation to analysis.

One cannot too much admire the marvelous presentiment he had of the rôle which the functions, which with him we call analytic, were to play; but we may confess that we stand astonished before the demonstration he believed to have given of the possibility of the development of a function in Taylor's series.

The exigencies in questions of pure analysis were less at this epoch. Confiding in intuition, one was content with certain probabilities, and agreed implicitly about certain hypotheses that it seemed useless to formulate in an explicit way; in reality, one had confidence in the ideas which so many times had shown themselves fecund, which is very nearly the *mot* of d'Alembert.

The demand for rigor in mathematics has had its successive approximations, and in this regard our sciences have not the absolute character so many people attribute to them.

III

We have now reached the first years of the nineteenth century. As we have explained, the great majority of the analytic researches had, in the eighteenth century, for occasion a problem of geometry, and especially of mechanics and of physics, and we have scarcely found the logical and æsthetic preoccupations which are to give a

physiognomy so different to so many mathematical works, above all in the latter two thirds of the nineteenth century.

Not to anticipate, however, after so many examples of the influences of physics on the developments of analysis, we meet still a new one, and one of the most memorable, in Fourier's theory of heat. He commences by forming the partial differential equations which govern temperature.

What are for a partial differential equation the conditions at the limits permitting the determination of a solution?

For Fourier, the conditions are suggested by the physical problem, and the methods that he followed have served as models to the physicist-geometers of the first half of the last century.

One of these consists in forming a series with certain simple solutions. Fourier thus obtained the first types of developments more general than the trigonometric developments, as in the problem of the cooling of a sphere, where he applies his theory to the terrestrial globe, and investigates the law which governs the variations of temperature in the ground, trying to go even as far as numerical applications.

In the face of so many beautiful results, we understand the enthusiasm of Fourier which scintillates from every line of his preliminary discourse. Speaking of mathematical analysis, he says, "There could not be a language more universal, more simple, more exempt from errors and from obscurities, that is to say, more worthy to express the invariable relations of natural things. Considered under this point of view, it is as extended as nature herself; it defines all sensible relations, measures times, spaces, forces, temperatures. This difficult science forms slowly, but it retains all the principles once acquired. It grows and strengthens without cease in the midst of so many errors of the human mind."

The eulogy is magnificent, but permeating it we see the tendency which makes all analysis uniquely an auxiliary, however incomparable, of the natural sciences, a tendency, in conformity, as we have seen, with the development of science during the preceding two centuries; but we reach just here an epoch where new tendencies appear.

Poisson having in a report on the Fundamenta recalled the reproach made by Fourier to Abel and Jacobi of not having occupied themselves preferably with the movement of heat, Jacobi wrote to Legendre: "It is true that Monsieur Fourier held the view that the principal aim of mathematics was public utility, and the explanation of natural phenomena; but a philosopher such as he should have known that the unique aim of science is the honor of the human spirit, and that from this point of view a question about numbers is as important as a question about the system of the

world." This was without doubt also the opinion of the grand geometer of Goettingen, who called mathematics the queen of the sciences, and arithmetic the queen of mathematics.

It would be ridiculous to oppose one to the other these two tendencies; the harmony of our science is in their synthesis.

The time was about to arrive when one would feel the need of inspecting the foundations of the edifice, and of making the inventory of accumulated wealth, using more of the critical spirit. Mathematical thought was about to gather more force by retiring into itself; the problems were exhausted for a time, and it is not well for all seekers to stay on the same road. Moreover, difficulties and paradoxes remaining unexplained made necessary the progress of pure theory.

The path on which this should move was traced in its large outlines, and there it could move with independence without necessarily losing contact with the problems set by geometry, mechanics, and physics.

At the same time more interest was to attach to the philosophic and artistic side of mathematics, confiding in a sort of preëstablished harmony between our logical and æsthetic satisfactions and the necessities of future applications.

Let us recall rapidly certain points in the history of the revision of principles where Gauss, Cauchy, and Abel likewise were laborers of the first hour. Precise definitions of continuous functions, and their most immediate properties, simple rules on the convergence of series, were formulated; and soon was established, under very general conditions, the possibility of trigonometric developments, legitimatizing thus the boldness of Fourier.

Certain geometric intuitions relative to areas and to arcs give place to rigorous demonstration. The geometers of the eighteenth century had necessarily sought to give account of the degree of the generality of the solution of ordinary differential equations. Their likeness to equations of finite differences led easily to the result; but the demonstration so conducted must not be pressed very close.

Lagrange, in his lessons on the calculus of functions, had introduced greater precision, and starting from Taylor's series, he saw that the equation of order m leaves indeterminate the function, and its $m - 1$ first derivatives for the initial value of the variable; we are not surprised that Lagrange did not set himself the question of convergence.

In twenty or thirty years the exigencies in the rigor of proofs had grown. One knew that the two preceding modes of demonstration are susceptible of all the precision necessary.

For the first, there was need of no new principle; for the second it was necessary that the theory should develop in a new way. Up to this point, the functions and the variables had remained real. The consideration of complex variables comes to extend the field of

analysis. The functions of a complex variable with unique derivative are necessarily developable in Taylor's series; we come back thus to the mode of development of which the author of the theory of analytic functions had understood the interest, but of which the importance could not be put fully in evidence in limiting one's self to real variables. They also owe the grand rôle that they have not ceased to play to the facility with which we can manage them, and to their convenience in calculation.

The general theorems of the theory of analytic functions permitted to reply with precision to questions remaining up to that time undecided, such as the degree of generality of the integrals of differential equations. It became possible to push even to the end the demonstration sketched by Lagrange for an ordinary differential equation. For a partial differential equation or a system of such equations, precise theorems were established. It is not that on this latter point the results obtained, however important they may be, resolve completely the diverse questions that may be set; because in mathematical physics, and often in geometry, the conditions at the limits are susceptible of forms so varied that the problem called Cauchy's appears often under very severe form. I will shortly return to this capital point.

IV

Without restricting ourselves to the historic order, we will follow the development of mathematical physics during the last century, in so far as it interests analysis.

The problems of calorific equilibrium lead to the equation already encountered by Laplace in the study of attraction. Few equations have been the object of so many works as this celebrated equation. The conditions at the limits may be of divers forms. The simplest case is that of the calorific equilibrium of a body of which we maintain the elements of the surface at given temperatures; from the physical point of view, it may be regarded as evident that the temperature, continuous within the interior since no source of heat is there, is determined when it is given at the surface.

A more general case is that where, the state remaining permanent, there might be radiation toward the outside with an emissive power varying on the surface in accordance with a given law; in particular the temperature may be given on one portion, while there is radiation on another portion.

These questions, which are not yet resolved in their greatest generality, have greatly contributed to the orientation of the theory of partial differential equations. They have called attention to types of determinations of integrals, which would not have presented themselves in remaining at a point of view purely abstract.

Laplace's equation had been met already in hydrodynamics and in the study of attraction inversely as the square of the distance. This latter theory has led to putting in evidence the most essential elements, such as the potentials of simple strata and of double strata. Analytic combinations of the highest importance were there met, which since have been notably generalized, such as Green's formula.

The fundamental problems of static electricity belong to the same order of ideas, and that was surely a beautiful triumph for theory, the discovery of the celebrated theorem on electric phenomena in the interior of hollow conductors, which later Faraday rediscovered experimentally, without having known of Green's memoir.

All this magnificent *ensemble* has remained the type of the theories already old of mathematical physics, which seem to us almost to have attained perfection, and which exercise still so happy an influence on the progress of pure analysis in suggesting to it the most beautiful problems. The theory of functions offers us another memorable affiliation.

There the analytic transformations which come into play are not distinct from those we have met in the permanent movement of heat. Certain fundamental problems of the theory of functions of a complex variable lost then their abstract enunciation to take a physical form, such as that of the distribution of temperature on a closed surface of any connection and not radiating, in calorific equilibrium with two sources of heat which necessarily correspond to flows equal and of contrary signs. Transposing, we face a question relative to Abelian integrals of the third species in the theory of algebraic curves.

The examples which precede, where we have envisaged only the equations of heat and of attraction, show that the influence of physical theories has been exercised not only on the general nature of the problems to be solved, but even in the details of the analytic transformations. Thus is currently designated in recent memoirs on partial differential equations under the name of Green's formula, a formula inspired by the primitive formula of the English physicist. The theory of dynamic electricity and that of magnetism, with Ampère and Gauss, have been the origin of important progress; the study of curvilinear integrals and that of the integrals of surfaces have taken thence all their developments, and formulas, such as that of Stokes which might also be called Ampère's formula, have appeared for the first time in memoirs on physics. The equations of the propagation of electricity, to which are attached the names of Ohm and Kirchoff, while presenting a great analogy with those of heat, offer often conditions at the limits a little different; we know

all that telegraphy by cables owes to the profound discussion of a Fourier's equation carried over into electricity.

The equations long ago written of hydrodynamics, the equations of the theory of electricity, those of Maxwell and of Hertz in electromagnetism, have offered problems analogous to those recalled above, but under conditions still more varied. Many unsurmounted difficulties are there met with; but how many beautiful results we owe to the study of particular cases, whose number one would wish to see increase. To be noted also as interesting at once to analysis and physics are the profound differences which the propagation may present according to the phenomena studied. With equations such as those of sound, we have propagation by waves; with the equation of heat, each variation is felt instantly at every distance, but very little at a very great distance, and we cannot then speak of velocity of propagation.

In other cases of which Kirchoff's equation relative to the propagation of electricity with induction and capacity offers the simplest type, there is a wave front with a velocity determined but with a remainder behind which does not vanish.

These diverse circumstances reveal very different properties of integrals; their study has been delved into only in a few particular cases, and it raises questions into which enter the most profound notions of modern analysis.

V

I will enter into certain analytic details especially interesting for mathematical physics.

The question of the generality of the solution of a partial differential equation has presented some apparent paradoxes. For the same equation, the number of arbitrary functions figuring in the general integral was not always the same, following the form of the integral envisaged. Thus Fourier, studying the equation of heat in an indefinite medium, considers as evident that a solution will be determined if its value for $t = 0$ is given, that is to say one arbitrary function of the three coördinates x, y, z; from the point of view of Cauchy, we may consider, on the contrary, that in the general solution there are two arbitrary functions of the three variables. In reality, the question, as it has long been stated, has not a precise signification.

In the first place, when it is a question only of analytic functions, any finite number of functions of any number of independent variables presents, from the arithmetical point of view, no greater generality than a single function of a single variable, since in the one case and in the other the *ensemble* of coefficients of the development forms an enumerable series. But there is something more. In reality, beyond the conditions which are translated by given functions, an

integral is subjected to conditions of continuity, or is to become infinite in a determined manner for certain elements; one may so be led to regard as equivalent to an arbitrary function the condition of continuity in a given space, and then we clearly see how badly formulated is the question of giving the number of the arbitrary functions. It is at times a delicate matter to demonstrate that conditions determine in a unique manner a solution, when we do not wish to be contented with probabilities; it is then necessary to make precise the manner in which the function and certain of its derivatives conduct themselves.

Thus in Fourier's problem relative to an indefinite medium certain hypotheses must be made about the function and its first derivatives at infinity, if we wish to establish that the solution is unique.

Formulas analogous to Green's render great services, but the demonstrations one deduces from them are not always entirely rigorous, implicitly supposing fulfilled for the limits conditions which are not, *a priori* at least, necessary. This is, after so many others, a new example of the evolution of exigencies in the rigor of proofs.

We remark, moreover, that the new study, rendered necessary, has often led to a better account of the nature of integrals.

True rigor is fecund, thus distinguishing itself from another purely formal and tedious, which spreads a shadow over the problems it touches.

The difficulties in the demonstration of the unity of a solution may be very different according as it is question of equations of which all the integrals are or are not analytic. This is an important point, and shows that even when we wish to put them aside, it is necessary sometimes to consider non-analytic functions.

Thus we cannot affirm that Cauchy's problem determines in a unique manner one solution, the data of the problem being general, that is to say not being characteristic.

This is surely the case, if we envisage only analytic integrals, but with non-analytic integrals there may be contacts of order infinite. And theory here does not outstrip applications; on the contrary, as the following example shows:

Does the celebrated theorem of Lagrange on the potentials of velocity in a perfect fluid hold good in a viscid fluid? Examples have been given where the coördinates of different points of a viscous fluid starting from rest are not expressible as analytic functions of the time starting from the initial instant of the motion, and where the nul rotations as well as all their derivatives with respect to the time at this instant are, however, not identically nul; Lagrange's theorem, therefore, does not hold true.

These considerations sufficiently show the interest it may have to be assured that all the integrals of a system of partial differential equations continuous as well as all their derivatives up to a determined order in a certain field of real variables are analytic functions; it is understood, we suppose, there are in the equations only analytic elements. We have for linear equations precise theorems, all the integrals being analytic, if the characteristics are imaginary, and very general propositions have also been obtained in other cases.

The conditions at the limits that one is led to assume are very different according as it is question of an equation of which the integrals are or are not analytic. A type of the first case is given by the problem generalized by Dirichlet; conditions of continuity there play an essential part, and, in general, the solution cannot be prolonged from the two sides of the continuum which serves as support to the data; it is no longer the same in the second case, where the disposition of this support in relation to the characteristics plays the principal rôle, and where the field of existence of the solution presents itself under wholly different conditions.

All these notions, difficult to make precise in ordinary language and fundamental for mathematical physics, are not of less interest for infinitesimal geometry.

It will suffice to recall that all the surfaces of constant positive curvature are analytic, while there exist surfaces of constant negative curvature not analytic.

From antiquity has been felt the confused sentiment of a certain economy in natural phenomena; one of the first precise examples is furnished by Fermat's principle relative to the economy of time in the transmission of light.

Then we came to recognize that the general equations of mechanics correspond to a problem of minimum, or more exactly of variation, and thus we obtained the principle of virtual velocities, then Hamilton's principle, and that of least action. A great number of problems appeared then as corresponding to minima of certain definite integrals.

This was a very important advance, because the existence of a minimum could in many cases be regarded as evident, and consequently the demonstration of the existence of the solution was effected.

This reasoning has rendered immense services; the greatest geometers, Gauss in the problem of the distribution of an attracting mass corresponding to a given potential, Riemann in his theory of Abelian functions, have been satisfied with it. To-day our attention has been called to the dangers of this sort of demonstration; it is possible for the minima to be simply limits and not to be actually attained by veritable functions possessing the necessary properties

of continuity. We are, therefore, no longer content with the prob-
abilities offered by the reasoning long classic.

Whether we proceed indirectly or whether we seek to give a rigor-
ous proof of the existence of a function corresponding to the mini-
mum, the route is long and arduous.

Further, not the less will it be always useful to connect a ques-
tion of mechanics or of mathematical physics with a problem of
minimum; in this first of all is a source of fecund analytic trans-
formations, and besides in the very calculations of the investigation
of variations useful indications may appear, relative to the condi-
tions at the limits; a beautiful example of it was given by Kirchoff
in the delicate investigation of the conditions at the limits of the
equilibrium of flexure of plates.

VI

I have been led to expand particularly on partial differential
equations.

Examples chosen in rational mechanics and in celestial mechanics
would readily show the rôle which ordinary differential equations
play in the progress of these sciences whose history, as we have seen,
has been so narrowly bound to that of analysis.

When the hope of integrating with simple functions was lost, one
strove to find developments permitting to follow a phenomenon as long
as possible, or at least to obtain information of its qualitative bearing.

For practice, the methods of approximation form an extremely
important part of mathematics, and it is thus that the highest parts
of theoretic arithmetic find themselves connected with the applied
sciences. As to series, the demonstrations themselves of the exist-
ence of integrals furnish them from the very first; thus Cauchy's
first method gives developments convergent as long as the integrals
and the differential coefficients remain continuous.

When any circumstance permits our foreseeing that such is always
the case, we obtain developments always convergent. In the pro-
blem of n bodies, we can in this way obtain developments valid so
long as there are no shocks.

If the bodies, instead of attracting, repel each other, this contin-
gency need not be feared and we should obtain developments valid
indefinitely; unhappily, as Fresnel said one day to Laplace, nature
is not concerned about analytic difficulties and the celestial bodies
attract instead of repelling each other.

One would even be tempted at times to go further than the great
physicist and say that nature has sown difficulties in the paths of
the analysts.

Thus, to take another example, we can generally decide, given a
system of differential equations of the first order, whether the gen-

eral solution is stable or not about a point, and to find developments in series valid for stable solutions it is only necessary that certain inequalities be verified.

But if we apply these results to the equations of dynamics to discuss stability, we find ourselves exactly in the particular case which is unfavorable. Nay, in general, here it is not possible to decide on the stability; in the case of a function of forces having a maximum, reasoning classic, but indirect, establishes the stability which cannot be deduced from any development valid for every value of the time.

Do not lament these difficulties; they will be the source of future progress.

Such are also the difficulties which still present to us, in spite of so many works, the equations of celestial mechanics; the astronomers have almost drawn from them, since Newton, by means of series practically convergent and approximations happily conducted, all that is necessary for the foretelling of the motions of the heavenly bodies.

The analysts would ask more, but they no longer hope to attain the integration by means of simple functions or developments always convergent.

What admirable recent researches have best taught them is the immense difficulty of the problem; a new way has, however, been opened by the study of particular solutions, such as the periodic solutions and the asymptotic solutions which have already been utilized. It is not perhaps so much because of the needs of practice as in order not to avow itself vanquished, that analysis will never resign itself to abandon, without a decisive victory, a subject where it has met so many brilliant triumphs; and again, what more beautiful field could the theories new-born or rejuvenated of the modern doctrine of functions find, to essay their forces, than this classic problem of n bodies?

It is a joy for the analyst to encounter in applications equations that he can integrate with known functions, with transcendents already classed.

Such encounters are unhapily rare; the problem of the pendulum, the classic cases of the motion of a solid body around a fixed point, are examples where the elliptic functions have permitted us to effect the integration.

It would also be extremely interesting to encounter a question of mechanics which might be the origin of the discovery of a new transcendent possessing some remarkable property; I should be embarrassed to give an example of it unless in carrying back to the pendulum the début of the theory of elliptic functions.

The interpenetration between theory and applications is here much less than in the questions of mathematical physics. Thus

is explained that, since forty years, the works on ordinary differential equations attached to analytic functions have had in great part a theoretic character altogether abstract.

The pure theory has notably taken the advance; we have had occasion to say that it was well it should be so, but evidently there is here a question of measure, and we may hope to see the old problems profit by the progress accomplished.

It would not be over-difficult to give some examples, and I will recall only those linear differential equations, where figure arbitrary parameters whose singular values are roots of entire transcendent functions; which in particular makes the successive harmonics of a vibrating membrane correspond to the poles of a meromorphic function.

It happens also that the theory may be an element of classification in leading to seek conditions for which the solution falls under a determined type, as for example that the integral may be uniform. There have been and there yet will be many interesting discoveries in this way, the case of the motion of a solid heavy body treated by Madame de Kovalevski and where the Abelian functions were utilized is a remarkable example.

VII

In studying the reciprocal relations of analysis with mechanics and mathematical physics, we have on our way more than once encountered the infinitesimal geometry, which has proposed so many celebrated problems; in many difficult questions, the happy combination of calculus and synthetic reasonings has realized considerable progress, as is shown by the theories of applicable surfaces and systems triply orthogonal.

It is another part of geometry which plays a grand rôle in certain analytic researches, I mean the geometry of situation or *analysis situs*. We know that Riemann made from this point of view a complete study of the continuum of two dimensions, on which rests his theory of algebraic functions and their integrals.

When this number of dimensions augments, the questions of *analysis situs* become necessarily complicated; the geometric intuition ceases, and the study becomes purely analytic, the mind being guided solely by analogies which may be misleading and need to be discussed very closely. The theory of algebraic functions of two variables, which transports us into a space of four dimensions, without getting from *analysis situs* an aid so fruitful as does the theory of functions of one variable, owes it, however, useful orientations.

There is also another order of questions where the geometry of situation intervenes; in the study of curves traced on a surface and

defined by differential equations, the connection of this surface plays an important rôle; this happens for geodesic lines.

The notion of connexity, moreover, presented itself long ago in analysis, when the study of electric currents and magnetism led to non-uniform potentials; in a more general manner certain multiform integrals of some partial differential equations are met in difficult theories, such as that of diffraction, and varied researches must continue in this direction.

From a different point of view, I must yet recall the relations of algebraic analysis with geometry, which manifest themselves so elegantly in the theory of groups of finite order.

A regular polyhedron, say an icosahedron, is on the one hand the solid that all the world knows; it is also, for the analyst, a group of finite order, corresponding to the divers ways of making the polyhedron coincide with itself.

The investigation of all the types of groups of motion of finite order interests not alone the geometers, but also the crystallographers; it goes back essentially to the study of groups of ternary linear substitutions of determinant $+1$, and leads to the thirty-two systems of symmetry of the crystallographers for the particular complex.

The grouping in systems of polyhedra corresponding so as to fill space exhausts all the possibilities in the investigation of the structure of crystals.

Since the epoch when the notion of group was introduced into algebra by Galois, it has taken, in divers ways, considerable development, so that to-day it is met in all parts of mathematics. In the applications, it appears to us above all as an admirable instrument of classification. Whether it is a question of substitution groups or of Sophus Lie's transformation groups, whether it is a question of algebraic equations or of differential equations, this comprehensive doctrine permits explanation of the degree of difficulty of the problems treated and teaches how to utilize the special circumstances which present themselves; thus it should interest as well mechanics and mathematical physics as pure analysis.

The degree of development of mechanics and physics has permitted giving to almost all their theories a mathematical form; certain hypotheses, the knowledge of elementary laws, have led to differential relations which constitute the last form under which these theories settle down, at least for a time. These latter have seen little by little their field enlarge with the principles of thermodynamics; to-day chemistry tends to take in its turn a mathematical form.

I will take as witness of it only the celebrated memoir of Gibbs on the equilibrium of chemical systems, so analytic in character,

and where it needed some effort on the part of the chemists to recognize, under their algebraic mantle, laws of high importance.

It seems that chemistry has to-day gotten out of the premathematic period, by which every science begins, and that a day must come when will be systematized grand theories, analogous to those of our present mathematical physics, but far more vast, and comprising the *ensemble* of physicochemic phenomena.

It would be premature to ask if analysis will find in their developments the source of new progress; we do not even know beforehand what analytic types one might find.

I have constantly spoken of differential equations ruling phenomena; will this always be the final form which condenses a theory? Of this I know nothing certain, but we should, however, remember that many hypotheses have been made of more or less experimental nature ; among them, one is what has been called the principle of *non-heredity*, which postulates that the future of a system depends only on its present state and its state at an instant infinitely near, or, more briefly, that accelerations depend only on positions and velocities.

We know that in certain cases this hypothesis is not admissible, at least with the magnitudes directly envisaged; one has sometimes misemployed on this subject the memory of matter, which recalls its past, and has spoken in affected terms of the life of a morsel of steel. Different attempts have been made to give a theory of these phenomena, where a distant past seems to interfere; of them I need not speak here. An analyst may think that in cases so complex it is necessary to abandon the form of differential equations, and resign one's self to envisage *functional equations*, where figure definite integrals which will be the witness of a sort of heredity.

To see the interest which is attached at this moment to functional equations, one might believe in a presentiment of the future needs of science.

VIII

After having spoken of non-heredity, I scarcely dare touch the question of the applications of analysis to biology.

It will be some time, no doubt, before one forms the functional equations of biologic phenomena; the attempts so far made are in a very modest order of ideas; yet efforts are being made to get out of the purely qualitative field, to introduce quantitative measures. In the question of the variation of certain characteristics, mensuration has been engaged in, and statistic measures which are translated by curves of frequency. The modifications of these curves with successive generations, their decompositions into distinct curves, may give the measure of the stability of species or of the rapidity

of mutations, and we know what interest attaches itself to these questions in recent botanic researches. In all this so great is the number of parameters that one questions whether the infinitesimal method itself could be of any service. Some laws of a simple arithmetic character like those of Mendel come occasionally to give renewed confidence in the old aphorism which I cited in the beginning, that all things are explained by numbers; but, in spite of legitimate hopes, it is clear that, in its totality, biology is still far from entering upon a period truly mathematical.

It is not so, according to certain economists, with potential economy. After Cournot, the Lausanne school made an effort extremely interesting to introduce mathematical analysis into political economy.

Under certain hypotheses, which fit at least limiting cases, we find in learned treatises an equation between the quantities of merchandise and their prices, which recalls the equation of virtual velocities in mechanics: this is the equation of economic equilibrium. A function of quantities plays in this theory an essential rôle recalling that of the potential function. Moreover, the best authorized representatives of the school insist on the analogy of economic phenomena with mechanical phenomena. "As rational mechanics," says one of them, "considers material points, pure economy considers the *homo oeconomicus.*"

Naturally, we find there also the analogues of Lagrange's equations, indispensable matrix of all mechanics.

While admiring these bold works, we fear lest the authors have neglected certain hidden masses, as Helmholtz and Hertz would have said. But although that may happen, there is in these doctrines a curious application of mathematics, which, at least, in well-circumscribed cases, has already rendered great services.

I have terminated, messieurs, this summary history of some of the applications of analysis, with the reflections which it has at moments suggested to me. It is far from being complete; thus I have omitted to speak of the calculus of probabilities, which demands so much subtlety of mind, and of which Pascal refused to explain the niceties to the Chevalier de Méré because he was not a geometer.

Its practical utility is of the first rank, its theoretic interest has always been great; it is further augmented to-day, thanks to the importance taken by the researches that Maxwell called *statistical* and which tend to envisage mechanics under a wholly new light.

I hope, however, to have shown, in this sketch, the origin and the reason of the bonds so profound which unite analysis to geometry and physics, more generally to every science bearing on quantities numerically measurable.

The reciprocal influence of analysis and physical theories has been in this regard particularly instructive.

What does the future hold?

Problems more difficult, corresponding to an approximation of higher order, will introduce complications which we can only vaguely forecast, in speaking, as I have just done, of functional equations replacing systematically our actual differential equations, or further of integrations of equations infinite in number with an infinity of unknown functions. But even though that happens, mathematical analysis will always remain that language which, according to the *mot* of Fourier, has no symbols to express confused notions, a language endowed with an admirable power of transformation and capable of condensing in its formulas an immense number of results.

ON PRESENT PROBLEMS OF ALGEBRA AND ANALYSIS

BY HEINRICH MASCHKE

[Heinrich **Maschke**, Associate Professor of Mathematics, University of Chicago. b. Breslau, Germany, October 24, 1853. A.B. Magdalenen Gymnasium, Breslau, 1872; Ph.D. Göttingen, 1880. Post-graduate Heidelberg, Breslau, Berlin, and Göttingen. Professor Mathematics Lvisenstädt. Gymnasium, Berlin, 1880–90; Electric Engineer at Weston Electric Company, Newark, New Jersey, 1890–92; Assistant Professor of Mathematics, University of Chicago, 1892–96.]

As set forth by the Committee directing the affairs of this International Congress, the address which I have the distinguished privilege of delivering to-day shall be on "Present Problems in Algebra and Analysis," — but it is *not* provided by the Committee how many of these problems shall be treated.

The different branches of algebra and analysis which have been investigated are so numerous that it would be quite impossible to give an approximately exhaustive representation even only of the most important problems, within the limits of the time allowed to me. I, therefore, have confined myself to the minimum admissible number, namely *one*, or rather one *group* of problems.

Of this one problem, however, this Section of Algebra and Analysis has the right to expect that it is neither purely algebraic nor purely analytic, but one which touches both fields; and at least in this respect I hope that my selection has been fortunate.

I purpose to speak to-day on the Theory of Invariants of Quadratic Differential Quantics. Invariants suggest at once algebra, differential quantics: analysis. At the same time the subject also leads into geometry, — it contains, for instance, a great part of differential geometry and of geometry of hyperspace. But is there, indeed, any algebraic or analytic problem which does not allow geometrical interpretation in some way or other? And when it comes to geometry of hyperspace, — it is then only geometrical language that we are using, — what we are actually considering are analytic or algebraic forms. Moreover, rigorous definitions and discussions of geometrical propositions of an invariant character in particular can only be given by tracing them back to their analytic origin.

In the following exposition I shall first speak on the various invariant expressions of differential quadratics as they occur in geometry of two and more dimensions, and then take up the purely analytic representation in the second part of the paper.

This corresponds also to the historical development of the sub-

ject: geometry has here as well as in many other branches of mathematics indicated the problems which in their later development turned out to be of paramount interest in pure analysis.

A few preliminary remarks concerning the nomenclature of the different types of invariant expressions will be necessary.

To a given differential quadratic form

$$A = \sum_{\iota,k=1}^{n} a_{ik} dx_i dx_k, (a_{ki} = a_{ik})$$

where the a_{ik}'s are functions of the n independent variables $x_1, x_2, \ldots x_n$, we apply a general point transformation of the variables x,

$$x_i = x_i(y_1, y_2, \ldots y_n).$$

We observe that the differentials dx are then transformed into linear expressions of the differentials dy with the Jacobian of the x's with respect to the y's as the substitution-determinant which we shall call r.

By this transformation A goes into

$$A' = \Sigma a'_{ik} dy_i dy_k.$$

Let now Φ be a function

(a) of the coefficients a_{ik} and their first, second, . . . derivatives,

(b) of $U, V, \ldots$ and their derivatives, where $U, V, \ldots$ are any arbitrary functions of $x_1, x_2, \ldots x_n$.

If then Φ remains the same whether formed for the new or for the old quantities, that is, if

$$\Phi(a'_{ik}, \frac{\partial a'_{ik}}{\partial y\lambda}, \ldots, U', \frac{\partial U'}{\partial y\lambda}, \ldots, V', \ldots) = \Phi(a_{ik}, \frac{\partial a_{ik}}{\partial x\lambda}, \ldots, U, \frac{\partial U}{\partial x\lambda},$$
$$\ldots V, \ldots)$$

we say that Φ is an invariant (in the wider sense) of A.

If Φ contains *only* the a_{ik}'s and their derivatives, we call it an *invariant proper*, and its order the order of the highest derivative occurring in it. If Φ contains also one or more arbitrary functions $U, V, \ldots$ we call it a *differential parameter*, the definition of order being the same as before.

If more than *one* differential quadratic is given it is easily understood what is meant by simultaneous invariants and simultaneous differential parameters.

In strict analogy with the algebraic theory of invariants we call *covariants* expressions Φ of the above invariantive nature, provided that we also allow the differentials dx to enter into Φ.

The first and the most important example of a differential quadratic quantic is the square of the arc-element on a surface

$$ds^2 = Edu^2 + 2Fdudv + Gdv^2.$$

It was Gauss who made (1827), in his *Disquisitiones generales circa superficies curvas*, this expression the fundamental object of

investigation. He also gave, in what has been called after him the Gaussian Curvature

$$K = (E, F, G, \frac{\partial E}{\partial u}, \cdots),$$

the first example of an invariant. Gauss defines this curvature geometrically and finds for it the analytic expression

$$\frac{LN - M^2}{EG - F^2}$$

which is a simultaneous invariant of two differential quantics,

namely, of ds^2 and of $\dfrac{ds^2}{\rho} = Ldu^2 + 2Mdudv + Ndv^2.$

This shows that K is independent of the u,v-system on the surface. And now Gauss expresses K in terms of E, F, G and the first and second derivatives of these quantities alone. A direct demonstration that K is an invariant proper of the differential quantic ds^2 *alone*, — that is, without passing through the second differential quantic $\dfrac{ds^2}{\rho}$, — is of course desirable.[1] Each one of the general methods of treating the theory of invariants, which will be discussed in the latter part of this paper, furnishes such a direct proof. In particular, the aspect of the formula for K, on p. 528, deduced by the symbolic method, shows immediately the invariant character of K.

Differential parameters were introduced into differential geometry by Beltrami in 1863. These are the well-known expressions

$$\Delta_1\varphi = \frac{E\left(\dfrac{\partial\varphi}{\partial v}\right) - 2F\dfrac{\partial\varphi}{\partial u}\dfrac{\partial\varphi}{\partial v} + G\left(\dfrac{\partial\varphi}{\partial u}\right)^2}{EG - F^2},$$

$$\nabla(\phi,\psi) = \frac{E\dfrac{\partial\varphi}{\partial v}\dfrac{\partial\psi}{\partial v} - F\left(\dfrac{\partial\varphi}{\partial u}\dfrac{\partial\psi}{\partial v} + \dfrac{\partial\varphi}{\partial v}\dfrac{\partial\psi}{\partial u}\right) + G\dfrac{\partial\varphi}{\partial u}\dfrac{\partial\psi}{\partial u}}{EG - F^2},$$

$$\Delta_2\varphi = \frac{1}{\sqrt{EG-F^2}}\left\{\frac{\partial}{\partial u}\left[\frac{G\dfrac{\partial\varphi}{\partial u} - F\dfrac{\partial\varphi}{\partial v}}{\sqrt{EG-F^2}}\right] + \frac{\partial}{\partial v}\left[\frac{E\dfrac{\partial\varphi}{\partial v} - F\dfrac{\partial\varphi}{\partial u}}{\sqrt{EG-F^2}}\right]\right\},$$

where φ and ψ are the arbitrary functions which take the place of U, V in our general definition of differential parameters. Beltrami adopted the name "differential parameters" and also the notation

¹ Cf. on this subject the interesting paper by Knoblauch: "Der Gauss'sche Satz vom Krümmungsmass," *Sitzungsberichte der Berliner Mathem. Gesellschaft.* April 27, 1904.

$\varDelta$ from Lamé, who, in his *Leçons sur les coördonnées curvilignes*, defined in 1859 his differential parameters

$$(\varDelta_1\varphi)^2 = \left(\frac{\partial\varphi}{\partial x}\right)^2 + \left(\frac{\partial\varphi}{\partial y}\right)^2 + \left(\frac{\partial\varphi}{\partial z}\right)^2$$

$$\varDelta_2\varphi = \frac{\partial^2\varphi}{\partial x^2} + \frac{\partial^2\varphi}{\partial y^2} + \frac{\partial^2\varphi}{\partial z^2}$$

for the three-dimensional case where the arc-element is of the form

$$ds^2 = dx^2 + dy^2 + dz^2.$$

Lamé recognized the fundamental importance of these quantities and made a systematical use of them on account of their invariance with respect to any point-transformation preserving the form ds^2.

The general theory of invariants defines the differential parameters $\varDelta_1$ and $\varDelta_2$ for the case of n variables. From these general expressions Beltrami's differential parameters are directly obtained for $n=2$, Lamé's quantities $(\varDelta_1)^2$ and $\varDelta_2$ for the special form of ds^2 in the case $n=3$.

The number of differential parameters is of course infinite, but Darboux in his *Leçons sur la théorie générale des surfaces* has proved that all of them are expressible by means of $\varDelta_1$, $\varDelta_2$, $\varGamma$ and the evident differential parameter

$$\theta(\varphi,\psi) = \frac{\dfrac{\partial\varphi}{\partial u}\dfrac{\partial\psi}{\partial v} - \dfrac{\partial\varphi}{\partial v}\dfrac{\partial\psi}{\partial u}}{\sqrt{EG - F^2}}$$

(by forming, for instance, $\varDelta_1(\varDelta_2\varphi)$ etc.) — an important theorem which has later been extended by Staeckel to an analogous theorem for the case of n variables.

The expression $\varDelta_1\varphi$ occurs already in Gauss's *Disquisitiones*. By taking as parameter curves a singly infinite system of geodesics and its orthogonal trajectories he transforms the arc-element into the form

$$ds^2 = dr^2 + m^2 d\varphi^2$$

and shows that r satisfies the differential equation

$$\varDelta_1 r = 1.$$

An important differential parameter is the geodesic curvature. Its expression was thrown by Bonnet into a form which is easily recognized as a differential parameter (of the second order). Its numerator $=0$ represents the differential equation of geodesic lines in an invariant form.

Since a transformation of the two independent variables u, v which preserves the same value of ds^2 can also be considered as a transformation of two surfaces which are applicable to each other, it follows that all invariants of ds^2 are also invariants of a surface with respect to the process of bending. From this reason these invariants have

been called by Weingarten and Knoblauch, who were among the first writers emphasizing and developing to a certain extent the invariantive side of differential geometry, in the case of invariants proper, " Biegungsinvarianten," in the case of differential parameters, " Biegungscovarianten," and this notation has been more or less generally adopted. The notation " Biegungscovarianten " does not agree with the definition of a covariant given above, but a differential parameter of ds^2 can easily be modified into a covariantive form by replacing according to the differential equation of the curve

$$U(u,v) = const.$$

the derivatives $\dfrac{\partial U}{\partial u}$ and $\dfrac{\partial U}{\partial v}$ by μdv and $-\mu du$.

A surface is completely defined, apart from its location in space, when in addition to the quadratic form ds^2 also

$$\frac{ds^2}{\rho} = L du^2 + 2M du dv + N dv^2$$

is given, where ρ denotes the radius of curvature along ds, — a theorem which was proved (1867) by Bonnet.

With these two differential quantics given, we can now at once form simultaneous invariants and differential parameters. The six coefficients, E, F, G, L, M, N are, however, not independent; they are related by three partial differential equations, — the Gaussian relation and the two Codazzi-Mainardi equations. These three relations are expressible in an invariantive form. The Gaussian relation is

$$\frac{LN - M^2}{EG - F^2} = (E, F, G, \frac{\delta E}{\delta u}, \cdots),$$

while the two Codazzi formulas are given by the identical vanishing of one simultaneous linear covariant.

As examples of simultaneous differential parameters and covariants I mention the expressions which, when set equal to zero, represent the differential equations of conjugate lines, asymptotic lines, and lines of curvature. The differential equation of lines of curvature, for instance, if written in terms of du, dv represents a linear simultaneous covariant; if written as a partial differential equation derived from

$$U(u,v) = const.$$

it represents a simultaneous differential parameter involving the arbitrary function U. The differential equation of conjugate lines, if written in two sets of differentials du, dv and δu, δv represents a bilinear simultaneous covariant; if written as a partial differential equation it represents a differential parameter involving two arbitrary functions U and V.

The theory of invariants of the above two differential quadratics,

together with the condition of the vanishing of one simultaneous invariant proper and one simultaneous covariant, dominates then, in a certain sense, the whole of differential geometry.

Passing now to the case of n variables we may consider the differential quadratic form

$$\sum_{i,k=1}^{n} a_{ik} dx_i dx_k = ds^2$$

as the square of the arc in a hyperspace of n dimensions.

The fundamental rôle which the Gaussian curvature plays in the case $n=2$ is here represented by an invariant expresson of ds^2 which — in a certain sense — might be regarded as a generalization of the Gaussian curvature, namely, the Riemann curvature of the hyperspace. Riemann's investigations on this subject are found in his paper, *Ueber die Hypothesen, welche der Geometrie zu Grunde liegen*, and in the mathematical supplement to it *Commentatio mathematica*, etc. in the prize-problem of the Parisian Academy, 1861.

The geometrical definition of the Riemann curvature is briefly the following: Starting from any point P with the coördinates x_i we consider two linear directions defined by the increments dx_i and δx_i. If we remain in the vicinity of P these two directions define a plane of two dimensions and the determinants

$$dx_i \delta x_k - dx_k \delta x_i$$

may be considered as the coördinates of this plane. If now we draw geodesic lines from the point P whose initial arc-elements lie all in this plane, then these geodesics define a surface of two dimensions and the Gaussian curvature of this geodesic surface at the point P is the Riemann curvature. The analytic expression for it is

$$R = -\tfrac{1}{2} \frac{\Sigma (ikrs)(dx_i \delta x_s - dx_s \delta x_i)(dx_k \delta x_r - dx_r \delta x_k)}{\Sigma (a_{ik} a_{rs} - a_{ir} a_{ks})(dx_i \delta x_s - dx_s \delta x_i)(dx_k \delta x_r - dx_r \delta x_k,}$$

where the sum is to be taken over all values of i, k, r, s from 1 to n with the exception of those for which $i=k$ or $r=s$.

The coefficients $(ikrs)$ are certain quantities depending on the coefficients a_{ik}, their first and second derivatives; they occur in the literature mostly under the name of the "Christoffel quadruple index symbols." A better, certainly shorter, notation would be the one used by Ricci, namely, "Riemann symbols."

The Riemann curvature R is an invariant expression, and as its form shows it is a covariant of two sets of differentials. For $n=2$ it is identical with the Gaussian curvature. For greater numbers n the value of R depends, at a given point, on the plane-direction at that point and in general varies with the plane. If it should be constant for all plane-directions through one point, and if this is so for all the points, then R is, as Schur has shown, altogether constant that is, for every point.

Spaces of constant Riemann curvature have been the object of numerous interesting investigations, but these are more or less of a specific geometric character.

If in particular R is zero, then all the Riemann symbols vanish and it can easily be shown that ds^2 can be transformed into the sum of n squares

$$ds^2 = \sum_{i=1}^{n} dy^2_i$$

The converse is true. In this case the hyperspace of n dimensions is called a *flat* or also *Euclidean* space.

In every case the quadratic

$$\sum_{i,k=1}^{n} a_{ik} dx_i dx_k$$

can be transformed into

$$\sum_{i=1}^{n+r} dy^2_i$$

where r has the maximum value $\frac{n(n-1)}{2}$. We might say then that the given hyperspace of n dimensions is always contained in an Euclidean space of $n+r$ dimensions, where r is one of the numbers, $0, 1, \cdots \frac{n(n-1)}{2}$.

The number r is evidently characteristic for the hyperspace the square of the arc-element of which is the given quadratic. This number r has been called by Ricci the *class* of the given differential quadratic quantic. It is evident that this class is an invariant number, and the condition that a given differential quadratic be of class r must certainly be an invariantive condition. For $r=0$ we have just seen that the condition is $R=0$. For higher values of r no attempt has yet been made, so far as I know, to establish this invariantive condition though this problem is certainly one of fundamental interest.

Beltrami, in his paper, *Teoria generale dei parametri differenziali*, has extended the definition of his differential parameters to the case of n variables. The definition, for instance, of the first differential parameters is

$$\Delta_1 \varphi = \frac{1}{a} \sum_{i,k=1}^{n} A_{ik} \frac{\delta \varphi}{\delta x_i} \frac{\delta \varphi}{\delta x_k}$$

where A_{ik} denotes the minor of the element a_{ik} in the determinant

$$|a_{ik}| = a.$$

Beltrami shows that by means of the geodesics emanating from one point and of the hypersurfaces orthogonal to them he can choose his parameters such that ds^2 is transformed into

$$ds^2 = dr^2 + \sum_{i,k=1}^{n-1} b_{ik} dy_i dy_k,$$

where r satisfies the equation $\Delta_1 r = 1$, and that thus Gauss's theorems

on geodesic polar coördinates for $n=2$ admit a perfect analogon in hyperspace. Also in hyperspace then the determination of systems of geodesics amounts to the integration of the partial differential equation

$$\Delta_1 \varphi = 1.$$

This leads now to the application of differential quadratics to analytic mechanics. If we write down the expression of the vis viva of a (holonomous) material system in terms of generalized coördinates $q_1, q_2, \cdots q_n$.

$$T = \tfrac{1}{2} \Sigma a_{ik} \frac{dq_i}{dt} \frac{dq_k}{dt}$$

we have at once in

$$2T dt^2 = ds^2$$

a differential quadratic before us.

If no external forces act on the system, then a geodesic line of ds^2 represents at once, as also Beltrami has shown, a path of the system. Thus the mechanical problem is practically reduced to the integration of the equation $\Delta_1 \varphi = 1$.

In the case of the existence of external forces having a potential U, the above differential quantic has to be replaced by

$$\Sigma (U+h) a_{ik} dq_i dq_k$$

and the mechanical problem is equivalent to the integration of the equation

$$\Delta_1 \varphi = U + h$$

where $\Delta_1 \varphi$ is the differential parameter of the quadratic form denoted before by ds^2.

A detailed exposition of the above-mentioned researches of Beltrami, as well as this application to mechanics, is given in the second volume of Darboux's *Leçons sur la théorie des surfaces*.

Passing now to the second part of my address, the purely analytic theory of invariants of differential quadratics, I have first to discuss that paper which forms the foundation of almost all later literature on the subject: Christoffel's article in *Crelle's Journal*, vol. LXX (1870), "Ueber die Transformation der homogenen Differentialausdrücke des zweiten Grades."

Christoffel puts his problem in this form: Given two differential quadratics

$$A = \Sigma a_{ik} dx_i dx_k \qquad \text{and} \quad A' = \Sigma a'_{ik} dy_i dy_k,$$

what are the necessary and sufficient conditions for the equivalence of the two quadratics, that is, for the existence of a transformation of one quantic into the other; and if these conditions are established how can the required transformation be determined? (I should mention that Lamé in his already quoted work, *Leçons sur les coör-*

données curvilignes, treats and solves the analogous problem for the case $A = dx^2 + dy^2 + dz^2$).

Since the differentials dx are substituted linearly in terms of the dy there exists one and only one *algebraic* condition for the transformation, namely,

$$|a'_{ik}| = r^2 |a_{ik}|.$$

This condition would be sufficient if the coefficients a_{ik} and the elements of the determinant r were constants. In our case, however, other conditions must be satisfied, namely, the conditions of integrability in order that the expressions for the dx's are complete differentials. This is the way in which Christoffel introduces his problem to the reader.

The difficulty lies in the fact that the integrability conditions lead at once to a great number of partial differential equations of an apparently highly complex character. But Christoffel succeeds in substituting for all these partial differential equations a purely *algebraic* problem: The equivalence of two finite systems of algebraic forms in the sense of the algebraic theory of invariants. If this equivalence is satisfied, — which is merely a question of algebra, — no further discussion of the integrability conditions is required; they are all taken care of by the equivalence of the two systems.

For the following it will be necessary to sketch briefly the character of these forms.

The first is the quadratic form A itself. The next form is a quadrilinear covariant G_4 in four sets of differentials dx^1, dx^2, dx^3, dx^4, the coefficients of which are precisely the quantities $(i\ k\ r\ s\ )$ — the "Christoffel quadruple index symbols" or the "Riemann symbols" — which occur in the expression for the Riemann curvature:

$$G_4 = \Sigma (ikrs)\ d'x_i d^2 x_k d^3 x_r d^4 x_s.$$

It is highly interesting to observe how the quantities $(i\ k\ r\ s\ )$ have entered into the theory from two so apparently different standpoints. Christoffel found these expressions quite independently. Though Riemann's paper was written in 1861, that is, before Christoffel's article which appeared in 1870, it was only published in 1876, ten years after Riemann's death, by Weber-Dedekind.

For the deduction of the following forms G_5, G_6, — . . . — these forms are covariants linear in resp. ₅, ₆, . . . sets of differentials — Christoffel uses a certain reduction process. The coefficients $(\lambda\ i\ k\ r\ s)$ for instance of G_5 are obtained from $(i\ k\ r\ s)$ first by differentiating $(i\ k\ r\ s)$ with respect to x_λ and then by the addition of a sum of $5n$ terms which are linear in the different symbols $(\ i\ k\ r\ s\ )$ with coefficients depending on the so-called Christoffel triple index symbols of the second kind — expressions involving the quantities a_{ik} and their first derivatives.

Continuing in this way Christoffel obtains a well-defined set of covariants G_5, G_6, . . . , and this is his final result: the necessary and sufficient condition for the equivalence of the two differential quadratics is the algebraic equivalence — in the sense of the algebraic theory of invariants — of the forms A, G_4, G_5, . . . G_μ, and A', G'_4, G'_5 . . . G'_μ, where μ is a certain finite number.

In several papers covering the period from about 1884 up to the present time Ricci has worked out in a systematical way the fundamental principles of Christoffel's investigation, and has applied his theory to many problems in analysis, geometry, mechanics, and mathematical physics. He recognized in particular the importance of Christoffel's deduction of the covariants $G_{\lambda+1}$ from G_λ. He found that this process of deduction can be applied with a proper modification to any functions of the x's and the a_{ik}'s and that whenever invariantive relations with respect to the fundamental differential quadratic A come into question, this process is always of vital importance. He calls this process *covariantive differentiation* with respect to the fundamental quadratic A. On the systematical use of this covariantive differentiation Ricci based a calculus which he called *Calcolo differenziale assoluto*.

A collection of all his various investigations is given in two places:

(1) In a paper published, together with Levi-Cività in the *Math. Annalen*, vol. LIV.

(2) In his *Lezioni sulla teoria delle superficie*, Verona, Padua, 1898.

In the introduction of these autographed lectures he presents a complete exposition of his absolute differential calculus. Characteristic is the way in which he treats in his *Lezioni* the differential geometry. He divides it into two parts:

(1) Properties of surfaces depending on the one differential quadratic ds^2.

(2) Properties of surfaces depending on the two quadratics

$$ds^2 \text{ and } \frac{ds^2}{\rho}.$$

We are here chiefly interested in his applications to the theory of differential invariants. This is the result in his language: In order to obtain all invariants proper and differential parameters of order μ, it is sufficient to determine the algebraic invariants of the system of the following forms:

(1) The fundamental differential quantic A.

(2) The covariantive derivatives of the arbitrary functions

U, V, . . . up to the order μ.

(3) (for $\mu > 1$) the quadrilinear covariant G_4 and its covariantive derivatives up to the order $\mu - 2$.

Another treatment of the invariant theory of differential quan-

tics was given by myself. I applied a symbolic method to the theory which consists chiefly in identifying the fundamental quadratic

$$\Sigma a_{ik}dx_i dx_k$$

with the square of a linear expression

$$(\Sigma f_i dx_i)^2$$

by setting $f_i f_k = a_{ik}$. This is strictly analogous to the introduction of symbols in the algebraic theory. The difference, of course, comes in at once when we have to consider also the derivatives of a_{ik}.

A systematic development leads to expressions and formulas which with respect to simplicity and shortness are as superior to the formulas of the ordinary notation as the formulas of the so-called symbolic notation in the algebraic theory are superior to the non-symbolic expressions.

As examples I give the most important invariant expressions for the case $n=2$.

Let us introduce the abbreviation

$$\frac{1}{\sqrt{a_{11}a_{22}-a^2{}_{12}}}\,(P_1\,Q_2 - P_2\,Q_1\,) = (PQ), \text{ where } P_k = \frac{\partial P}{\partial x_k} \text{ etc.;}$$

let further $f, \varphi, \psi \, \ldots$ be symbols of A, so that

$$f_i f_k = \varphi_i \varphi_k = \psi_i \psi_k = \cdot \, \cdot \, \cdot = a_{ik}$$

and let $U, V \, \ldots$ be arbitrary functions of x_1, x_2.

Then we have

$$(fU)^2 = \varDelta_1\,U,$$
$$(fU)\,(fV) = \Gamma(UV),$$
$$(f(fU)) \quad = \varDelta_2\,U,$$
$$(\varphi\psi)\,((f\varphi)\,(f\psi)) \quad = 2K \text{ (Gaussian curvature),}$$

$(f\varphi)\,(\varphi U)\,((fU)U) : (\varDelta_1\,U)^{\frac{3}{2}} =$ Geodesic curvature of curve $U = const.$
To give also some examples of simultaneous invariant expressions let $f, \varphi, \, \ldots$ be as before symbols of

$$Edu^2 + 2Fdudv + Gdv^2$$

and $F, \varPhi \, \ldots$ symbols of

$$Ldu^2 + 2Mdudv + Ndv^2,$$

Then:

$$(F\varPhi)^2 = 2K,$$
$$(fF)^2 = \text{mean curvature.}$$

The differential equations

of asymptotic curves $U = c$ are $(FU)^2 = 0$,
of conjugate curves $U = c$, $V = c$: $(FU)(FV) = 0$,
of lines of curvature $U = c$: $(fF)(fU)(FU) = 0$.

The equation $(f\varphi)(\varphi F)((f\varphi)\,U) = 0$ gives the two Cadazzi formulas by setting the coefficients of U_1 and U_2 separately equal to zero.

In these examples the invariant expressions always appear as products of factors of the type (PR). The general theorem holds that any product of factors of this type represents always an in-

variant expression provided that the symbols f, φ, ..., F, Φ, ... occur in such a connection as to permit actual meaning.

The symbolic representation of invariant expressions suggested by the case $n=2$ can without essential difficulty be extended to the general case of n variables. In this treatment of the subject all the essential quantities entering into the theory present themselves quite naturally; they lie, so to say, on the surface; so, for instance, all the Christoffel symbols of the different kinds including the Riemann symbols and in particular also the process of covariantive differentiation.

The results of my investigation are chiefly laid down in the paper "A symbolic treatment of the theory of invariants of quadratic differential quantics of n variables," *Transactions of the American Mathematical Society*, vol. IV.

A third method of investigation of our theory of invariants is based on Lie's theory of continuous groups. The general point transformation by which A is transformed into A' defines a so-called "infinite" continuous group. In order to obtain the invariants of A, this group must first be "extended" in Lie's sense to include the coefficients a_{ik} of A and also the arbitrary functions involved in the differential parameters.

Lie himself developed a short outline of the determination of invariants in the second volume of the *Mathematische Annalen* for the case $n=2$, and indicated in particular how the Gaussian curvature and the parameter $\Delta_1\varphi$ could be found. The general plan of investigation was taken up in the sixteenth volume of the *Acta Mathematica* by Zorowski, who studied the case $n=2$ in detail, adding the complete computation of the Gaussian curvature and the most important differential parameters.

An extension of Lie's methods to the general case of n variables as far as the actual determination of invariants is concerned has, so far as I know, not yet been made; only the problem of determining the *number* of functionally independent invariants of a given order has been taken up. It seems that Lie's method is especially well adapted to this particular problem. In a paper in the *Atti del Reale Instituto Veneto* (1897), Levi-Cività found a lower limit for the number of invariants of a given order. The actual number was determined by Haskins in the *Transactions of the American Mathematical Society*, vol. III, for the case of invariants proper (including also simultaneous invariants) and in vol. V, of differential parameters.

I am at the end of my paper. I have attempted to show, in a compendious way, what has been done in this attractive field of research which is so closely connected with various interesting parts

of pure and applied mathematics. The number of problems that remain to be solved are numerous. Excepting the lowest cases as to the number of variables and the order of the invariants, not much more than the mere existence of the invariants is known, so that we have hardly the right to speak of a *theory* of these invariants.

When it comes to the question which of the different methods will be best adapted to a further systematical study of the subject, it seems probable that a combination of two or more of them will be the most promising one. But here, as always, it is the *man*, not the *method*, that solves the problem.

SHORT PAPERS

The Section of Algebra and Analysis attracted wide interest and caused many supplementary papers on various topics to be submitted. It is impossible to give a résumé of these, as their analytical nature demands that they be printed in full or not at all.

The first paper was presented by Professor G. A. Miller, of Leland Stanford Jr. University, on the " Bearing of Several Recent Theorems on Group Theory."

The second paper was read by Professor James Birney Shaw, of Milliken University, on "Linear Associative Algebra."

The third paper was presented by Professor M. W. Haskell, of the University of California, on "The Reduction of any Collineation to a Product of Perspective Collineations."

The fourth paper was presented by Professor M. B. Porter, of the University of Texas, "On Functions defined by an Infinite Series of Analytic Functions of a Complex Variable."

The fifth paper was presented by Professor Edward V. Huntington, of Harvard University, on "A Set of Postulates for Real Algebra comprising Postulates for a One Dimensional Continuum and for the Theory of Groups."

The sixth paper was presented by Professor J. I. Hutchinson, of Cornell University, on "Uniformizing of Algebraic Functions."

The seventh paper was read by Professor E. R. Hedrick, of the University of Missouri, on "Generalization of the Analytic Functions of a Complex Variable."

SECTION B — GEOMETRY

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A STUDY OF THE DEVELOPMENT OF GEOMETRIC METHODS

BY M. JEAN GASTON DARBOUX

(*Translated from the French by Professor George Bruce Halsted, Kenyon College*)

[Jean Gaston Darboux, Perpetual Secretary Academy of Sciences, Paris; Doyen
Honorary, Professor of Higher Geometry of the Faculty of Sciences, Paris.
b. August 13, 1842, Nîmes, France. Dr.Sc., LL.D., University of Cambridge,
University of Christiania, University of Heidelberg, *et al.* Professor of
Special Mathematics, Lycée Louis le Grand, 1867–73; Master of Confer-
ences in Superior Normal Schools, Paris, 1873–81; Professor Suppléant of
Rational Mechanics and Higher Geometry, The Sorbonne, 1873–81; since
1881, Professor Titulaire of the Faculty of Sciences, and Doyen of the Fac-
ulty of Sciences since 1889; also Professor in Higher Normal School for
Schools of Science ; Member of Bureau des Longitudes; President of the
First General Assembly of the International Association of Academies; and
Honorary Vice-President for France of the Congress of Arts and Science;
Member of Institute of France, Royal Society of London; Academies of
Berlin, St. Petersburg, Rome, Amsterdam, Munich, Stockholm; American
Philosophical Society, *et al.* Author of many publications and addresses
on Mathematics, and editor of the *Bulletin of Science of Mathematics*.]

I

To appreciate the progress geometry has made during the cen-
tury just ended, it is of advantage to cast a rapid glance over the
state of mathematical science at the beginning of the nineteenth
century.

We know that, in the last period of his life, Lagrange, fatigued by
the researches in analysis and mechanics, which assured him, however,
an immortal glory, neglected mathematics for chemistry (which,
according to him, was easy as algebra), for physics, for philosophic
speculations.

This mood of Lagrange we almost always find at certain moments
of the life of the greatest savants. The new ideas which came to
them in the fecund period of youth and which they introduced into
the common domain have given them all they could have expected;
they have fulfilled their task and feel the need of turning their

mental activity towards wholly new subjects. This need, as we recognize, manifested itself with particular force at the epoch of Lagrange. At this moment, in fact, the programme of researches opened to geometers by the discovery of the infinitesimal calculus appeared very nearly finished up. Some differential equations more or less complicated to integrate, some chapters to add to the integral calculus, and one seemed about to touch the very outmost bounds of science.

Laplace had achieved the explanation of the system of the world and laid the foundations of molecular physics. New ways opened before the experimental sciences and prepared the astonishing development they received in the course of the century just ended. Ampère, Poisson, Fourier, and Cauchy himself, the creator of the theory of imaginaries, were occupied above all in studying the application of the analytic methods to molecular physics, and seemed to believe that outside this new domain, which they hastened to cover, the outlines of theory and science were finally fixed.

Modern geometry, a glory we must claim for it, came, after the end of the eighteenth century, to contribute in large measure to the renewing of all mathematical science, by offering to research a way new and fertile, and above all in showing us, by brilliant successes, that general methods are not everything in science, and that even in the simplest subject there is much for an ingenious and inventive mind to do.

The beautiful geometric demonstrations of Huygens, of Newton, and of Clairaut were forgotten or neglected. The fine ideas introduced by Desargues and Pascal had remained without development and appeared to have fallen on sterile ground.

Carnot, by his *Essai sur les transversales* and his *Géométrie de position*, above all Monge, by the creation of descriptive geometry and by his beautiful theories on the generation of surfaces, came to renew a chain which seemed broken. Thanks to them, the conceptions of the inventors of analytic geometry, Descartes and Fermat, retook alongside the infinitesimal calculus of Leibnitz and Newton the place they had lost, yet should never have ceased to occupy. With his geometry, said Lagrange, speaking of Monge, this demon of a man will make himself immortal.

And, in fact, not only has descriptive geometry made it possible to coördinate and perfect the procedures employed in all the arts where precision of form is a condition of success and of excellence for the work and its products; but it appeared as the graphic translation of a geometry, general and purely rational, of which numerous and important researches have demonstrated the happy fertility.

Moreover, beside the *Géométrie descriptive* we must not forget to place that other masterpiece, the *Application de l'analyse à la*

géométrie ; nor should we forget that to Monge are due the notion of lines of curvature and the elegant integration of the differential equation of these lines for the case of the ellipsoid, which, it is said, Lagrange envied him. To be stressed is this character of unity of the work of Monge.

The renewer of modern geometry has shown us from the beginning, what his successors have perhaps forgotten, that the alliance of geometry and analysis is useful and fruitful, that this alliance is perhaps for each a condition of success.

II

In the school of Monge were formed many geometers: Hachette, Brianchon, Chappuis, Binet, Lancret, Dupin, Malus, Gaultier de Tours, Poncelet, Chasles, *et al.* Among these Poncelet takes first rank. Neglecting, in the works of Monge, everything pertaining to the analysis of Descartes or concerning infinitesimal geometry, he devoted himself exclusively to developing the germs contained in the purely geometric researches of his illustrious predecessor.

Made prisoner by the Russians in 1813 at the passage of the Dnieper and incarcerated at Saratoff, Poncelet employed the leisure captivity left him in the demonstration of the principles which he has developed in the *Traité des propriétés projectives des figures,* issued in 1822, and in the great memoirs on reciprocal polars and on harmonic means, which go back nearly to the same epoch. So we may say the modern geometry was born at Saratoff.

Renewing the chain broken since Pascal and Desargues, Poncelet introduced at the same time homology and reciprocal polars, putting thus in evidence, from the beginning, the fruitful ideas on which the science has evolved during fifty years.

Presented in opposition to analytic geometry, the methods of Poncelet were not favorably received by the French analysts. But such were their importance and their novelty, that without delay they aroused, from divers sides, the most profound researches.

Poncelet had been alone in discovering the principles; on the contrary, many geometers appeared almost simultaneously to study them on all sides and to deduce from them the essential results which they implicitly contained.

At this epoch, Gergonne was brilliantly editing a periodical which has to-day for the history of geometry an inestimable value. The *Annales de Mathématiques,* published at Nîmes from 1810 to 1831, was during more than fifteen years the only journal in the entire world devoted exclusively to mathematical researches.

Gergonne, who, in many regards, was a model editor for a scientific journal, had the defects of his qualities; he collaborated, often

against their will, with the authors of the memoirs sent him, rewrote them, and sometimes made them say more or less than they would have wished. Be that as it may, he was greatly struck by the originality and range of Poncelet's discoveries.

In geometry some simple methods of transformation of figures were already known; homology even had been employed in the plane, but without extending it to space, as did Poncelet, and especially without recognizing its power and fruitfulness. Moreover, all these transformations were *punctual;* that is to say, they made correspond a point to a point.

In introducing polar reciprocals, Poncelet was in the highest degree creative, because he gave the first example of a transformation in which to a point corresponded something other than a point.

Every method of transformation enables us to multiply the number of theorems, but that of polar reciprocals had the advantage of making correspond to a proposition another proposition of wholly different aspect. This was a fact essentially new. To put it in evidence, Gergonne invented the system, which since has had so much success, of memoirs printed in double columns with correlative propositions in juxtaposition; and he had the idea of substituting for Poncelet's demonstrations, which required an intermediary curve or surface of the second degree, the famous "principle of duality," of which the signification, a little vague at first, was sufficiently cleared up by the discussions which took place on this subject between Gergonne, Poncelet, and Pluecker.

Bobillier, Chasles. Steiner, Lamé, Sturm, and many others whose names escape me, were, at the same time as Pluecker and Poncelet, assiduous collaborators of the *Annales de Mathématiques.* Gergonne, having become rector of the Academy of Montpellier, was forced to suspend in 1831 the publication of his journal. But the success it had obtained, the taste for research it had contributed to develop, had commenced to bear their fruit. Quételet had established in Belgium the *Correspondance mathématique et physique.* Crelle, from 1826, brought out at Berlin the first sheets of his celebrated journal, where he published the memoirs of Abel, of Jacobi, of Steiner.

A great number of separate works began also to appear, wherein the principles of modern geometry were powerfully expounded and developed.

First came in 1827 the *Barycentrische Calcul* of Moebius, a work truly original, remarkable for the profundity of its conceptions, the elegance and the rigor of its exposition; then in 1828 the *Analytisch-geometrische Entwickelungen* of Pluecker, of which the second part appeared in 1831, and which was soon followed by the *System der analytischen Geometrie* of the same author, published at Berlin in 1835.

In 1832 Steiner brought out at Berlin his great work: *Systematische Entwickelung der Abhaengigkeit der geometrischen Gestalten von einander*, and, the following year, *Die geometrischen Konstruktionen ausgefuehrt mittels der geraden Linie und eines festen Kreises*, where was confirmed by the most elegant examples a proposition of Poncelet's relative to the employment of a single circle for the geometric constructions.

Finally, in 1830, Chasles sent to the Academy of Brussels, which happily inspired had offered a prize for a study of the principles of modern geometry, his celebrated *Aperçu historique sur l'origine et le développement des méthodes en géométrie*, followed by *Mémoire sur deux principes généraux de la science : la dualité et l'homographie*, which was published only in 1837.

Time would fail us to give a worthy appreciation of these beautiful works and to apportion the share of each. Moreover, to what would such a study conduct us, but to a new verification of the general laws of the development of science? When the times are ripe, when the fundamental principles have been recognized and enunciated, nothing stops the march of ideas ; the same discoveries, or discoveries almost equivalent, appear at nearly the same instant, and in places the most diverse. Without undertaking a discussion of this sort, which, besides, might appear useless or become irritating, it is, however, of importance to bring out a fundamental difference between the tendencies of the great geometers, who, about 1830, gave to geometry a scope before unknown.

III

Some, like Chasles and Steiner, who consecrated their entire lives to research in pure geometry, opposed what they called *synthesis* to *analysis*, and, adopting in the *ensemble* if not in detail the tendencies of Poncelet, proposed to constitute an independent doctrine, rival of Descartes's analysis.

Poncelet could not content himself with the insufficient resources furnished by the method of projections; to attain imaginaries he created that famous *principle of continuity* which gave birth to such long discussions between him and Cauchy.

Suitably enunciated, this principle is excellent and can render great service. Poncelet was wrong in refusing to present it as a simple consequence of analysis; and Cauchy, on the other hand, was not willing to recognize that his own objections, applicable without doubt to certain transcendent figures, were without force in the applications made by the author of the *Traité des propriétés projectives*.

Whatever be the opinion of such a discussion, it showed at least in the clearest manner that the geometric system of Poncelet rested

on an analytic foundation, and besides we know, by the untoward publication of the manuscripts of Saratoff, that by the aid of Descartes's analysis were established the principles which serve as foundation for the *Traité des propriétés projectives*.

Younger than Poncelet, who besides abandoned geometry for mechanics where his works had a preponderant influence, Chasles, for whom was created in 1847 a chair of *Géométrie supérieure* in the Faculty of Science of Paris, endeavored to constitute a geometric doctrine entirely independent and autonomous. He has expounded it in two works of high importance, the *Traité de géométrie supérieure*, which dates from 1852, and the *Traité des sections coniques*, unhappily unfinished and of which the first part alone appeared in 1865.

In the preface of the first of these works he indicates very clearly the three fundamental points which permit the new doctrine to share the advantages of analysis and which to him appear to mark an advance in the cultivation of the science. These are: (1) The introduction of the principle of signs, which simplifies at once the enunciations and the demonstrations, and gives to Carnot's analysis of transversals all the scope of which it is susceptible; (2) the introduction of imaginaries, which supplies the place of the principle of continuity and furnishes demonstrations as general as those of analytic geometry; (3) the simultaneous demonstration of propositions which are correlative, that is to say, which correspond in virtue of the principle of duality.

Chasles studies indeed in his work homography and correlation; but he avoids systematically in his exposition the employment of transformations of figures, which, he thinks, cannot take the place of direct demonstrations since they mask the origin and the true nature of the properties obtained by their means.

There is truth in this judgment, but the advance itself of the science permits us to declare it too severe. If it happens often that, employed without discernment, transformations multiply uselessly the number of theorems, it must be recognized that they often aid us to better understand the nature of the propositions even to which they have been applied. Is it not the employment of Poncelet's projection which has led to the so fruitful distinction between projective properties and metric properties, which has taught us also the high importance of that cross-ratio whose essential property is found already in Pappus, and of which the fundamental rôle has begun to appear after fifteen centuries only in the researches of modern geometry?

The introduction of the principle of signs was not so new as Chasles supposed at the time he wrote his *Traité de Géométrie supérieure*.

Moebius, in his *Barycentrische Calcul*, had already given issue to a *desideratum* of Carnot, and employed the signs in a way the largest

and most precise, defining for the first time the sign of a segment and even that of an area.

Later he succeeded in extending the use of signs to lengths not laid off on the same straight line and to angles not formed about the same point.

Besides Grassmann, whose mind has so much analogy to that of Moebius, had necessarily employed the principle of signs in the definitions which serve as basis for his methods, so original, of studying the properties of space.

The second characteristic which Chasles assigns to his system of geometry is the employment of imaginaries. Here, his method was really new, and he illustrates it by examples of high interest. One will always admire the beautiful theories he has left us on homofocal surfaces of the second degree, where all the known properties and others new, as varied as elegant, flow from the general principle that they are inscribed in the same developable circumscribed to the circle at infinity.

But Chasles introduced imaginaries only by their symmetric functions, and consequently would not have been able to define the cross-ratio of four elements when these ceased to be real in whole or in part. If Chasles had been able to establish the notion of the cross-ratio of imaginary elements, a formula he gives in the *Géométrie supérieure* (p. 118 of the new edition) would have immediately furnished him that beautiful definition of angle as logarithm of a cross-ratio which enabled Laguerre, our regretted confrère, to give the complete solution, sought so long, of the problem of the transformation of relations which contain at the same time angles and segments in homography and correlation.

Like Chasles, Steiner, the great and profound geometer, followed the way of pure geometry; but he has neglected to give us a complete exposition of the methods upon which he depended. However, they may be characterized by saying that they rest upon the introduction of those elementary geometric forms which Desargues had already considered, on the development he was able to give to Bobillier's theory of polars, and finally on the construction of curves and surfaces of higher degrees by the aid of sheaves or nets of curves of lower orders. In default of recent researches, analysis would suffice to show that the field thus embraced has just the extent of that into which the analysis of Descartes introduces us without effort.

IV

While Chasles, Steiner, and, later, as we shall see, von Staudt, were intent on constituting a rival doctrine to analysis and set in some sort altar against altar, Gergonne, Bobillier, Sturm, and above all Pluecker, perfected the geometry of Descartes and constituted an

analytic system in a manner adequate to the discoveries of the geometers. It is to Bobillier and to Pluecker that we owe the method called *abridged notation*. Bobillier consecrated to it some pages truly new in the last volumes of the *Annales* of Gergonne.

Pluecker commenced to develop it in his first work, soon followed by a series of works where are established in a fully conscious manner the foundations of the modern analytic geometry. It is to him that we owe tangential coördinates, trilinear coördinates, employed with homogeneous equations, and finally the employment of canonical forms whose validity was recognized by the method, so deceptive sometimes, but so fruitful, called the *enumeration of constants*.

All these happy acquisitions infused new blood into Descartes's analysis and put it in condition to give their full signification to the conceptions of which the geometry called *synthetic* had been unable to make itself completely mistress.

Pluecker, to whom it is without doubt just to adjoin Bobillier, carried off by a premature death, should be regarded as the veritable initiator of those methods of modern analysis where the employment of homogeneous coördinates permits treating simultaneously and, so to say, without the reader perceiving it, together with one figure all those deducible from it by homography and correlation.

V

Parting from this moment, a period opens brilliant for geometric researches of every nature.

The analysts interpret all their results and are occupied in translating them by constructions.

The geometers are intent on discovering in every question some general principle, usually undemonstrable without the aid of analysis, in order to make flow from it without effort a crowd of particular consequences, solidly bound to one another and to the principle whence they are derived. Otto Hesse, brilliant disciple of Jacobi, develops in an admirable manner that method of homogeneous coördinates to which Pluecker perhaps had not attached its full value. Boole discovers in the polars of Bobillier the first notion of a covariant; the theory of forms is created by the labors of Cayley, Sylvester, Hermite, Brioschi. Later Aronhold, Clebsch and Gordan, and other geometers still living, gave to it its final notation, established the fundamental theorem relative to the limitation of the number of covariant forms and so gave it all its amplitude.

The theory of surfaces of the second order, built up principally by the school of Monge, was enriched by a multitude of elegant properties, established principally by O. Hesse, who found later in Paul Serret a worthy emulator and continuer.

The properties of the polars of algebraic curves are developed by Pluecker and above all by Steiner. The study, already old, of curves of the third order is rejuvenated and enriched by a crowd of new elements. Steiner, the first, studies by pure geometry the double tangents of curves of the fourth order, and Hesse, after him, applies the methods of algebra to this beautiful question, as well as to that of points of inflection of curves of the third order.

The notion of *class* introduced by Gergonne, the study of a paradox in part elucidated by Poncelet and relative to the respective degrees of two curves reciprocal polars one of the other, give birth to the researches of Pluecker relative to the singularities called *ordinary* of algebraic plane curves. The celebrated formulas to which Pluecker is thus conducted are later extended by Cayley and by other geometers to algebraic skew curves, by Cayley again and by Salmon to algebraic surfaces.

The singularities of higher order are in their turn taken up by the geometers; contrary to an opinion then very widespread, Halphen demonstrates that each of these singularities cannot be considered as equivalent to a certain group of ordinary singularities, and his researches close for a time this difficult and important question.

Analysis and geometry, Steiner, Cayley, Salmon, Cremona, meet in the study of surfaces of the third order, and, in conformity with the anticipations of Steiner, this theory becomes as simple and as easy as that of surfaces of the second order.

The algebraic ruled surfaces, so important for applications, are studied by Chasles, by Cayley, of whom we find the influence and the mark in all mathematical researches, by Cremona, Salmon, La Gournerie; so they will be later by Pluecker in a work to which we must return.

The study of the general surface of the fourth order would seem to be still too difficult; but that of the particular surfaces of this order with multiple points or multiple lines is commenced, by Pluecker for the surface of waves, by Steiner, Kummer, Cayley, Moutard, Laguerre, Cremona, and many other investigators.

As for the theory of algebraic skew curves, grown rich in its elementary parts, it receives finally, by the labors of Halphen and of Noether, whom it is impossible for us here to separate, the most notable extensions.

A new theory with a great future is born by the labors of Chasles, of Clebsch, and of Cremona; it concerns the study of all the algebraic curves which can be traced on a determined surface.

Homography and correlation, those two methods of transformation which have been the distant origin of all the preceding researches, receive from them in their turn an unexpected extension; they are not the only methods which make a single element correspond to a

single element, as might have shown a particular transformation briefly indicated by Poncelet in the *Traité des propriétés projectives*.

Pluecker defines the *transformation by reciprocal radii vectores* or *inversion*, of which Sir W. Thomson and Liouville hasten to show all the importance, as well for mathematical physics as for geometry.

A contemporary of Moebius and Pluecker, Magnus believed he had found the most general transformation which makes a point correspond to a point, but the researches of Cremona show us that the transformation of Magnus is only the first term of a series of birational transformations which the great Italian geometer teaches us to determine methodically, at least for the figures of plane geometry.

The Cremona transformations long retained a great interest, though later researches have shown us that they reduce always to a series of successive applications of the transformation of Magnus.

VI

All the works we have enumerated, others to which we shall return later, find their origin and, in some sort, their first motive in the conceptions of modern geometry; but the moment has come to indicate rapidly another source of great advances for geometric studies. Legendre's theory of elliptic functions, too much neglected by the French geometers, is developed and extended by Abel and Jacobi. With these great geometers, soon followed by Riemann and Weierstrass, the theory of Abelian functions which, later, algebra would try to follow solely with its own resources, brought to the geometry of curves and surfaces a contribution whose importance will continue to grow.

Already, Jacobi had employed the analysis of elliptic functions in the demonstration of Poncelet's celebrated theorems on inscribed and circumscribed polygons, inaugurating thus a chapter since enriched by a multitude of elegant results; he had obtained also, by methods pertaining to geometry, the integration of Abelian equations.

But it was Clebsch who first showed in a long series of works all the importance of the notion of *deficiency* (*Geschlecht, genre*) of a curve, due to Abel and Riemann, in developing a crowd of results and elegant solutions that the employment of Abelian integrals would seem, so simple was it, to connect with their veritable point of departure.

The study of points of inflection of curves of the third order, that of double tangents of curves of the fourth order, and, in general, the theory of osculation on which the ancients and the moderns had so often practiced, were connected with the beautiful problem of the division of elliptic functions and Abelian functions.

In one of his memoirs, Clebsch had studied the curves which are

rational or of deficiency zero; this led him, toward the end of his too short life, to envisage what may be called also *rational* surfaces, those which can be simply represented by a plane. This was a vast field for research, opened already for the elementary cases by Chasles, and in which Clebsch was followed by Cremona and many other savants. It was on this occasion that Cremona, generalizing his researches on plane geometry, made known not indeed the totality of birational transformations of space, but certain of the most interesting among these transformations.

The extension of the notion of deficiency to algebraic surfaces is already commenced; already also works of high value have shown that the theory of integrals, simple or multiple, of algebraic differentials will find, in the study of surfaces as in that of curves, an ample field of important applications; but it is not proper for the reporter on geometry to dilate on this subject.

VII

While thus were constituted the mixed methods whose principal applications we have just indicated, the pure geometers were not inactive. Poinsot, the creator of the theory of couples, developed, by a method purely geometric, "that, where one never for a moment loses from view the object of the research," the theory of the rotation of a solid body that the researches of d'Alembert, Euler, and Lagrange seemed to have exhausted; Chasles made a precious contribution to kinematic by his beautiful theorems on the displacement of a solid body, which have since been extended by other elegant methods to the case where the motion has divers degrees of freedom. He made known those beautiful propositions on attraction in general, which figure without disadvantage beside those of Green and Gauss. Chasles and Steiner met in the study of the attraction of ellipsoids and showed thus once more that geometry has its designated place in the highest questions of the integral calculus.

Steiner did not disdain at the same time to occupy himself with the elementary parts of geometry. His researches on the contacts of circles and conics, on isoperimetric problems, on parallel surfaces, on the centre of gravity of curvature, excited the admiration of all by their simplicity and their depth.

Chasles introduced his principle of correspondence between two variable objects which has given birth to so many applications; but here analysis retook its place to study the principle in its essence, make it precise and generalize it.

It was the same concerning the famous theory of *characteristics* and the numerous researches of de Jonquières. Chasles, Cremona, and still others, which gave the foundations of a new branch of the science, *Enumerative Geometry*.

During many years, the celebrated postulate of Chasles was admitted without any objection: a crowd of geometers believed they had established it in a manner irrefutable.

But, as Zeuthen then said, it is very difficult to recognize whether, in demonstrations of this sort, there does not exist always some weak point that their author has not perceived; and, in fact, Halphen, after fruitless efforts, crowned finally all these researches by clearly indicating in what cases the postulate of Chasles may be admitted and in what cases it must be rejected.

VIII

Such are the principal works which restored geometric synthesis to honor and assured to it, in the course of the last century, the place belonging to it in mathematical research. Numerous and illustrious workers took part in this great geometric movement, but we must recognize that its chiefs and leaders were Chasles and Steiner. So brilliant were their marvelous discoveries that they threw into the shade, at least momentarily, the publications of other modest geometers, less preoccupied perhaps in finding brilliant applications, fitted to evoke love for geometry than to establish this science itself on an absolutely solid foundation. Their works have received perhaps a recompense more tardy, but their influence grows each day; it will assuredly increase still more. To pass them over in silence would be without doubt to neglect one of the principal factors which will enter into future researches. We allude at this moment above all to von Staudt. His geometric works were published in two books of great interest: the *Geometrie der Lage*, issued in 1847, and the *Beitrage zur Geometrie der Lage*, published in 1856, that is to say, four years after the *Géométrie supérieure*. Chasles, as we have seen, had devoted himself to constituting a body of doctrine independent of Descartes's analysis and had not completely succeeded. We have already indicated one of the criticisms that can be made upon this system: the imaginary elements are there defined only by their symmetric functions, which necessarily exclude them from a multitude of researches. On the other hand, the constant employment of cross-ratio, of transversals, and of involution, which requires frequent analytic transformations, gives to the *Géométrie supérieure* a character almost exclusively metric which removes it notably from the methods of Poncelet. Returning to these methods, von Staudt devoted himself to constituting a geometry freed from all metric relation and resting exclusively on relations of situation.

This is the spirit in which was conceived his first work, the *Geometrie der Lage* of 1847. The author there takes as point of departure the harmonic properties of the complete quadrilateral and those of homologic triangles, demonstrated uniquely by considerations

of geometry of three dimensions, analogous to those of which the school of Monge made such frequent use.

In this first part of his work, von Staudt neglected entirely imaginary elements. It is only in the *Beitrage*, his second work, that he succeeds, by a very original extension of the method of Chasles, in defining geometrically an isolated imaginary element and distinguishing it from its conjugate.

This extension, although rigorous, is difficult and very abstract. It may be defined in substance as follows: Two conjugate imaginary points may always be considered as the double points of an involution on a real straight; and just as one passes from an imaginary to its conjugate by changing i into $-i$, so one may distinguish the two imaginary points by making correspond to each of them one of the two different senses which may be attributed to the straight. In this there is something a little artificial; the development of the theory erected on such foundations is necessarily complicated. By methods purely projective, von Staudt establishes a calculus of cross-ratios of the most general imaginary elements. Like all geometry, the projective geometry employs the notion of order and order engenders number; we are not astonished therefore that von Staudt has been able to constitute his calculus; but we must admire the ingenuity displayed in attaining it. In spite of the efforts of distinguished geometers who have essayed to simplify its exposition, we fear that this part of the geometry of von Staudt, like the geometry otherwise so interesting of the profound thinker Grassmann, cannot prevail against the analytical methods which have won to-day favor almost universal. Life is short; geometers know and also practice the principle of least action. Despite these fears, which should discourage no one, it seems to us that under the first form given it by von Staudt, projective geometry must become the necessary companion of descriptive geometry, that it is called to renovate this geometry in its spirit, its procedures, and its applications.

This has already been comprehended in many countries, and notably in Italy, where the great geometer Cremona did not disdain to write for the schools an elementary treatise on projective geometry.

IX

In the preceding articles, we have essayed to follow and bring out clearly the most remote consequences of the methods of Monge and Poncelet. In creating tangential coördinates and homogeneous coördinates, Pluecker seemed to have exhausted all that the method of projections and that of reciprocal polars give to analysis.

It remained for him, toward the end of his life, to return to his first researches to give them an extension enlarging to an unexpected degree the domain of geometry.

Preceded by innumerable researches on systems of straight lines, due to Poinsot, Moebius, Chasles, Dupin, Malus, Hamilton, Krummer, Transon, above all to Cayley, who first introduced the notion of the coördinates of the straight, researches originating perhaps in statics and kinematics, perhaps in geometrical optics, Pluecker's geometry of the straight line will always be regarded as the part of his work where are met the newest and most interesting ideas.

Pluecker first set up a methodic study of the straight line, which already is important, but that is nothing beside what he discovered. It is sometimes said that the principle of duality shows that the plane as well as the point may be considered as a space element. That is true; but in adding the straight line to the plane and point as possible space element, Pluecker was led to recognize that any curve, any surface, may also be considered as space element, and so was born a new geometry which already has inspired a great number of works, which will raise up still more in the future.

A beautiful discovery, of which we shall speak further on, has already connected the geometry of spheres with that of straight lines and permits the introduction of the notion of coördinates of a sphere.

The theory of systems of circles is already commenced; it will be developed without doubt when one wishes to study the representation, which we owe to Laguerre, of an imaginary point in space by an oriented circle.

But before expounding the development of these new ideas which have vivified the infinitesimal methods of Monge, it is necessary to go back to take up the history of branches of geometry that we have neglected until now.

X

Among the works of the school of Monge, we have hitherto confined ourselves to the consideration of those connected with *finite* geometry; but certain of the disciples of Monge devoted themselves above all to developing the new notions of infinitesimal geometry applied by their master to curves of double curvature, to lines of curvature, to the generation of surfaces, notions expounded at least in part in the *Application de l'Analyse à la Géométrie*. Among these we must cite Lancret, author of beautiful works on skew curves, and above all Charles Dupin, the only one perhaps who followed all the paths opened by Monge.

Among other works, we owe to Dupin two volumes Monge would not have hesitated to sign: *Les Développements de Géométrie pure*, issued in 1813, and *Les Applications de Géométrie et de Mécanique*, dating from 1822.

There we find the notion of *indicatrix*, which was to renovate, after Euler and Meunier, all the theory of curvature, that of conjugate

tangents, of asymptotic lines which have taken so important a place in recent researches. Nor should we forget the determination of the surface of which all the lines of curvature are circles, nor above all the memoir on triple systems of orthogonal surfaces where is found, together with the discovery of the triple system formed by surfaces of the second degree, the celebrated theorem to which the name of Dupin will remain attached.

Under the influence of these works and of the renaissance of synthetic methods, the geometry of infinitesimals retook in all researches the place Lagrange had wished to take away from it forever.

Singular thing, the geometric methods thus restored were to receive the most vivid impulse in consequence of the publication of a memoir which, at least at first blush, would appear connected with the purest analysis; we mean the celebrated paper of Gauss, *Disquisitiones generales circa superficies curvas*, which was presented in 1827 to the Göttingen Society, and whose appearance marked, one may say, a decisive date in the history of infinitesimal geometry.

From this moment, the infinitesimal method took in France a free scope before unknown.

Frenet, Bertrand, Molins, J. A. Serret, Bouquet, Puiseux, Ossian Bonnet, Paul Serret, develop the theory of skew curves. Liouville, Chasles, Minding, join them to pursue the methodic study of the memoir of Gauss.

The integration made by Jacobi of the differential equation of the geodesic lines of the ellipsoid started a great number of researches. At the same time the problems studied in the *Application de l'Analyse* of Monge were greatly developed.

The determination of all the surfaces having their lines of curvature plane or spheric completed in the happiest manner certain partial results already obtained by Monge.

At this moment, one of the most penetrating of geometers, according to the judgment of Jacobi, Gabriel Lamé, who, like Charles Sturm, had commenced with pure geometry and had already made to this science contributions the most interesting by a little book published in 1817 and by memoirs inserted in the *Annales* of Gergonne, utilized the results obtained by Dupin and Binet on the system of confocal surfaces of the second degree, and, rising to the idea of curvilinear coördinates in space, became the creator of a wholly new theory destined to receive in mathematical physics the most varied applications.

XI

Here again, in this infinitesimal branch of geometry are found the two tendencies we have pointed out à *propos* of the geometry of finite quantities.

Some, among whom must be placed J. Bertrand and O. Bonnet, wish to constitute an independent method resting directly on the employment of infinitesimals. The grand *Traité de Calcul différentiel*, of Bertrand, contains many chapters on the theory of curves and of surfaces, which are, in some sort, the illustration of this conception.

Others follow the usual analytic ways, being only intent to clearly recognize and put in evidence the elements which figure in the first plan. Thus did Lamé in introducing his theory of *differential parameters*. Thus did Beltrami in extending with great ingenuity the employment of these differential invariants to the case of two independent variables, that is to say, to the study of surfaces.

It seems that to-day is accepted a mixed method whose origin is found in the works of Ribaucour, under the name *périmorphie*. The rectangular axes of analytic geometry are retained, but made mobile and attached as seems best to the system to be studied. Thus disappear most of the objections which have been made to the method of coördinates. The advantages of what is sometimes called *intrinsic* geometry are united to those resulting from the use of the regular analysis. Besides, this analysis is by no means abandoned; the complications of calculation which it almost always carries with it, in its applications to the study of surfaces and rectilinear coördinates, usually disappear if one employs the notion on the invariants and the covariants of quadratic powers of differentials which we owe to the researches of Lipschitz and Christoffel, inspired by Riemann's studies on the non-Euclidean geometry.

XII

The results of so many labors were not long in coming. The notion of geodesic curvature which Gauss already possessed, but without having published it, was given by Bonnet and Liouville; the theory of surfaces of which the radii of curvature are functions one of the other, inaugurated in Germany by two propositions which would figure without disadvantage in the memoir of Gauss, was enriched by Ribaucour, Halphen, S. Lie, and others, with a multitude of propositions, some concerning these surfaces envisaged in a general manner; others applying to particular cases where the relation between the radii of curvature takes a form particularly simple; to minimal surfaces for example, and also to surfaces of constant curvature, positive or negative.

The minimal surfaces were the object of works which make of their study the most attractive chapter of infinitesimal geometry. The integration of their partial differential equation constitutes one of the most beautiful discoveries of Monge; but because of the imperfection of the theory of imaginaries, the great geometer could not

get from its formulas any mode of generation of these surfaces, nor even any particular surface. We will not here retrace the detailed history which we have presented in our *Leçons sur la théorie des surfaces ;* but it is proper to recall the fundamental researches of Bonnet which have given us, in particular, the notion of *surfaces associated with a given surface*, the formulas of Weierstrass which establish a close bond between the minimal surfaces and the functions of a complex variable, the researches of Lie by which it was established that just the formulas of Monge can to-day serve as foundation for a fruitful study of minimal surfaces.

In seeking to determine the minimal surfaces of smallest classes or degrees, we were led to the notion of double minimal surfaces which is dependent on *analysis situs.*

Three problems of unequal importance have been studied in this theory.

The first, relative to the determination of minimal surfaces inscribed along a given contour in a developable equally given, was solved by celebrated formulas which have led to a great number of propositions. For example, every straight traced on such a surface is an axis of symmetry.

The second, set by S. Lie, concerns the determination of all the algebraic minimal surfaces inscribed in an algebraic developable, without the curve of contact being given. It also has been entirely elucidated.

The third and the most difficult is what the physicists solve experimentally, by plunging a closed contour into a solution of glycerine. It concerns the determination of the minimal surface passing through a given contour.

The solution of this problem evidently surpasses the resources of geometry. Thanks to the resources of the highest analysis, it has been solved for particular contours in the celebrated memoir of Riemann and in the profound researches which have followed or accompanied this memoir.

For the most general contour, its study has been brilliantly begun; it will be continued by our successors.

After the minimal surfaces, the surfaces of constant curvature attracted the attention of geometers. An ingenious remark of Bonnet connects with each other the surfaces of which one or the other of the two curvatures, mean curvature or total curvature, is constant.

Bour announced that the partial differential equation of surfaces of constant curvature could be completely integrated. This result has not been secured; it would seem even very doubtful if we consider a research where S. Lie has essayed in vain to apply a general method of integration of partial differential equations to the particular equation of surfaces of constant curvature.

But, if it is impossible to determine in finite terms all these surfaces, it has at least been possible to obtain certain of them, characterized by special properties, such as that of having their lines of curvature plane or spheric; and it has been shown, by employing a method which succeeds in many other problems, that from every surface of constant curvature may be derived an infinity of other surfaces of the same nature, by employing operations clearly defined which require only quadratures.

The theory of the deformation of surfaces in the sense of Gauss has been also much enriched. We owe to Minding and to Bour the detailed study of that special deformation of ruled surfaces which leaves the generators rectilineal. If we have not been able, as has been said, to determine the surfaces applicable on the sphere, other surfaces of the second degree have been attacked with more success, and, in particular, the paraboloid of revolution.

The systematic study of the deformation of general surfaces of the second degree is already entered upon; it is one of those which will give shortly the most important results.

The theory of infinitesimal deformation constitutes to-day one of the most finished chapters of geometry. It is the first somewhat extended application of a general method which seems to have a great future.

Being given a system of differential or partial differential equations, suitable to determine a certain number of unknowns, it is advantageous to associate with it a system of equations which we have called *auxiliary system*, and which determines the systems of solutions infinitely near any given system of solutions. The auxiliary system being necessarily linear, its employment in all researches gives precious light on the properties of the proposed system and on the possibility of obtaining its integration.

The theory of lines of curvature and of asymptotic lines has been notably extended. Not only have been determined these two series of lines for particular surfaces such as the tetrahedral surfaces of Lamé; but also, in developing Moutard's results relative to a particular class of linear partial differential equations of the second order, it proved possible to generalize all that had been obtained for surfaces with lines of curvature plane or spheric, in determining completely all the classes of surfaces for which could be solved the problem of *spheric representation*.

Just so has been solved the correlative problem relative to asymptotic lines in making known all the surfaces of which the infinitesimal deformation can be determined in finite terms. Here is a vast field for research whose exploration is scarcely begun.

The infinitesimal study of rectilinear congruences, already commenced long ago by Dupin, Bertrand, Hamilton, Kummer, has come

to intermingle in all these researches. Ribaucour, who has taken in it a preponderant part, studied particular classes of rectilinear congruences and, in particular, the congruences called *isotropes*, which intervene in the happiest way in the study of minimal surfaces.

The triply orthogonal systems which Lamé used in mathematical physics have become the object of systematic researches. Cayley was the first to form the partial differential equation of the third order on which the general solution of this problem was made to depend.

The system of homofocal surfaces of the second degree has been generalized and has given birth to that theory of general *cyclides* in which may be employed at the same time the resources of metric geometry, of projective geometry, and of infinitesimal geometry. Many other orthogonal systems have been made known. Among these it is proper to signalize the *cyclic* systems of Ribaucour, for which one of the three families admits circles as orthogonal trajectories and the more general systems for which these orthogonal trajectories are simply plane curves.

The systematic employment of imaginaries, which we must be careful not to exclude from geometry, has permitted the connection of all these determinations with the study of the finite deformation of a particular surface.

Among the methods which have permitted the establishment of all these results, it is proper to note the systematic employment of linear partial differential equations of the second order and of systems formed of such equations. The most recent researches show that this employment is destined to renovate most of the theories.

Infinitesimal geometry could not neglect the study of the two fundamental problems set it by the calculus of variations.

The problem of the shortest path on a surface was the object of masterly studies by Jacobi and by Ossian Bonnet. The study of geodesic lines has been followed up; we have learned to determine them for new surfaces. The theory of *ensembles* has come to permit the following of these lines in their course on a given surface.

The solution of a problem relative to the representation of two surfaces one on the other has greatly increased the interest of discoveries of Jacobi and of Liouville relative to a particular class of surfaces of which the geodesic lines could be determined. The results concerning this particular case led to the examination of a new question: to investigate all the problems of the calculus of variations of which the solution is given by curves satisfying a given differential equation.

Finally, the methods of Jacobi have been extended to space of three dimensions and applied to the solution of a question which presented the greatest difficulties: the study of properties of mini-

mum appertaining to the minimal surface passing through a given contour.

XIII

Among the inventors who have contributed to the development of infinitesimal geometry, Sophus Lie distinguishes himself by many capital discoveries which place him in the first rank.

He was not one of those who show from infancy the most characteristic aptitudes, and at the moment of quitting the University of Christiania in 1865, he still hesitated between philology and mathematics.

It was the works of Pluecker which gave him for the first time full consciousness of his true calling.

He published in 1869 a first work on the interpretation of imaginaries in geometry, and from 1870 he was in possession of the directing ideas of his whole career. I had at this time the pleasure of seeing him often, of entertaining him at Paris, where he had come with his friend F. Klein.

A course by M. Sylow followed by Lie had revealed to him all the importance of the theory of substitutions; the two friends studied this theory in the great treatise of C. Jordan; they were fully conscious of the important rôle it was called on to play in so many branches of mathematical science where it had not yet been applied.

They have both had the good fortune to contribute by their works to impress upon mathematical studies the direction which to them appeared the best.

In 1870, Sophus Lie presented to the Academy of Sciences of Paris a discovery extremely interesting. Nothing bears less resemblance to a sphere than a straight line, and yet Lie had imagined a singular transformation which made a sphere correspond to a straight line, and permitted, consequently, the connecting of every proposition relative to straight lines with a proposition relating to spheres, and *vice versa*.

In this so curious method of transformation, each property relative to the lines of curvature of a surface furnishes a proposition relative to the asymptotic lines of the surface attained.

The name of Lie will remain attached to these deep-lying relations which join to one another the straight line and the sphere, those two essential and fundamental elements of geometric research. He developed them in a memoir full of new ideas which appeared in 1872.

The works which followed this brilliant début of Lie fully confirmed the hopes it had aroused. Pluecker's conception relative to the generation of space by straight lines, by curves or surfaces arbitrarily chosen, opens to the theory of algebraic forms a field which has not yet been explored, which Clebsch scarcely began to recognize and settle the boundaries of. But, from the side of infini-

tesimal geometry, this conception has been given its full value by Sophus Lie. The great Norwegian geometer was able to find in it first the notion of congruences and complexes of curves, and afterward that of *contact transformations* of which he had found, for the case of the plane, the first germ in Pluecker. The study of these transformations led him to perfect, at the same time with M. Mayer, the methods of integration which Jacobi had instituted for partial differential equations of the first order; but above all it threw the most brilliant light on the most difficult and the most obscure parts of the theories relative to partial differential equations of higher order. It permitted Lie, in particular, to indicate all the cases in which the method of characteristics of Monge is fully applicable to equations of the second order with two independent variables.

In continuing the study of these special transformations, Lie was led to construct progressively his masterly theory of continuous groups of transformations and to put in evidence the very important rôle that the notion of group plays in geometry. Among the essential elements of his researches, it is proper to signalize the infinitesimal transformations, of which the idea belongs exclusively to him.

Three great books published under his direction by able and devoted collaborators contain the essential part of his works and their applications to the theory of integration, to that of complex units and to the non-Euclidean geometry.

XIV

By an indirect way I have arrived at that non-Euclidean geometry the study of which takes in the researches of geometers a place which grows greater each day.

If I were the only one to talk with you about geometry, I should take pleasure in recalling to you all that has been done on this subject since Euclid or at least from Legendre to our days.

Envisaged successively by the greatest geometers of the last century, the question has progressively enlarged.

It commenced with the celebrated *postulatum* relative to parallels; it ends with the totality of geometric axioms.

The *Elements* of Euclid, which have withstood the action of so many centuries, will have at least the honor before ending of arousing a long series of works admirably enchained which will contribute, in the most effective way, to the progress of mathematics, at the same time that they furnish to the philosophers the most precise and the most solid points of departure for the study of the origin and of the formation of our cognitions.

I am assured in advance that my distinguished collaborator will not forget, among the problems of the present time, this one, which is perhaps the most important, and with which he has occupied himself

with so much success; and I leave to him the task of developing it with all the amplitude which it assuredly merits.

I have just spoken of the elements of geometry. They have received in the last hundred years extensions which must not be forgotten. The theory of polyhedrons has been enriched by the beautiful discoveries of Poinsot on the star polyhedrons and those of Moebius on polyhedrons with a single face. The methods of transformation have enlarged the exposition. We may say to-day that the first book contains the theory of translation and of symmetry, that the second amounts to the theory of rotation and of displacement, that the third rest on homothety and inversion. But it must be recognized that it is due to analysis that the *Elements* have been enriched by their most beautiful propositions.

It is to the highest analysis that we owe the inscription of regular polygons of seventeen sides and analogous polygons. To it we owe the demonstrations, so long sought, of the impossibility of the quadrature of the circle, of the impossibility of certain geometric constructions with the aid of the ruler and the compasses; and to it finally we owe the first rigorous demonstrations of the properties of maximum and of minimum of the sphere. It will belong to geometry to enter upon this ground where analysis has preceded it.

What will be the elements of geometry in the course of the century which has just commenced? Will there be a single elementary book of geometry? It is perhaps America, with its schools free from all programme and from all tradition, which will give us the best solution of this important and difficult question.

Von Staudt has sometimes been called the *Euclid of the nineteenth century;* I would prefer to call him the *Euclid of projective geometry;* but is projective geometry, interesting though it may be, destined to furnish the unique foundation of the future elements?

XV

The moment has come to close this over-long recital, and yet there is a crowd of interesting researches that I have been, so to say, forced to neglect.

I would have loved to talk with you about those geometries of any number of dimensions of which the notion goes back to the first days of algebra, but of which the systematic study was commenced only sixty years ago by Cayley and by Cauchy. This kind of researches has found favor in your country and I need not recall that our illustrious president, after having shown himself the worthy successor of Laplace and Le Verrier, in a space which he considers with us as being endowed with three dimensions, has not disdained to publish, in the *American Journal*, considerations of great interest on the geometries of n dimensions.

A single objection can be made to studies of this sort, and was already formulated by Poisson: the absence of all real foundation, of all *substratum* permitting the presentation, under aspects visible and in some sort palpable, of the results obtained.

The extension of the methods of descriptive geometry, and above all the employment of Pluecker's conceptions on the generation of space, will contribute to take away from this objection much of its force.

I would have liked to speak to you also of the method of equi-pollences, of which we find the germ in the posthumous works of Gauss, of Hamilton's quaternions, of Grassmann's methods, and in general of systems of complex units, of the *analysis situs*, so intimately connected with the theory of functions, of the geometry called *kinematic*, of the theory of abaci, of geometrography, of the applications of geometry to natural philosophy or to the arts. But I fear, if I branched out beyond measure, some analyst, as has happened before, would accuse geometry of wishing to monopolize everything.

My admiration for analysis, grown so fruitful and so powerful in our time, would not permit me to conceive such a thought. But if some reproach of this sort could be formulated to-day, it is not to geometry, it is to analysis it would be proper, I believe, to address it. The circle in which the mathematical studies appeared to be inclosed at the beginning of the nineteenth century has been broken on all sides.

The old problems present themselves to us under a new form, new problems offer themselves, whose study occupies legions of workers.

The number of those who cultivate pure geometry has become prodigiously restricted. Therein is a danger against which it is important to provide. We must not forget that, if analysis has acquired means of investigation which it lacked heretofore, it owes them in great part to the conceptions introduced by the geometers. Geometry must not remain in some sort entombed in its triumph. It is in its school we have learned; our successors must learn never to be blindly proud of methods too general, to envisage the questions in themselves and to find, in the conditions particular to each problem, perhaps a direct way towards a solution, perhaps the means of applying in an appropriate manner the general procedures which every science should gather.

As Chasles said at the beginning of the *Aperçu historique*, "The doctrines of pure geometry offer often, and in a multitude of questions, that simple and natural way which, penetrating to the very source of the truths, lays bare the mysterious chain which binds them to each other and makes us know them individually in the way most luminous and most complete."

Cultivate therefore geometry, which has its own advantages, without wishing, on all points, to make it equal to its rival.

For the rest, if we were tempted to neglect it, it would soon find in the applications of mathematics, as it did once before, means to rise up again and develop itself anew. It is like the giant Antæus who recovered his strength in touching the earth.

THE PRESENT PROBLEMS OF GEOMETRY

BY DR. EDWARD KASNER

[Edward Kasner, Instructor in Mathematics, Columbia University. b. New York City, 1877. B.S. College of the City of New York, 1896; A.M. Columbia University, 1899; Ph.D. *ibid.* 1899. Post-graduate, Fellow in Mathematics, Columbia University, 1897–99 ; Student, University of Göttingen, 1899–1900; Tutor in Mathematics, Columbia University, 1900–05; Instructor, 1905; Member American Mathematical Society; Fellow American Association for Advancement of Science. **Associate editor,** *Transactions American Mathematical Society.*]

In spite of the richness and power of recent geometry, it is noticeable that the geometer himself has become more modest. It was the ambition of Descartes and Leibnitz to discover universal methods, applicable to all conceivable questions; later, the *Ausdehnungslehre* of Grassmann and the quaternion theory of Hamilton were believed by their devotees to be ultimate geometric analyses; and Chasles attributed to the principles of duality and homography the same rôle in the domain of pure space as that of the law of gravitation in celestial mechanics. To-day, the mathematician admits the existence and the necessity of many theories, many geometries, each appealing to certain interests, each to be developed by the most appropriate methods; and he realizes that, no matter how large his conceptions and how powerful his methods, they will be replaced before long by others larger and more powerful.

Aside from the conceivability of other spaces with just as self-consistent properties as those of the so-called ordinary space, such diverse theories arise, in the first place, on account of the variety of objects demanding consideration, — curves, surfaces, congruences and complexes, correspondences, fields of differential elements, and so on in endless profusion. The totality of configurations is indeed not thinkable in the sense of an ordinary assemblage, since the totality itself would have to be admitted as a configuration, that is, an element of the assemblage.

However, more essential in most respects than the diversity in the material treated is the diversity in the points of view from which it may be regarded. Even the simplest figure, a triangle or a circle, has an infinity of properties — indeed, recalling the unity of the physical world, the complete study of a single figure would involve its relations to all other figures and thus not be distinguishable from the whole of geometry. For the past three decades the ruling thought in this connection has been the principle (associated with the names of Klein and Lie) that the properties which are deemed of interest in the various geometric theories may be classified according to the

groups of transformations which leave those properties unchanged. Thus almost all discussions on algebraic curves are connected with the group of displacements (more properly the so-called principal group), or the group of projective transformations, or the group of birational transformations; and the distinction between such theories is more fundamental than the distinction between the theories of curves, of surfaces, and of complexes.

Historically, the advance has been, in general, from small to larger groups of transformations. The change thus produced may be likened to the varying appearance of a painting, at first viewed closely in all its details, then at a distance in its significant features. The analogy also suggests the desirability of viewing an object from several standpoints, of studying geometric configurations with respect to various groups. It is indeed true, though in a necessarily somewhat vague sense, that the more essential properties are those invariant under the more extensive groups; and it is to be expected that such groups will play a predominating rôle in the not far distant future.

The domain of geometry occupies a position, as indicated in the programme of the Congress, intermediate between the domain of analysis on the one hand and of mathematical physics on the other; and in its development it continually encroaches upon these adjacent fields. The concepts of transformation and invariant, the algebraic curve, the space of n dimensions, owe their origin primarily to the suggestions of analysis; while the null-system, the theory of vector fields, the questions connected with the applicability and deformation of surfaces, have their source in mechanics. It is true that some mathematicians regard the discussion of point sets, for example, as belonging exclusively to the theory of functions, and others look upon the composition of displacements as a part of mechanics. While such considerations show the difficulty, if not impossibility, of drawing strict limits about any science, it is to be observed that the consequent lack of definiteness, deplored though it be by the formalist, is more than compensated by the fact that such overlapping is actually the principal means by which the different realms of knowledge are bound together.

If a mathematician of the past, an Archimedes or even a Descartes, could view the field of geometry in its present condition, the first feature to impress him would be its lack of concreteness. There are whole classes of geometric theories which proceed, not merely without models and diagrams, but without the slightest (apparent) use of the spatial intuition. In the main this is due, of course, to the power of the analytic instruments of investigation as compared with the purely geometric. The formulas move in advance of thought, while the intuition often lags behind; in the oft-quoted words of d'Alembert, " L'algèbre est généreuse, elle donne souvent plus qu'on

lui demande." As the field of research widens, as we proceed from
the simple and definite to the more refined and general, we naturally
cease to picture our processes and even our results. It is often neces-
sary to close our eyes and go forward blindly if we wish to advance
at all. But admitting the inevitableness of such a change in the
spirit of any science, one may still question the attitude of the geo-
meter who rests content with his blindness, who does not at least
strive to intensify and enlarge the intuition. Has not such an inten-
sification and enlargement been the main contribution of geometry
to the race, its very *raison d'être* as a separate part of mathematics,
and is there any ground for regarding this service as completed?

From the point of view here referred to, a problem is not to be
regarded as completely solved until we are in position to construct
a model of the solution, or at least to conceive of such a construction.
This requires the interpretation, not merely of the results of a geo-
metric investigation, but also, as far as possible, of the intermediate
processes — an attitude illustrated most strikingly in the works of
Lie. This duty of the geometer, to make the ground won by means
of analysis really geometric, and as far as possible concretely intui-
tive, is the source of many problems of to-day, a few of which will
be referred to in the course of this address.

The tendency to generalization, so characteristic of modern geo-
metry, is counteracted in many cases by this desire for the concrete,
in others by the desire for the exact, the rigorous (not to be con-
fused with the rigid). The great mathematicians have acted on the
principle "Devinez avant de démontrer," and it is certainly true
that almost all important discoveries are made in this fashion. But
while the demonstration comes after the discovery, it cannot there-
fore be disregarded. The spirit of rigor, which tended at first to the
arithmetization of all mathematics and now tends to its exhibition
in terms of pure logic, has always been more prominent in analysis
than in geometry. Absolute rigor may be unattainable, but it can-
not be denied that much remains to be done by the geometers, judg-
ing even by elementary standards. We need refer only to the loose
proofs based upon the invaluable but insufficient enumeration of
constants, the so-called principle of the conservation of number, and
the discussions which confine themselves to the "general case."
Examples abound in every field of geometry. The theorem announced
by Chasles concerning the number of conics satisfying five arbitrary
conditions was proved by such masters as Clebsch and Halphen be-
fore examples invalidating the result were devised. Picard recently
called attention to the need of a new proof of Noether's theorem that
upon the general algebraic surface of degree greater than three every
algebraic curve is a complete intersection with another algebraic
surface. The considerations given by Noether render the result

highly probable, but do not constitute a complete proof; while the exact meaning of the term general can be determined only from the context.

The reaction against such loose methods is represented by Study [1] in algebraic geometry, and Hilbert in differential geometry. The tendency of a considerable portion of recent work is towards the exhaustive treatment of definite questions, including the consideration of the special or degenerate cases ordinarily passed over as unimportant. Another aspect of the same tendency is the discussion of converses of familiar problems, with the object of obtaining conditions at once necessary and sufficient, that is, completely characteristic results. [2]

Another set of problems is suggested by the relation of geometry to physics. It is the duty of the geometer to abstract from the physical sciences those domains which may be expressed in terms of pure space, to study the geometric foundations (or, as some would put it, the skeletons) of the various branches of mechanics and physics. Most of the actual advance, it is true, has hitherto come from the physicists themselves, but undoubtedly the time has arrived for more systematic discussions by the mathematicians. In addition to the importance which is due to possible applications of such work, it is to be noticed that we meet, in this way, configurations as interesting and remarkable as those created by the geometer's imagination. Even in this field, one is tempted to remark, truth is stranger than fiction.

We have now considered, briefly and inadequately, some of the leading ideals and influences which are at work towards both the widening and the deepening of geometry in general; and turn to our proper topic, a survey of the leading problems or groups of problems in certain selected (but it is hoped representative) fields of contemporaneous investigation.

Foundations

The most striking development of geometry during the past decade relates to the critical revision of its foundations, more precisely, its *logical* foundations. There are, of course, other points of view, for

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[1] "[Es ist eine] tief eingewurzelte Gewohnheit vieler Geometer, Satze zu formulieren, die 'im allgemeinen' gelten sollen. d. h. einen klaren Sinn *überhaupt nicht* haben, zudem noch häufig als *allgemein* gültig hingestellt oder mangelhaft begründet werden. [Dies Verfahren wird], trotz etwanigen Verweisungen auf Träger sehr berühmter Namen, späteren Geschlechtern sicher als ganz unzulässig erscheinen, scheint aber in unserem 'kritischen' Zeitalter von vielen als eine *berechtigte Eigentümlichkeit der Geometrie* betrachtet zu werden . . ." *Jahr. Deut. Math.-Ver.*, vol. xi (1902), p. 100.

[2] As an example may be mentioned the theorem of Malus and Dupin, known for almost a century, that the rays emanating from a point are converted, by any refraction, into a normal congruence. Quite recently, Levi-Cività succeeded in showing that this property is characteristic; that is, any normal congruence may be refracted into a bundle.

example, the physical, the physiological, the psychological, the meta-physical, but the interest of mathematicians has been confined to the purely logical aspect. The main results in this direction are due to Peano and his co-workers; but the whole field was first brought prominently to the attention of the mathematical world by the appearance, five years ago, of Hilbert's elegant *Festschrift*.

The central problem is to lay down a system of primitive (unde-fined) concepts or symbols and primitive (unproved) propositions or postulates, from which the whole body of geometry (that is, the geometry considered) shall follow by purely deductive processes. No appeal to intuition is then necessary. "We might put the axioms into a reasoning apparatus like the logical machine of Stanley Jevons, and see all geometry come out of it" (Poincaré). Such a system of concepts and postulates may be obtained in a great (indeed end-less) variety of ways: the main question, at present, concerns the comparison of various systems, and the possibility of imposing lim-itations so as to obtain a unique and perhaps simplest basis.

The first requirement of a system is that it shall be consistent. The postulates must be compatible with one another. No one has yet deduced contradictory results from the axioms of Euclid, but what is our guarantee that this will not happen in the future? The only method of answering this question which has suggested itself is the exhibition of some object (whose existence is admitted) which fulfills the conditions imposed by the postulates. Hilbert succeeded in con-structing such an ideal object out of numbers; but remarks that the difficulty is merely transferred to the field of arithmetic. The most far-reaching result is the definition of number in terms of logical classes as given by Pieri and Russell; but no general agreement is yet to be expected in these discussions. Will the ultimate conclu-sion be the impossibility of a direct proof of compatibility?

More accessible is the question concerning the independence of postulates (and the analogous question of the irreducibility of con-cepts). Most of the work of the last few years has been concentrated on this point. In Hilbert's original system the various groups of axioms (relating respectively to combination, order, parallels, con-gruence, and continuity) are shown to be independent, but the dis-cussion is not carried out completely for the individual axioms. In Dr. Veblen's recently published system of twelve postulates, each is proved independent of the remaining eleven.[1] This marks an ad-vance, but, of course, it does not terminate the problem. In what respect does a group of propositions differ from what is termed a single proposition? Is it possible to define the notion of an absolutely simple postulate? The statement that any two points determine a straight line involves an infinity of statements, and its fulfillment for

[1] *Trans. Amer. Math. Soc.*, vol. v (1904).

certain pairs of points may necessitate its fulfillment for all pairs. If in Euclid's system the postulate of parallels is replaced by the postulate concerning the sum of the angles of a triangle, a well-known example of such a reduction is obtained; for it is sufficient to assume the new postulate for a single triangle, the general result being then deducible. As other examples we may mention Peano's reduction of the Euclidean definition of the plane; and the definition of a collineation which demands, instead of the conversion of *all* straight lines into straight lines, the existence of four simply infinite systems of such straight lines.[1]

These examples illustrate the difficulty, if not the impossibility, of formulating a really fundamental, that is, absolute standard of independence and irreducibility. It is probable that the guiding ideas will be obtained in the discussion of simpler deductive theories, in particular, the systems for numbers and groups.

Two features are especially prominent in the actual development of the body of geometry from its fundamental system. First, the consideration of what may be termed the collateral geometries, which arise by replacing one of the original postulates by its opposite, or otherwise varying the system. Such theories serve to show the limitation of that point of view which restricts the term general geometry (pangeometry) to the Euclidean and non-Euclidean geometries. The variety of possible abstract geometries is, of course, inexhaustible; this is the central fact brought to light by the exhibition of such systems as the non-Archimedean and the non-arguesian. In the second place, much valuable work is being done in discussing the various methods by which the same theorem may be deduced from the postulates, the ideal being to use as few of the postulates as possible. Here again the question of *simplicity* (simplest proof), though it baffles analysis, forces itself upon the attention.

Among the minor problems in this field, it is sufficient to consider that concerning the relation of the theory of volume to the axiom of continuity. This axiom need not be used in establishing the theory of areas of polygons; but after Dehn and others had proved the existence of polyhedra having the same volume though not decomposable into mutually congruent parts (even after the addition of congruent polyhedra), it was stated by Hilbert, and deemed evident generally, that reference to continuity could not be avoided in three dimensions. In a recent announcement [2] of Vahlen's forthcoming *Abstrakte Geometrie* this conclusion is declared unsound. It seems probable, however, that the difference is merely one concerning the interpretation to be given to the term continuity.

[1] Together with certain continuity assumptions. Cf. *Bull. Amer. Math. Soc.*, vol. IX (1903), p. 545.
[2] *Jahr. Deut. Math.-Ver.*, vol. XIII (1904), p. 395.

The work on logical foundations has been confined almost entirely to the Euclidean and projective geometries. It is desirable, however, that other geometric theories should be treated in a similar deductive fashion. In particular, it is to be hoped that we shall soon have a really systematic foundation for the so-called inversion geometry, dealing with properties invariant under circular transformations. This theory is of interest, not only for its own sake and for its applications in function theory, but also because its study serves to free the mind from what is apt to become, without some check, slavery to the projective point of view.

The Curve Concept — Analysis Situs

Although curves and surfaces have constituted the almost exclusive material of the geometric investigation of the thirty centuries of which we have record, it can hardly be claimed that the concepts themselves have received their final analysis. Certain vague notions are suggested by the naïve intuition. It is the duty of mathematicians to create perfectly precise concepts which agree more or less closely with such intuitions, and at the same time, by the reaction of the concepts, to refine the intuition. The problem, evidently, is not at all determinate. It would be of interest to trace the evolution which has actually produced several distinct curve concepts defining more or less extensive classes of curves, agreeing in little beyond the possession of an infinite number of points.

The more familiar special concepts or classes of curves are defined in terms of the corresponding equation $y=f(x)$ or function $f(x)$. Such are, for example: (1) algebraic curves; (2) analytic curves; (3) graphs of functions possessing derivatives of all orders; (4) the curves considered in the usual discussions of infinitesimal geometry, in which the existence of first and second derivatives is assumed; (5) the so-called regular curves with a continuously turning tangent (except for a finite number of corners); (6) the so-called ordinary curves possessing a tangent and having only a finite number of oscillations (maxima and minima) in any finite interval; (7) curves with tangents; (8) the graphs of continuous functions.

How far are such distinctions accessible to the intuition? Of course there are limitations. For over two centuries, from Descartes to the publication of Weierstrass's classic example, the intuition of mathematicians declared the classes (7) and (8) to be identical. Still later it was found that such extraordinary (pathological or crinkly) curves may present themselves in class (7). However, even here partially successful attempts to connect with intuition have been made by Wiener, Hilbert, Schoenflies, Moore, and others.

Let us consider a simpler extension in the field of ordinary curves. If the function $f(x)$ is continuous except for a certain value of x

where there is an ordinary discontinuity, this is indicated by a break in the graph; if f is continuous, but the derivative f' has such a discontinuity, this shows itself by a sharp turn in the curve; if the discontinuity is only in the second derivative, there is a sudden change in the radius of curvature, which is, however, relatively difficult to observe from the figure; finally, if the third derivative is discontinuous, the effect upon the curve is no longer apparent. Does this mean that it is impossible to picture it? Does it not rather indicate a limitation in the usual geometric training which goes only as far as relations expressible in terms of tangency and curvature? For the interpretation of the third derivative it is necessary to consider say the osculating parabola at each point of the curve: in the case referred to, as we pass over the critical point, the tangent line and osculating circle change continuously, but there is a sudden change in the osculating parabola. If in fact our intuition were trained to picture osculating algebraic curves of all orders, it would detect a discontinuity in a derivative of any order. A partial equivalent would be the ability to picture the successive evolutes of a given curve; a complete equivalent would be the picturing of the successive slope curves $y = f'(x)$, $y = f''(x)$, etc. All this requires, evidently, only an increase in the intensity of our intuition, not a change in its nature.

This, however, would not apply to all questions. There are functions which, while possessing derivatives of all orders (then necessarily continuous), are not analytic (that is, not expressible by power series). What is it that distinguishes the analytic curves among this larger class? Is it possible to put the distinction in a form capable of assimilation by an idealized intuition? In short, what is the really geometric definition of an analytic curve? [1]

Much recent work in function theory has had for its point of departure a more general basis than the theory of curves, namely, the theory of sets or assemblages of points, with special reference to the notions of derived set and the various contents or areas. The geometry of point sets must indeed be regarded as one of the most important and promising in the whole field of mathematics. It receives its distinctive character, as compared with the general abstract theory of assemblages (*Mengenlehre*), from the fact that it operates not with all one-to-one correspondences, but with the group of *analysis situs*, the group of *continuous* one-to-one correspondences. From the point of view of the larger group, there is no distinction between a one-dimensional and a two- or many-dimensional continuum (Cantor). This is still the case if the correspondence

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[1] One method of attack would be the interpretation of Pringsheim's conditions; this requires not merely the individual derivative curves, but the limit of the system.

is continuous but not one-to-one (Peano, 1890). In the domain of continuous one-to-one correspondence, however, spaces of different dimensions are not equivalent (Jürgens, 1899).

An important class of curves, much more general than those referred to above, consists of those point sets which are equivalent (in the sense of *analysis situs*) to the straight line or segment of a straight line. This is Hurwitz's simple and elegant geometric formulation of the concept originally treated analytically by Jordan, the most fundamental curve concept of to-day. The closed Jordan curves are defined in analogous fashion as equivalent to the perimeter of a square (or the circumference of a circle).

A curve of this kind divides the remaining points of the plane into two simply connected continua, an inside and an outside. The necessity for proof of this seemingly obvious result is seen from the fact that the Jordan class includes such extraordinary types as the curve with positive content constructed recently by Osgood.[1] Such a separation of the plane may, however, be thought about by other than Jordan curves: the concept of the boundary of a connected region gives perhaps the most extensive class of point sets which deserve to be called curve. Schoenflies proposes a definition for the idea of a simple closed curve which makes it appear as the natural extension, in a certain sense, of the polygon: a perfect set of points *P* which separates the plane into an exterior region *E* and an interior region *I* such that any *E* point can be connected with any *I* point by a path (*Polygonstrecke*) having only one point in common with *P*. This is in effect a converse of Jordan's theorem, and shows precisely how the Jordan curve is distinguished from other types of boundaries of connected regions.

These discussions are mentioned here simply as aspects of a really fundamental problem: the revision of the concepts and results of that division of geometry which has been variously termed *analysis situs*, theory of connection, topology, geometry of situation — a revision to be carried out in the light of the theory of assemblages.[2]

Algebraic Surfaces and Birational Transformations

After the demonstration of the power of the methods based upon projective transformation, — the chief contribution due to the geometers of the first half of the nineteenth century, — attempts were made to introduce other types of one-to-one correspondence or transformation into algebraic geometry; in particular the inversion of William Thomson and Liouville, and the quadratic transformation of Magnus. The general theory of such Cremona transformations was inaugurated by the Italian geometer in his memoir *Sulle tras-*

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[1] *Trans. Amer. Math. Soc.*, vol. IV (1903), p. 107.
[2] Cf. Schoenflies, *Math. Annalen*, vols. LVIII, LIX (1903, 1904).

formazioni geometriche delle figure piane, published in 1863. Within
a few years, Clifford, Noether, and Rosanes, working independently,
established the remarkable result that every Cremona transforma-
tion in a plane can be decomposed into a succession of quadratic
transformations, thus bringing to light the fact that there are at
bottom only two types of algebraic one-to-one correspondence, the
homographic and the quadratic.[1]

The development of a corresponding theory in space has been one
of the chief aims of the geometers of Italy, Germany, and England
for the last thirty years, but the essential question of decomposition
still remains unanswered. Is it possible to reduce the general Cremona
transformation of space to a finite number of fundamental types?

In its application to the study of the properties of algebraic
curves and surfaces, the theory of the Cremona transformation
is usually merged in the more general theory of the birational trans-
formation. By means of the latter, a correspondence is established
which is one-to-one for the points of the particular figure considered
and the transformed figure, but not for all the points of space. In
the plane theory an important result is that a curve with the most
complicated singularities can, by means of Cremona transformations,
be converted into a curve whose only singularities are multiple
points with distinct tangents (Noether); furthermore, by means of
birational transformations, the singularities may be reduced to the
very simplest type, ordinary double points (Bertini). The known
theory of space curves is also, in this aspect, quite complete. The
analogous problem of the reduction of higher singularities of a sur-
face has been considered by Noether, Del Pezzo, Segre, Kobb, and
others, but no ultimate conclusion has yet been obtained.

One principal source of difficulty is that, while in case of two
birationally equivalent curves the correspondence is one-to-one
without exception, on the other hand, in the case of two surfaces,
there may be isolated points which correspond to curves, and just
such irregular phenomena escape the ordinary methods. Again,
not only singular points require consideration, as is the case in the
plane theory, but also singular lines, and the points may be isolated
or superimposed on the lines. Most success is to be expected from
further application of the method of projection from a higher space
due to Clifford and Veronese. In this direction the most important
result hitherto obtained is the theorem, of Picard and Simart, that
any algebraic surface (in ordinary space) can be regarded as the
projection of a surface free from singularities situated in five-dimen-
sional space.

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[1] Segre recently called attention to a case where the usual methods of discus-
sion fail to apply; the proof has been completed by Castelnuovo. Cf. *Atti di
Torino*, vol. XXXVI (1901).

A question which awaits solution even in the case of the plane is that relating to the invariants of the group of Cremona transformations proper. The genus and the moduli of a curve are unaltered by all birational transformations, but the problem arises: Are there properties of curves which remain unchanged by Cremona, although not by other birational transformations? From the fact that birationally equivalent curves need not be equivalent under the Cremona group, it would seem that such invariants — Cremona invariants proper — do exist, but no actual examples have yet been obtained. The problem may be restated in the form: What are the necessary and sufficient conditions which must be fulfilled by two curves if they are to be equivalent with respect to Cremona transformations? Equality of genera and moduli, as already remarked, is necessary but not sufficient.

The invariant theory of birational transformations has for its principal object the study of the linear systems of point groups on a given algebraic curve, that is, the point groups cut out by linear systems of curves. Its foundations were implicitly laid by Riemann in his discussion of the equivalent theory of algebraic functions on a Riemann surface, though the actual application to curves is due to Clebsch. Most of the later work has proceeded along the algebraic-geometric lines developed by Brill and Noether, the promising purely geometric treatment inaugurated by Segre being rather neglected.

The extension of this type of geometry to space, that is, the development of a systematic geometry on a fundamental algebraic surface (especially as regards the linear systems of curves situated thereon), is one of the main tasks of recent mathematics. The geometric treatment is given in the memoirs of Enriques and Castelnuovo, while the corresponding functional aspect is the subject of the treatise of Picard and Simart on algebraic functions of two variables, at present in course of publication.

The most interesting feature of the investigations belonging in this field is the often unexpected light which they throw on the inter-relations of distinct fields of mathematics, and the advantage derived from such relations. For example, Picard (as he himself relates on presenting the second volume of his treatise to the Paris Academy a few months ago) [1] for a long time was unable to prove directly that the integrals of algebraic total differentials can be reduced, in general, to algebraic-logarithmic combinations, until finally a method for deciding the matter was suggested by a theorem on surfaces which Noether had stated some twenty years earlier. Again, in the enumeration of the double integrals of the second species, Picard arrived at a certain result, which was soon noticed

[1] *Comptes Rendus*, February 1, 1904.

to be essentially equivalent to one obtained by Castelnuovo in his investigations on linear systems; and thus there was established a connection between the so-called numerical and linear genera of a surface, and the number of distinct double integrals.[1]

A closely related set of investigations, originating with Clebsch's theorems on intersections and Liouville's on confocal quadrics, may be termed the "geometry of Abel's theorem." As later applications we can merely mention Humbert's memoirs on certain metric properties of curves, and Lie's determination of surfaces of translation.

Investigations in analysis have often suggested the introduction of new types of configurations into geometry. The field of algebraic surfaces is especially fruitful in this respect. Thus, while in the case of curves (excluding the rational) there always exist integrals everywhere finite, this holds for only a restricted class of surfaces; their determination depends on the solution of a partial differential equation which has been discussed in a few special cases.

In addition to such relations between analysis and geometry, important relations arise between various fields of geometry. Just as an algebraic function of one variable is pictured by either a plane curve or a Riemann surface (according as the independent and dependent variables are taken to be real or complex), so an algebraic function of two independent variables may be represented by either a surface in ordinary space or a Riemannian four-dimensional manifold in space of five dimensions. In the case of one variable, the single invariant number (deficiency or genus p) which arises is capable of definition in terms of the characteristics of the curve or the connectivity of the Riemann surface. In passing to two variables, however, it is necessary to consider several arithmetical invariants — just how many is an unsettled question. For the algebraic surface we have, for instance, the geometric genus of Clebsch, the numerical genus of Cayley, and the so-called second genus, each of which may be regarded as a generalization, from a certain point of view, of the single genus of a curve; all are invariant with respect to birational transformation.

The other geometric interpretation, by means of a Riemannian manifold, has rendered necessary the study of the *analysis situs* of higher spaces. The connection of such a manifold is no longer expressed by a single number as in the case of an ordinary surface, but by a set of two or more, the so-called numbers of Betti and Riemann. The detailed theory of these connectivities, difficult and delicate because it must be derived with little aid from the intuition, has been made the subject of an extensive series of memoirs by Poincaré.

From the point of view of analysis, the chief interest in these investigations is the fact that the connectivities are related to the

[1] *Comptes Rendus*, February 22, 1904.

number of integrals of certain types. The chief problem for the geometer, however, is the discovery of the precise relations between the connectivities of the Riemann manifold and the various genera of the algebraic surface. That relations do exist between such diverse geometries — the one operating with all *continuous*, the other with the *algebraic*, one-to-one correspondence — is one of the most striking results of recent mathematics.

Geometry of Multiple Forms

For some time after its origin, the linear invariant theory of Boole, Cayley, and Sylvester confined itself to forms containing a single set of variables. The needs of both analysis and geometry, however, have emphasized the importance and the necessity of further development of the theory of forms containing two or more sets of variables (of the same or different type), so-called multiple forms.

In the plane we have both point coördinates (x) and line coördinates (u). A form in x corresponds to a point curve (locus), a form in u to a line curve (envelope), and a form involving both x and u to a connex. The latter was introduced into geometry, some thirty years ago, by Clebsch, the suggestion coming from the fact that, even in the study of a simple form in x, covariants in x and u present themselves, so that it seemed desirable to deal with such forms *ab initio*.

Passing to space, we meet three simple elements, the point (x), the plane (u), and the line (p). Forms in a single set of variables represent, respectively, a surface as point locus, a surface as plane envelope, and a complex of lines. The compound elements composed of two simple elements are the point-plane, the point-line, and the plane-line. The first type, leading to point-plane connexes, has been studied extensively during the past few years; the second to a more limited degree; the third is merely the dual of the second. To complete the series, the case of the point-line-plane as element, or forms involving x, u, and p, requires investigation.

In the corresponding n-dimensional theory it is necessary to take account of n simple elements and the various compound elements formed by their combinations.

The importance of such work is twofold: First, on account of connection with the algebra of invariants. A fundamental theorem of Clebsch states that, in the investigation of complete systems of comitants, it is sufficient to consider forms involving not more than one set of variables of each type: if in the given forms the types are involved in any manner, it is possible to find an equivalent reduced system of the kind described. On the other hand, it is impossible to reduce the system further, so that the introduction of the n types

of variables is necessary for the algebraically complete discussion. Geometry must accordingly extend itself to accommodate the configurations defined by the new elements.

Second, on account of connection with the theory of differential equations. The ordinary plane connex in x, u, assigns to each point of the plane a certain number of directions (represented by the tangents drawn to the corresponding curve), and thus gives rise to an (algebraic) differential equation of the first order in two variables; the point-plane connex in space, associating with each point a single infinity of incident planes, defines a partial differential equation of the first order; the point-line connex yields a Monge equation. The point-line-plane case has not yet been interpreted from this point of view.

One special problem in this field deserves mention, on account of its many applications. This is the study of the system composed of a quadric form in any number of variables and a bilinear form in contragredient variables, that is, a quadric manifold and an arbitrary (not merely automorphic) collineation in n-space. For $n=6$, for example, this corresponds to the general linear transformation of line or sphere coördinates.

In addition to forms containing variables of different types, the forms involving several sets of variables of the same type require consideration. Forms in two sets of line coördinates present themselves in connection with the pfaffian problem of differential systems. The main interest attaches, however, to forms in sets of point coördinates, since it is these which occur in the theory of contact transformations and of multiple correspondences. For example, while the ordinary homography on a line is represented by a bilinear form in binary variables, the trilinear form in similar variables gives rise to a new geometric variety, the so-called homography of the second class (associating with any two points a unique third point), which has applications to the generation of cubic surfaces and to the constructions at the basis of photogrammetry. The theory of multilinear forms in general deserves more attention than it has yet received.

Other important problems, connected with the geometric phases of linear invariant theory, can merely be mentioned: (1) The general geometric interpretation of what appears algebraically as the simplest projective relation, namely, apolarity. (2) The invariant discussion of the simpler discontinuous varieties, for example, the polygon considered as n-point or as n-line.[1] (3) The establishment of a system of forms corresponding to the general space curve. (4) The study of the properties and the groups of the configurations cor-

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[1] Cf. F. Morley "On the geometry whose element is the 3-point of a plane," *Trans. Amer. Math. Soc.*, vol. v (1904). E. Study in his *Geometrie der Dynamen* develops a new foundation for kinematics by employing as element the *Soma* or trirectangular trihedron.

responding in hyperspace to the simpler systems of invariants. (5) Complete systems of orthogonal or metric invariants for the simpler curves.[1]

Transcendental Curves

To reduce to systematic order the chaos of non-algebraic curves has been the aspiration of many a mathematician; but, despite all efforts, we have no theory comparable with that of algebraic curves. The very vagueness and apparent hopelessness of the question is apt to repel the modern mathematician, to cause him to return to the more familiar field. The resulting concentration has led to the powerful methods, already referred to, for studying algebraic varieties. In the transcendental domain, on the other hand, we have a multitude of interesting but particular geometric forms, — some suggested by mechanics and physics, others derived from their relation to algebraic curves, or by the interpretation of analytic results — a few thousands of which have been considered of sufficient importance to deserve specific names.[2] The problem at issue is then a practical one (comparable with corresponding discussions in natural history): to formulate a principle of classification which will apply, not to all possible curves, but to as many as possible of the usual important transcendental curves.

The most fruitful suggestion hitherto applied has come from the consideration of differential equations: almost all the important transcendental curves satisfy algebraic differential equations, and these in the great majority of cases are of the first order. Hence the need of a systematic discussion of the curves defined by any algebraic equation $F(x, y, y') = 0$, the so-called *panalgebraic* curves of Loria. If F is of degree n in y' and of degree ν in x, y, the curve is said to belong to a system with the characteristics (n, ν), and we thus have an important basis for classification. Closely related is the theory of the Clebsch connex; this figure, it is true, is considered as belonging to algebraic geometry, but it defines (by means of its principal coincidence) a system of usually transcendental panalgebraic curves.

Both points of view appear to characterize certain systems of curves rather than individual curves. The following interpretation may serve as a simple geometric definition of the curves considered.

With any plane curve C we may associate a space curve in this way: at each point of C erect a perpendicular to the plane whose length represents the slope of the curve at that point; the locus of the end points of these perpendiculars is the associated space curve

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[1] Here would belong in particular the theory of algebraic curves based on linkages. Little advance has been made beyond the existence theorems of Kempe and Koenigs. An important unsolved problem is the determination of the linkage with minimum number of pieces by which a given curve can be described.

[2] Cf. Loria, *Spezielle Kurven*, Leipzig, 1902.

C'. Not every space curve is obtained in this way, but only those whose tangents belong to a certain linear complex. If C is algebraic so is C', and then an infinite number of algebraic surfaces may be passed through the latter. If C is transcendental, so is C', and usually no algebraic surface can be passed through it. Sometimes, however, one such algebraic surface F exists. (If there were two, C' and C would be algebraic.) It is precisely in this case that the curve C is panalgebraic in the sense of Loria's theory. That such a curve belongs to a definite system is seen from the fact that while the surface F is unique, it contains a singly infinite number of curves whose tangents belong to the linear complex mentioned, and the orthogonal projections of these curves constitute the required system.

The principal problems in this field which require treatment are: first, the exhaustive discussion of the simplest systems, corresponding to small values of the characteristics n and ν; second, the study of the general case in connection with (1) algebraic differential equations, (2) connexes, and (3) algebraic surfaces and linear complexes.

Natural or Intrinsic Geometry

In spite of the immediate triumph of the Cartesian system at the time of its introduction into mathematics, rebellion against what may be termed the tyranny of extraneous coördinates, first expressed in the *Characteristica geometrica* of Leibnitz, has been an ever-present though often subdued influence in the development of geometry. Why should the properties of a curve be expressed in terms of x's and y's which are defined not by the curve itself, but by its relation to certain arbitrary elements of reference? The same curve in different positions may have unlike equations, so that it is not a simple matter to decide whether given equations represent really distinct or merely congruent curves. The idea of the so-called natural or intrinsic coördinates had its birth during the early years of the nineteenth century, but it is only the systematic treatment of recent years which has created a new field of geometry.

For a plane curve there is at each point the arc s measured from some fixed point on the curve, and the radius of curvature ρ; these intrinsic coördinates are connected by a relation $\rho = f(s)$ which is precisely characteristic of the curve, that is, the curves corresponding to the equation differ only in position. There is, however, still something arbitrary in the point taken as origin. This is eliminated by taking as coördinates ρ and its derivative δ taken with respect to the arc; so that the final intrinsic equation is of the form $\delta = F(\rho)$. There is no difficulty in extending the method to space curves. The two natural equations necessary are here $\tau = \phi(\rho)$, $\delta = \psi(\rho)$, where ρ and τ are the radii of first and second curvature and δ is the arc derivative of ρ.

The application to surfaces is not so evident. Thus, in Cesaro's standard work, while the discussion of curves is consistently intrinsic, this is true to only a slight extent in the treatment of surfaces. The natural geometry of surfaces is in fact only in process of formation. Bianchi proposes as intrinsic the familiar representation by means of the two fundamental quadratic differential forms; but, although it is true that the surfaces corresponding to a given pair of forms are necessarily congruent, there is the disadvantage, arising from the presence of arbitrary parameters, that the same surface may be represented by distinct pairs of forms. One way of overcoming this difficulty is to introduce the common feature of all pairs corresponding to a surface, that is, the invariants of the forms: in this direction we may cite Ricci's principle of covariant differentiation and Maschke's recent application of symbolic methods.

The basis of natural geometry is, essentially, the theory of differential invariants. Under the group of motions, a given configuration assumes ∞^r positions, where r is in general 6, but may be smaller in certain cases. The r parameters which thus enter in the analytic representation may be eliminated by the formation of differential equations. The aim of natural geometry is to express these differential equations in terms of the simplest geometric elements of the given configuration.

The beginning of such a discussion of surfaces was given by Sophus Lie in 1896 and his work has been somewhat simplified by Scheffers. As natural coördinates we may take the principal radii of curvature R_1, R_2, at a point of the surface, and their derivatives

$$\delta_{11} = \frac{dR_1}{ds_2} \qquad \delta_{12} = \frac{dR_1}{ds_2} \qquad \delta_{21} = \frac{dR_2}{ds_1} \qquad \delta_{22} = \frac{dR_2}{ds_2}$$

taken in the principal directions. For a given surface (excluding the Weingarten class) the radii are independent, and there are four relations of the form

$$\delta_{11} = f_{11}(R_1,\ R_1), \quad \delta_{12} = f_{12}(R_1,\ R_2), \quad \delta_{21} = f_{21}(R_1,\ R_2),$$
$$\delta_{22} = f_{22}(R_1,\ R_2).$$

Conversely, these equations are not satisfied by any surfaces except those congruent or symmetric to the given surface.

It is to be noticed that four equations thus appear to be necessary to define a surface, although two are sufficient for a twisted curve. If a single equation in the above-mentioned natural coördinates is considered, it is not, as in the case of ordinary coördinates, characteristic: surfaces not congruent or symmetric to the given surface would satisfy the equation. The apparent inconsistency which arises is removed, however, by the fact that the four natural equations are

dependent.[1] It is just this that makes the subject difficult as compared with the theory of curves, in which the defining equations are entirely arbitrary. The questions demanding treatment fall under these two headings: first, the derivation of the natural equations of the familiar types of surfaces, and second, the study of the new types that correspond to equations of simple form. The natural geometry of the Weingarten class of surfaces requires a distinct basis.

The fact that intrinsic⁕ coördinates are, at bottom, differential invariants with respect to the group of motions, suggests the extension of the same idea to the other groups. Thus in the projective geometry of arbitrary (algebraic or transcendental) curves, coördinates are required which, unlike the distances and angles ordinarily used, are invariant under projection. These might, for example, be introduced as follows. At each point of the general curve C, there is a unique osculating cubic and a unique osculating W (self-projective) curve. Connected with each of these osculating curves is an absolute projective invariant defined as an anharmonic ratio. These ratios may then be taken as natural projective coördinates γ and ω, and the natural equation on the curve is of the form $\gamma = f(\omega)$. The principal advantage of such a representation is that the necessary and sufficient condition for the equivalence of two curves under projective transformations is simply the identity of the corresponding equations.

Returning to the theory of surfaces, natural coördinates may be introduced so as to fit into the so-called geometry of a flexible but inextensible surface, originated by Gauss, in which the criterion of equivalence is applicability, or, according to the more accurate phraseology of Voss, isometry. Intrinsic coördinates must then be invariant with respect to bending (*Biegungsinvariante*). This property is fulfilled, for example, by the Gaussian curvature κ and the differential parameters connected with it $\lambda = \Delta (\kappa, \kappa)$, $\mu = \Delta(\kappa, \lambda)$, $\nu = \Delta(\lambda, \lambda)$, all capable of simple geometric interpretation. The intrinsic equations are then of the form $\mu = \phi(\kappa, \lambda)$, $\nu = \phi(\kappa, \lambda)$.

A pair of equations of this kind thus represent, not so much a single surface S, as the totality of all surfaces applicable on S (or into which S may be bent) — a totality which is termed a complete group G, since no additional surfaces are obtained when the same process is applied to any member of the totality. The discussion of such groups is ordinarily based on the first fundamental form (representing the squared element of length), since this is the same for isometric surfaces; though of course it changes on the introduction of new parameters.

The simplest example of a complete isometric group is the group

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[1] The three relations connecting the functions f_{11}, f_{12}, f_{21}, f_{22} have been worked out recently by S. Heller, *Math. Annalen*, vol. LVIII (1904).

typified by the plane, consisting of all the developable surfaces. In this case the equations of the group may be obtained explicitly, in terms of eliminations, differentiations, and quadratures. This is, however, quite exceptional; thus, even in the case of the surfaces applicable on the unit sphere (surfaces of constant Gaussian curvature $+1$), the differential equation of the group has not been integrated explicitly. In fact, until the year 1866, not a single case analogous to that of the developable surfaces was discovered. Weingarten, by means of his theory of evolutes, then succeeded in determining the complete group of the catenoid and of the paraboloid of revolution, and, some twenty years later, a fourth group defined in terms of minimal surfaces.

During the past decade, the French geometers have concentrated their efforts in this field mainly on the arbitrary paraboloid (and to some extent on the arbitrary quadric). The difficulties even in this extremely restricted and apparently simple case are great, and are only gradually being conquered by the use of almost the whole wealth of modern analysis and the invention of new methods which undoubtedly have wider fields of application. The results obtained exhibit, for example, connections with the theories of surfaces of constant curvature, isometric surfaces, Backlund transformations, and motions with two degrees of freedom. The principal workers are Darboux, Goursat, Bianchi, Thybaut, Cosserat, Servant, Guichard, and Raffy.

Geometry im Grossen

The questions we have just been considering, in common with almost all the developments of general or infinitesimal geometry, deal with the properties of the figure studied *im Kleinen*, that is, in the sufficiently small neighborhood of a given point. Algebraic geometry, on the other hand, deals with curves and surfaces in their entirety. This distinction, however, is not inherent in the subject-matter, but is rather a subjective one due to the limitations of our analysis: our results being obtained by the use of power series are valid only in the region of convergence. The properties of a curve or surface (assumed analytic) considered as a whole are represented not by means of function elements, but by means of the entire functions obtained say by analytic continuation.

Only the merest traces of such a transcendental geometry *im Grossen* are in existence, but the interest of many investigators is undoubtedly tending in this direction. The difficulty of the problems which arise (in spite of their simple and natural character) and the delicacy of method necessary in their treatment may be compared to the corresponding problems and methods of celestial mechanics. The calculation of the ephemeris of a planet for a limited time is

a problem *im Kleinen*, while the discovery of periodic orbits and the theory of the stability of the solar system are typical problems *im Grossen*.

The principal problems in this field of geometry are connected with closed curves and surfaces. Of special importance are the investigations relating to the closed geodesic lines which can be drawn on a given surface, since these are apt to lead to the invention of methods applicable to the wider field of dynamics. Geodesics may in fact be defined dynamically as trajectories of a particle constrained to the surface and acted upon either by no force or by a force due to a force function U whose first differential parameter is expressible in terms of U. The few general theorems known in this connection are due in the main to Hadamard (*Journal de Mathématiques*, 1897, 1898). Thus, on a closed surface whose curvature is everywhere positive, a point describing a geodesic must cross any existing closed geodesic an infinite number of times, so that, in particular, two closed geodesics necessarily intersect.[1] On a surface of negative curvature, under certain restrictions, there exist closed geodesics of various topological types, as well as geodesics which approach these asymptotically.

As regards surfaces all of whose geodesics are closed, the investigations have been confined entirely to the case of surfaces of revolution, the method employed being that suggested by Darboux in the *Cours de Mécanique* of Despeyrons. Last year Zoll[2] succeeded in determining such a surface (beyond the obvious sphere) which differs from the other known solutions in not having any singularities. Analogous problems in connection with closed lines of curvature and asymptotic lines will probably soon secure the consideration they deserve.

A problem of different type is the determination of applicability criteria valid for entire surfaces. The ordinary conditions (in terms of differential parameters) assert, for example, the applicability of any surface of constant positive curvature upon a sphere; but the bending is actually possible only for a sufficiently small portion of the surface. A spherical surface as a whole cannot be applied on any other surface, that is, cannot be bent without extension or tearing. This result is analogous to the theorem known to Euclid, although first proved by Cauchy, that a closed convex polyhedral surface is necessarily rigid. Lagrange, Minding, and Jellet stated the result for all closed convex surfaces, but the complete discussion is due to H. Liebmann.[3] The theory of the deformation of concave surfaces

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[1] In a paper read before the St. Louis meeting of the American Mathematical Society, Poincaré stated reasons which make very probable the existence of at least three closed geodesics on a surface of this kind.

[2] *Math. Annalen*, vol. LVII (1903).

[3] *Göttingen Nachrichten*, 1899; *Math. Annalen*, vols. LIII, LIV.

is far more complicated, and awaits solution even in the case of polyhedral surfaces.

Beltrami's visualization of Lobachevsky's geometry by picturing the straight lines of the Lobachevsky plane as geodesics on a surface of constant negative curvature is well known. However, since the known surfaces of this kind, like the pseudosphere, have singular lines, this method really depicts only part of the plane. In fact Hilbert (*Transactions of the American Mathematical Society for 1900*), by very refined considerations, has shown that an analytic surface of constant negative curvature which is everywhere regular does not exist, so that the entire Lobachevsky plane cannot be depicted by any analytic surface.[1] There remains undecided the possibility of a complete representation by means of a non-analytic surface. The partial differential equation of the surfaces of negative constant curvature is of the hyperbolic type and hence does admit non-analytic solutions.[2] (This is not true for surfaces of positive curvature, since the equation is then of elliptic type.) The discussion of non-analytic curves and surfaces will perhaps be one of the really new features of future geometry, but it is not yet possible to indicate the precise direction of such a development.[3]

Other theories belonging essentially to geometry *im Grossen* are the questions of *analysis situs*, or topology, to which reference has been made on several occasions, and the properties of the very general convex surfaces introduced by Minkowski in connection with his *Geometrie der Zahlen*.

Systems of Curves — Differential Equations

Although projective geometry has for its domain the investigation of all properties unaltered by collineation, attention has been confined almost exclusively to the algebraic configuration, so that projective is often confused with algebraic geometry. To the more general projective geometry belong, for example, the ideas of osculating conic of an arbitrary curve and the asymptotic lines of an arbitrary surface, and Mehmke's theorem which asserts that when two surfaces touch each other, the ratio of their Gaussian curvatures at the point of contact is an (absolute) projective invariant. The field for investigation in this direction is of course very extensive, but we may mention as a problem of special importance the deriva-

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[1] The entire projective plane, on the other hand, can be so depicted on a surface devised by W. Boy (*Inaugural Dissertation*, Göttingen, 1901).

[2] According to Bernstein (*Math. Annalen*, vol. LIX, 1904, p. 72), the proof given by Lütkemeyer (*Inaugural Dissertation*, Göttingen, 1902) is not valid, though the conclusion is correct.

[3] Lebesgue (*Comptes Rendus*, 1900, and *Thèse*, 1902) has examined the theory of surfaces applicable on a plane without assuming the existence of derivatives for the defining functions, and thereby obtains an example of a non-ruled developable.

tion of the conditions for the projective equivalence of surfaces in terms of their fundamental quadratic forms.

Coördinate with what has just been stated, that general configurations may be studied from the projective point of view, is the fact that algebraic configurations may be studied in relation to general transformation theory. One may object that, with respect to the group of all (analytic) point transformations, the algebraic configurations do not form a *body*,[1] that is, are not converted into algebraic configurations; but such a *body* is obtained by adjoining to the algebraic all those transcendental configurations which are equivalent to algebraic. As this appears to have been overlooked, it seems desirable to give a few concrete instances, of interest in showing the effect of looking at familiar objects from a new and more general point of view.

As a first example, consider the idea of a linear system of plane curves. In algebraic geometry, a linear system is understood to be one represented by an equation of the form

$$F_0 + \lambda_1 F_1 + \lambda_2 F_2 + \cdots + \lambda_\kappa F_\kappa = 0,$$

where the λ's are parameters and the F's are polynomials in x, y. On the other hand, in general (infinitesimal) geometry, a system is defined to be linear when it can be reduced (by the introduction of new parameters) to the same form where the F's are arbitrary functions. The first definition is invariant under the projective group; the second, under the group of all point transformations. If now we apply the second definition to algebraic curves, the result does not coincide with that given by the first definition. Thus, every one-parameter system is linear in the general sense, while only pencils of curves are linear in the projective sense. The first case of real importance is, however, the two-parameter system, since here each point of view gives restricted, though not identical, types. An example in point is furnished by the vertical parabolas tangent to a fixed line, the equation of the system being $y = (ax + b)^2$. From the algebraic or projective point of view, this is a quadratic system since the parameters are involved to the second degree; but the system is linear from the general point of view since its equation may be written $ax + b - \sqrt{y} = 0$. This suggests the problem: Determine the systems of algebraic curves which are linear in the general sense.

As a second example, consider, from both points of view, the equivalence of pencils of straight lines in the plane. By means of collineations any two pencils may be converted into any other two;

[1] The most extensive group for which the algebraic configurations form a body consists of all algebraic transformations. It is rather remarkable that even this theory has received no development.

[2] Halphen, Laguerre, Forsyth. This theory has been extended to simultaneous equations and applied geometrically by E. J. Wilczynski (*Trans. Amer. Math. Soc.*, 1901–1904).

but if three pencils are given, it is necessary to distinguish the case where the three base points are in a straight line from the case where they are not so situated.   We thus have two projectively distinct cases, which may be represented canonically by: (1) $x =$ const., $y =$ const., $x + y =$ const., and (2) $x =$ const., $y =$ const., $y/x =$ const. The first type may, however, be converted into the second by the transcendental transformation $x_1 = e^x$, $y_1 = e^y$, so that, in the general group of point transformations, all sets of three pencils are equivalent. The discussion for four or more pencils yields the rather surprising result that the projective classification remains valid for the larger group.

Dropping these special considerations on algebraic systems, let us pass to the theory of arbitrary systems of curves, or, what is equivalent, the geometry of differential equations. While belonging to the cycle of theories due primarily to Sophus Lie, it has received little development in the purely geometric direction. Most attention has been devoted to special classes of differential equations with respect to special groups of transformations. Thus there is an extensive theory of the homogeneous linear equations with respect to the group $x_1 = \xi(x)$, $y_1 = y\eta(x)$ which leaves the entire class invariant.[1] A special theory which deserves development is that of equations of the first order with respect to the infinite group of conformal transformations.

As regards the general group of all point transformations, all equations of the first order are equivalent, so that the first case of interest is the theory of the two-parameter systems. The invariants of the differential equation of second order have been discussed most completely in the prize essay of A. Tresse (submitted to the Jablonowski Gesellschaft in 1896), with application to the equivalence problem. A specially important class, treated earlier by Lie and R. Liouville, consists of the equations of cubic type
$$y'' = Ay'^3 + By'^2 + Cy' + D,$$
where the coefficients are functions of x, y. It includes, in particular, the general linear system and all systems capable of representing the geodesics of any surface. While the analytical conditions which characterize these subclasses are known, little advance has been made in their geometric interpretation.

Perhaps the simplest configuration belonging to the field considered, that is, having properties invariant under all point transformations, is that composed of three simply infinite systems of curves, which may be represented analytically by an equation of third degree in y' with one-valued functions of x, y for coefficients. In the case of equations of the fourth and higher degree in y', certain invariants

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[1] The elementary (metric) theory of curve systems has been too much neglected; it may be compared in interest and extent with the usual theory of surfaces.

may be found immediately from the fact that when x and y undergo an arbitrary transformation, the derivative y' undergoes a fractional linear transformation (of special type). The invariants found from this algebraic principle are, however, in a sense, trivial, and the real problem remains almost untouched: to determine the essential invariants due to the differential relations connecting the coefficients in the linear transformation of the derivative.

General Theory of Transformations

Closely connected with the geometry of differential equations that we have been considering is the geometry of point transformations. In the former theory the transformations enter only as instruments, in the latter these instruments are made the subject-matter of the investigation. The distinction is parallel to that which occurs in projective geometry between the theory of projective properties of curves and surfaces and the properties of collineations. (It may be remarked, however, that although a transformation is generally regarded as dynamic and a configuration as static, the distinction is not at all essential. Thus a point transformation or correspondence between the points of a plane may be viewed as simply a double infinity of point pairs; on the other hand, a curve in the plane may be regarded as the equivalent of a correspondence between the points of two straight lines.[1])

We consider first two problems concerning the general (analytic) point transformation which are of interest and importance from the theoretic standpoint. The one relates to the discussion of the character of such a transformation in the neighborhood of a given point. Transon's theorem states that the effect of any analytic transformation upon an infinitesimal region is the same as that of a projective transformation. This is true, however, only in general; it ceases to hold when the derivatives of the defining functions vanish at the point considered. What is the character of the transformation in the neighborhood of such singular points ?

A more fundamental problem relates to the theory of equivalence. Consider a transformation T which puts in correspondence the points P and Q of a plane. Let the entire plane be subjected to a transformation S which converts P into P' and Q into Q'. We thus obtain a new transformation T' in which P' and Q' are corresponding points. This is termed the transform of T by means of S, the relation being expressed symbolically by $T' = S^{-1}TS$. The question then arises whether all transformations are equivalent, that is, can any one be converted into any other in the manner defined. The answer depends on certain functional equations which also arise in connection

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[1] Geometry on a straight line, in its entirety, is as rich as geometry in a plane or in space of any number of dimensions.

with the question whether an arbitrary transformation belongs to a continuous group. The problem deserves treatment not merely for the analytic transformations, but also for the algebraic and for the continuous transformations.[1]

Aside from such fundamental questions, further development is desirable both in the study of the general properties (associated curve systems and contact relations) of an arbitrary transformation, and in the introduction of new special types of transformation, for instance, those which may be regarded as natural extensions of familiar types.

The main problems in the theory of point transformation are connected with certain fields of application which we now pass in review.

1. *Cartography.* A map may be regarded, abstractly, as the point by point representation of one surface upon another, the case of especial practical importance being, of course, the representation of a spherical or spheroidal surface upon the plane. As it is impossible to map any but the developable surfaces without distortion upon a plane, the chief types of available representation are characterized by the invariance of certain elements, as angles or areas, or the simple depiction of certain curves, as of geodesics by straight lines. Most attention has been devoted to the conformal type, but the question proposed by Gauss remains unsolved: what is the *best* conformal representation of a given surface on the plane, that is, the one accompanied by the minimum distortion? The answer, of course, depends on the criterion adopted for measuring the degree of distortion, and it is in this direction that progress is to be expected.

2. *Mathematical theory of elasticity.* As a geometric foundation for the mechanics of continua, it is necessary to study the most general deformation of space, defined say by putting x_1, y_1, z_1 equal to arbitrary functions of x, y, z. The most elegant analytical representation, as given for instance in the memoir of E. and F. Cosserat (*Annales de Toulouse*, volume 10), is obtained by introducing the elements of length ds and ds_1 before and after deformation, and the related quadratic differential form $ds_1^2 - ds^2 = 2\epsilon_1 dx^2 + 2\epsilon_2 dy^2 + 2\epsilon_3 dz^2 + 2\gamma_1\, dydz + 2\gamma_2\, dxdz + 2\gamma_3\, dxdy$. The theory is thus seen to be analogous to though of course more complicated than the usual theory of surfaces. The six functions of x, y, z which appear as coefficients in this form are termed the components of the deformation. Their

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[1] This problem is not to be confused with the similar (but simpler) question connected with Lie's division of (analytic) groups into *demokratisch* and *aristokratisch*. In those of the first kind all the infinitesimal transformations are equivalent, in those of the second there exist non-equivalent infinitesimal transformations. Lie shows that all finite groups are *aristokratisch*, while the groups of all (analytic) point and contact transformations are *demokratisch*. Cf. *Leipziger Berichte*, vol. XLVII (1895), p. 271.

importance is due to the fact that they vanish only when the transformation is a rigid displacement, so that two deformations have the same components when, and only when, they differ by a displacement. The case where the components are constants leads to the homogeneous deformation (or affine transformation of the geometers), the type considered almost exclusively in the usual discussions of elasticity. It would seem desirable to study in detail the next case which presents itself, namely, that in which the components are linear functions of x, y, z.

In the general deformation, the six components are not independent, but are connected by nine differential equations analogous to those of Codazzi. The fact that a transformation is defined by three independent functions indicates, however, that there should be only three distinct relations between the components. This means that the nine equations of condition which occur in the standard theory are themselves interdependent; but their relations (analogous to syzygies among syzygies in the algebra of forms) do not appear to have been worked out.

3. *Vector fields.* From its beginning in the Faraday-Maxwell theory of electricity until the present day, the course which the discussion of vector fields has followed has been guided almost entirely by external considerations, namely, the physical applications. While this is advantageous in many respects, it cannot be denied that it has led to lack of symmetry and generality. The time seems to be ripe for a more systematic mathematical development. The vector field deserves to be introduced as a standard form into geometry.

Abstractly, such a field is equivalent to a point transformation of space, since each is represented by three scalar relations in six variables. Instead of taking these variables as the coördinates of corresponding points, it is more convenient to consider three as the coördinates x, y, z of a particle and the other three as components u, v, w of its velocity; we thus picture the set of functional relations by means of the steady motion of a hypothetical space-filling fluid. This image should be of service even in abstract analysis; for its rôle is analogous to that of the curve in dealing with a single relation between two variables. The streaming of a material fluid is, of course, not sufficiently general for such a purpose, since, in virtue of the equation of continuity, it images only a particular class of vector fields.

In addition to the ordinary vector fields, physics makes use of so-called hypervector fields, which, geometrically, lead to configurations consisting of a triply infinite system of quadric surfaces, one for each point of space. In the special case of interest in hydrodynamics (irrotational motion), the configuration simplifies in that the quadrics are ellipsoids about the corresponding points as centres.

This is equivalent to the *tensor* field which arises in studying the moments of inertia of an arbitrary distribution of mass. The more general case actually arises in Maxwell's theory of magnetism.

4. As a final domain of application we mention the class of questions which have received systematic treatment, under the title of *nomography*, only during the past few years. This subject deals with the methods of representing graphically, in a plane, functional relations containing any number of variables. Thus a function of two independent variables, $z = f(x, y)$, may be represented by the system of plane curves $f(x, y) = c$, each marked with the corresponding value of the parameter. This "parametered" system is then a cartesian graphical table, which is the simplest type of abacus or nomogram.

By means of any point transformation, one nomogram is converted into another which may serve to represent the same functional relation. The importance of this process of conversion (the so-called *anamorphosis* of Lalanne and Massau) depends on the fact that it may replace a complicated table by a simpler. The problems which arise (for example, the determination of all relations between three variables which can be represented by a nomogram composed of three systems of straight lines[1]) are of both practical and theoretical interest. The literature is scattered through the French, Italian, and German technological journals, but a systematic presentation of the main results is to be found in the *Traité de Nomographie* of d'Ocagne (Paris, 1899).

We return to the abstract theory of transformations. The type of transformation we have been considering, converting point into point, is only a special case of more general types. The most important extension hitherto made depends upon the introduction of differential elements. Thus the lineal element or directed point (x, y, y') leads to transformations which in general convert a point into a system of elements; when the latter form a curve, every curve is converted into a curve and the result is termed a contact transformation. Backlund has shown that no extension results from the elements of second or higher order: osculation transformations are necessarily contact transformations. The discussion of elements of infinitely high order, defined by an infinite set of coördinates $(x, y, y', y'', \cdots)$, may perhaps lead to a real extension. The question may be put in this form: Are there transformations (in addition to ordinary contact transformations) which convert analytic curves into analytic curves in such a way that contact is an invariant relation? The idea of curve transformation in general will probably be worked

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[1] The case of three systems of circles has also been discussed. See d'Ocagne, *Journal de l'Ecole Polytechnique*, 1902.

out in the near future: what is the most general mode of setting up a correspondence which associates with every Jordan curve another Jordan curve? Such discussions are aspects of geometry with an infinite number of dimensions.

After a review of the kind given in this paper, one is tempted to ask: What is it which influences the mathematician in selecting *certain* (out of an infinite number of equally conceivable) problems for investigations? It is true, of course, that his subject is ideal, self-created, and that "Das Wesen der Mathematik liegt in. ihrer Freiheit." Georg Cantor would indeed replace the term *pure* mathematics by *free* mathematics. This freedom, however, is not entirely caprice. The investigators of each age have always felt it their duty to deal with the unsolved questions and to generalize the results and conceptions inherited from the past, to correlate with other fields of contemporaneous thought, to keep in contact, as far as possible, with the whole body of truth. This is not all, however. The influence of æsthetic considerations, though less subject to analysis, has been, and still is, of at least equal importance in guiding the course of mathematical development.

SHORT PAPERS

The Section of Geometry was very fully attended and productive of extended discussion and a number of supplementary papers. For the same reason as in the Section of Algebra and Analysis it is impossible to give a satisfactory résumé of the short papers on this subject owing to their close technical reasoning.

The first paper was presented by Professor Harris Hancock, of the University of Cincinnati, on "Algebraic Minimal Surfaces."

The second paper was presented by Professor H. T. Blichfeldt, of Leland Stanford Jr. University, on the subject "Concerning some Geometrical Properties of Surfaces of Revolution."

The third paper was presented by Professor George Bruce Halsted, of Kenyon College, on "Non-Euclidean Spherics."

The fourth paper was presented by Professor Arnold Emch, of the University of Colorado, on "The Configuration of the Points of Inflection of a Plane Cubic and their Harmonic Polars."

The fifth paper was presented by Professor H. P. Manning, of Brown University, on "Representation of Complex Variables in Space of Four Dimensions."

The sixth paper was read by Professor G. A. Bliss, of the University of Missouri, on "Concerning Calcidus of Variations."

The seventh paper was presented by Professor L. W. Dowling, of the University of Wisconsin, on "Certain Universal Curves."

SECTION C — APPLIED MATHEMATICS
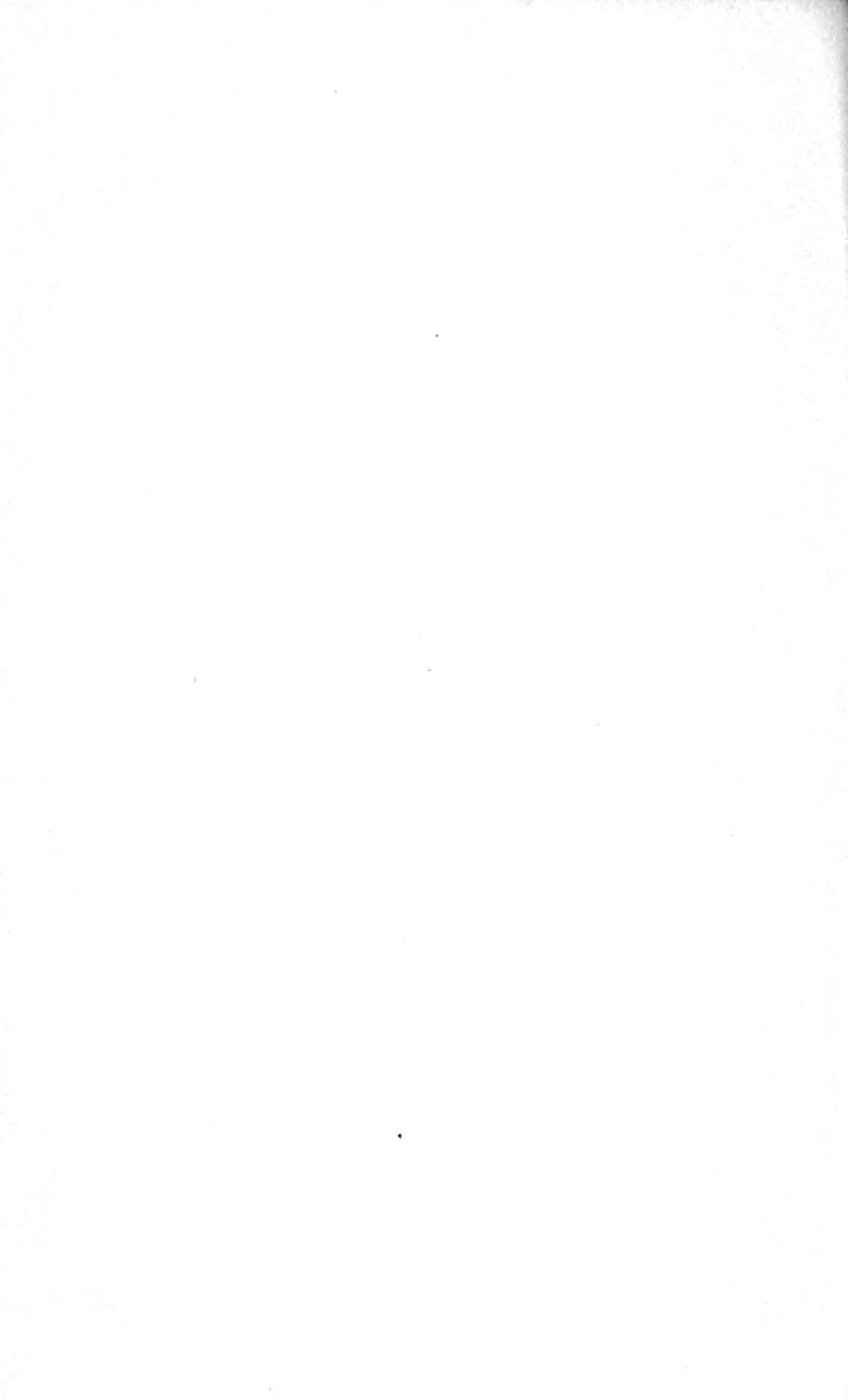
SECTION C — APPLIED MATHEMATICS

(Hall 7, September 24, 3 p. m.)

CHAIRMAN: PROFESSOR ARTHUR G. WEBSTER, Clark University, Worcester, Mass.
SPEAKERS: PROFESSOR LUDWIG BOLTZMANN, University of Vienna.
PROFESSOR HENRI POINCARÉ, The Sorbonne; Member of the Institute of France.
SECRETARY: PROFESSOR HENRY T. EDDY, University of Minnesota.

THE RELATIONS OF APPLIED MATHEMATICS

BY LUDWIG BOLTZMANN

(Translated from the German by Professor S. Epsteen, University of Chicago)

[**Ludwig Boltzmann,** Professor of Physics, University of Vienna, since 1902. b. Vienna, Austria, 1840. Studied, Vienna, Heidelberg, and Berlin. Professor of Physico-Mathematics, University of Gratz, 1869–73; Professor of Mathematics, University of Vienna, 1873–76; Professor of Experimental Physics, University of Gratz, 1876–90; Professor of Theoretical Physics, University of Munich, 1891–95; *ibid.* University of Vienna, 1895–1900; Professor of Physics, University of Leipzig, 1900–02. **Author of** *Vorlesungen über Maxwell's Theorie der Elekt rizitat und des Lichts; Vorlesungen über Kinetische Gastheorie; Vorlesungen über die Prinzipe der Mechanik.*]

MY present lecture has been put under the heading of applied mathematics, while my activity as a teacher and investigator belongs to the science of physics. The immense gap which divides the latter science into two distinct camps has almost nowhere been so sharply emphasized as in the division of the lecture material of this scientific congress, which covers such an enormous range of subjects that one may designate it as a flood, or, to preserve local coloring, as a Niagara of scientific lectures. I speak of the division of physics into theoretical and experimental. Although I have been assigned, as representative of theoretical physics, to "A. — Normative Science," experimental physics appears much later under " C.—Physical Science." Between them lie history, science of language, literature, art, and science of religion. Over all this, however, the theoretical physicist must extend his hand to the experimental physicist. We shall therefore not be able to avoid entirely the question of the justification of dividing physics into two parts and, in particular, into theoretical and experimental.

Let us listen first of all to an investigator of a time when natural science had not yet grown beyond its first beginnings, to Emmanuel Kant. Kant requires of each science that it should be developed

logically from unified principles and firmly established theories. Natural science seems to him a primary science only in so far as it rests on a mathematical basis. Thus, he does not reckon the chemistry of his day among the sciences, because it rests merely upon an empirical basis and lacks a unified, regulative principle.

From this point of view theoretical physics is preferred to experimental physics, and occupies, in a sense, a higher rank. Experimental physics was merely to gather the material, but it remained for the theoretical physics to form the structure.

But the succession in the order of rank becomes reversed when we take into account the acquisitions of the last decades as well as the progress which is to be expected in the immediate future. The chain of experimental discoveries of the last century received a fitting completion with the discovery of the Röntgen rays. Connected with these there appear in the present century a multitude of new rays, with the most enigmatical properties, which have the profoundest effects upon our conceptions of nature. The more enigmatical these newly discovered facts are, and the more they seem at first to contradict our present conceptions, the greater the successes which they promise for the future. But this is not the occasion for the discussion of these experimental triumphs. I must leave to the representatives of experimental physics at this Congress the prolific problem of portraying all of the fruits which have hitherto been gathered in this domain, one might almost say, daily, and those which are to be expected.

The representative of theoretical physics scarcely finds himself in an equally fortunate position. Great activity does indeed prevail in this domain. One could almost say that it is in process of revolution. Only how much less tangible are the results here attained in comparison with those in experimental physics! It appears here that in a certain sense experimentation deserves precedence over all theory. An immediate fact is at once comprehensible. Its fruits may become evident in the shortest time, such as the various applications of the Röntgen rays and the utilization of the Hertz waves in wireless telegraphy. The battle which the theories have to fight is, however, an infinitely wearisome one; indeed, it seems as if certain disputed questions which existed from the beginning will live as long as the science.

Every firmly established fact remains forever unchangeable; at most, it may be generalized, completed, additions may be made, but it cannot be completely upset. Thus it is explained why the development of experimental physics is continuously progressive, never making a sudden jump, and never visited by great tremblings and revolutions. It occurs only in rare instances that something which was regarded as a fact turns out afterwards to have been an

error, and in such cases the explanations of the errors follow soon, and they are not of great influence on the structure of the science as a whole.

It is, indeed, strongly emphasized that every established and logically recognized truth must remain incontrovertible. Although this cannot be doubted, experience teaches that the structure of our theories is by no means composed entirely of such incontrovertibly established truths. They are composed rather of many arbitrary pictures of the connections between phenomena, of so-called hypotheses.

Without some departure, however slight, from direct observation, a theory or even an intelligibly connected practical description for predicting the facts of nature cannot exist. This is equally true of the old theories whose foundations have become questionable, and of the most modern ones, which are resigning themselves to a great illusion if they regard themselves as free from hypotheses.

The hypotheses may perhaps be indefinite, or may be in the shape of mathematical formulae, or the thought may be equivalent to the latter, but expressed in words. In the latter cases the agreement with given data may be checked step by step; a complete revolution of that previously constructed is indeed not absolutely impossible, as, for example, if the law of the conservation of energy should turn out to be incorrect. But such a revolution will be exceedingly rare and highly improbable.

Such an indefinite, slightly specialized theory might serve as a guiding thread for experiments whose purpose is a detailed development of knowledge previously acquired and which is proceeding in barren channels; beyond this its usefulness does not reach.

In contradistinction to these are the hypotheses which give the imagination room for play and by boldly going beyond the material at hand afford continual inspiration for new experiments, and are thus pathfinders for the most unexpected discoveries. Such a theory will indeed be subject to change, a very complicated mass of information will be brought together and will then be replaced by a new and more comprehensive theory in which the old one will be the picture of a limited type of phenomena. Examples of this are the theory of emission in regard to the description of the phenomena of catoptrics and dioptrics, the hypothesis of an elastic ether in the representation of the phenomena of interference and refraction of light, and the notion of the electric fluid in the description of the phenomena of electrostatics.

Moreover the theories which proudly designate themselves as free from hypotheses are not exempt from great revolutions; thus, no one will doubt that the so-called theory of energy will have completely to alter its form if it desires to remain effective.

The accusation has been made that physical hypotheses have sometimes proved injurious and have delayed the progress of the science. This accusation is based chiefly upon the rôle which the hypothesis of the electric fluid has played in the development of the theory of electricity. This hypothesis was brought to a high stage of perfection by Wilhelm Weber, and the general recognition which his works found in Germany did indeed stand in the road of the theory of Maxwell. In a similar manner Newton's emanation theory stood in the way of the theory of undulations. But such inconveniences can scarcely be entirely avoided in the future. It will always be the tendency to complete as far as possible the prevailing view, and to make it self-sufficient whenever such a theory is self-consistent and does not in any way lead to a contradiction, whether it consist of mechanical models, of geometrical pictures, or of mathematical formulas. It will always be possible that a new theory will arise which has not yet been tested by experiment and which will represent a much larger field of phenomena. In such cases the older theory will count the largest following until this field of phenomena is brought into the range of experiment, and decisive tests demonstrate the superiority of the newer one. It is certainly useful, if the theory of Weber be always held up as a warning example, that one should bear in mind the essential progressiveness of the intellect. The services of Weber are not decreased by this, however; Maxwell himself speaks of his theory with the greatest wonder. Indeed, this instance cannot be taken into consideration against the usefulness of hypotheses, since Maxwell's theory contained as much of the hypothetical as any other. And this was eliminated only after it became generally known through Hertz, Poynting, and others.

The accusation has also been raised against hypotheses in physics that the creation and development of mathematical methods for the computation of the hypothetical molecular motions has been useless and even harmful. This accusation I cannot recognize as substantiated. Were it so, the theme selected for my present lecture would be an unfortunate one; and this fact may excuse me for having lingered on this much-discussed subject and for having sought to justify the use of hypotheses in physics.

I have not chosen for the thesis of my present lecture the entire development of physical theory. Several years ago I treated this subject at the German *Naturforscherversammlung* in Munich, and although some new developments have taken place since then, I should have to repeat myself a great deal. Moreover, one who has committed himself to one faction is not in a position to judge the other factions in a completely objective manner. I do not refer to a criticism of its value; my lecture shall not criticise, but shall judge. I am also convinced of the value of the views of my opponents and

only arise to repel them when they attempt to belittle mine. But one can scarcely give as complete an account according to subject-matter, and an exposition of the inter-relations of all ideas in the views of another, as in his own.

I shall therefore select as the goal of my lecture to-day not merely the kinetic theory of molecules, but, moreover, a highly specialized branch of it. Far from denying that it contains hypotheses, I must rather characterize it as a bold advance beyond the facts of observation. And I nevertheless do not consider it unworthy of this occasion; this much faith do I have in hypotheses which present certain peculiarities of observation in a new light or which bring forth relations among them which cannot be reached by other methods. We must indeed be mindful of the fact that hypotheses require and are capable of continuous development, and are only then to be abandoned when all the relations which they represent can be better understood in some other manner.

To the above-mentioned problems, which are as old as the science and still unsolved, belongs the one if matter is continuous, or if it is to be considered as made up of discrete parts, of very many, but not in the mathematical sense infinite, individuals. This is one of the difficult questions which form the boundary of philosophy and physics.

Even some decades ago, scientists felt very shy of going deeply into the discussion of such questions. The one before us is too real to be entirely avoided; but one cannot discuss it without touching on some profounder still, such as upon the nature of the law of causation, of matter, of force, and so forth. The latter are the ones of which it was said that they did not trouble the scientist, that they belonged entirely to philosophy. To-day the situation is different, there is evident a tendency among scientists to consider philosophic questions, and properly so. One of the first rules of science is never to trust blindly to the instrument with which one works, but to test it in all directions. How, then, are we to trust blindly to inherited and historically developed conceptions, particularly when there are instances known where they led us into error? But in the examination of even the simplest elements, where is the boundary between science and philosophy at which we should pause?

I hope that none of the philosophers present will take offense or perceive an accusation, if I say boldly that by assigning this question to philosophy the resulting success has been rather meagre. Philosophy has done noticeably little toward the explanation of these questions, and from her own one-sided point of view she can do so just as little as natural science can from hers. If real progress is possible, it is only to be expected by coöperation of both of these sciences. May I therefore be pardoned if I touch slightly upon these questions

although not a specialist; their connection with the aim of my lecture is very intimate.

Let us consult the famous thinker already quoted, Emmanuel Kant, on the question if matter is continuous, or if it is composed of atoms. He treats of this in his *Antimonies*. Of all the questions there raised, he shows that both the *pro* and *con* can be logically demonstrated. It can be shown rigorously that there is no limit to the divisibility of matter while an infinite divisibility contradicts the laws of logic. Kant shows likewise that a beginning and end of time, a boundary where space ceases, are as inconceivable as absolutely endless duration, absolutely endless extension.

This is by no means the sole instance where philosophical thought becomes tangled in contradictions; indeed, one finds them at every step. The ordinary things of philosophy are sources of insolvable riddles; to explain our perceptions it invents the concept of matter and then finds that it is altogether unsuited to possess perception itself or to generate perception in a spirit. With consummate acumen it constructs the concept of space, or of time, and finds that it is absolutely impossible that things should exist in this space, that events should occur during this time. It finds insurmountable difficulties in the relation of cause to effect, of body and soul, in the possibility of consciousness, in short, everywhere and in everything. Indeed, it finally finds it inexplicable and self-contradictory that anything can exist at all, that something originated and is capable of continuing, that everything can be classified according to our categories, nor that there is a quite perfect classification. Such a classification will always be a variable one and adapted to the requirements of the moment. Also the breaking up of physics into theoretical and experimental is merely a consequence of the prevalent division of methods and will not last forever.

My present thesis is quite different from the one that certain questions are beyond the boundary of human comprehension. For according to the latter, there is a deficiency, an incompleteness in the human intelligence, while I consider the existence of these questions, these problems, as an illusion. By superficial consideration it seems astonishing, after this illusion is recognized, that the impulse to answer those questions does not cease. Habit of thought is much too powerful to release us.

It is here as with the ordinary illusion which continues operative after its cause is recognized. In consequence of this is the feeling of uncertainty, of want of satisfaction which the scientist feels when he philosophizes. These illusions will yield but very slowly and gradually, and I consider it as one of the chief problems of philosophy to set forth clearly the uselessness of reaching beyond the limits of our habits of thought and to strive, in the choice and combination of

concepts and words, to give the most useful expression of facts in a manner which is independent of our inherited habits. Then all these complications and contradictions must vanish. It must be made clear what is stone in the structure of our thoughts and what is mortar, and the oppressive sentiment, that the simplest things are the most inexplicable and the most trivial are the most mysterious, becomes mere imagination-change.

To call upon logic seems to me as if one were to put on for a trip into the mountains a long flowing robe, which always wrapped itself about the feet so that one fell at the first steps while on the level. The source of this kind of logic is the immoderate trust in the so-called laws of thought. It is certain that we could not gather experience did we not have certain forms of connecting phenomena, that is to say, of thought, innate. If we wish to call these laws of thought, they are indeed *a priori* to the extent that they accompany every experience in our soul, or if we prefer, in our brain. Only nothing seems to me less reasonable than the conclusion from the reasoning in this sense to certainty, to infallibility. These laws of thought have been developed according to the same laws of evolution as the optical apparatus of the eye, the acoustic apparatus of the ear, and the pumping arrangements of the heart. In the course of human development everything useless was eliminated, and thus a unity and finish arose which might be mistaken for infallibility. Thus the perfection of the eye, of the ear, of the arrangement of the heart excite our admiration, without the absolute perfection of these organs being emphasized, however. Just so little should the laws of thought be regarded as absolutely infallible. They are the very ones which have developed with regard to seizing that which is most necessary and practically useful in the maintenance of life. With these, the results of experimental investigation show more relation than the examination of the mechanism of thought. We should, therefore, not be surprised that the customary forms of thought for the abstract are not entirely suited to practical applications in far removed problems of philosophy, and that they have not become applicable since the days of Thales. Therefore the simplest things seem to be the most puzzling to the philosopher. And he finds everywhere contradictions; these are nothing more, however, than useless, incorrect facsimiles of that which is given us through our thoughts. In facts there can be no contradictions. As soon as contradictions seem unavoidable we must test, extend, and seek to modify that which we call laws of thought, but which are only inherited, customary representations, preserved for aeons, for the description of practical needs. Just as to the inherited discoveries of the cylinder, the carriage, the plow, numerous artificial ones have been consciously added, so must we improve, artificially and con-

sciously, our likewise inherited concepts. Our problem cannot be to quote facts before the judgment seat of our laws of thought, but to fit our mental representations and concepts to the facts. Since we attempt to express with clearness such complicated processes merely by words, written, spoken, or inwardly thought, it might also be said that we should combine the words in such wise as to give the most appropriate expression of the facts, that the relations indicated by our words should be most adequate for the relations among the actualities. When the problem is enunciated in this fashion, its appropriate solution may still offer the greatest difficulties, but one knows then the end in view and will not stumble on self-made difficulties.

Much that is useless in the usage and in the bearing of the nature of life is brought forth by a method of treatment which, being useful in most cases, becomes through habit a second nature, until one cannot set it aside when it becomes inapplicable somewhere. I say that the adaptability goes beyond the point aimed at. This happens frequently in the commonplaces of thought, and becomes the source of apparent contradictions between the laws of thought and the world, as well as between the laws of thought themselves.

Thus, the regularity of the phenomena of nature is the fundamental condition for all cognition; thus comes the habit of inquiring of everything the cause, the non-resisting compulsion, and we inquire also concerning the cause, why everything must have a cause. In fact people strove for a long time to determine if cause and effect is a necessary bond or merely an accidental sequence, and if it did or did not have a unique meaning to say that a certain particular phenomenon was connected with, and a necessary consequence of, a definite group of other phenomena.

Similarly, something is said to be useful, valuable, if it satisfies the needs of the individual or of humanity; but we go beyond the mark if we ask concerning the value of life itself, if such it seem to have, because it has no purpose outside of itself. The same happens when we strive vainly to explain the simplest concepts, out of which all others are built, by means of simpler ones still, to explain the simplest fundamental laws.

We should not attempt to deduce nature from our concepts, but should adapt the latter to the former. We should not believe our inherited rules of thought to be conditions preceding our more complicated experiences, for they are not so for the simplest essentials. They arose slowly in connection with simple experiences and passed, by heredity, to the more highly organized being. Thus is explained how synthetic judgments arise which were formed by our ancestors and were born in us, and are in this sense *a priori*. Their great power is also seen in this way, but not their infallibility.

In saying that such judgments as "everything is red or is not red" are results of experience, I do not mean that every person checks this empty truth by experience, but that he learns that his parents called everything either red or not red and that he preserves this nomenclature.

It might seem as if we had gone somewhat deeply into philosophical questions, but I believe that the views we have reached could not have been attained in a shorter and simpler manner. For we have reached an impartial judgment how the question of the atomistic structure of matter is to be viewed. We shall not invoke the law of thought that there is no limit to the divisibility of matter. This law is of no more value than if a naïve person were to say that no matter where he went upon the earth the plumb-line directions seemed always to be parallel and therefore there were no antipodes.

On the one hand we shall start from facts only, and on the other we shall take nothing into consideration except the effort to attain to the most adequate expression of these facts.

Regarding the first point, the numerous facts of the theory of heat, of chemistry, of crystallography, show that bodies which are apparently continuous do not by any means fill the entire volume indistinguishably and uniformly with matter. Indeed, it appears that the space which they occupy is filled with innumerably many individuals, molecules, and atoms, which are extraordinarily small, but not infinitely small in the mathematical sense. Their sizes can be computed in different manners and always with the same result.

The fruitfulness of this line of thought has been verified in the most recent time. All the phenomena which are observed with the cathode rays, the Becquerel rays, etc., indicate that we are dealing with diminutive, moving particles, electrons. After a vigorous battle, this view vanquished completely the opposing explanation of these phenomena by the theory of undulations. Not only did the former theory give a better explanation of the previously known facts, it inspired new experiments and permitted the prediction of unknown phenomena, and thus it developed into an atomistic theory of electricity. If it continue to develop with the same success as in past years, if phenomena, such as the one observed by Ramsay on the transmutation of radium into helium, do not remain isolated, this theory promises deductions concerning the nature and structure of atoms as yet undreamed of. Computation shows that electrons are much smaller than the atoms of ponderable matter; and the hypothesis that the atoms are built up of many elements, as well as various interesting views on the character and structure of this composition, is to-day on every tongue. The word atom should not lead us into error, it comes from a past time; no physicist ascribes indivisibility to the atoms.

It is not my intention to confine the thought merely to the above facts and their resulting consequences; these are not sufficient to carry through the question as to the finite or infinite divisibility of matter. If we are going to think of the atoms of chemistry as made up of electrons, what would hinder us from considering the electrons as particles filled with rarefied, continuous matter? We shall adhere faithfully to the previously developed philosophical principles and shall examine in the most unhampered manner the concepts themselves in order to express them in a consistent and most useful form.

It appears now, that we are unable to define the infinite in any other way except as the limit of continually increasing magnitudes, at least no one has hitherto been able to set up any other intelligible conception of the infinite. Should we desire a verbal picture of the continuum, we must first think of a large finite number of particles which are endowed with certain properties and study the totality of these particles. Certain properties of this totality may approach a definite limit as the number of particles is increased, and their size decreased. It can be asserted, concerning these properties, that they belong to the continuum, and it is my opinion that this is the only self-consistent definition of a continuum which is endowed with certain properties.

The question if matter is composed of atoms or is continuous becomes then the question if the observed properties are accurately satisfied by the assumption of an exceedingly great number of such particles or, by increasing number, their limit. We have not indeed answered the old philosophical question, but we are cured of the effort to answer it in a senseless and hopeless manner. The thought-process, that we must investigate the properties of a finite totality and then let the number of members of this totality increase greatly, remains the same in both cases. It is nothing other than the abbreviated expression in algebraic symbols of exactly the same thought when, as often happens, differential equations are made the basis of a mathematical-physical theory.

The members of the totality which we select as the picture of the material body cannot be thought of as absolutely at rest, for there would then be no motion of any kind, nor can the members be thought of as relatively at rest in one and the same body, for in this case it would be impossible to account for the fluids. No effort has been made to subject them to anything more than to the general laws of mechanics. In order to explain nature we shall therefore select a totality of an exceedingly large number of very minute funda-mental individuals which are constantly in motion, and which are subject to the laws of mechanics. But an objection is raised that will be an appropriate introduction to the final considerations of

this lecture. The fundamental equations of mechanics do not alter
their form in the slightest way when the algebraic sign of the time is
changed. All pure mechanical events can therefore occur equally
well in one sense as in its opposite, that is, in the sense of increasing
time or of diminishing time. We remark, however, that in ordinary
life future and past do not coincide as completely as the directions
right and left, but that the two are distinctly different.

This becomes still more definite by means of the second law of the
mechanical theory of heat, which asserts that when an arbitrary
system of bodies is left to itself, uninfluenced by other bodies, the
sense in which changes of condition occur can be assigned. A certain
function of the condition of all the bodies, the entropy. can be
determined, which is such that every change that occurs must be in
the sense of carrying with it an increase of this function; thus,
with increasing time the entropy increases. This law is indeed an
abstraction, just as the principle of Galileo; for it is impossible, in
strict rigor, to isolate a system of bodies from all others. But since
it has given correct results hitherto, in connection with all the other
laws, we assume it to be correct, just as in the case of the principle of
Galileo.

It follows from this law that every closed system of bodies must
tend toward a definite final condition for which the entropy is a
maximum. The outcome of this law, that the universe must come
to a final state in which nothing more can occur, has caused aston-
ishment; but this outcome is only comprehensible on the assump-
tion that the universe is finite and subject to the second law of the
mechanical theory of heat. If one regards the universe as infinite.
the above-mentioned difficulties of thought arise again if one does
not consider the infinite as a mere limit of the finite. Since there is
nothing analogous to the second law in the differential equations
of mechanics, it follows that it can be represented mechanically only
by the initial conditions. In order to find the assumptions suit-
able for this purpose, we must reflect that, to explain the appar-
ent continuity of bodies, we had to assume that every family
of atoms, or more generally, of mechanical individuals, existed in
incredibly many different initial positions. In order to treat this
assumption mathematically, a new science was founded whose pro-
blem is, not the study of the motion of a single mechanical system,
but of the properties of complexes of very many mechanical systems
which begin with a great variety of initial conditions. The task of
systematizing this science, of compiling it into a large book. and of
giving it a characteristic name, was executed by one of the greatest
American scholars, and in regard to abstract thinking, purely theo-
retic investigation, perhaps the greatest, Willard Gibbs, the recently
deceased professor at Yale University. He called this science statis-

tical mechanics, and it falls naturally into two parts. The first investigates the conditions under which the outwardly visible properties of a complex of very many mechanical individuals is not in any wise altered; this first part I shall call statistical statics. The second part investigates the gradual changes of these outwardly visible properties when those conditions are not fulfilled; it may be called statistical dynamics. At this point we may allude to the broad view which is opened by applying this science to the statistics of animated beings, of human society, of sociology, etc., and not merely upon mechanical particles. A development of the details of this science would only be possible in a series of lectures and by means of mathematical formulas. Apart from mathematical difficulties it is not free from difficulties of principle. It is based upon the theory of probabilities. The latter is as exact as any other branch of mathematics if the concept of equal probabilities, which cannot be deduced from the other fundamental notions, is assumed. It is here as in the method of least squares which is only free from objection when certain definite assumptions are made concerning the equal probability of elementary errors. The existence of this fundamental difficulty explains why the simplest result of statistical statics, the proof of Maxwell's speed law among the molecules of a gas, is still being disputed.

The theorems of statistical mechanics are rigorous consequences of the assumptions and will always remain valid, just as all well-founded mathematical theorems. But its application to the events of nature is the prototype of a physical hypothesis. Starting from the simplest fundamental assumption of the equal probabilities, we find that aggregates of very many individuals behave quite analogously as experience shows of the material world. Progressive or visible rotary motion must always go over into invisible motion of the minutest particles, into heat, as Helmholtz characteristically says: ordered motion tends always to go over into not ordered motion; the mixture of different substances as well as of different temperatures, the points of greater or less intense molecular motion, must always tend toward homogeneity. That this mixture was not complete from the start, that the universe began in such an improbable state, belongs to the fundamental hypotheses of the entire theory; and it may be said that the reason for this is as little known as the reason why the universe is just so and not otherwise. But we may take a different point of view. Conditions of great mixture and great differences in temperature are not absolutely impossible according to the theory but are very highly improbable. If the universe be considered as large enough there will be, according to the laws of probability, here and there places of the size of fixed stars, of altogether improbable distributions. The development of such

a spot would be one-sided both in its structure and subsequent dissolution. Were there thinking beings at such a spot their impressions of time would be the same as ours, although the course of events in the universe as a whole would not be one-sided. The above-developed theory does indeed go boldly beyond our experience, but it has the merit which every such theory should have of showing us the facts of experience in an entirely new light and of inspiring us to new thought and reflection. In contradistinction to the first fundamental law, the second one is merely based on probability, as Gibbs pointed out in the '70's of the last century.

I have not avoided philosophical questions, in the firm hope that coöperation between philosophy and natural science will give new sustenance to both; indeed, that only in this manner a consistent argument can be carried through. I agree with Schiller when he says to the scientists and philosophers of his day, "Let there be strife between you, and the union will come speedily;" I believe that the time for this union has now arrived.

THE PRINCIPLES OF MATHEMATICAL PHYSICS

BY JULES HENRI POINCARÉ

[Jules Henri Poincaré, Professor University of Paris, and the Polytechnic
School; Member of Bureau of Longitude. **b.** Nancy, April 29, 1854. D.Sc.
August 3, 1879; D.Sc. Cambridge and Oxford, 1879; Charge of the Course
of the Faculty of Sciences at Caen; Master of Conference of the Faculty of
Sciences of Paris, 1881; Professor of the same Faculty, 1886; Member of the
Institute of France, 1887; Corresponding Member of the National Academy
of Washington; Philosophical Society of Philadelphia; the Academies of
Berlin, London, St. Petersburg, Vienna, Rome, Munich, Göttingen, Bologna,
Turin, Naples, Venice, Amsterdam, Copenhagen, Stockholm, etc. Written
books and numerous articles for reviews and periodicals.]

WHAT is the actual state of mathematical physics? What are the
problems it is led to set itself? What is its future? Is its orientation
on the point of modifying itself?

Will the aim and the methods of this science appear in ten years
to our immediate successors in the same light as to ourselves; or,
on the contrary, are we about to witness a profound transformation?
Such are the questions we are forced to raise in entering to-day upon
our investigation.

If it is easy to propound them, to answer is difficult.

If we feel ourselves tempted to risk a prognostication, we have,
to resist this temptation, only to think of all the stupidities the
most eminent savants of a hundred years ago would have uttered,
if one had asked them what the science of the nineteenth century
would be. They would have believed themselves bold in their pre-
dictions, yet after the event how very timid we should have found
them.

Mathematical physics, we know, was born of celestial mechanics,
which engendered it at the end of the eighteenth century, at the
moment when the latter was attaining its complete development.
During its first years especially, the infant resembled in a striking
way its mother.

The astronomic universe is formed of masses, very great without
doubt, but separated by intervals so immense that they appear to
us only as material points. These points attract each other in the
inverse ratio of the square of the distances, and this attraction is
the sole force which influences their movements. But if our senses
were sufficiently subtle to show us all the details of the bodies which
the physicist studies, the spectacle we should there discover would
scarcely differ from what the astronomer contemplates. There also
we should see material points, separated one from another by inter-

vals enormous in relation to their dimensions, and describing orbits following regular laws.

These infinitesimal stars are the atoms. Like the stars properly so called, they attract or repel each other, and this attraction or this repulsion directed following the straight line which joins them, depends only on the distance. The law according to which this force varies as function of the distance is perhaps not the law of Newton, but it is an analogous law; in place of the exponent — 2, we have probably a different exponent, and it is from this change of exponent that springs all the diversity of physical phenomena, the variety of qualities and of sensations, all the world colored and sonorous which surrounds us, — in a word, all nature.

Such is the primitive conception in all its purity. It only remains to seek in the different cases what value should be given to this exponent in order to explain all the facts. It is on this model that Laplace, for example, constructed his beautiful theory of capillarity; he regards it only as a particular case of attraction, or as he says of universal gravitation, and no one is astonished to find it in the middle of one of the five volumes of the *Mécanique céleste*.

More recently Briot believed he had penetrated the final secret of optics in demonstrating that the atoms of ether attract each other in the inverse ratio of the sixth power of the distance; and does not Maxwell himself say somewhere that the atoms of gases repel each other in the inverse ratio of the fifth power of the distance? We have the exponent — 6, or — 5 in place of the exponent — 2, but it is always an exponent.

Among the theories of this period, one alone is an exception, that of Fourier; in it are indeed atoms, acting at a distance one upon the other; they mutually transmit heat, but they do not attract, they never budge. From this point of view, the theory of Fourier must have appeared to the eyes of his contemporaries, even to Fourier himself, as imperfect and provisional.

This conception was not without grandeur; it was seductive, and many among us have not finally renounced it; we know that we shall attain the ultimate elements of things only by patiently disentangling the complicated skein that our senses give us; that it is necessary to advance step by step, neglecting no intermediary; that our fathers were wrong in wishing to skip stations; but we believe that when we shall have arrived at these ultimate elements, there again will be found the majestic simplicity of celestial mechanics.

Neither has this conception been useless; it has rendered us an inestimable service, since it has contributed to make precise in us the fundamental notion of the physical law.

I will explain myself; how did the ancients understand law? It was for them an internal harmony, static, so to say, and immutable;

or it was like a model that nature constrained herself to **imitate.** A law for us is not that at all; it is a constant relation between the phenomenon of to-day and that of to-morrow; in a word, it is a differential equation.

The ideal form of physical law is the law of Newton which first covered it; and then how has one to adapt this form to physics? by copying as much as possible this law of Newton, that is, in imitating celestial mechanics.

Nevertheless, a day arrived when the conception of central forces no longer appeared sufficient, and this is the first of those crises of which I just now spoke.

Then investigators gave up trying to penetrate into the detail of the structure of the universe, to isolate the pieces of this vast mechanism, to analyze one by one the forces which put them in motion, and were content to take as guides certain general principles which have precisely for their object the sparing us this minute study.

How so? Suppose that we have before us any machine; the initial wheel-work and the final wheel-work alone are visible, but the transmission, the intermediary wheels by which the movement is communicated from one to the other are hidden in the interior and escape our view; we do not know whether the communication is made by gearing or by belts, by connecting-rods or by other dispositives.

Do we say that it is impossible for us to understand anything about this machine so long as we are not permitted to take it to pieces? You know well we do not, and that the principle of the conservation of energy suffices to determine for us the most interesting point. We easily ascertain that the final wheel turns ten times less quickly than the initial wheel, since these two wheels are visible; we are able thence to conclude that a couple applied to the one will be balanced by a couple ten times greater applied to the other. For that there is no need to penetrate the mechanism of this equilibrium and to know how the forces compensate each other in the interior of the machine; it suffices to be assured that this compensation cannot fail to occur.

Well, in regard to the universe, the principle of the conservation of energy is able to render us the same service. This is also a machine, much more complicated than all those of industry, and of which almost all the parts are profoundly hidden from us; but in observing the movement of those that we can see, we are able, by aid of this principle, to draw conclusions which remain true whatever may be the details of the invisible mechanism which animates them.

The principle of the conservation of energy, or the principle of Mayer, is certainly the most important, but it is not the only one;

there are others from which we are able to draw the same advantage. These are:

> The principle of Carnot, or the principle of the degradation of energy.

> The principle of Newton, or the principle of the equality of action and reaction.

> The principle of relativity, according to which the laws of physical phenomena should be the same, whether for an observer fixed, or for an observer carried along in a uniform movement of translation; so that we have not and could not have any means of discerning whether or not we are carried along in such a motion.

> The principle of the conservation of mass, or principle of Lavoisier.

I would add the principle of least action.

The application of these five or six general principles to the different physical phenomena is sufficient for our learning of them what we could reasonably hope to know of them.

The most remarkable example of this new mathematical physics is, beyond contradiction, Maxwell's electro-magnetic theory of light.

We know nothing of the ether, how its molecules are disposed, whether they attract or repel each other; but we know that this medium transmits at the same time the optical perturbations and the electrical perturbations; we know that this transmission should be made conformably to the general principles of mechanics, and that suffices us for the establishment of the equations of the electromagnetic field.

These principles are results of experiments boldly generalized; but they seem to derive from their generality itself an eminent degree of certitude.

In fact the more general they are, the more frequently one has the occasion to check them, and the verifications, in multiplying themselves, in taking forms the most varied and the most unexpected, finish by no longer leaving place for doubt.

Such is the second phase of the history of mathematical physics, and we have not yet emerged from it.

Do we say that the first has been useless? that during fifty years science went the wrong way, and that there is nothing left but to forget so many accumulated efforts as vicious conceptions condemned in advance to non-success?

Not the least in the world; the second phase could not have come into existence without the first?

The hypothesis of central forces contained all the principles; it involved them as necessary consequences; it involved both the con-

servation of energy and that of masses, and the equality of action and reaction; and the law of least action, which would appear, it is true, not as experimental verities, but as theorems, and of which the enunciation would have at the same time a something more precise and less general than under their actual form.

It is the mathematical physics of our fathers which has familiarized us little by little with these divers principles; which has taught us to recognize them under the different vestments in which they disguise themselves. One has to compare them to the data of experience, to find how it was necessary to modify their enunciation so as to adapt them to these data; and by these processes they have been enlarged and consolidated.

So we have been led to regard them as experimental verities; the conception of central forces became then a useless support, or rather an embarrassment, since it made the principles partake of its hypothetical character.

The frames have not therefore broken, because they were elastic; but they have enlarged; our fathers, who established them, did not work in vain, and we recognize in the science of to-day the general traits of the sketch which they traced.

Are we about to enter now upon the eve of a second crisis? Are these principles on which we have built all about to crumble away in their turn? For some time, this may well have been asked.

In hearing me speak thus, you think without doubt of radium, that grand revolutionist of the present time, and in fact I will come back to it presently; but there is something else.

It is not alone the conservation of energy which is in question; all the other principles are equally in danger, as we shall see in passing them successively in review.

Let us commence with the principle of Carnot. This is the only one which does not present itself as an immediate consequence of the hypothesis of central forces; more than that, it seems, if not directly to contradict that hypothesis, at least not to be reconciled with it without a certain effort.

If physical phenomena were due exclusively to the movements of atoms whose mutual attraction depended only on the distance, it seems that all these phenomena should be reversible; if all the initial velocities were reversed, these atoms, always subjected to the same forces, ought to go over their trajectories in the contrary sense, just as the earth would describe in the retrograde sense this same elliptic orbit which it describes in the direct sense, if the initial conditions of its movement had been reversed. On this account, if a physical phenomenon is possible, the inverse phenomenon should be equally so, and one should be able to reascend the course of time.

But it is not so in nature, and this is precisely what the principle of Carnot teaches us; heat can pass from the warm body to the cold body; it is impossible afterwards to make it reascend the inverse way and reëstablish differences of temperature which have been effaced.

Motion can be wholly dissipated and transformed into heat by friction; the contrary transformation can never be made except in a partial manner.

We have striven to reconcile this apparent contradiction. If the world tends toward uniformity, this is not because its ultimate parts, at first unlike, tend to become less and less different, it is because, shifting at hazard, they end by blending. For an eye which should distinguish all the elements, the variety would remain always as great, each grain of this dust preserves its originality and does not model itself on its neighbors; but as the blend becomes more and more intimate, our gross senses perceive no more than the uniformity. Behold why, for example, temperatures tend to a level, without the possibility of turning backwards.

A drop of wine falls into a glass of water; whatever may be the law of the internal movements of the liquid, we soon see it colored to a uniform rosy tint, and from this moment, however well we may shake the vase, the wine and the water do not seem capable of further separation. Observe what would be the type of the reversible physical phenomenon: to hide a grain of barley in a cup of wheat is easy; afterwards to find it again and get it out is practically impossible.

All this Maxwell and Boltzmann have explained; the one who has seen it most clearly, in a book too little read because it is a little difficult to read, is Gibbs, in his *Elementary Principles of Statistical Mechanics*.

For those who take this point of view, the principle of Carnot is only an imperfect principle, a sort of concession to the infirmity of our senses; it is because our eyes are too gross that we do not distinguish the elements of the blend; it is because our hands are too gross that we cannot force them to separate; the imaginary demon of Maxwell, who is able to sort the molecules one by one, could well constrain the world to return backward. Can it return of itself? That is not impossible; that is only infinitely improbable.

The chances are that we should long await the concourse of circumstances which would permit a retrogradation, but soon or late they would be realized, after years whose number it would take millions of figures to write.

These reservations, however, all remained theoretic and were not very disquieting, and the principle of Carnot retained all its practical value.

But here the scene changes.

The biologist, armed with his microscope, long ago noticed in his preparations disorderly movements of little particles in suspension: this is the Brownian movement; he first thought this was a vital phenomenon, but he soon saw that the inanimate bodies danced with no less ardor than the others; then he turned the matter over to the physicists. Unhappily, the physicists remained long uninterested in this question; the light is focused to illuminate the microscopic preparation, thought they; with light goes heat; hence inequalities of temperature and interior currents produce the movements in the liquid of which we speak.

M. Gouy, however, looked more closely, and he saw, or thought he saw, that this explanation is untenable, that the movements become more brisk as the particles are smaller, but that they are not influenced by the mode of illumination.

If, then, these movements never cease, or rather are reborn without ceasing, without borrowing anything from an external source of energy, what ought we to believe? To be sure, we should not renounce our belief in the conservation of energy, but we see under our eyes now motion transformed into heat by friction, now heat changed inversely into motion, and that without loss since the movement lasts forever. This is the contrary of the principle of Carnot.

If this be so, to see the world return backward, we no longer have need of the infinitely subtle eye of Maxwell's demon; our microscope suffices us. Bodies too large, those, for example, which are a tenth of a millimeter, are hit from all sides by moving atoms, but they do not budge, because these shocks are very numerous and the law of chance makes them compensate each other: but the smaller particles receive too few shocks for this compensation to take place with certainty and are incessantly knocked about. And thus already one of our principles is in peril.

We come to the principle of relativity: this not only is confirmed by daily experience, not only is it a necessary consequence of the hypothesis of central forces, but it is imposed in an irresistible way upon our good sense, and yet it also is battered.

Consider two electrified bodies; though they seem to us at rest, they are both carried along by the motion of the earth; an electric charge in motion, Rowland has taught us, is equivalent to a current; these two charged bodies are, therefore, equivalent to two parallel currents of the same sense and these two currents should attract each other. In measuring this attraction, we measure the velocity of the earth; not its velocity in relation to the sun or the fixed stars, but its absolute velocity.

I know it will be said that it is not its absolute velocity that is measured, but its velocity in relation to the ether. How unsatis-

factory that is! Is it not evident that from a principle so understood we could no longer get anything? It could no longer tell us anything just because it would no longer fear any contradiction.

If we succeed in measuring anything, we should always be free to say that this is not the absolute velocity in relation to the ether, it might always be the velocity in relation to some new unknown fluid with which we might fill space.

Indeed, experience has taken on itself to ruin this interpretation of the principle of relativity; all attempts to measure the velocity of the earth in relation to the ether have led to negative results. This time experimental physics has been more faithful to the principle than mathematical physics; the theorists, to put in accord their other general views, would not have spared it; but experiment has been stubborn in confirming it.

The means have been varied in a thousand ways and finally Michelson has pushed precision to its last limits; nothing has come of it. It is precisely to explain this obstinacy that the mathematicians are forced to-day to employ all their ingenuity.

Their task was not easy, and if Lorentz has gotten through it, it is only by accumulating hypotheses.

The most ingenious idea has been that of local time.

Imagine two observers who wish to adjust their watches by optical signals; they exchange signals, but as they know that the transmission of light is not instantaneous, they take care to cross them.

When the station B perceives the signal from the station A, its clock should not mark the same hour as that of the station A at the moment of sending the signal, but this hour augmented by a constant representing the duration of the transmission. Suppose, for example, that the station A sends its signal when its clock marks the hour 0, and that the station B perceives it when its clock marks the hour t. The clocks are adjusted if the slowness equal to t represents the duration of the transmission, and to verify it the station B sends in its turn a signal when its clock marks 0; then the station A should perceive it when its clock marks t. The time-pieces are then adjusted. And in fact, they mark the same hour at the same physical instant, but on one condition, namely, that the two stations are fixed. In the contrary case the duration of the transmission will not be the same in the two senses, since the station A, for example, moves forward to meet the optical perturbation emanating from B, while the station B flies away before the perturbation emanating from A. The watches adjusted in that manner do not mark, therefore, the true time; they mark what one may call the *local time*, so that one of them goes slow on the other. It matters little, since we have no means of perceiving it. All the phenomena which happen

at A, for example, will be late, but all will be equally so, and the observer who ascertains them will not perceive it, since his watch is slow; so, as the principle of relativity would have it, he will have no means of knowing whether he is at rest or in absolute motion.

Unhappily, that does not suffice, and complementary hypotheses are necessary; it is necessary to admit that bodies in motion undergo a uniform contraction in the sense of the motion. One of the diameters of the earth, for example, is shrunk by $\frac{1}{200,000,000}$ in consequence of the motion of our planet, while the other diameter retains its normal length. Thus, the last little differences find themselves compensated. And then there still is the hypothesis about forces. Forces, whatever be their origin, gravity as well as elasticity, would be reduced in a certain proportion in a world animated by a uniform translation; or, rather, this would happen for the components perpendicular to the translation; the components parallel would not change.

Resume, then, our example of two electrified bodies; these bodies repel each other, but at the same time if all is carried along in a uniform translation, they are equivalent to two parallel currents of the same sense which attract each other. This electro-dynamic attraction diminishes, therefore, the electro-static repulsion, and the total repulsion is more feeble than if the two bodies were at rest. But since to measure this repulsion we must balance it by another force, and all these other forces are reduced in the same proportion, we perceive nothing.

Thus, all is arranged, but are all the doubts dissipated?

What would happen if one could communicate by non-luminous signals whose velocity of propagation differed from that of light? If, after having adjusted the watches by the optical procedure, one wished to verify the adjustment by the aid of these new signals, then would appear divergences which would render evident the common translation of the two stations. And are such signals inconceivable, if we admit with Laplace that universal gravitation is transmitted a million times more rapidly than light?

Thus, the principle of relativity has been valiantly defended in these latter times, but the very energy of the defense proves how serious was the attack.

Let us speak now of the principle of Newton, on the equality of action and reaction.

This is intimately bound up with the preceding, and it seems indeed that the fall of the one would involve that of the other. Thus we should not be astonished to find here the same difficulties.

Electrical phenomena, we think, are due to the displacements of little charged particles, called electrons, immersed in the medium that we call ether. The movements of these electrons produce perturbations in the neighboring ether; these perturbations propagate

themselves in every direction with the velocity of light, and in turn other electrons, originally at rest, are made to vibrate when the perturbation reaches the parts of the ether which touch them.

The electrons, therefore, act upon one another, but this action is not direct, it is accomplished through the ether as intermediary.

Under these conditions can there be compensation between action and reaction, at least for an observer who should take account only of the movements of matter, that is to say, of the electrons, and who should be ignorant of those of the ether that he could not see? Evidently not. Even if the compensation should be exact, it could not be simultaneous. The perturbation is propagated with a finite velocity; it, therefore, reaches the second electron only when the first has long ago entered upon its rest.

This second electron, therefore, will undergo, after a delay, the action of the first, but certainly it will not react on this, since around this first electron nothing any longer budges.

The analysis of the facts permits us to be still more precise. Imagine for example, a Hertzian generator, like those employed in wireless telegraphy; it sends out energy in every direction; but we can provide it with a parabolic mirror, as Hertz did with his smallest generators, so as to send all the energy produced in a single direction.

What happens, then, according to the theory? It is that the apparatus recoils as if it were a gun and as if the energy it has projected were a bullet; and that is contrary to the principle of Newton, since our projectile here has no mass, it is not matter, it is energy.

It is still the same, moreover, with a beacon light provided with a reflector, since light is nothing but a perturbation of the electromagnetic field. This beacon light should recoil as if the light it sends out were a projectile. What is the force that this recoil should produce? It is what one has called the Maxwell-Bartholdi pressure. It is very minute, and it has been difficult to put it into evidence even with the most sensitive radiometers; but it suffices that it exists.

If all the energy issuing from our generator falls on a receiver, this will act as if it had received a mechanical shock, which will represent in a sense the compensation of the recoil of the generator; the reaction will be equal to the action, but it will not be simultaneous; the receiver will move on but not at the moment when the generator recoils. If the energy propagates itself indefinitely without encountering a receiver, the compensation will never be made.

Do we say that the space which separates the generator from the receiver and which the perturbation must pass over in going from the one to the other is not void, that it is full not only of ether, but of air; or even in the interplanetary spaces of some fluid subtle but still ponderable; that this matter undergoes the shock like the

receiver at the moment when the energy reaches it, and recoils in its turn when the perturbation quits it? That would save the principle of Newton, but that is not true.

If energy in its diffusion remained always attached to some material substratum, then matter in motion would carry along light with it, and Fizeau has demonstrated that it does nothing of the sort, at least for air. This is what Michelson and Morley have since confirmed.

One may suppose also that the movements of matter, properly so called, are exactly compensated by those of the ether; but that would lead us to the same reflections as just now. The principle so extended would explain everything, since whatever might be the visible movements, we should always have the power of imagining hypothetical movements which compensated them.

But if it is able to explain everything, this is because it does not permit us to foresee anything; it does not enable us to decide between different possible hypotheses, since it explains everything beforehand. It therefore becomes useless.

And then the suppositions that it would be necessary to make on the movements of the ether are not very satisfactory.

If the electric charges double, it would be natural to imagine that the velocities of the divers atoms of ether double also, and for the compensation, it would be necessary that the mean velocity of the ether quadruple.

This is why I have long thought that these consequences of theory, contrary to the principle of Newton, would end some day by being abandoned, and yet the recent experiments on the movements of the electrons issuing from radium seem rather to confirm them.

I arrive at the principle of Lavoisier on the conservation of masses: in truth this is one not to be touched without unsettling all mechanics.

And now certain persons believe that it seems true to us only because we consider in mechanics merely moderate velocities, but that it would cease to be true for bodies animated by velocities comparable to that of light. These velocities, it is now believed, have been realized; the cathode rays or those of radium may be formed of very minute particles or of electrons which are displaced with velocities smaller no doubt than that of light, but which might be its one tenth or one third.

These rays can be deflected, whether by an electric field, or by a magnetic field, and we are able by comparing these deflections, to measure at the same time the velocity of the electrons and their mass (or rather the relation of their mass to their charge). But when it was seen that these velocities approached that of light, it was decided that a correction was necessary.

These molecules, being electrified, could not be displaced without agitating the ether; to put them in motion it is necessary to overcome a double inertia, that of the molecule itself and that of the ether. The total or apparent mass that one measures is composed, therefore, of two parts: the real or mechanical mass of the molecule and the electro-dynamic mass representing the inertia of the ether.

The calculations of Abraham and the experiments of Kaufmann have then shown that the mechanical mass, properly so called, is null, and that the mass of the electrons, or, at least, of the negative electrons, is of exclusively electro-dynamic origin. This forces us to change the definition of mass; we cannot any longer distinguish mechanical mass and electro-dynamic mass, since then the first would vanish; there is no mass other than electro-dynamic inertia. But in this case the mass can no longer be constant, it augments with the velocity, and it even depends on the direction, and a body animated by a notable velocity will not oppose the same inertia to the forces which tend to deflect it from its route, as to those which tend to accelerate or to retard its progress.

There is still a resource; the ultimate elements of bodies are electrons, some charged negatively, the others charged positively. The negative electrons have no mass, this is understood; but the positive electrons, from the little we know of them, seem much greater. Perhaps they have, besides their electro-dynamic mass, a true mechanical mass. The veritable mass of a body would, then, be the sum of the mechanical masses of its positive electrons, the negative electrons not counting; mass so defined could still be constant.

Alas, this resource also evades us. Recall what we have said of the principle of relativity and of the efforts made to save it. And it is not merely a principle which it is a question of saving, such are the indubitable results of the experiments of Michelson.

Lorentz has been obliged to suppose that all the forces, whatever be their origin, were affected with a coefficient in a medium animated by a uniform translation; this is not sufficient; it is still necessary, says he, that *the masses of all the particles be influenced by a translation to the same degree as the electro-magnetic masses of the electrons.*

So the mechanical masses will vary in accordance with the same laws as the electro-dynamic masses; they cannot, therefore, be constant.

Need I point out that the fall of the principle of Lavoisier involves that of the principle of Newton? This latter signifies that the centre of gravity of an isolated system moves in a straight line; but if there is no longer a constant mass, there is no longer a centre

of gravity, we no longer know even what this is.   This is why I said above that the experiments on the cathode rays appeared to justify the doubts of Lorentz on the subject of the principle of Newton.

From all these results, if they are confirmed, would arise an entirely new mechanics, which would be, above all, characterized by this fact, that no velocity could surpass that of light, any more than any temperature could fall below the zero absolute, because bodies would oppose an increasing inertia to the causes, which would tend to accelerate their motion; and this inertia would become infinite when one approached the velocity of light.

Nor for an observer carried along himself in a translation he did not suspect could any apparent velocity surpass that of light; there would then be a contradiction, if we recall that this observer would not use the same clocks as a fixed observer, but, indeed, clocks marking "local time."

Here we are then facing a question I content myself with stating. If there is no longer any mass, what becomes of the law of Newton?

Mass has two aspects, it is at the same time a coefficient of inertia and an attracting mass entering as factor into Newtonian attraction.   If the coefficient of inertia is not constant, can the attracting mass be?   That is the question.

At least, the principle of the conservation of energy yet remains to us, and this seems more solid.   Shall I recall to you how it was in its turn thrown into discredit?   This event has made more noise than the preceding and it is in all the records.

From the first works of Becquerel, and, above all, when the Curies had discovered radium, one saw that every radio-active body was an inexhaustible source of radiations.   Its activity would seem to subsist without alteration throughout the months and the years. This was already a strain on the principles; these radiations were in fact energy, and from the same morsel of radium this issued and forever issued.   But these quantities of energy were too slight to be measured; at least one believed so and was not much disquieted.

The scene changed when Curie bethought himself to put radium into a calorimeter; it was seen then that the quantity of heat incessantly created was very notable.

The explanations proposed were numerous; but in so far as no one of them has prevailed over the others, we cannot be sure there is a good one among them.

Sir William Ramsay has striven to show that radium is in process of transformation, that it contains a store of energy enormous but not inexhaustible.

The transformation of radium, then, would produce a million times more of heat than all known transformations; radium would

wear itself out in 1250 years; you see that we are at least certain to be settled on this point some hundreds of years from now. While waiting our doubts remain.

In the midst of so many ruins what remains standing? The principle of least action has hitherto remained intact, and Larmor appears to believe that it will long survive the others; in reality, it is still more vague and more general.

In presence of this general ruin of the principles, what attitude will mathematical physics take?

And first, before too much perplexity, it is proper to ask if all this is really true. All these apparent contradictions to the principles are encountered only among infinitesimals; the microscope is necessary to see the Brownian movement; electrons are very light; radium is very rare, and no one has ever seen more than some milligrams of it at a time.

And, then, it may be asked if, beside the infinitesimal seen, there be not another infinitesimal unseen counterpoise to the first.

So, there is an interlocutory question, and, as it seems, only experiment can solve it. We have, therefore, only to hand over the matter to the experimenters, and, while waiting for them to determine the question finally, not to preoccupy ourselves with these disquieting problems, but quietly continue our work, as if the principles were still uncontested. We have much to do without leaving the domain where they may be applied in all security; we have enough to employ our activity during this period of doubts.

And as to these doubts, is it indeed true that we can do nothing to disembarrass science of them? It may be said, it is not alone experimental physics that has given birth to them; mathematical physics has well contributed. It is the experimenters who have seen radium throw out energy, but it is the theorists who have put in evidence all the difficulties raised by the propagation of light across a medium in motion; but for these it is probable we should not have become conscious of them. Well, then, if they have done their best to put us into this embarrassment, it is proper also that they help us to get out of it.

They must subject to critical examination all these new views I have just outlined before you, and abandon the principles only after having made a loyal effort to save them.

What can they do in this sense? That is what I will try to explain.

Among the most interesting problems of mathematical physics, it is proper to give a special place to those relating to the kinetic theory of gases. Much has already been done in this direction, but much still remains to be done. This theory is an eternal paradox. We have reversibility in the premises and irreversibility in the con-

clusions; and between the two an abyss. Statistic considerations, the law of great numbers, do they suffice to fill it? Many points still remain obscure to which it is necessary to return, and doubtless many times. In clearing them up, we shall understand better the sense of the principle of Carnot and its place in the *ensemble* of dynamics, and we shall be better armed to interpret properly the curious experiment of Gouy, of which I spoke above.

Should we not also endeavor to obtain a more satisfactory theory of the electro-dynamics of bodies in motion? It is there especially, as I have sufficiently shown above, that difficulties accumulate. Evidently we must heap up hypotheses, we cannot satisfy all the principles at once; heretofore, one has succeeded in safeguarding some only on condition of sacrificing the others; but all hope of obtaining better results is not yet lost. Let us take, therefore, the theory of Lorentz, turn it in all senses, modify it little by little, and perhaps everything will arrange itself.

Thus in place of supposing that bodies in motion undergo a contraction in the sense of the motion, and that this contraction is the same whatever be the nature of these bodies and the forces to which they are otherwise submitted, could we not make an hypothesis more simple and more natural?

We might imagine, for example, that it is the ether which is modified when it is in relative motion in reference to the material medium which it penetrates, that when it is thus modified, it no longer transmits perturbations with the same velocity in every direction. It might transmit more rapidly those which are propagated parallel to the medium, whether in the same sense or in the opposite sense, and less rapidly those which are propagated perpendicularly. The wave surfaces would no longer be spheres, but ellipsoids, and we could dispense with that extraordinary contraction of all bodies.

I cite that only as an example, since the modifications one might essay would be evidently susceptible of infinite variation.

It is possible also that the astronomer may some day furnish us data on this point; he it was in the main who raised the question in making us acquainted with the phenomenon of the aberration of light. If we make crudely the theory of aberration, we reach a very curious result. The apparent positions of the stars differ from their real positions because of the motion of the earth, and as this motion is variable, these apparent positions vary. The real position we cannot know, but we can observe the variations of the apparent position. The observations of the aberration show us, therefore, not the movement of the earth, but the variations of this movement; they cannot, therefore, give us information about the absolute motion of the earth. At least this is true in first approximation, but it would be no longer the same if we could appreciate the thousandths

of a second. Then it would be seen that the amplitude of the oscillation depends not alone on the variation of the motion, variation which is well known, since it is the motion of our globe on its elliptic orbit, but on the mean value of this motion; so that the constant of aberration would not be altogether the same for all the stars, and the differences would tell us the absolute motion of the earth in space.

This, then, would be, under another form, the ruin of the principle of relativity. We are far, it is true, from appreciating the thousandths of a second, but after all, say some, the total absolute velocity of the earth may be much greater than its relative velocity with respect to the sun. If, for example, it were 300 kilometers per second in place of 30, this would suffice to make the phenomena observable.

I believe that in reasoning thus we admit a too simple theory of aberration. Michelson has shown us, I have told you, that the physical procedures are powerless to put in evidence absolute motion; I am persuaded that the same will be true of the astronomic procedures, however far one pushes precision.

However that may be, the data astronomy will furnish us in this regard will some day be precious to the physicist. While waiting, I believe the theorists, recalling the experience of Michelson, may anticipate a negative result, and that they would accomplish a useful work in constructing a theory of aberration which would explain this in advance.

But let us come back to the earth. There also we may aid the experimenters. We can, for example, prepare the ground by studying profoundly the dynamics of electrons; not, be it understood, in starting from a single hypothesis, but in multiplying hypotheses as much as possible. It will be, then, for the physicists to utilize our work in seeking the crucial experiment to decide between these different hypotheses.

This dynamics of electrons can be approached from many sides, but among the ways leading thither is one which has been somewhat neglected, and yet this is one of those which promise us most of surprises. It is the movements of the electrons which produce the line of the emission spectra; this is proved by the phenomenon of Zeemann; in an incandescent body, what vibrates is sensitive to the magnet, therefore electrified. This is a very important first point, but no one has gone farther; why are the lines of the spectrum distributed in accordance with a regular law?

These laws have been studied by the experimenters in their least details; they are very precise and relatively simple. The first study of these distributions recalled the harmonics encountered in acoustics; but the difference is great. Not only the numbers of vibrations are not the successive multiples of one number, but we do not

even find anything analogous to the roots of those transcendental equations to which so many problems of mathematical physics conduct us: that of the vibrations of an elastic body of any form, that of the Hertzian oscillations in a generator of any form, the problem of Fourier for the cooling of a solid body.

The laws are simpler, but they are of wholly other nature, and to cite only one of these differences, for the harmonics of high order the number of vibrations tends toward a finite limit, instead of increasing indefinitely.

That has not yet been accounted for, and I believe that there we have one of the most important secrets of nature. Lindemann has made a praiseworthy attempt, but, to my mind, without success; this attempt should be renewed. Thus we shall penetrate, so to say, into the inmost recess of matter. And from the particular point of view which we to-day occupy, when we know why the vibrations of incandescent bodies differ from ordinary elastic vibrations, why the electrons do not behave themselves like the matter which is familiar to us, we shall better comprehend the dynamics of electrons and it will be perhaps more easy for us to reconcile it with the principles.

Suppose, now, that all these efforts fail, and after all I do not believe they will, what must be done? Will it be necessary to seek to mend the broken principles in giving what we French call a *coup de pouce*? That is evidently always possible, and I retract nothing I have formerly said.

Have you not written, you might say if you wished to seek a quarrel with me, have you not written that the principles, though of experimental origin, are now unassailable by experiment because they have become conventions? And now you have just told us the most recent conquests of experiment put these principles in danger. Well, formerly I was right and to-day I am not wrong.

Formerly I was right, and what is now happening is a new proof of it. Take, for example, the calorimeter experiment of Curie on radium. Is it possible to reconcile that with the principle of the conservation of energy?

It has been attempted in many ways; but there is among them one I should like you to notice.

It has been conjectured that radium was only an intermediary, that it only stored radiations of unknown nature which flashed through space in every direction, traversing all bodies, save radium, without being altered by this passage and without exercising any action upon them. Radium alone took from them a little of their energy and afterward gave it out to us in divers forms.

What an advantageous explanation, and how convenient! First, it is unverifiable and thus irrefutable. Then again it will serve to

account for any derogation whatever to the principle of Mayer; it responds in advance not only to the objection of Curie, but to all the objections that future experimenters might accumulate. This new and unknown energy would serve for everything. This is just what I have said, and we are thereby shown that our principle is unassailable by experiment.

And after all, what have we gained by this *coup de pouce ?*

The principle is intact, but thenceforth of what use is it?

It permitted us to foresee that in such or such circumstance we could count on such a total quantity of energy; it limited us; but now where there is put at our disposition this indefinite provision of new energy, we are limited by nothing; and as I have written elsewhere, if a principle ceases to be fecund, experiment, without contradicting it directly, will be likely to condemn it.

This, therefore, is not what would have to be done, it would be necessary to rebuild anew.

If we were cornered down to this necessity, we should moreover console ourselves. It would not be necessary to conclude that science can weave only a Penelope's web, that it can build only ephemeral constructions, which it is soon forced to demolish from top to bottom with its own hands.

As I have said, we have already passed through a like crisis. I have shown you that in the second mathematical physics, that of the principles, we find traces of the first, that of the central forces; it will be just the same if we must learn a third.

When an animal exuviates, and breaks its too narrow carapace to make itself a fresh one, we easily recognize under the new envelope the essential traits of the organism which have existed.

We cannot foresee in what way we are about to expand; perhaps it is the kinetic theory of gases which is about to undergo development and serve as model to the others. Then, the facts which first appeared to us as simple, thereafter will be merely results of a very great number of elementary facts which only the laws of chance make coöperate for a common end. Physical law will then take an entirely new aspect; it will no longer be solely a differential equation, it will take the character of a statistical law.

Perhaps, likewise, we should construct a whole new mechanics, of which we only succeed in catching a glimpse, where inertia increasing with the velocity, the velocity of light would become an impassable limit.

The ordinary mechanics, more simple, would remain a first approximation, since it would be true for velocities not too great, so that we should still find the old dynamics under the new.

We should not have to regret having believed in the principles, and even, since velocities too great for the old formulas would always

be only exceptional, the surest way in practice would be still to act as if we continued to believe in them. They are so useful, it would be necessary to keep a place for them. To determine to exclude them altogether would be to deprive one's self of a precious weapon. I hasten to say in conclusion we are not yet there, and as yet nothing proves that the principles will not come forth from the combat victorious and intact.

SHORT PAPERS

Three short papers were read in the Section of Applied Mathematics, the first by Professor Henry T. Eddy, of the University of Minnesota, on "The Electromagnetic Theory and the Velocity of Light."

The second paper was presented by Professor Alexander Macfarlane, of Chatham, Ontario, "On the Exponential Notation in Vector-analysis."

The third paper was presented by Professor James McMahon, of Cornell University, "On the Use of N-fold Riemann Spaces in Applied Mathematics."

WORKS OF REFERENCE

(PREPARED THROUGH THE COURTESY OF PROFESSOR GEORGE BRUCE HALSTED,
OF KENYON COLLEGE, AND PROFESSOR LUDWIG BOLTZMANN, OF THE UNIVERSITY
OF VIENNA)

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SCIENCE AND HYPOTHESIS

SCIENCE AND HYPOTHESIS[1]

BY PROF. JULES HENRI POINCARE, UNIVERSITY OF PARIS

PART I — NUMBER AND MAGNITUDE

On the Nature of Mathematical Reasoning

THE very possibility of mathematical science seems an insoluble contradiction. If this science is only deductive in appearance, from whence is derived that perfect rigor which is challenged by none? If, on the contrary, all the propositions which it enunciates may be derived in order by the rules of formal logic, how is it that mathematics is not reduced to a gigantic tautology? The syllogism can teach us nothing essentially new, and if everything must spring from the principle of identity, then everything should be capable of being reduced to that principle. Are we then to admit that the enunciations of all the theorems with which so many volumes are filled, are only indirect ways of saying that A is A?

No doubt we may refer back to axioms which are at the source of all these reasonings. If it is felt that they cannot be reduced to the principle of contradiction, if we decline to see in them any more than experimental facts which have no part or lot in mathematical necessity, there is still one resource left to us: we may class them among *à priori* synthetic views. But this is no solution of the difficulty — it is merely giving it a name; and even if the nature of the synthetic views had no longer for us any mystery, the contradiction would not have disappeared; it would have only been shirked. Syllogistic reasoning remains incapable of adding anything to the data that are given it; the data are reduced to axioms, and that is all we should find in the conclusions.

No theorem can be new unless a new axiom intervenes in its demonstration; reasoning can only give us immediately evident truths borrowed from direct intuition; it would only be an intermediary

parasite. Should we not therefore have reason for asking if the syllogistic apparatus serves only to disguise what we have borrowed?

The contradiction will strike us the more if we open any book on mathematics; on every page the author announces his intention of generalizing some proposition already known. Does the mathematical method proceed from the particular to the general, and, if so, how can it be called deductive?

Finally, if the science of number were merely analytical, or could be analytically derived from a few synthetic intuitions, it seems that a sufficiently powerful mind could with a single glance perceive all its truths; nay, one might even hope that some day a language would be invented simple enough for these truths to be made evident to any person of ordinary intelligence.

Even if these consequences are challenged, it must be granted that mathematical reasoning has of itself a kind of creative virtue, and is therefore to be distinguished from the syllogism. The difference must be profound. We shall not, for instance, find the key to the mystery in the frequent use of the rule by which the same uniform operation applied to two equal numbers will give identical results. All these modes of reasoning, whether or not reducible to the syllogism, properly so called, retain the analytical character, and *ipso facto*, lose their power.

The argument is an old one. Let us see how Leibnitz tried to show that two and two make four. I assume the number one to be defined, and also the operation $x+1$—*i.e.*, the adding of unity to a given number x. These definitions, whatever they may be, do not enter into the subsequent reasoning. I next define the numbers 2, 3, 4 by the equalities:—

(1) $1+1=2$; (2) $2+1=3$; (3) $3+1=4$, and in the same way I define the operation $x+2$ by the relation; (4) $x+2=(x+1)+1$.
Given this, we have:—

$$2+2=(2+1)+1; \quad \text{(def. 4)}.$$
$$(2+1)+1=3+1 \quad \quad \text{(def. 2)}.$$
$$3+1=4 \quad \quad \quad \text{(def. 3)}.$$

whence $2+2=4$ Q.E.D.

It cannot be denied that this reasoning is purely analytical. But if we ask a mathematician, he will reply: " This is not a demonstration properly so called; it is a verification." We have confined ourselves to bringing together one or other of two purely conventional definitions, and we have verified their identity; nothing new has been learned. *Verification* differs from proof precisely because it is analytical, and because it leads to nothing. It leads to nothing because the conclusion is nothing but the premises translated into another language. A real proof, on the other hand, is fruitful, because the conclusion is in a sense more general than the premises. The equality

2+2=4 can be verified because it is particular. Each individual enunciation in mathematics may be always verified in the same way. But if mathematics could be reduced to a series of such verifications it would not be a science. A chess-player, for instance, does not create a science by winning a piece. There is no science but the science of the general. It may even be said that the object of the exact sciences is to dispense with these direct verifications.

Let us now see the geometer at work, and try to surprise some of his methods. The task is not without difficulty; it is not enough to open a book at random and to analyze any proof we may come across. First of all, geometry must be excluded, or the question becomes complicated by difficult problems relating to the rôle of the postulates, the nature and the origin of the idea of space. For analogous reasons we cannot avail ourselves of the infinitesimal calculus. We must seek mathematical thought where it has remained pure—*i.e.*, in Arithmetic. But we still have to choose; in the higher parts of the theory of numbers the primitive mathematical ideas have already undergone so profound an elaboration that it becomes difficult to analyze them.

It is therefore at the beginning of Arithmetic that we must expect to find the explanation we seek; but it happens that it is precisely in the proofs of the most elementary theorems that the authors of classic treatises have displayed the least precision and rigor. We may not impute this to them as a crime; they have obeyed a necessity. Beginners are not prepared for real mathematical rigor; they would see in it nothing but empty, tedious subtleties. It would be waste of time to try to make them more exacting; they have to pass rapidly and without stopping over the road which was trodden slowly by the founders of the science.

Why is so long a preparation necessary to habituate oneself to this perfect rigor, which it would seem should naturally be imposed on all minds? This is a logical and psychological problem which is well worthy of study. But we shall not dwell on it; it is foreign to our subject. All I wish to insist on is, that we shall fail in our purpose unless we reconstruct the proofs of the elementary theorems, and give them, not the rough form in which they are left so as not to weary the beginner, but the form which will satisfy the skilled geometer.

Definition of Addition

I assume that the operation $x+1$ has been defined; it consists in adding the number 1 to a given number x. Whatever may be said of this definition, it does not enter into the subsequent reasoning.

We now have to define the operation $x+a$, which consists in adding the number a to any given number x. Suppose that we have defined the operation $x+(a-1)$; the operation $x+a$ will be defined by

the equality: (1) $x + a = [x + (a-1)] + 1$. We shall know what $x + a$ is when we know what $x + (a-1)$ is, and as I have assumed that to start with we know what $x + 1$ is, we can define successively and "by recurrence" the operations $x + 2$, $x + 3$, etc. This definition deserves a moment's attention; it is of a particular nature which distinguishes it even at this stage from the purely logical definition; the equality (1), in fact, contains an infinite number of distinct definitions, each having only one meaning when we know the meaning of its predecessor.

Properties of Addition

Associative.— I say that $a + (b + c) = (a + b) + c$; in fact the theorem is true for $c = 1$. It may then be written $a + (b + 1) = (a + b) + 1$; which, remembering the difference of notation, is nothing but the equality (1) by which I have just defined addition. Assume the theorem true for $c = \gamma$, I say that it will be true for $c = \gamma + 1$. Let $(a + b) + \gamma = a + (b + \gamma)$, it follows that $[(a + b) + \gamma] + 1 = [a + (b + \gamma)] + 1$; or by def. (1)—$(a + b) + (\gamma + 1) = a + (b + \gamma + 1) = a + [b + (\gamma + 1)]$, which shows by a series of purely analytical deductions that the theorem is true for $\gamma + 1$. Being true for $c = 1$, we see that it is successively true for $c = 2$, $c = 3$, etc.

Commutative. — (1) I say that $a + 1 = 1 + a$. The theorem is evidently true for $a = 1$; we can *verify* by purely analytical reasoning that if it is true for $a = \gamma$ it will be true for $a = \gamma + 1$.[1] Now, it is true for $a = 1$, and therefore is true for $a = 2$, $a = 3$, and so on. This is what is meant by saying that the proof is demonstrated "by recurrence."

(2) I say that $a + b = b + a$. The theorem has just been shown to hold good for $b = 1$, and it may be verified analytically that if it is true for $b = \beta$, it will be true for $b = \beta + 1$. The proposition is thus established by recurrence.

Definition of Multiplication

We shall define multiplication by the equalities: (1) $a \times 1 = a$. (2) $a \times b = [a \times (b-1)] + a$. Both of these include an infinite number of definitions; having defined $a \times 1$, it enables us to define in succession $a \times 2$, $a \times 3$, and so on.

Properties of Multiplication

Distributive. — I say that $(a + b) \times c = (a \times c) + (b \times c)$. We can verify analytically that the theorem is true for $c = 1$; then if it is true for $c = \gamma$, it will be true for $c = \gamma + 1$. The proposition is then proved by recurrence.

[1] For $(\gamma+1)+1=(1+\gamma)+1=1+(\gamma+1)$.—[Tr.]

Commutative. — (1) I say that $a \times 1 = 1 \times a$. The theorem is obvious for $a = 1$. We can verify analytically that if it is true for $a = a$, it will be true for $a = a + 1$.

(2) I say that $a \times b = b \times a$. The theorem has just been proved for $b = 1$. We can verify analytically that if it be true for $b = \beta$ it will be true for $b = \beta + 1$.

This monotonous series of reasonings may now be laid aside; but their very monotony brings vividly to light the process, which is uniform, and is met again at every step. The process is proof by recurrence. We first show that a theorem is true for $n=1$; we then show that if it is true for $n-1$ it is true for n, and we conclude that it is true for all integers. We have now seen how it may be used for the proof of the rules of addition and multiplication — that is to say, for the rules of the algebraical calculus. This calculus is an instrument of transformation which lends itself to many more different combinations than the simple syllogism; but it is still a purely analytical instrument, and is incapable of teaching us anything new. If mathematics had no other instrument, it would immediately be arrested in its development; but it has recourse anew to the same process — *i.e.,* to reasoning by recurrence, and it can continue its forward march. Then if we look carefully, we find this mode of reasoning at every step, either under the simple form which we have just given to it, or under a more or less modified form. It is therefore mathematical reasoning *par excellence,* and we must examine it closer.

The essential characteristic of reasoning by recurrence is that it contains, condensed, so to speak, in a single formula, an infinite number of syllogisms. We shall see this more clearly if we enunciate the syllogisms one after another. They follow one another, if one may use the expression, in a cascade. The following are the hypothetical syllogisms:—The theorem is true of the number 1. Now, if it is true of 1, it is true of 2; therefore it is true of 2. Now, if it is true of 2, it is true of 3; hence it is true of 3, and so on. We see that the conclusion of each syllogism serves as the minor of its successor. Further, the majors of all our syllogisms may be reduced to a single form. If the theorem is true of $n-1$, it is true of n.

We see, then, that in reasoning by recurrence we confine ourselves to the enunciation of the minor of the first syllogism, and the general formula which contains as particular cases all the majors. This unending series of syllogisms is thus reduced to a phrase of a few lines.

It is now easy to understand why every particular consequence of a theorem may, as I have above explained, be verified by purely analytical processes. If, instead of proving that our theorem is true for all numbers we only wish to show that it is true for the number 6 for instance, it will be enough to establish the first five

syllogisms in our cascade. We shall require 9 if we wish to prove it
for the number 10; for a greater number we shall require more still;
but however great the number may be we shall always reach it, and the
analytical verification will always be possible. But however far we went
we should never reach the general theorem applicable to all numbers,
which alone is the object of science. To reach it we should require
an infinite number of syllogisms, and we should have to cross an abyss
which the patience of the analyst, restricted to the resources of formal
logic, will never succeed in crossing.

I asked at the outset why we cannot conceive of a mind powerful
enough to see at a glance the whole body of mathematical truth. The
answer is now easy. A chess-player can combine for four or five
moves ahead; but, however extraordinary a player he may be, he
cannot prepare for more than a finite number of moves. If he applies
his faculties to Arithmetic, he cannot conceive its general truths by
direct intuition alone; to prove even the smallest theorem he must use
reasoning by recurrence, for that is the only instrument which enables
us to pass from the finite to the infinite. This instrument is always
useful, for it enables us to leap over as many stages as we wish; it frees
us from the necessity of long, tedious, and monotonous verifications
which would rapidly become impracticable. Then when we take in
hand the general theorem it becomes indispensable, for otherwise we
should ever be approaching the analytical verification without ever
actually reaching it. In this domain of Arithmetic we may think our-
selves very far from the infinitesimal analysis, but the idea of math-
ematical infinity is already playing a preponderating part, and with-
out it there would be no science at all, because there would be nothing
general.

The views upon which reasoning by recurrence is based may be
exhibited in other forms; we may say, for instance, that in any finite
collection of different integers there is always one which is smaller
than any other. We may readily pass from one enunciation to an-
other, and thus give ourselves the illusion of having proved that
reasoning by recurrence is legitimate. But we shall always be brought
to a full stop — we shall always come to an indemonstrable axiom,
which will at bottom be but the proposition we had to prove translated
into another language. We cannot therefore escape the conclusion
that the rule of reasoning by recurrence is irreducible to the principle
of contradiction. Nor can the rule come to us from experiment. Ex-
periment may teach us that the rule is true for the first ten or the first
hundred numbers, for instance; it will not bring us to the indefinite
series of numbers, but only to a more or less long, but always limited,
portion of the series.

Now, if that were all that is in question, the principle of contradic-
tion would be sufficient, it would always enable us to develop as many

syllogisms as we wished. It is only when it is a question of a single formula to embrace an infinite number of syllogisms that this principle breaks down, and there, too, experiment is powerless to aid. This rule, inaccessible to analytical proof and to experiment, is the exact type of the *à priori* synthetic intuition. On the other hand, we cannot see in it a convention as in the case of the postulates of geometry.

Why then is this view imposed upon us with such an irresistible weight of evidence? It is because it is only the affirmation of the power of the mind which knows it can conceive of the indefinite repetition of the same act, when the act is once possible. The mind has a direct intuition of this power, and experiment can only be for it an opportunity of using it, and thereby of becoming conscious of it.

But it will be said, if the legitimacy of reasoning by recurrence cannot be established by experiment alone, is it so with experiment aided by induction? We see successively that a theorem is true of the number 1, of the number 2, of the number 3, and so on — the law is manifest, we say, and it is so on the same ground that every physical law is true which is based on a very large but limited number of observations.

It cannot escape our notice that here is a striking analogy with the usual processes of induction. But an essential difference exists. Induction applied to the physical sciences is always uncertain, because it is based on the belief in a general order of the universe, an order which is external to us. Mathematical induction — *i.e.*, proof by recurrence — is, on the contrary, necessarily imposed on us, because it is only the affirmation of a property of the mind itself.

Mathematicians, as I have said before, always endeavor to generalize the propositions they have obtained. To seek no further example, we have just shown the equality, $a+1=1+a$, and we then used it to establish the equality, $a+b=b+a$, which is obviously more general. Mathematics may, therefore, like the other sciences, proceed from the particular to the general. This is a fact which might otherwise have appeared incomprehensible to us at the beginning of this study, but which has no longer anything mysterious about it, since we have ascertained the analogies between proof by recurrence and ordinary induction.

No doubt mathematical recurrent reasoning and physical inductive reasoning are based on different foundations, but they move in parallel lines and in the same direction — namely, from the particular to the general.

Let us examine the case a little more closely. To prove the equality $a+2=2+a$......(1), we need only apply the rule $a+1=1+a$, twice, and write $a+2=a+1+1=1+a+1=1+1+a=2+a$......(2).

The equality thus deduced by purely analytical means is not, how-

ever, a simple particular case. It is something quite different. We may not therefore even say in the really analytical and deductive part of mathematical reasoning that we proceed from the general to the particular in the ordinary sense of the words. The two sides of the equality (2) are merely more complicated combinations than the two sides of the equality (1), and analysis only serves to separate the elements which enter into these combinations and to study their relations.

Mathematicians therefore proceed " by construction," they " construct" more complicated combinations. When they analyze these combinations, these aggregates, so to speak, into their primitive elements, they see the relations of the elements and deduce the relations of the aggregates themselves. The process is purely analytical, but it is not a passing from the general to the particular, for the aggregates obviously cannot be regarded as more particular than their elements.

Great importance has been rightly attached to this process of " construction," and some claim to see in it the necessary and sufficient condition of the progress of the exact sciences. Necessary, no doubt, but not sufficient! For a construction to be useful and not mere waste of mental effort, for it to serve as a stepping-stone to higher things, it must first of all possess a kind of unity enabling us to see something more than the juxtaposition of its elements. Or more accurately, there must be some advantage in considering the construction rather than the elements themselves. What can this advantage be? Why reason on a polygon, for instance, which is always decomposable into triangles, and not on elementary triangles? It is because there are properties of polygons of any number of sides, and they can be immediately applied to any particular kind of polygon. In most cases it is only after long efforts that those properties can be discovered, by directly studying the relations of elementary triangles. If the quadrilateral is anything more than the juxtaposition of two triangles, it is because it is of the polygon type.

A construction only becomes interesting when it can be placed side by side with other analogous constructions for forming species of the same genus. To do this we must necessarily go back from the particular to the general, ascending one or more steps. The analytical process " by construction " does not compel us to descend, but it leaves us at the same level. We can only ascend by mathematical induction, for from it alone can we learn something new. Without the aid of this induction, which in certain respects differs from, but is as fruitful as, physical induction, construction would be powerless to create science.

Let me observe, in conclusion, that this induction is only possible if the same operation can be repeated indefinitely. That is why the theory of chess can never become a science, for the different moves of the same piece are limited and do not resemble each other.

Mathematical Magnitude and Experiment

If we want to know what the mathematicians mean by a continuum, it is useless to appeal to geometry. The geometer is always seeking, more or less, to represent to himself the figures he is studying, but his representations are only instruments to him; he uses space in his geometry just as he uses chalk: and further, too much importance must not be attached to accidents which are often nothing more than the whiteness of the chalk.

The pure analyst has not to dread this pitfall. He has disengaged mathematics from all extraneous elements, and he is in a position to answer our question: — " Tell me exactly what this continuum is, about which mathematicians reason." Many analysts who reflect on their art have already done so — M. Tannery, for instance, in his *Introduction à la théorie des Fonctions d'une variable*.

Let us start with the integers. Between any two consecutive sets, intercalate one or more intermediary sets, and then between these sets others again, and so on indefinitely. We thus get an unlimited number of terms, and these will be the numbers which we call fractional, rational, or commensurable. But this is not yet all; between these terms, which, be it marked, are already infinite in number, other terms are intercalated, and these are called irrational or incommensurable.

Before going any further, let me make a preliminary remark. The continuum thus conceived is no longer a collection of individuals arranged in a certain order, infinite in number, it is true, but external the one to the other. This is not the ordinary conception in which it is supposed that between the elements of the continuum exists an intimate connection making of it one whole, in which the point has no existence previous to the line, but the line does exist previous to the point. Multiplicity alone subsists, unity has disappeared — " the continuum is unity in multiplicity," according to the celebrated formula. The analysts have even less reason to define their continuum as they do, since it is always on this that they reason when they are particularly proud of their rigor. It is enough to warn the reader that the real mathematical continuum is quite different from that of the physicists and from that of the metaphysicians.

It may also be said, perhaps, that mathematicians who are contented with this definition are the dupes of words, that the nature of each of these sets should be precisely indicated, that it should be explained how they are to be intercalated, and that it should be shown how it is possible to do it. This, however, would be wrong; the only property of the sets which comes into the reasoning is that of preceding or succeeding these or those other sets; this alone should therefore intervene in the definition. So we need not concern ourselves with the manner in which the sets are intercalated, and no one will

doubt the possibility of the operation if he only remembers that " possible " in the language of geometers simply means exempt from contradiction. But our definition is not yet complete, and we come back to it after this rather long digression.

Definition of Incommensurables. — The mathematicians of the Berlin school, and Kronecker in particular, have devoted themselves to constructing this continuous scale of irrational and fractional numbers without using any other materials than the integer. The mathematical continuum from this point of view would be a pure creation of the mind in which experiment would have no part.

The idea of rational number not seeming to present to them any difficulty, they have confined their attention mainly to defining incommensurable numbers. But before reproducing their definition here, I must make an observation that will allay the astonishment which this will not fail to provoke in readers who are but little familiar with the habits of geometers.

Mathematicians do not study objects, but the relations between objects; to them it is a matter of indifference if these objects are replaced by others, provided that the relations do not change. Matter does not engage their attention, they are interested by form alone.

If we did not remember it, we could hardly understand that Kronecker gives the name of incommensurable number to a simple symbol — that is to say, something very different from the idea we think we ought to have of a quantity which should be measurable and almost tangible.

Let us see now what is Kronecker's definition. Commensurable numbers may be divided into classes in an infinite number of ways, subject to the condition that any number whatever of the first class is greater than any number of the second. It may happen that among the numbers of the first class there is one which is smaller than all the rest; if, for instance, we arrange in the first class all the numbers greater than 2, and 2 itself, and in the second class all the numbers smaller than 2, it is clear that 2 will be the smallest of all the numbers of the first class. The number 2 may therefore be chosen as the symbol of this division.

It may happen, on the contrary, that in the second class there is one which is greater than all the rest. This is what takes place, for example, if the first class comprises all the numbers greater than 2, and if, in the second, are all the numbers less than 2, and 2 itself. Here again the number 2 might be chosen as the symbol of this division.

But it may equally well happen that we can find neither in the first class a number smaller than all the rest, nor in the second class a number greater than all the rest. Suppose, for instance, we place in the first class all the numbers whose squares are greater than 2, and in

the second all the numbers whose squares are smaller than 2. We know that in neither of them is a number whose square is equal to 2. Evidently there will be in the first class no number which is smaller than all the rest, for however near the square of a number may be to 2, we can always find a commensurable whose square is still nearer to 2. From Kronecker's point of view, the incommensurable number of 2 is nothing but the symbol of this particular method of division of commensurable numbers; and to each mode of repartition corresponds in this way a number, commensurable or not, which serves as a symbol. But to be satisfied with this would be to forget the origin of these symbols; it remains to explain how we have been led to attribute to them a kind of concrete existence, and on the other hand, does not the difficulty begin with fractions? Should we have the notion of these numbers if we did not previously know a matter which we conceive as infinitely divisible — *i.e.,* as a continuum?

The Physical Continuum. — We are next led to ask if the idea of the mathematical continuum is not simply drawn from experiment. If that be so, the rough data of experiment, which are our sensations, could be measured. We might, indeed, be tempted to believe that this is so, for in recent times there has been an attempt to measure them, and a law has even been formulated, known as Fechner's law, according to which sensation is proportional to the logarithm of the stimulus. But if we examine the experiments by which the endeavor has been made to establish this law, we shall be led to a diametrically opposite conclusion. It has, for instance, been observed that a weight A of 10 grammes and a weight B of 11 grammes produced identical sensations, that the weight B could no longer be distinguished from a weight C of 12 grammes, but that the weight A was readily distinguished from the weight C. Thus the rough results of the experiments may be expressed by the following relations: $A = B$, $B = C$, $A < C$, which may be regarded as the formula of the physical continuum. But here is an intolerable disagreement with the law of contradiction, and the necessity of banishing this disagreement has compelled us to invent the mathematical continuum. We are therefore forced to conclude that this notion has been created entirely by the mind, but it is experiment that has provided the opportunity. We cannot believe that two quantities which are equal to a third are not equal to one another, and we are thus led to suppose that A is different from B and B from C, and that if we have not been aware of this, it is due to the imperfections of our senses.

The Creation of the Mathematical Continuum: First Stage. — So far it would suffice, in order to account for facts, to intercalate between A and B a small number of terms which would remain discrete. What happens now if we have recourse to some instrument to make up for the weakness of our senses? If, for example, we use a microscope?

Such terms as A and B, which before were indistinguishable from one another, appear now to be distinct: but between A and B, which are distinct, is intercalated another new term D, which we can distinguish neither from A nor from B. Although we may use the most delicate methods, the rough results of our experiments will always present the characters of the physical continuum with the contradiction which is inherent in it. We only escape from it by incessantly intercalating new terms between the terms already distinguished, and this operation must be pursued indefinitely. We might conceive that it would be possible to stop if we could imagine an instrument powerful enough to decompose the physical continuum into discrete elements, just as the telescope resolves the Milky Way into stars. But this we cannot imagine; it is always with our senses that we use our instruments; it is with the eye that we observe the image magnified by the microscope, and this image must therefore always retain the characters of visual sensation, and therefore those of the physical continuum.

Nothing distinguishes a length directly observed from half that length doubled by the microscope. The whole is homogeneous to the part; and there is a fresh contradiction — or rather there would be one if the number of the terms were supposed to be finite; it is clear that the part containing less terms than the whole cannot be similar to the whole. The contradiction ceases as soon as the number of terms is regarded as infinite. There is nothing, for example, to prevent us from regarding the aggregate of integers as similar to the aggregate of even numbers, which is however only a part of it; in fact, to each integer corresponds another even number which is its double. But it is not only to escape this contradiction contained in the empiric data that the mind is led to create the concept of a continuum formed of an indefinite number of terms.

Here everything takes place just as in the series of the integers. We have the faculty of conceiving that a unit may be added to a collection of units. Thanks to experiment, we have had the opportunity of exercising this faculty and are conscious of it; but from this fact we feel that our power is unlimited, and that we can count indefinitely, although we have never had to count more than a finite number of objects. In the same way, as soon as we have intercalated terms between two consecutive terms of a series, we feel that this operation may be continued without limit, and that, so to speak, there is no intrinsic reason for stopping. As an abbreviation, I may give the name of a mathematical continuum of the first order to every aggregate of terms formed after the same law as the scale of commensurable numbers. If, then, we intercalate new sets according to the laws of incommensurable numbers, we obtain what may be called a continuum of the second order.

Second Stage. — We have only taken our first step. We have explained the origin of continuums of the first order; we must now see why this is not sufficient, and why the incommensurable numbers had to be invented.

If we try to imagine a line, it must have the characters of the physical continuum — that is to say, our representation must have a certain breadth. Two lines will therefore appear to us under the form of two narrow bands, and if we are content with this rough image, it is clear that where two lines cross they must have some common part. But the pure geometer makes one further effort; without entirely renouncing the aid of his senses, he tries to imagine a line without breadth and a point without size. This he can do only by imagining a line as the limit towards which tends a band that is growing thinner and thinner, and the point as the limit towards which is tending an area that is growing smaller and smaller. Our two bands, however narrow they may be, will always have a common area; the smaller they are the smaller it will be, and its limit is what the geometer calls a point. This is why it is said that the two lines which cross must have a common point, and this truth seems intuitive.

But a contradiction would be implied if we conceived of lines as continuums of the first order — *i.e.*, the lines traced by the geometer should only give us points, the co-ordinates of which are rational numbers. The contradiction would be manifest if we were, for instance, to assert the existence of lines and circles. It is clear, in fact, that if the points whose co-ordinates are commensurable were alone regarded as real, the in-circle of a square and the diagonal of the square would not intersect, since the co-ordinates of the point of intersection are incommensurable.

Even then we should have only certain incommensurable numbers, and not all these numbers.

But let us imagine a line divided into two half-rays (*demi-droites*). Each of these half-rays will appear to our minds as a band of a certain breadth; these bands will fit close together, because there must be no interval between them. The common part will appear to us to be a point which will still remain as we imagine the bands to become thinner and thinner, so that we admit as an intuitive truth that if a line be divided into two half-rays the common frontier of these half-rays is a point. Here we recognize the conception of Kronecker, in which an incommensurable number was regarded as the common frontier of two classes of rational numbers. Such is the origin of the continuum of the second order, which is the mathematical continuum properly so called.

Summary. — To sum up, the mind has the faculty of creating symbols, and it is thus that it has constructed the mathematical continuum, which is only a particular system of symbols. The only limit

to its power is the necessity of avoiding all contradiction; but the mind only makes use of it when experiment gives a reason for it.

In the case with which we are concerned, the reason is given by the idea of the physical continuum, drawn from the rough data of the senses. But this idea leads to a series of contradictions from each of which in turn we must be freed. In this way we are forced to imagine a more and more complicated system of symbols. That on which we shall dwell is not merely exempt from internal contradiction, — it was so already at all the steps we have taken, — but it is no longer in contradiction with the various propositions which are called intuitive, and which are derived from more or less elaborate empirical notions.

Measurable Magnitude. — So far we have not spoken of the *measure* of magnitudes; we can tell if any one of them is greater than any other, but we cannot say that it is two or three times as large.

So far, I have only considered the order in which the terms are arranged; but that is not sufficient for most applications. We must learn how to compare the interval which separates any two terms. On this condition alone will the continuum become measurable, and the operations of arithmetic be applicable. This can only be done by the aid of a new and special convention; and this convention is, that in such a case the interval between the terms A and B is equal to the interval which separates C and D. For instance, we started with the integers, and between two consecutive sets we intercalated n intermediary sets; by convention we now assume these new sets to be equidistant. This is one of the ways of defining the addition of two magnitudes; for if the interval AB is by definition equal to the interval CD, the interval AD will by definition be the sum of the intervals AB and AC. This definition is very largely, but not altogether, arbitrary. It must satisfy certain conditions — the commutative and associative laws of addition, for instance; but, provided the definition we choose satisfies these laws, the choice is indifferent, and we need not state it precisely.

Remarks. — We are now in a position to discuss several important questions.

(1) Is the creative power of the mind exhausted by the creation of the mathematical continuum? The answer is in the negative, and this is shown in a very striking manner by the work of Du Bois Reymond.

We know that mathematicians distinguish between infinitesimals of different orders, and that infinitesimals of the second order are infinitely small, not only absolutely so, but also in relation to those of the first order. It is not difficult to imagine infinitesimals of fractional or even of irrational order, and here once more we find the mathematical continuum which has been dealt with in the preceding

pages. Further, there are infinitesimals which are infinitely small with reference to those of the first order, and infinitely large with respect to the order $1 + \epsilon$, however small ϵ may be. Here, then, are new terms intercalated in our series; and if I may be permitted to revert to the terminology used in the preceding pages, a terminology which is very convenient, although it has not been consecrated by usage, I shall say that we have created a kind of continuum of the third order.

It is an easy matter to go further, but it is idle to do so, for we would only be imagining symbols without any possible application, and no one will dream of doing that. This continuum of the third order, to which we are led by the consideration of the different orders of infinitesimals, is in itself of but little use and hardly worth quoting. Geometers look on it as a mere curiosity. The mind only uses its creative faculty when experiment requires it.

(2) When we are once in possession of the conception of the mathematical continuum, are we protected from contradictions analogous to those which gave it birth? No, and the following is an instance: —

He is a *savant* indeed who will not take it as evident that every curve has a tangent; and, in fact, if we think of a curve and a straight line as two narrow bands, we can always arrange them in such a way that they have a common part without intersecting. Suppose now that the breadth of the bands diminishes indefinitely: the common part will still remain, and in the limit, so to speak, the two lines will have a common point, although they do not intersect — *i.e.,* they will touch. The geometer who reasons in this way is only doing what we have done when we proved that two lines which intersect have a common point, and his intuition might also seem to be quite legitimate. But this is not the case. We can show that there are curves which have no tangent, if we define such a curve as an analytical continuum of the second order. No doubt some artifice analogous to those we have discussed above would enable us to get rid of this contradiction, but as the latter is only met with in very exceptional cases, we need not trouble to do so. Instead of endeavoring to reconcile intuition and analysis, we are content to sacrifice one of them, and as analysis must be flawless, intuition must go to the wall.

The Physical Continuum of Several Dimensions. — We have discussed above the physical continuum as it is derived from the immediate evidence of our senses — , or, if the reader prefers, from the rough results of Fechner's experiments; I have shown that these results are summed up in the contradictory formulæ: — A = B, B = C, A > C.

Let us now see how this notion is generalized, and how from it may be derived the concept of continuums of several dimensions. Consider any two aggregates of sensations. We can either distinguish between

them, or we cannot; just as in Fechner's experiments the weight of 10 grammes could be distinguished from the weight of 12 grammes, but not from the weight of 11 grammes. This is all that is required to construct the continuum of several dimensions.

Let us call one of these aggregates of sensations an *element*. It will be in a measure analogous to the *point* of the mathematicians, but will not be, however, the same thing. We cannot say that our element has no size, for we cannot distinguish it from its immediate neighbors, and it is thus surrounded by a kind of fog. If the astronomical comparison may be allowed, our " elements " would be like nebulæ, whereas the mathematical points would be like stars.

If this be granted, a system of elements will form a continuum if we can pass from any one of them to any other by a series of consecutive elements such that each cannot be distinguished from its predecessor. This *linear* series is to the *line* of the mathematician what the isolated *element* was to the point.

Before going further, I must explain what is meant by a *cut*. Let us consider a continuum C, and remove from it certain of its elements, which for a moment we shall regard as no longer belonging to the continuum. We shall call the aggregate of elements thus removed a *cut*. By means of this cut, the continuum C will be *subdivided* into several distinct continuums; the aggregate of elements which remain will cease to form a single continuum. There will then be on C two elements, A and B, which we must look upon as belonging to two distinct continuums; and we see that this must be so, because it will be impossible to find a linear series of consecutive elements of C (each of the elements indistinguishable from the preceding, the first being A and the last B), *unless one of the elements of this series is indistinguishable from one of the elements of the cut.*

It may happen, on the contrary, that the cut may not be sufficient to subdivide the continuum C. To classify the physical continuums, we must first of all ascertain the nature of the cuts which must be made in order to subdivide them. If a physical continuum, C, may be subdivided by a cut reducing to a finite number of elements, all distinguishable the one from the other (and therefore forming neither one continuum nor several continuums), we shall call C a continuum *of one dimension*. If, on the contrary, C can only be subdivided by cuts which are themselves continuums, we shall say that C is of several dimensions; if the cuts are continuums of one dimension, then we shall say that C has two dimensions; if cuts of two dimensions are sufficient, we shall say that C is of three dimensions, and so on. Thus the notion of the physical continuum of several dimensions is defined, thanks to the very simple fact, that two aggregates of sensations may be distinguishable or indistinguishable.

The Mathematical Continuum of Several Dimensions. — The con-

ception of the mathematical continuum of n dimensions may be led up to quite naturally by a process similar to that which we discussed at the beginning of this chapter. A point of such a continuum is defined by a system of n distinct magnitudes which we call its co-ordinates.

The magnitudes need not always be measurable; there is, for instance, one branch of geometry independent of the measure of magnitudes, in which we are only concerned with knowing, for example, if, on a curve A B C, the point B is between the points A and C, and in which it is immaterial whether the arc A B is equal to or twice the arc B C. This branch is called *Analysis Situs*. It contains quite a large body of doctrine which has attracted the attention of the greatest geometers, and from which are derived, one from another, a whole series of remarkable theorems. What distinguishes these theorems from those of ordinary geometry is that they are purely qualitative. They are still true if the figures are copied by an unskilful draughtsman, with the result that the proportions are distorted and the straight lines replaced by lines which are more or less curved.

As soon as measurement is introduced into the continuum we have just defined, the continuum becomes space, and geometry is born. But the discussion of this is reserved for Part II.

Non-Euclidean Geometries

Every conclusion presumes premisses. These premisses are either self-evident and need no demonstration, or can be established only if based on other propositions; and, as we cannot go back in this way to infinity, every deductive science, and geometry in particular, must rest upon a certain number of indemonstrable axioms. All treatises of geometry begin therefore with the enunciation of these axioms. But there is a distinction to be drawn between them. Some of these, for example, "Things which are equal to the same thing are equal to one another," are not propositions in geometry but propositions in analysis. I look upon them as analytical *à priori* intuitions, and they concern me no further. But I must insist on other axioms which are special to geometry. Of these most treatises explicitly enunciate three: — (1) Only one line can pass through two points; (2) a straight line is the shortest distance between two points; (3) through one point only one parallel can be drawn to a given straight line. Although we generally dispense with proving the second of these axioms, it would be possible to deduce it from the other two, and from those much more numerous axioms which are implicitly admitted without enunciation, as I shall explain further on. For a long time a proof of the third axiom known as Euclid's postulate was sought in vain. It is impossible to imagine the efforts that have been spent in pursuit of this chimera. Finally, at the beginning of the nineteenth century, and almost simultaneously, two scientists, a Russian and a Bulgarian, Lobatschewsky and Bolyai, showed irrefutably that this proof is impossible. They have nearly rid us of inventors of geometries without a postulate, and ever since the Académie des Sciences receives only about one or two new dmonstrations a year. But the question was not exhausted, and it was not long before a great step was taken by the celebrated memoir of Riemann, entitled: *Ueber die Hypothesen welche der Geometrie zum Grunde liegen.* This little work has inspired most of the recent treatises to which I shall later on refer, and among which I may mention those of Beltrami and Helmholtz.

The Geometry of Lobatschewsky. — If it were possible to deduce

Euclid's postulate from the several axioms, it is evident that by reject-
ing the postulate and retaining the other axioms we should be led to
contradictory consequences.   It would be, therefore, impossible to
found on those premisses a coherent geometry.   Now, this is pre-
cisely what Lobatschewsky has done.   He assumes at the outset that
several parallels may be drawn through a point to a given straight
line, and he retains all the other axioms of Euclid.   From these
hypotheses he deduces a series of theorems between which it is im-
possible to find any contradiction, and he constructs a geometry as
impeccable in its logic as Euclidean geometry.   The theorems are very
different, however, from those to which we are accustomed, and at first
will be found a little disconcerting.   For instance, the sum of the
angles of a triangle is always less than two right angles, and the differ-
ence between that sum and two right angles is proportional to the
area of the triangle.   It is impossible to construct a figure similar to a
given figure but of different dimensions.   If the circumference of a
circle be divided into n equal parts, and tangents be drawn at the
points of intersection, the n tangents will form a polygon if the
radius of the circle is small enough, but if the radius is large enough
they will never meet.   We need not multiply these examples.   Lobat-
schewsky's propositions have no relation to those of Euclid, but they
are none the less logically interconnected.

Riemann's Geometry. — Let us imagine to ourselves a world only
peopled with beings of no thickness, and suppose these " infinitely
flat " animals are all in one and the same plane, from which they
cannot emerge.   Let us further admit that this world is sufficiently
distant from other worlds to be withdrawn from their influence, and
while we are making these hypotheses it will not cost us much to en-
dow these beings with reasoning power, and to believe them capable
of making a geometry.   In that case they will certainly attribute to
space only two dimensions.   But now suppose that these imaginary
animals, while remaining without thickness, have the form of a spher-
ical, and not of a plane figure, and are all on the same sphere, from
which they cannot escape.   What kind of a geometry will they con-
struct?   In the first place, it is clear that they will attribute to
space only two dimensions.   The straight line to them will be the
shortest distance from one point on the sphere to another — that is
to say, an arc of a great circle.   In a word, their geometry will be
spherical geometry.   What they will call space will be the sphere on
which they are confined, and on which take place all the phenomena
with which they are acquainted.   Their space will therefore be
unbounded, since on a sphere one may always walk forward without
ever being brought to a stop, and yet it will be *finite;* the end will
never be found, but the complete tour can be made.   Well, Riemann's
geometry is spherical geometry extended to three dimensions.   **To**

construct it, the German mathematician had first of all to throw over-
board, not only Euclid's postulate, but also the first axiom that *only
one line can pass through two points*. On a sphere, through two given
points, we can *in general* draw only one great circle which, as we have
just seen, would be to our imaginary beings a straight line. But
there was one exception. If the two given points are at the ends of
a diameter, an infinite number of great circles can be drawn through
them. In the same way, in Riemann's geometry — at least in one of
its forms — through two points only one straight line can in general
be drawn, but there are exceptional cases in which through two
points an infinite number of straight lines can be drawn. So there
is a kind of opposition between the geometries of Riemann and Lo-
batschewsky. For instance, the sum of the angles of a triangle is
equal to two right angles in Euclid's geometry, less than two right
angles in that of Lobatschewsky, and greater than two right angles
in that of Riemann. The number of parallel lines that can be drawn
through a given point to a given line is one in Euclid's geometry, none
in Riemann's, and an infinite number in the geometry of Lobatschew-
sky. Let us add that Riemann's space is finite, although unbounded
in the sense which we have above attached to these words.

Surfaces with Constant Curvature. — One objection, however, re-
mains possible. There is no contradiction between the theorems
of Lobatschewsky and Riemann; but however numerous are the other
consequences that these geometers have deduced from their hypothe-
ses, they had to arrest their course before they exhausted them all,
for the number would be infinite; and who can say that if they had
carried their deductions further they would not have eventually
reached some contradiction? This difficulty does not exist for Rie-
mann's geometry, provided it is limited to two dimensions. As we
have seen, the two-dimensional geometry of Riemann, in fact, does
not differ from spherical geometry, which is only a branch of ordinary
geometry, and is therefore outside all contradiction. Beltrami, by
showing that Lobatschewsky's two-dimensional geometry was only a
branch of ordinary geometry, has equally refuted the objection as
far as it is concerned. This is the course of his argument: Let us
consider any figure whatever on a surface. Imagine this figure to be
traced on a flexible and inextensible canvas applied to the surface, in
such a way that when the canvas is displaced and deformed the differ-
ent lines of the figure change their form without changing their
length. As a rule, this flexible and inextensible figure cannot be
displaced without leaving the surface. But there are certain surfaces
for which such a movement would be possible. They are surfaces of
constant curvature. If we resume the comparison that we made
just now, and imagine beings without thickness living on one of these
surfaces, they will regard as possible the motion of a figure all the

lines of which remain of a constant length. Such a movement would appear absurd, on the other hand, to animals without thickness living on a surface of variable curvature. These surfaces of constant curvature are of two kinds. The curvature of some is *positive,* and they may be deformed so as to be applied to a sphere. The geometry of these surfaces is therefore reduced to spherical geometry — namely, Riemann's. The curvature of others is *negative.* Beltrami has shown that the geometry of these surfaces is identical with that of Lobatschewsky. Thus the two-dimensional geometries of Riemann and Lobatschewsky are connected with Euclidean geometry.

Interpretation of Non-Euclidean Geometries. — Thus vanishes the objection so far as two-dimensional geometries are concerned. It would be easy to extend Beltrami's reasoning to three dimensional geometries, and minds which do not recoil before space of four dimensions will see no difficulty in it; but such minds are few in number. I prefer, then, to proceed otherwise. Let us consider a certain plane, which I shall call the fundamental plane, and let us construct a kind of dictionary by making a double series of terms written in two columns, and corresponding each to each, just as in ordinary dictionaries the words in two languages which have the same signification correspond to one another: —

Space ...   .   The portion of space situated above the fundamental plane.

Plane ...   .   Sphere cutting orthogonally the fundamental plane.

Line   ...   .   Circle cutting orthogonally the fundamental plane.

Sphere...   ...   ...   Sphere.

Circle ...   .   Circle.

Angle ...   .   Angle.

Distance   between
    two   points ......   Logarithm of the anharmonic ratio of these two points and of the intersection of the fundamental plane with the circle passing through these two points and cutting it orthogonally.

Etc.   Etc.

Let us now take Lobatschewsky's theorems and translate them by the aid of this dictionary, as we would translate a German text with the aid of a German-French dictionary. *We shall then obtain the theorems of ordinary geometry.* For instance. Lobatschewsky's theorem: "The sum of the angles of a triangle is less than two right angles," may be translated thus: "If a curvilinear triangle has for its sides arcs of circles which if produced would cut orthogonally the fundamental plane, the sum of the angles of this curvilinear triangle

will be less than two right angles." Thus, however far the consequences of Lobatschewsky's hypotheses are carried, they will never lead to a contradiction; in fact, if two of Lobatschewsky's theorems were contradictory, the translations of these two theorems made by the aid of our dictionary would be contradictory also. But these translations are theorems of ordinary geometry, and no one doubts that ordinary geometry is exempt from contradiction. Whence is the certainty derived, and how far is it justified? That is a question upon which I cannot enter here, but it is a very interesting question, and I think not insoluble. Nothing, therefore, is left of the objection I formulated above. But this is not all. Lobatschewsky's geometry being susceptible of a concrete interpretation, ceases to be a useless logical exercise, and may be applied. I have no time here to deal with these applications, nor with what Herr Klein and myself have done by using them in the integration of linear equations. Further, this interpretation is not unique, and several dictionaries may be constructed analogous to that above, which will enable us by a simple translation to convert Lobatschewsky's theorems into the theorems of ordinary geometry.

Implicit Axioms. — Are the axioms implicitly enunciated in our text-books the only foundation of geometry? We may be assured of the contrary when we see that, when they are abandoned one after another, there are still left standing some propositions which are common to the geometries of Euclid, Lobatschewsky, and Riemann. These propositions must be based on premises that geometers admit without enunciation. It is interesting to try and extract them from the classical proofs.

John Stuart Mill asserted [1] that every definition contains an axiom, because by defining we implicitly affirm the existence of the object defined. That is going rather too far. It is but rarely in mathematics that a definition is given without following it up by the proof of the existence of the object defined, and when this is not done it is generally because the reader can easily supply it; and it must not be forgotten that the word " existence " has not the same meaning when it refers to a mathematical entity as when it refers to a material object.

A mathematical entity exists provided there is no contradiction implied in its definition, either in itself, or with the propositions previously admitted. But if the observation of John Stuart Mill cannot be applied to all definitions, it is none the less true for some of them. A plane is sometimes defined in the following manner : — The plane is a surface such that the line which joins any two points upon it lies wholly on that surface. Now, there is obviously a new axiom concealed in this definition. It is true we might change it, and that

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[1] *Logic*, c. viii., cf. Definitions, § 5·6.—TR.

would be preferable, but then we should have to enunciate the axiom explicitly. Other definitions may give rise to no less important reflections, such as, for example, that of the equality of two figures. Two figures are equal when they can be superposed. To superpose them, one of them must be displaced until it coincides with the other. But how must it be displaced? If we asked that question, no doubt we should be told that it ought to be done without deforming it, and as an invariable solid is displaced. The vicious circle would then be evident. As a matter of fact, this definition defines nothing. It has no meaning to a being living in a world in which there are only fluids. If it seems clear to us, it is because we are accustomed to the properties of natural solids which do not much differ from those of the ideal solids, all of whose dimensions are invariable. However, imperfect as it may be, this definition implies an axiom. The possibility of the motion of an invariable figure is not a self-evident truth. At least it is only so in the application to Euclid's postulate, and not as an analytical à priori intuition would be. Moreover, when we study the definitions and the proofs of geometry, we see that we are compelled to admit without proof not only the possibility of this motion, but also some of its properties. This first arises in the definition of the straight line. Many defective definitions have been given, but the true one is that which is understood in all the proofs in which the straight line intervenes. "It may happen that the motion of an invariable figure may be such that all the points of a line belonging to the figure are motionless, while all the points situate outside that line are in motion. Such a line would be called a straight line." We have deliberately in this enunciation separated the definition from the axiom which it implies. Many proofs such as those of the cases of the equality of triangles, of the possibility of drawing a perpendicular from a point to a straight line, assume propositions the enunciations of which are dispensed with, for they necessarily imply that it is possible to move a figure in space in a certain way.

The Fourth Geometry. — Among these explicit axioms there is one which seems to me to deserve some attention, because when we abandon it we can construct a fourth geometry as coherent as those of Euclid, Lobatschewsky, and Riemann. To prove that we can always draw a perpendicular at a point A to a straight line A B, we consider a straight line A C movable about the point A, and initially identical with the fixed straight line A B. We then can make it turn about the point A until it lies in A B produced. Thus we assume two propositions — first, that such a rotation is possible, and then that it may continue until the two lines lie the one in the other produced. If the first point is conceded and the second rejected, we are led to a series of theorems even stranger than those of Lobatschewsky and Riemann, but equally free from contradiction. I shall give only one

of these theorems, and I shall not choose the least remarkable of them. *A real straight line may be perpendicular to itself.*

Lie's Theorem. — The number of axioms implicity introduced into classical proofs is greater than necessary, and it would be interesting to reduce them to a minimum. It may be asked, in the first place, if this reduction is possible — if the number of necessary axioms and that of imaginable geometries is not infinite? A theorem due to Sophus Lie is of weighty importance in this discussion. It may be enunciated in the following manner: — Suppose the following premisses are admitted: (1) space has n dimensions; (2) the movement of an invariable figure is possible; (3) p conditions are necessary to determine the position of this figure in space.

The number of geometries compatible with these premisses will be limited. I may even add that if n is given, a superior limit can be assigned to p. If, therefore, the possibility of the movement is granted, we can only invent a finite and even a rather restricted number of three-dimensional geometries.

Riemann's Geometries. — However, this result seems contradicted by Riemann, for that scientist constructs an infinite number of geometries, and that to which his name is usually attached is only a particular case of them. All depends, he says, on the manner in which the length of a curve is defined. Now, there is an infinite number of ways of defining this length, and each of them may be the starting-point of a new geometry. That is perfectly true, but most of these definitions are incompatible with the movement of a variable figure such as we assume to be possible in Lie's theorem. These geometries of Riemann, so interesting on various grounds, can never be, therefore, purely analytical, and would not lend themselves to proofs analogous to those of Euclid.

On the Nature of Axioms. — Most mathematicians regard Lobatschewsky's geometry as a mere logical curiosity. Some of them have, however, gone further. If several geometries are possible, they say, is it certain that our geometry is the one that is true? Experiment no doubt teaches us that the sum of the angles of a triangle is equal to two right angles, but this is because the triangles we deal with are too small. According to Lobatschewsky, the difference is proportional to the area of the triangle, and will not this become sensible when we operate on much larger triangles, and when our measurements become more accurate? Euclid's geometry would thus be a provisory geometry. Now, to discuss this view we must first of all ask ourselves, what is the nature of geometrical axioms? Are they synthetic *à priori* intuitions, as Kant affirmed? They would then be imposed upon us with such a force that we could not conceive of the contrary proposition, nor could we build upon it a theoretical edifice. There would be no non-Euclidean geometry. To convince ourselves of this,

let us take a true synthetic *à priori* intuition — the following, for
instance, which played an important part in the first chapter : — **If a
theorem is true for the number 1, and if it has been proved that it is
true of *n*+1, provided it is true of *n*,** it will be true for all positive
integers. Let us next try to get rid of this, and while rejecting this
proposition let us construct a false arithmetic analogous to non-
Euclidean geometry. We shall not be able to do it. We shall be
even tempted at the outset to look upon these intuitions as analytical.
Besides, to take up again our fiction of animals without thickness, we
can scarcely admit that these beings, if their minds are like ours,
would adopt the Euclidean geometry, which would be contradicted by
all their experience. Ought we, then, to conclude that the axioms of
geometry are experimental truths? But we do not make experiments
on ideal lines or ideal circles; we can only make them on material
objects. On what, therefore, would experiments serving as a founda-
tion for geometry be based? The answer is easy. We have seen above
that we constantly reason as if the geometrical figures behaved like
solids. What geometry would borrow from experiment would be
therefore the properties of these bodies. The properties of light and
its propagation in a straight line have also given rise to some of the
propositions of geometry, and in particular to those of projective
geometry, so that from that point of view one would be tempted to
say that metrical geometry is the study of solids, and projective
geometry that of light. But a difficulty remains, and is unsurmount-
able. If geometry were an experimental science, it would not be
an exact science. It would be subjected to continual revision. Nay,
it would from that day forth be proved to be erroneous, for we
know that no rigorously invariable solid exists. *The geometrical
axioms are therefore neither synthetic à priori intuitions nor experi-
mental facts.* They are conventions. Our choice among all possible
conventions is *guided* by experimental facts; but it remains *free,* and
is only limited by the necessity of avoiding every contradiction, and
thus it is that postulates may remain rigorously true even when the
experimental laws which have determined their adoption are only ap-
proximate. In other words, *the axioms of geometry* (I do not speak of
those of arithmetic) *are only definitions in disguise.* What, then, are
we to think of the question : Is Euclidean geometry true? It has
no meaning. We might as well ask if the metric system is true,
and if the old weights and measures are false; if Cartesian co-ordinates
are true and polar co-ordinates false. One geometry cannot be more
true than another; it can only be more convenient. Now Euclidean
geometry is, and will remain, the most convenient: 1st, because it is
the simplest, and it is not so only because of our mental habits or
because of the kind of direct intuition that we have of Euclidean
space; it is the simplest in itself, just as a polynomial of the first de-

gree is simpler than a polynomial of the second degree; 2nd, because it sufficiently agrees with the properties of natural solids, those bodies which we can compare and measure by means of our senses.

Space and Geometry

Let us begin with a little paradox. Beings whose minds were made as ours, and with senses like ours, but without any preliminary education, might receive from a suitably-chosen external world impressions which would lead them to construct a geometry other than that of Euclid, and to localize the phenomena of this external world in a non-Euclidean space, or even in space of four dimensions. As for us, whose education has been made by our actual world, if we were suddenly transported into this new world, we should have no difficulty in referring phenomena to our Euclidean space. Perhaps somebody may appear on the scene some day who will devote his life to it, and be able to represent to himself the fourth dimension.

Geometrical Space and Representative Space.— It is often said that the images we form of external objects are localized in space, and even that they can only be formed on this condition. It is also said that this space, which thus serves as a kind of framework ready prepared for our sensations and representations, is identical with the space of the geometers, having all the properties of that space. To all clear-headed men who think in this way, the preceding statement might well appear extraordinary; but it is as well to see if they are not the victims of some illusion which closer analysis may be able to dissipate. In the first place, what are the properties of space properly so called? I mean of that space which is the object of geometry, and which I shall call geometrical space. The following are some of the more essential :—

1st, it is continuous; 2nd, it is infinite; 3rd, it is of three dimensions; 4th, it is homogeneous — that is to say, all its points are identical one with another; 5th, it is isotropic. Compare this now with the framework of our representations and sensations, which I may call *representative space.*

Visual Space. — First of all let us consider a purely visual impression, due to an image formed on the back of the retina. A cursory analysis shows us this image as continuous, but as possessing only two dimensions, which already distinguishes purely visual from what may be called geometrical space. On the other hand, the image is enclosed within a limited framework; and there is a no less important difference: *this pure visual space is not homogeneous.* All the points on the retina, apart from the images which may be formed, do not play the same rôle. The yellow spot can in no way be regarded as identical with a point on the edge of the retina. Not only does the

same object produce on it much brighter impressions, but in the whole of the *limited* framework the point which occupies the centre will not appear identical with a point near one of the edges. Closer analysis no doubt would show us that this continuity of visual space and its two dimensions are but an illusion. It would make visual space even more different than before from geometrical space, but we may treat this remark as incidental.

However, sight enables us to appreciate distance, and therefore to perceive a third dimension. But every one knows that this perception of the third dimension reduces to a sense of the effort of accommodation which must be made, and to a sense of the convergence of the two eyes, that must take place in order to perceive an object distinctly. These are muscular sensations quite different from the visual sensations which have given us the concept of the two first dimensions. The third dimension will therefore not appear to us as playing the same rôle as the two others. What may be called *complete visual space* is not therefore an isotropic space. It has, it is true, exactly three dimensions; which means that the elements of our visual sensations (those at least which concur in forming the concept of extension) will be completely defined if we know three of them; or, in mathematical language, they will be functions of three independent variables. But let us look at the matter a little closer. The third dimension is revealed to us in two different ways: by the effort of accommodation, and by the convergence of the eyes. No doubt these two indications are always in harmony; there is between them a constant relation; or, in mathematical language, the two variables which measure these two muscular sensations do not appear to us as independent. Or, again, to avoid an appeal to mathematical ideas which are already rather too refined, we may go back to the language of the preceding chapter and enunciate the same fact as follows: — If two sensations of convergence A and B are indistinguishable, the two sensations of accommodation A' and B' which accompany them respectively will also be indistinguishable. But that is, so to speak, an experimental fact. Nothing prevents us *à priori* from assuming the contrary, and if the contrary takes place, if these two muscular sensations both vary independently, we must take into account one more independent variable, and complete visual space will appear to us as a physical continuum of four dimensions. And so in this there is also a fact of *external* experiment. Nothing prevents us from assuming that a being with a mind like ours, with the same sense-organs as ourselves, may be placed in a world in which light would only reach him after being passed through refracting media of complicated form. The two indications which enable us to appreciate distances would cease to be connected by a constant relation. A being educating his senses in such a

world would no doubt attribute four dimensions to complete visual space.

Tactile and Motor Space. — " Tactile space " is more complicated still than visual space, and differs even more widely from geometrical space.   It is useless to repeat for the sense of touch my remarks on the sense of sight.   But outside the data of sight and touch there are other sensations which contribute as much and more than they do to the genesis of the concept of space.   They are those which everybody knows, which accompany all our movements, and which we usually call muscular sensations.   The corresponding framework constitutes what may be called *motor space.*   Each muscle gives rise to a special sensation which may be increased or diminished so that the aggregate of our muscular sensations will depend upon as many variables as we have muscles.   From this point of view *motor space would have as many dimensions as we have muscles.*   I know that it is said that if the muscular sensations contribute to form the concept of space, it is because we have the sense of the *direction* of each movement, and that this is an integral part of the sensation.   If this were so, and if a muscular sense could not be aroused unless it were accompanied by this geometrical sense of direction, geometrical space would certainly be a form imposed upon our sensitiveness.   But I do not see this at all when I analyze my sensations.   What I do see is that the sensations which correspond to movements in the same direction are connected in my mind by a simple *association of ideas.*   It is to this association that what we call the sense of direction is reduced.   We cannot therefore discover this sense in a single sensation.   This association is extremely complex, for the contraction of the same muscle may correspond, according to the position of the limbs, to very different movements of direction.   Moreover, it is evidently acquired; it is like all associations of ideas, the result of a *habit.*   This habit itself is the result of a very large number of *experiments,* and no doubt if the education of our senses had taken place in a different medium, where we would have been subjected to different impressions, then contrary habits would have been acquired, and our muscular sensations would have been associated according to other laws.

Characteristics of Representative Space. — Thus representative space in its triple form — visual, tactile, and motor — differs essentially from geometrical space.   It is neither homogeneous nor isotropic; we cannot even say that it is of three dimensions.   It is often said that we " project " into geometrical space the objects of our external perception; that we " localize " them.   Now, has that any meaning, and if so what is that meaning?   Does it mean that we *represent* to ourselves external objects in geometrical space?   Our representations are only the reproduction of our sensations; they cannot therefore be arranged in the same framework — that is to say, in

representative space. It is also just as impossible for us to represent to ourselves external objects in geometrical space, as it is impossible for a painter to paint on a flat surface objects with their three dimensions. Representative space is only an image of geometrical space, an image deformed by a kind of perspective, and we can only represent to ourselves objects by making them obey the laws of this perspective. Thus we do not *represent* to ourselves external bodies in geometrical space, but we *reason* about these bodies as if they were situated in geometrical space. When it is said, on the other hand, that we "localize" such an object in such a point of space, what does it mean? *It simply means that we represent to ourselves the movements that must take place to reach that object.* And it does not mean that to represent to ourselves these movements they must be projected into space, and that the concept of space must therefore pre-exist. When I say that we represent to ourselves these movements, I only mean that we represent to ourselves the muscular sensations which accompany them, and which have no geometrical character, and which therefore in no way imply the pre-existence of the concept of space.

Changes of State and Changes of Position. — But, it may be said, if the concept of geometrical space is not imposed upon our minds, and if, on the other hand, none of our sensations can furnish us with that concept, how then did it ever come into existence? This is what we have now to examine, and it will take some time; but I can sum up in a few words the attempt at explanation which I am going to develop. *None of our sensations, if isolated, could have brought us to the concept of space; we are brought to it solely by studying the laws by which those sensations succeed one another.* We see at first that our impressions are subject to change; but among the changes that we ascertain, we are very soon led to make a distinction. Sometimes we say that the objects, the causes of these impressions, have changed their state, sometimes that they have changed their position, that they have only been displaced. Whether an object changes its state or only its position, this is always translated for us in the same manner, *by a modification in an aggregate of impressions.* How then have we been enabled to distinguish them? If there were only change of position, we could restore the primitive aggregate of impressions by making movements which would confront us with the movable object in the same *relative* situation. We thus *correct* the modification which was produced, and we re-establish the initial state by an inverse modification. If, for example, it were a question of the sight, and if an object be displaced before our eyes, we can "follow it with the eye," and retain its image on the same point of the retina by appropriate movements of the eyeball. These movements we are conscious of because they are voluntary, and because they are accompanied by muscular sensations. But that does not mean that we

represent them to ourselves in geometrical space. So what characterizes change of position, what distinguishes it from change of state, is that it can always be *corrected* by this means. It may therefore happen that we pass from the aggregate of impressions A to the aggregate B in two different ways. First, involuntarily and without experiencing muscular sensations — which happens when it is the object that is displaced; secondly, voluntarily, and with muscular sensation — which happens when the object is motionless, but when we displace ourselves in such a way that the object has relative motion with respect to us. If this be so, the translation of the aggregate A to the aggregate B is only a change of position. It follows that sight and touch could not have given us the idea of space without the help of the " muscular sense." Not only could this concept not be derived from a single sensation, or even from *a series of sensations;* but a *motionless* being could never have acquired it, because, not being able to correct by his movements the effects of the change of position of external objects, he would have had no reason to distinguish them from changes of state. Nor would he have been able to acquire it if his movements had not been voluntary, or if they were unaccompanied by any sensations whatever.

Conditions of Compensation.—How is such a compensation possible in such a way that two changes, otherwise mutually independent, may be reciprocally corrected? A mind *already familiar with geometry* would reason as follows : — If there is to be compensation, the different parts of the external object on the one hand, and the different organs of our senses on the other, must be in the same *relative* position after the double change. And for that to be the case, the different parts of the external body on the one hand, and the different organs of our senses on the other, must have the same relative position to each other after the double change; and so with the different parts of our body with respect to each other. In other words, the external object in the first change must be displaced as an invariable solid would be displaced, and it must also be so with the whole of our body in the second change, which is to correct the first. Under these conditions compensation may be produced. But we who as yet know nothing of geometry, whose ideas of space are not yet formed, we cannot reason in this way — we cannot predict *à priori* if compensation is possible. But experiment shows us that it sometimes does take place, and we start from this experimental fact in order to distinguish changes of state from changes of position.

Solid Bodies and Geometry. — Among surrounding objects there are some which frequently experience displacements that may be thus corrected by a *correlative* movement of our own body — namely, *solid bodies.* The other objects, whose form is variable, only in exceptional circumstances undergo similar displacement (change of position without change of form). When the displacement of a body takes place

with deformation, we can no longer by appropriate movements place the organs of our body in the same *relative* situation with respect to this body; we can no longer, therefore, reconstruct the primitive aggregate of impressions.

It is only later, and after a series of new experiments, that we learn how to decompose a body of variable form into smaller elements such that each is displaced approximately according to the same laws as solid bodies. We thus distinguish " deformations " from other changes of state. In these deformations each element undergoes a simple change of position which may be corrected; but the modification of the aggregate is more profound, and can no longer be corrected by a correlative movement. Such a concept is very complex even at this stage, and has been relatively slow in its appearance. It would not have been conceived at all had not the observation of solid bodies shown us beforehand how to distinguish changes of position.

If, then, there were no solid bodies in nature there would be no geometry.

Another remark deserves a moment's attention. Suppose a solid body to occupy successively the positions a and β; in the first position it will give us an aggregate of impressions A, and in the second position the aggregate of impressions B. Now let there be a second solid body, of qualities entirely different from the first — of different color, for instance. Assume it to pass from the position a, where it gives us the aggregate of impressions A' to the position β, where it gives the aggregate of impressions B'. In general, the aggregate A will have nothing in common with the aggregate A', nor will the aggregate B have anything in common with the aggregate B'. The transition from the aggregate A to the aggregate B, and that of the aggregate A' to the aggregate B', are therefore two changes which *in themselves* have in general nothing in common. Yet we consider both these changes as displacements; and, further, we consider them the *same* displacement. How can this be? It is simply because they may be both corrected by the *same* correlative movement of our body. " Correlative movement," therefore, constitutes the *sole connection* between two phenomena which otherwise we should never have dreamed of connecting.

On the other hand, our body, thanks to the number of its articulations and muscles, may have a multitude of different movements, but all are not capable of " correcting " a modification of external objects; those alone are capable of it in which our whole body, or at least all those in which the organs of our senses enter into play are displaced *en bloc* — *i.e.,* without any variation of their relative positions, as in the case of a solid body.

To sum up:

1. In the first place, we distinguish two categories of phenomena: —

The first involuntary, unaccompanied by muscular sensations, and attributed to external objects — they are external changes; the second, of opposite character and attributed to the movements of our own body, are internal changes.

2. We notice that certain changes of each in these categories may be corrected by a correlative change of the other category.

3. We distinguish among external changes those that have a correlative in the other category — which we call displacements; and in the same way we distinguish among the internal changes those which have a correlative in the first category.

Thus by means of this reciprocity is defined a particular class of phenomena called displacements. *The laws of these phenomena are the object of geometry.*

Law of Homogeneity. — The first of these laws is the law of homogeneity. Suppose that by an external change we pass from the aggregate of impressions A to the aggregate B, and that then this change a is corrected by a correlative voluntary movement β, so that we are brought back to the aggregate A. Suppose now that another external change a' brings us again from the aggregate A to the aggregate B. Experiment then shows us that this change a', like the change a, may be corrected by a voluntary correlative movement β', and that this Momevent β' corresponds to the same muscular sensations as the movement β which corrected a.

This fact is usually enunciated as follows: — *Space is homogeneous and isotropic.* We may also say that a movement which is once produced may be repeated a second and a third time, and so on, without any variation of its properties. In the first chapter, in which we discussed the nature of mathematical reasoning, we saw the importance that should be attached to the possibility of repeating the same operation indefinitely. The virtue of mathematical reasoning is due to this repetition; by means of the law of homogeneity geometrical facts are apprehended. To be complete, to the law of homogeneity must be added a multitude of other laws, into the details of which I do not propose to enter, but which mathematicians sum up by saying that these displacements form a " group."

The Non-Euclidean World. — If geometrical space were a framework imposed on *each* of our representations considered individually, it would be impossible to represent to ourselves an image without this framework, and we should be quite unable to change our geometry. But this is not the case; geometry is only the summary of the laws by which these images succeed each other. There is nothing, therefore, to prevent us from imagining a series of representations, similar in every way to our ordinary representations, but succeeding one another according to laws which differ from those to which we are accustomed. We may thus conceive that beings whose education has

taken place in a medium in which those laws would be so different, might have a very different geometry from ours.

Suppose, for example, a world enclosed in a large sphere and subject to the following laws: — The temperature is not uniform; it is greatest at the centre, and gradually decreases as we move towards the circumference of the sphere, where it is absolute zero. The law of this temperature is as follows: — If R be the radius of the sphere, and r the distance of the point considered from the centre, the absolute temperature will be proportional to $R^2 - r^2$. Further, I shall suppose that in this world all bodies have the same co-efficient of dilatation, so that the linear dilatation of any body is proportional to its absolute temperature. Finally, I shall assume that a body transported from one point to another of different temperature is instantaneously in thermal equilibrium with its new environment. There is nothing in these hypotheses either contradictory or unimaginable. A moving object will become smaller and smaller as it approaches the circumference of the sphere. Let us observe, in the first place, that although from the point of view of our ordinary geometry this world is finite, to its inhabitants it will appear infinite. As they approach the surface of the sphere they become colder, and at the same time smaller and smaller. The steps they take are therefore also smaller and smaller, so that they can never reach the boundary of the sphere. If to us geometry is only the study of the laws according to which invariable solids move, to these imaginary beings it will be the study of the laws of motion of solids *deformed by the differences of temperature* alluded to.

No doubt, in our world, natural solids also experience variations of form and volume due to differences of temperature. But in laying the foundations of geometry we neglect these variations; for besides being but small they are irregular, and consequently appear to us to be accidental. In our hypothetical world this will no longer be the case, the variations will obey very simple and regular laws. On the other hand, the different solid parts of which the bodies of these inhabitants are composed will undergo the same variations of form and volume.

Let me make another hypothesis: suppose that light passes through media of different refractive indices, such that the index of refraction is inversely proportional to $R^2 - r^2$. Under these conditions it is clear that the rays of light will no longer be rectilinear but circular. To justify what has been said, we have to prove that certain changes in the position of external objects may be corrected by correlative movements of the beings which inhabit this imaginary world; and in such a way as to restore the primitive aggregate of the impressions experienced by these sentient beings. Suppose, for example, that an object is displaced and deformed, not like an invariable solid, but like

a solid subjected to unequal dilatations in exact conformity with the law of temperature assumed above. To use an abbreviation, we shall call such a movement a non-Euclidean displacement.

If a sentient being be in the neighborhood of such a displacement of the object, his impressions will be modified; but by moving in a suitable manner, he may reconstruct them. For this purpose, all that is required is that the aggregate of the sentient being and the object, considered as forming a single body, shall experience one of those special displacements which I have just called non-Euclidean. This is possible if we suppose that the limbs of these beings dilate according to the same laws as the other bodies of the world they inhabit.

Although from the point of view of our ordinary geometry there is a deformation of the bodies in this displacement, and although their different parts are no longer in the same relative position, nevertheless we shall see that the impressions of the sentient being remain the same as before; in fact, though the mutual distances of the different parts have varied, yet the parts which at first were in contact are still in contact. It follows that tactile impressions will be unchanged. On the other hand, from the hypothesis as to refraction and the curvature of the rays of light, visual impressions will also be unchanged. These imaginary beings will therefore be led to classify the phenomena they observe, and to distinguish among them the "changes of position," which may be corrected by a voluntary correlative movement, just as we do.

If they construct a geometry, it will not be like ours, which is the study of the movements of our invariable solids; it will be the study of the changes of position which they will have thus distinguished, and will be " non-Euclidean displacements," and *this will be non-Euclidean geometry*. So that beings like ourselves, educated in such a world, will not have the same geometry as ours.

The World of Four Dimensions. — Just as we have pictured to ourselves a non-Euclidean world, so we may picture a world of four dimensions.

The sense of light, even with one eye, together with the muscular sensations relative to the movements of the eyeball, will suffice to enable us to conceive of space of three dimensions. The images of external objects are painted on the retina, which is a plane of two dimensions; these are *perspectives*. But as eye and objects are movable, we see in succession different perspectives of the same body taken from different points of view. We find at the same time that the transition from one perspective to another is often accompanied by muscular sensations. If the transition from the perspective A to the perspective B, and that of the perspective A′ to the perspective B′ are accompanied by the same muscular sensations, we connect them as we do other operations of the same nature. Then when

we study the laws according to which these operations are combined
we see that they form a group, which has the same structure as that
of the movements of invariable solids. Now, we have seen that it is
from the properties of this group that we derive the idea of geomet-
rical space and that of three dimensions. We thus understand how
these perspectives gave rise to the conception of three dimensions,
although each perspective is of only two dimensions, — because *they
succeed each other according to certain laws.* Well, in the same way
that we draw the perspective of a three-dimensional figure on a plane,
so we can draw that of a four-dimensional figure on a canvas of three
(or two) dimensions. To a geometer this is but child's play. We can
even draw several perspectives of the same figure from several different
points of view. We can easily represent to ourselves these perspective,
since they are of only three dimensions. Imagine that the different
perspectives of one and the same object occur in succession, and
that the transition from one to the other is accompanied by muscular
sensations. It is understood that we shall consider two of these tran-
sitions as two operations of the same nature when they are associated
with the same muscular sensations. There is nothing, then, to prevent
us from imagining that these operations are combined according to
any law we choose — for instance, by forming a group with the same
structure as that of the movements of an invariable four-dimensional
solid. In this there is nothing that we cannot represent to ourselves,
and, moreover, these sensations are those which a being would experi-
ence who has a retina of two dimensions, and who may be displaced
in space of four dimensions. In this sense we may say that we can
represent to ourselves the fourth dimension.

Conclusions. — It is seen that experiment plays a considerable rôle
in the genesis of geometry; but it would be a mistake to conclude from
that that geometry is, even in part, an experimental science. If it were
experimental, it would only be approximate and provisory. And
what a rough approximation it would be! Geometry would be only
the study of the movements of solid bodies; but, in reality, it is not
concerned with natural solids: its object is certain ideal solids, abso-
lutely invariable, which are but a greatly simplified and very remote
image of them. The concept of these ideal bodies is entirely mental,
and experiment is but the opportunity which enables us to reach the
idea. The object of geometry is the study of a particular " group " ;
but the general concept of group pre-exists in our minds, at least
potentially. It is imposed on us not as a form of our sensitiveness, but
as a form of our understanding; only, from among all possible groups,
we must choose one that will be the *standard,* so to speak, to which
we shall refer natural phenomena.

Experiment guides us in this choice, which it does not impose on
us. It tells us not what is the truest, but what is the most convenient

geometry. It will be noticed that my description of these fantastic worlds has required no language other than that of ordinary geometry. Then, were we transported to those worlds, there would be no need to change that language. Beings educated there would no doubt find it more convenient to create a geometry different from ours, and better adapted to their impressions; but as for us, in the presence of the same impressions, it is certain that we should not find it more convenient to make a change.

Experiment and Geometry

1. I have on several occasions in the preceding pages tried to show how the principles of geometry are not experimental facts, and that in particular Euclid's postulate cannot be proved by experiment. However convincing the reasons already given may appear to me, I feel I must dwell upon them, because there is a profoundly false conception deeply rooted in many minds.

2. Think of a material circle, measure its radius and circumference, and see if the ratio of the two lengths is equal to π. What have we done? We have made an experiment on the properties of the matter with which this *roundness* has been realized, and of which the measure we used is made.

3. *Geometry and Astronomy.* — The same question may also be asked in another way. If Lobatschewsky's geometry is true, the parallax of a very distant star will be finite. If Riemann's is true, it will be negative. These are the results which seem within the reach of experiment, and it is hoped that astronomical observations may enable us to decide between the two geometries. But what we call a straight line in astronomy is simply the path of a ray of light. If, therefore, we were to discover negative parallaxes, or to prove that all parallaxes are higher than a certain limit, we should have a choice between two conclusions: we could give up Euclidean geometry, or modify the laws of optics, and suppose that light is not rigorously propagated in a straight line. It is needless to add that every one would look upon this solution as the more advantageous. Euclidean geometry, therefore, has nothing to fear from fresh experiments.

4. Can we maintain that certain phenomena which are possible in Euclidean space would be impossible in non-Euclidean space, so that experiment in establishing these phenomena would directly contradict the non-Euclidean hypothesis? I think that such a question cannot be seriously asked. To me it is exactly equivalent to the following, the absurdity of which is obvious: — There are lengths which can be expressed in metres and centimetres, but cannot be measured in toises, feet, and inches; so that experiment, by ascertaining the existence of these lengths, would directly contradict this hypothesis, that

there are toises divided into six feet. Let us look at the question a little more closely. I assume that the straight line in Euclidean space possesses any two properties, which I shall call A and B; that in non-Euclidean space it still possesses the property A, but no longer possesses the property B; and, finally, I assume that in both Euclidean and non-Euclidean space the straight line is the only line that possesses the property A. If this were so, experiment would be able to decide between the hypotheses of Euclid and Lobatschewsky. It would be found that some concrete object, upon which we can experiment — for example, a pencil of rays of light — possesses the property A. We should conclude that it is rectilinear, and we should then endeavor to find out if it does, or does not, possess the property B. But *it is not so*. There exists no property which can, like this property A, be an absolute criterion enabling us to recognize the straight line, and to distinguish it from every other line. Shall we say, for instance, " This property will be the following: the straight line is a line such that a figure of which this line is a part can move without the mutual distances of its points varying, and in such a way that all the points in this straight line remain fixed " ? Now, this is a property which in either Euclidean or non-Euclidean space belongs to the straight line, and belongs to it alone. But how can we ascertain by experiment if it belongs to any particular concrete object? Distances must be measured, and how shall we know that any concrete magnitude which I have measured with my material instrument really represents the abstract distance? We have only removed the difficulty a little farther off. In reality, the property that I have just enunciated is not a property of the straight line alone; it is a property of the straight line and of distance. For it to serve as an absolute criterion, we must be able to show, not only that it does not also belong to any other line than the straight line and to distance, but also that it does not belong to any other line than the straight line, and to any other magnitude than distance. Now, that is not true, and if we are not convinced by these considerations, I challenge any one to give me a concrete experiment which can be interpreted in the Euclidean system, and which cannot be interpreted in the system of Lobatschewsky. As I am well aware that this challenge will never be accepted, I may conclude that no experiment will ever be in contradiction with Euclid's postulate; but, on the other hand, no experiment will ever be in contradiction with Lobatschewsky's postulate.

5. But it is not sufficient that the Euclidean (or non-Euclidean) geometry can ever be directly contradicted by experiment. Nor could it happen that it can only agree with experiment by a violation of the principle of sufficient reason, and of that of the relativity of space. Let me explain myself. Consider any material system whatever. We have to consider on the one hand the " state " of the various bodies of

this system — for example, their temperature, their electric potential, etc.; and on the other hand their position in space. And among the data which enable us to define this position we distinguish the mutual distances of these bodies that define their relative positions, and the conditions which define the absolute position of the system and its absolute orientation in space. The law of the phenomena which will be produced in this system will depend on the state of these bodies, and on their mutual distances; but because of the relativity and the inertia of space, they will not depend on the absolute position and orientation of the system. In other words, the state of the bodies and their mutual distances at any moment will solely depend on the state of the same bodies and on their mutual distances at the initial moment, but will in no way depend on the absolute initial position of the system and on its absolute initial orientation. This is what we shall call, for the sake of abbreviation, *the law of relativity*.

So far I have spoken as a Euclidean geometer. But I have said that an experiment, whatever it may be, requires an interpretation on the Euclidean hypothesis; it equally requires one on the non-Euclidean hypothesis. Well, we have made a series of experiments. We have interpreted them on the Euclidean hypothesis, and we have recognized that these experiments thus interpreted do not violate this "law of relativity." We now interpret them on the non-Euclidean hypothesis. This is always possible, only the non-Euclidean distances of our different bodies in this new interpretation will not generally be the same as the Euclidean distances in the primitive interpretation. Will our experiment interpreted in this new manner be still in agreement with our "law of relativity," and if this agreement had not taken place, would we not still have the right to say that experiment has proved the falsity of non-Euclidean geometry? It is easy to see that this is an idle fear. In fact, to apply the law of relativity in all its rigor, it must be applied to the entire universe; for if we were to consider only a part of the universe, and if the absolute position of this part were to vary, the distances of the other bodies of the universe would equally vary; their influence on the part of the universe considered might therefore increase or diminish, and this might modify the laws of the phenomena which take place in it. But if our system is the entire universe, experiment is powerless to give us any opinion on its position and its absolute orientation in space. All that our instruments, however perfect they may be, can let us know will be the state of the different parts of the universe, and their mutual distances. Hence, our law of relativity may be enunciated as follows: — The readings that we can make with our instruments at any given moment will depend only on the readings that we were able to make on the same instruments at the initial moment. Now such an enunciation is

independent of all interpretation by experiments. If the law is true
in the Euclidean interpretation, it will be also true in the non-Eucli-
dean interpretation. Allow me to make a short digression on this
point. I have spoken above of the data which define the position of
the different bodies of the system. I might also have spoken of those
which define their velocities. I should then have to distinguish the
velocity with which the mutual distances of the different bodies are
changing, and on the other hand the velocities of translation and
rotation of the system; that is to say, the velocities with which its
absolute position and orientation are changing. For the mind to be
fully satisfied, the law of relativity would have to be enunciated as
follows: — The state of bodies and their mutual distances at any
given moment, as well as the velocities with which those distances are
changing at that moment, will depend only on the state of those bodies,
on their mutual distances at the initial moment, and on the velocities
with which those distances were changing at the initial moment. But
they will not depend on the absolute initial position of the system
nor on its absolute orientation, nor on the velocities with which that
absolute position and orientation were changing at the initial moment.
Unfortunately, the law thus enunciated does not agree with experi-
ments — at least, as they are ordinarily interpreted. Suppose a man
were translated to a planet, the sky of which was constantly covered
with a thick curtain of clouds, so that he could never see the other
stars. On that planet he would live as if it were isolated in space.
But he would notice that it revolves, either by measuring its ellipticity
(which is ordinarily done by means of astronomical observations, but
which could be done by purely geodesic means), or by repeating the
experiment of Foucault's pendulum. The absolute rotation of this
planet might be clearly shown in this way. Now, here is a fact which
shocks the philosopher, but which the physicist is compelled to accept.
We know from this fact Newton concluded the existence of absolute
space. I myself cannot accept this way of looking at it. I shall ex-
plain why in Part III., but for the moment it is not my intention to
discuss this difficulty. I must therefore resign myself, in the enun-
ciation of the law of relativity, to including velocities of every kind
among the data which define the state of the bodies. However that
may be, the difficulty is the same for both Euclid's geometry and for
Lobatschewsky's. I need not therefore trouble about it further, and
I have only mentioned it incidentally. To sum up, whichever way we
look at it, it is impossible to discover in geometric empiricism a ra-
tional meaning.

6. Experiments only teach us the relations of bodies to one another.
They do not and cannot give us the relations of bodies and space, nor
the mutual relations of the different parts of space. "Yes!" you
reply, "a single experiment is not enough, because it only gives us

one equation with several unknowns; but when I have made enough experiments I shall have enough equations to calculate all my unknowns." If I know the height of the main-mast, that is not sufficient to enable me to calculate the age of the captain. When you have measured every fragment of wood in a ship you will have many equations, but you will be no nearer knowing the captain's age. All your measurements bearing on your fragments of wood can tell you only what concerns those fragments; and similarly, your experiments, however numerous they may be, referring only to the relations of bodies with one another, will tell you nothing about the mutual relations of the different parts of space.

7. Will you say that if the experiments have reference to the bodies, they at least have reference to the geometrical properties of the bodies? First, what do you understand by the geometrical properties of bodies? I assume that it is a question of the relations of the bodies to space. These properties therefore are not reached by experiments which only have reference to the relations of bodies to one another, and that is enough to show that it is not of those properties that there can be a question. Let us therefore begin by making ourselves clear as to the sense of the phrase: geometrical properties of bodies. When I say that a body is composed of several parts, I presume that I am thus enunciating a geometrical property, and that will be true even if I agree to give the improper name of points to the very small parts I am considering. When I say that this or that part of a certain body is in contact with this or that part of another body, I am enunciating a proposition which concerns the mutual relations of the two bodies, and not their relations with space. I assume that you will agree with me that these are not geometrical properties. I am sure that at least you will grant that these properties are independent of all knowledge of metrical geometry. Admitting this, I suppose that we have a solid body formed of eight thin iron rods, *oa, ob, oc, od, oe, of, og, oh,* connected at one of their extremities, *o.* And let us take a second solid body — for example, a piece of wood, on which are marked three little spots of ink which I shall call *a β γ.* I now suppose that we find that we can bring into contact *a β γ* with *ago;* by that I mean *a* with *a,* and at the same time *β* with *g,* and *γ* with *o.* Then we can successively bring into contact *aβγ* with *bgo, cgo, dgo, ego, fgo,* then with *aho, bho, cho, dho, eho, fho;* and then *aγ* successively with *ab, bc, cd, de, ef, fa.* Now these are observations that can be made without having any idea beforehand as to the form or the metrical properties of space. They have no reference whatever to the " geometrical properties of bodies." These observations will not be possible if the bodies on which we experiment move in a group having the same structure as the Lobatschewskian group (I mean according to the same laws as solid bodies in Lobatschewsky's geo-

metry). They therefore suffice to prove that these bodies move according to the Euclidean group; or at least that they do not move according to the Lobatschewskian group. That they may be compatible with the Euclidean group is easily seen; for we might make them so if the body $\alpha\beta\gamma$ were an invariable solid of our ordinary geometry in the shape of a right-angled triangle and if the points *abcdefgh* were the vertices of the polyhedron formed of two regular hexagonal pyramids of our ordinary geometry having *abcdef* as their common base, and having the one *g* and the other *h* as their vertices. Suppose now, instead of the previous observations, we note that we can as before apply $\alpha\beta\gamma$ successively to *ago, bgo, cgo, dgo, ego, fgo, aho, bho, cho, dho, eho, fho*, and then that we can apply $\alpha\beta$ (and no longer $\alpha\gamma$) successively to *ab, bc, cd, de, ef*, and *fa*. These are observations that could be made if non-Euclidean geometry were true, if the bodies $\alpha\beta\gamma$, *oabcdefgh* were invariable solids, if the former were a right-angled triangle, and the latter a double regular hexagonal pyramid of suitable dimensions. These new verifications are therefore impossible if the bodies move according to the Euclidean group; but they become possible if we suppose the bodies to move according to the Lobatschewskian group. They would therefore suffice to show, if we carried them out, that the bodies in question do not move according to the Euclidean group. And so, without making any hypothesis on the form and the nature of space, on the relations of the bodies and space, and without attributing to bodies any geometrical property, I have made observations which have enabled me to show in one case that the bodies experimented upon move according to a group, the structure of which is Euclidean, and in the other case that they move in a group, the structure of which is Lobatschewskian. It cannot be said that all the first observations would constitute an experiment proving that space is Euclidean, and the second an experiment proving space is non-Euclidean: in fact, it might be imagined (note that I use the word *imagined*) that there are bodies moving in such a manner as to render possible the second series of observations: and the proof is that the first mechanic who came our way could construct it if he would only take the trouble. But you must not conclude, however, that space is non-Euclidean. In the same way, just as ordinary solid bodies would continue to exist when the mechanic had constructed the strange bodies I have just mentioned, he would have to conclude that space is both Euclidean and non-Euclidean. Suppose, for instance, that we have a large sphere of radius R, and that its temperature decreases from the centre to the surface of the sphere according to the law of which I spoke when I was describing the non-Euclidean world. We might have bodies whose dilatation is negligible, and which would behave as ordinary invariable solids; and, on the other hand, we might have very dilatable bodies, which

would behave as non-Euclidean solids. We might have two double pyramids *oabcdefgh* and *o' a' b' c' d' e' f' g' h'*, and two triangles *a β γ* and *a' β' γ'*. The first double pyramid would be rectilinear, and the second curvilinear. The triangle *aβγ* would consist of undilatable matter, and the other of very dilatable matter. We might therefore make our first observations with the double pyramid *o' a' h'* and the triangle *a' β' γ'*.

And then the experiment would seem to show — first, that Euclidean geometry is true, and then that it is false. Hence, *experiments have reference not to space but to bodies.*

8. To round the matter off, I ought to speak of a very delicate question, which will require considerable development; but I shall confine myself to summing up what I have written in the *Revue de Métaphysique et de Morale* and in the *Monist*. When we say that space has three dimensions, what do we mean? We have seen the importance of these " internal changes " which are revealed to us by our muscular sensations. They may serve to characterize the different attitudes of our body. Let us take arbitrarily as our origin one of these attitudes, A. When we pass from this initial attitude to another attitude B we experience a series of muscular sensations, and this series S of muscular sensations will define B. Observe, however, that we shall often look upon two series S and S' as defining the same attitude B (since the initial and final attitudes A and B remaining the same, the intermediary attitudes of the corresponding sensations may differ). How then can we recognize the equivalence of these two series? Because they may serve to compensate for the same external change, or more generally, because when it is a question of compensation for an external change, one of the series may be replaced by the other. Among these series we have distinguished those which can alone compensate for an external change, and which we have called " displacements." As we cannot distinguish two displacements which are very close together, the aggregate of these displacements presents the characteristics of a physical continuum. Experience teaches us that they are the characteristics of a physical continuum of six dimensions; but we do not know as yet how many dimensions space itself possesses, so we must first of all answer another question. What is a point in space? Every one thinks he knows, but that is an illusion. What we see when we try to represent to ourselves a point in space is a black spot on white paper, a spot of chalk on a blackboard, always an object. The question should therefore be understood as follows: — What do I mean when I say the object B is at the point which a moment before was occupied by the object A? Again, what criterion will enable me to recognize it? I mean that *although I have not moved* (my muscular sense tells me this), my finger, which just now touched the object A, is now touching the object B. I might have

used other criteria — for instance, another finger or the sense of sight — but the first criterion is sufficient. I know that if it answers in the affirmative all other criteria will give the same answer. I know it from experiment. I cannot know it *à priori*. For the same reason I say that touch cannot be exercised at a distance; that is another way of enunciating the same experimental fact. If I say, on the contrary, that sight is exercised at a distance, it means that the criterion furnished by sight may give an affirmative answer while the others reply in the negative.

To sum up. For each attitude of my body my finger determines a point, and it is that and that only which defines a point in space. To each attitude corresponds in this way a point. But it often happens that the same point corresponds to several different attitudes (in this case we say that our finger has not moved, but the rest of our body has). We distinguish, therefore, among changes of attitude those in which the finger does not move. How are we led to this? It is because we often remark that in these changes the object which is in touch with the finger remains in contact with it. Let us arrange then in the same class all the attitudes which are deduced one from the other by one of the changes that we have thus distinguished. To all these attitudes of the same class will correspond the same point in space. Then to each class will correspond a point, and to each point a class. Yet it may be said that what we get from this experiment is not the point, but the class of changes, or, better still, the corresponding class of muscular sensations. Thus, when we say that space has three dimensions, we merely mean that the aggregate of these classes appears to us with the characteristics of a physical continuum of three dimensions. Then if, instead of defining the points in space with the aid of the first finger, I use, for example, another finger, would the results be the same? That is by no means *à priori* evident. But, as we have seen, experiment has shown us that all our criteria are in agreement, and this enables us to answer in the affirmative. If we recur to what we have called displacements, the aggregate of which forms, as we have seen, a group, we shall be brought to distinguish those in which a finger does not move, and by what has preceded, those are the displacements which characterize a point in space, and their aggregate will form a sub-group of our group. To each sub-group of this kind, then, will correspond a point in space. We might be tempted to conclude that experiment has taught us the number of dimensions of space; but in reality our experiments have referred not to space, but to our body and its relations with neighboring objects. What is more, our experiments are exceeding crude. In our mind the latent idea of a certain number of groups pre-existed; these are the groups with which Lie's theory is concerned. Which

shall we choose to form a kind of standard by which to compare natural phenomena? And when this group is chosen, which of the subgroups shall we take to characterize a point in space? Experiment has guided us by showing us what choice adapts itself best to the properties of our body; but there its rôle ends.

PART III — FORCE

The Classical Mechanics.

THE English teach mechanics as an experimental science; on the Continent it is taught always more or less as a deductive and *à priori* science. The English are right, no doubt. How is it that the other method has been persisted in for so long; how is it that Continental scientists who have tried to escape from the practice of their predecessors have in most cases been unsuccessful? On the other hand, if the principles of mechanics are only of experimental origin, are they not merely approximate and provisory? May we not be some day compelled by new experiments to modify or even to abandon them? These are the questions which naturally arise, and the difficulty of solution is largely due to the fact that treatises on mechanics do not clearly distinguish between what is experiment, what is mathematical reasoning, what is convention, and what is hypothesis. This is not all.

1. There is no absolute space, and we only conceive of relative motion; and yet in most cases mechanical facts are enunciated as if there is an absolute space to which they can be referred.

2. There is no absolute time. When we say that two periods are equal, the statement has no meaning, and can only acquire a meaning by a convention.

3. Not only have we no direct intuition of the equality of two periods, but we have not even direct intuition of the simultaneity of two events occurring in two different places. I have explained this in an article entitled " Mesure du Temps."[1]

4. Finally, is not our Euclidean geometry in itself only a kind of convention of language? Mechanical facts might be enunciated with reference to a non-Euclidean space which would be less convenient but quite as legitimate as our ordinary space; the enunciation would become more complicated, but it still would be possible.

Thus, absolute space, absolute time, and even geometry are not conditions which are imposed on mechanics. All these things no more existed before mechanics than the French language can be logically said to have existed before the truths which are expressed in French.

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[1] *Revue de Métaphysique et de Morale*, t. vi., pp. 1-13, January, 1898.

We might endeavor to enunciate the fundamental law of mechanics in a language independent of all these conventions; and no doubt we should in this way get a clearer idea of those laws in themselves. This is what M. Andrade has tried to do, to some extent at any rate, in his *Leçons de Mécanique physique*. Of course the enunciation of these laws would become much more complicated, because all these conventions have been adopted for the very purpose of abbreviating and simplifying the enunciation. As far as we are concerned, I shall ignore all these difficulties; not because I disregard them, far from it; but because they have received sufficient attention in the first two parts of the book. Provisionally, then, we shall admit absolute time and Euclidean geometry.

The Principle of Inertia. — A body under the action of no force can only move uniformly in a straight line. Is this a truth imposed on the mind *à priori*? If this be so, how is it that the Greeks ignored it? How could they have believed that motion ceases with the cause of motion? or, again, that every body, if there is nothing to prevent it, will move in a circle, the noblest of all forms of motion?

If it be said that the velocity of a body cannot change, if there is no reason for it to change, may we not just as legitimately maintain that the position of a body cannot change, or that the curvature of its path cannot change, without the agency of an external cause? Is, then, the principle of inertia, which is not an *à priori* truth, an experimental fact? Have there ever been experiments on bodies acted on by no forces? and, if so, how did we know that no forces were acting? The usual instance is that of a ball rolling for a very long time on a marble table; but why do we say it is under the action of no force? Is it because it is too remote from all other bodies to experience any sensible action? It is not further from the earth than if it were thrown freely into the air; and we all know that in that case it would be subject to the attraction of the earth. Teachers of mechanics usually pass rapidly over the example of the ball, but they add that the principle of inertia is verified indirectly by its consequences. This is very badly expressed; they evidently mean that various consequences may be verified by a more general principle, of which the principle of inertia is only a particular case. I shall propose for this general principle the following enunciation:—The acceleration of a body depends only on its position and that of neighboring bodies, and on their velocities. Mathematicians would say that the movements of all the material molecules of the universe depend on differential equations of the second order. To make it clear that this is really a generalization of the law of inertia we may again have recourse to our imagination. The law of inertia, as I have said above, is not imposed on us *à priori*; other laws would be just as compatible with the principle of sufficient reason. If a body is not acted upon

by force, instead of supposing that its velocity is unchanged we may suppose that its position or its acceleration is unchanged.

Let us for a moment suppose that one of these two laws is a law of nature, and substitute it for a law of inertia: what will be the natural generalization? A moment's reflection will show us. In the first case, we may suppose that the velocity of a body depends only on its position and that of neighboring bodies; in the second case, that the variation of the acceleration of a body depends only on the position of the body and of neighboring bodies, on their velocities and accelerations; or, in mathematical terms, the differential equations of the motion would be of the first order in the first case and of the third order in the second.

Let us now modify our supposition a little. Suppose a world analogous to our solar system, but one in which by a singular chance the orbits of all the planets have neither eccentricity nor inclination; and further, suppose that the masses of the planets are too small for their mutual perturbations to be sensible. Astronomers living in one of these planets would not hesitate to conclude that the orbit of a star can only be circular and parallel to a certain plane; the position of a star at a given moment would then be sufficient to determine its velocity and path. The law of inertia which they would adopt would be the former of the two hypothetical laws I have mentioned.

Now, imagine this system to be some day crossed by a body of vast mass and immense velocity coming from distant constellations. All the orbits would be profoundly disturbed. Our astronomers would not be greatly astonished. They would guess that this new star is in itself quite capable of doing all the mischief; but, they would say, as soon as it has passed by, order will again be established. No doubt the distances of the planets from the sun will not be the same as before the cataclysm, but the orbits will become circular again as soon as the disturbing cause has disappeared. It would be only when the perturbing body is remote, and when the orbits, instead of being circular are found to be elliptical, that the astronomers would find out their mistake, and discover the necessity of reconstructing their mechanics.

I have dwelt on these hypotheses, for it seems to me that we can clearly understand our generalized law of inertia only by opposing it to a contrary hypothesis.

Has this generalized law of inertia been verified by experiment, and can it be so verified? When Newton wrote the *Principia,* he certainly regarded this truth as experimentally acquired and demonstrated. It was so in his eyes, not only from the anthropomorphic conception to which I shall later refer, but also because of the work of Galileo. It was so proved by the laws of Kepler. According to those laws, in fact, the path of a planet is entirely determined by its initial position and

initial velocity; this, indeed, is what our generalized law of inertia requires.

For this principle to be only true in appearance — lest we should fear that some day it must be replaced by one of the analogous principles which I opposed to it just now — we must have been led astray by some amazing chance such as that which had led into error our imaginary astronomers. Such an hypothesis is so unlikely that it need not delay us. No one will believe that there can be such chances; no doubt the probability that two eccentricities are both exactly zero is not smaller than the probability that one is 0.1 and the other 0.2. The probability of a simple event is not smaller than that of a complex one. If, however, the former does occur, we shall not attribute its occurrence to chance; we shall not be inclined to believe that nature has done it deliberately to deceive us. The hypothesis of an error of this kind being discarded, we may admit that so far as astronomy is concerned our law has been verified by experiment.

But Astronomy is not the whole of Physics. May we not fear that some day a new experiment will falsify the law in some domain of physics? An experimental law is always subject to revision; we may always expect to see it replaced by some other and more exact law. But no one seriously thinks that the law of which we speak will ever be abandoned or amended. Why? Precisely because it will never be submitted to a decisive test.

In the first place, for this test to be complete, all the bodies of the universe must return with their initial velocities to their initial positions after a certain time. We ought then to find that they would resume their original paths. But this test is impossible; it can be only partially applied, and even when it is applied there will still be some bodies which will not return to their original positions. Thus there will be a ready explanation of any breaking down of the law.

Yet this is not all. In Astronomy we see the bodies whose motion we are studying, and in most cases we grant that they are not subject to the action of other invisible bodies. Under these conditions, our law must certainly be either verified or not. But it is not so in Physics. If physical phenomena are due to motion, it is to the motion of molecules which we cannot see. If, then, the acceleration of bodies we cannot see depends on something else than the positions or velocities of other visible bodies or of invisible molecules, the existence of which we have been led previously to admit, there is nothing to prevent us from supposing that this something else is the position or velocity of other molecules of which we have not so far suspected the existence. The law will be safeguarded. Let me express the same thought in another form in mathematical language. Suppose we are observing n molecules, and find that their $3n$ co-ordinates satisfy a system of $3n$ differential equations of the fourth order (and not of

the second, as required by the law of inertia). We know that by introducing $3n$ variable auxiliaries, a system of $3n$ equations of the fourth order may be reduced to a system of $6n$ equations of the second order. If, then, we suppose that the $3n$ auxiliary variables represent the coordinates of n invisible molecules, the result is again conformable to the law of inertia. To sum up, this law, verified experimentally in some particular cases, may be extended fearlessly to the most general cases; for we know that in these general cases it can neither be confirmed nor contradicted by experiment.

The Law of Acceleration. — The acceleration of a body is equal to the force which acts on it divided by its mass.

Can this law be verified by experiment? If so, we have to measure the three magnitudes mentioned in the enunciation: acceleration, force, and mass. I admit that acceleration may be measured, because I pass over the difficulty arising from the measurement of time. But how are we to measure force and mass? We do not even know what they are. What is mass? Newton replies: " The product of the volume and the density." " It were better to say," answer Thomson and Tait, " that density is the quotient of the mass by the volume." What is force? " It is," replies Lagrange, " that which moves or tends to move a body." " It is," according to Kirchoff, " the product of the mass and the acceleration." Then why not say that mass is the quotient of the force by the acceleration? These difficulties are insurmountable.

When we say force is the cause of motion, we are talking metaphysics; and this definition, if we had to be content with it, would be absolutely fruitless, would lead to absolutely nothing. For a definition to be of any use it must tell us how to measure force; and that is quite sufficient, for it is by no means necessary to tell what force is in itself, nor whether it is the cause or the effect of motion. We must therefore first define what is meant by the equality of two forces. When are two forces equal? We are told that it is when they give the same acceleration to the same mass, or when acting in opposite directions they are in equilibrium. This definition is a sham. A force applied to a body cannot be uncoupled and applied to another body as an engine is uncoupled from one train and coupled to another. It is therefore impossible to say what acceleration such a force, applied to such a body, would give to another body if it were applied to it. It is impossible to tell how two forces which are not acting in exactly opposite directions would behave if they were acting in opposite directions. It is this definition which we try to materialize, as it were, when we measure a force with a dynamometer or with a balance. Two forces, F and F', which I suppose, for simplicity, to be acting vertically upwards, are respectively applied to two bodies, C and C'. I attach a body weighing P first to C and then to C'; if

there is equilibrium in both cases I conclude that the two forces F and F′ are equal, for they are both equal to the weight of the body P. But am I certain that the body P has kept its weight when I transferred it from the first body to the second? Far from it. I am certain of the contrary. I know that the magnitude of the weight varies from one point to another, and that it is greater, for instance, at the pole than at the equator. No doubt, the difference is very small, and we neglect it in practice; but a definition must have mathematical rigor; this rigor does not exist. What I say of weight would apply equally to the force of the spring of a dynamometer, which would vary according to temperature and many other circumstances. Nor is this all. We cannot say that the weight of the body P is applied to the body C and keeps in equilibrium the force F. What is applied to the body C is the action of the body P on the body C. On the other hand, the body P is acted on by its weight, and by the reaction R of the body C on P the forces F and A are equal, because they are in equilibrium; the forces A and R are equal by virtue of the principle of action and reaction; and finally, the force R and the weight P are equal because they are in equilibrium. From these three equalities we deduce the equality of the weight P and the force F.

Thus we are compelled to bring into our definition of the equality of two forces the principle of the equality of action and reaction; *hence this principle can no longer be regarded as an experimental law but only as a definition.*

To recognize the equality of two forces we are then in possession of two rules: the equality of two forces in equilibrium and the equality of action and reaction. But, as we have seen, these are not sufficient, and we are compelled to have recourse to a third rule, and to admit that certain forces — the weight of a body, for instance — are constant in magnitude and direction. But this third rule is an experimental law. It is only approximately true: *it is a bad definition.* We are therefore reduced to Kirchoff's definition: force is the product of the mass and the acceleration. This law of Newton in its turn ceases to be regarded as an experimental law, it is now only a definition. But as a definition it is insufficient, for we do not know what mass is. It enables us, no doubt, to calculate the ratio of two forces applied at different times to the same body, but it tells us nothing about the ratio of two forces applied to two different bodies. To fill up the gap we must have recourse to Newton's third law, the equality of action and reaction, still regarded not as an experimental law but as a definition. Two bodies, A and B, act on each other; the acceleration of A, multiplied by the mass of A, is equal to the action of B on A: in the same way the acceleration of B, multiplied by the mass of B, is equal to the reaction of A on B. As, by definition, the action and the reaction are equal, the masses of A and B are respectively in

the inverse ratio of their masses. Thus is the ratio of the two masses defined, and it is for experiment to verify that the ratio is constant.

This would do very well if the two bodies were alone and could be abstracted from the action of the rest of the world; but this is by no means the case. The acceleration of A is not solely due to the action of B, but to that of a multitude of other bodies, C, D, . . . To apply the preceding rule we must decompose the acceleration of A into many components, and find out which of these components is due to the action of B. The decomposition would still be possible if we suppose that the action of C on A is simply added to that of B on A, and that the presence of the body C does not in any way modify the action of B on A, or that the presence of B does not modify the action of C on A; that is, if we admit that any two bodies attract each other, that their mutual action is along their join, and is only dependent on their distance apart; if, in a word, we admit the *hypothesis of central forces.*

We know that to determine the masses of the heavenly bodies we adopt quite a different principle. The law of gravitation teaches us that the attraction of two bodies is proportional to their masses; if r is their distance apart, m and m' their masses, k, a constant, then their attraction will be kmm'/r^2. What we are measuring is therefore not mass, the ratio of the force to the acceleration, but the attracting mass; not the inertia of the body, but its attracting power. It is an indirect process, the use of which is not indispensable theoretically. We might have said that the attraction is inversely proportional to the square of the distance, without being proportional to the product of the masses, that it is equal to f/r^2 and not to kmm'. If it were so, we should nevertheless, by observing the *relative* motion of the celestial bodies, be able to calculate the masses of these bodies.

But have we any right to admit the hypothesis of central forces? Is this hypothesis rigorously accurate? Is it certain that it will never be falsified by experiment? Who will venture to make such an assertion? And if we must abandon this hypothesis, the building which has been so laboriously erected must fall to the ground.

We have no longer any right to speak of the component of the acceleration of A which is due to the action of B. We have no means of distinguishing it from that which is due to the action of C or of any other body. The rule becomes inapplicable in the measurement of masses. What then is left of the principle of the equality of action and reaction? If we reject the hypothesis of central forces this principle must go too; the geometrical resultant of all the forces applied to the different bodies of a system abstracted from all external action will be zero. In other words, *the motion of the centre of gravity of this system will be uniform and in a straight line.* Here would seem to be a means of defining mass. The position of the centre of gravity evidently depends on the values given to the masses; we must select

these values so that the motion of the centre of gravity is uniform and rectilinear. This will always be possible if Newton's third law holds good, and it will be in general possible only in one way. But no system exists which is abstracted from all external action; every part of the universe is subject, more or less, to the action of the other parts. *The law of the motion of the centre of gravity is only rigorously true when applied to the whole universe.*

But then, to obtain the values of the masses we must find the motion of the centre of gravity of the universe. The absurdity of this conclusion is obvious; the motion of the centre of gravity of the universe will be forever to us unknown. Nothing, therefore, is left, and our efforts are fruitless. There is no escape from the following definition, which is only a confession of failure: *Masses are co-efficients, which it is found convenient to introduce into calculations.*

We could reconstruct our mechanics by giving to our masses different values. The new mechanics would be in contradiction neither with experiment nor with the general principles of dynamics (the principle of inertia, proportionality of masses and accelerations, equality of action and reaction, uniform motion of the centre of gravity in a straight line, and areas). But the equations of this mechanics *would not be so simple.* Let us clearly understand this. It would be only the first terms which would be less simple — *i.e.,* those we already know through experiment; perhaps the small masses could be slightly altered without the *complete* equations gaining or losing in simplicity.

Hertz has inquired if the principles of mechanics are rigorously true. "In the opinion of many physicists it seems inconceivable that experiment will ever alter the impregnable principles of mechanics; and yet, what is due to experiment may always be rectified by experiment." From what we have just seen these fears would appear to be groundless. The principles of dynamics appeared to us first as experimental truths, but we have been compelled to use them as definitions. It is *by definition* that force is equal to the product of the mass and the acceleration; this is a principle which is henceforth beyond the reach of any future experiment. Thus it is by definition that action and reaction are equal and opposite. But then it will be said, these unverifiable principles are absolutely devoid of any significance. They cannot be disproved by experiment, but we can learn from them nothing of any use to us; what then is the use of studying dynamics? This somewhat rapid condemnation would be rather unfair. There is not in Nature any system *perfectly* isolated, perfectly abstracted from all external action; but there are systems which are *nearly* isolated. If we observe such a system, we can study not only the relative motion of its different parts with respect to each other, but the motion of its centre of gravity with respect to the other parts of the universe.

We then find that the motion of its centre of gravity is *nearly* uniform and rectilinear in conformity with Newton's Third Law. This is an experimental fact, which cannot be invalidated by a more accurate experiment. What, in fact, would a more accurate experiment teach us? It would teach us that the law is only approximately true, and we know that already. *Thus is explained how experiment may serve as a basis for the principles of mechanics, and yet will never invalidate them.*

Anthropomorphic Mechanics. — It will be said that Kirchoff has only followed the general tendency of mathematicians towards nominalism; from this his skill as a physicist has not saved him. He wanted a definition of a force, and he took the first that came handy; but we do not require a definition of force; the idea of force is primitive, irreducible, indefinable; we all know what it is; of it we have direct intuition. This direct intuition arises from the idea of effort which is familiar to us from childhood. But in the first place, even if this direct intuition made known to us the real nature of force in itself, it would prove to be an insufficient basis for mechanics; it would, moreover, be quite useless. The important thing is not to know what force is, but how to measure it. Everything which does not teach us how to measure it is as useless to the mechanician as, for instance, the subjective idea of heat and cold to the student of heat. This subjective idea cannot be translated into numbers, and is therefore useless: a scientist whose skin is an absolutely bad conductor of heat, and who, therefore, has never felt the sensation of heat or cold, would read a thermometer in just the same way as any one else, and would have enough material to construct the whole of the theory of heat.

Now this immediate notion of effort is of no use to us in the measurement of force. It is clear, for example, that I shall experience more fatigue in lifting a weight of 100 lb. than a man who is accustomed to lifting heavy burdens. But there is more than this. This notion of effort does not teach us the nature of force; it is definitely reduced to a recollection of muscular sensations, and no one will maintain that the sun experiences a muscular sensation when it attracts the earth. All that we can expect to find from it is a symbol, less precise and less convenient than the arrows (to denote direction) used by geometers, and quite as remote from reality.

Anthropomorphism plays a considerable historic rôle in the genesis of mechanics; perhaps it may yet furnish us with a symbol which some minds may find convenient; but it can be the foundation of nothing of a really scientific or philosophical character.

The Thread School.—M. Andrade, in his *Leçons de Mécanique physique,* has modernized anthropomorphic mechanics. To the school

of mechanics with which Kirchoff is identified, he opposes a school which is quaintly called the "Thread School."

This school tries to reduce everything to the consideration of certain material systems of negligible mass, regarded in a state of tension and capable of transmitting considerable effort to distant bodies — systems of which the ideal type is the fine string, wire, or *thread*. A thread which transmits any force is slightly lengthened in the direction of that force; the direction of the thread tells us the direction of the force, and the magnitude of the force is measured by the lengthening of the thread.

We may imagine such an experiment as the following : — A body A is attached to a thread; at the other extremity of the thread acts a force which is made to vary until the length of the thread is increased by a, and the acceleration of the body A is recorded. A is then detached, and a body B is attached to the same thread, and the same or another force is made to act until the increment of length again is a, and the acceleration of B is noted. The experiment is then renewed with both A and B until the increment of length is β. The four accelerations observed should be proportional. Here we have an experimental verification of the law of acceleration enunciated above. Again, we may consider a body under the action of several threads in equal tension, and by experiment we determine the direction of those threads when the body is in equilibrium. This is an experimental verification of the law of the composition of forces. But, as a matter of fact, what have we done? We have defined the force acting on the string by the deformation of the thread, which is reasonable enough; we have then assumed that if a body is attached to this thread, the effort which is transmitted to it by the thread is equal to the action exercised by the body on the thread; in fact, we have used the principle of action and reaction by considering it, not as an experimental truth, but as the very definition of force. This definition is quite as conventional as that of Kirchoff, but it is much less general.

All the forces are not transmitted by the thread (and to compare them they would all have to be transmitted by identical threads). If we even admitted that the earth is attached to the sun by an invisible thread, at any rate it will be agreed that we have no means of measuring the increment of the thread. Nine times out of ten, in consequence, our definition will be in default; no sense of any kind can be attached to it, and we must fall back on that of Kirchoff. Why then go on in this roundabout way? You admit a certain definition of force which has a meaning only in certain particular cases. In those cases you verify by experiment that it leads to the law of acceleration. On the strength of these experiments you then take the law of acceleration as a definition of force in all the other cases.

Would it not be simpler to consider the law of acceleration as a

definition in all cases, and to regard the experiments in question, not as verifications of that law, but as verifications of the principle of action and reaction, or as proving the deformations of an elastic body depend only on the forces acting on that body? Without taking into account the fact that the conditions in which your definition could be accepted can only be very imperfectly fulfilled, that a thread is never without mass, that it is never isolated from all other forces than the reaction of the bodies attached to its extremities.

The ideas expounded by M. Andrade are none the less very interesting. If they do not satisfy our logical requirements, they give us a better view of the historical genesis of the fundamental ideas of mechanics. The reflections they suggest show us how the human mind passed from a naïve anthropomorphism to the present conception of science.

We see that we end with an experiment which is very particular, and as a matter of fact very crude, and we start with a perfectly general law, perfectly precise, the truth of which we regard as absolute. We have, so to speak, freely conferred this certainty on it by looking upon it as a convention.

Are the laws of acceleration and of the composition of forces only arbitrary conventions? Conventions, yes; arbitrary, no — they would be so if we lost sight of the experiments which led the founders of the science to adopt them, and which, imperfect as they were, were sufficient to justify their adoption. It is well from time to time to let our attention dwell on the experimental origin of these conventions.

Relative and Absolute Motion

The Principle of Relative Motion. — Sometimes endeavors have been made to connect the law of acceleration with a more general principle. The movement of any system whatever ought to obey the same laws, whether it is referred to fixed axes or to the movable axes which are implied in uniform motion in a straight line. This is the principle of relative motion; it is imposed upon us for two reasons: the commonest experiment confirms it; the consideration of the contrary hypothesis is singularly repugnant to the mind.

Let us admit it then, and consider a body under the action of a force. The relative motion of this body with respect to an observer moving with a uniform velocity equal to the initial velocity of the body, should be identical with what would be its absolute motion if it started from rest. We conclude that its acceleration must not depend upon its absolute velocity, and from that we attempt to deduce the complete law of acceleration.

For a long time there have been traces of this proof in the regulations for the degree of B.Sc. It is clear that the attempt has

failed. The obstacle which prevented us from proving the law of acceleration is that we have no definition of force. This obstacle subsists in its entirety, since the principle invoked has not furnished us with the missing definition. The principle of relative motion is none the less very interesting, and deserves to be considered for its own sake. Let us try to enunciate it in an accurate manner. We have said above that the accelerations of the different bodies which form part of an isolated system only depend on their velocities and their relative positions, and not on their velocities and their absolute positions, provided that the movable axes to which the relative motion is referred move uniformly in a straight line; or, if it is preferred, their accelerations depend only on the differences of their velocities and the differences of their co-ordinates, and not on the absolute values of these velocities and co-ordinates. If this principle is true for relative accelerations, or rather for differences of acceleration, by combining it with the law of reaction we shall deduce that it is true for absolute accelerations. It remains to be seen how we can prove that differences of acceleration depend only on differences of velocities and co-ordinates; or, to speak in mathematical language, that these differences of co-ordinates satisfy differential equations of the second order. Can this proof be deduced from experiment or from *à priori* conditions? Remembering what we have said before, the reader will give his own answer. Thus enunciated, in fact, the principle of relative motion curiously resembles what I called above the generalized principle of inertia; it is not quite the same thing, since it is a question of differences of co-ordinates, and not of the co-ordinates themselves. The new principle teaches us something more than the old, but the same discussion applies to it, and would lead to the same conclusions. We need not recur to it.

Newton's Argument. — Here we find a very important and even slightly disturbing question. I have said that the principle of relative motion was not for us simply a result of experiment; and that *à priori* every contrary hypothesis would be repugnant to the mind. But, then, why is the principle only true if the motion of the movable axes is uniform and in a straight line? It seems that it should be imposed upon us with the same force if the motion is accelerated, or at any rate if it reduces to a uniform rotation. In these two cases, in fact, the principle is not true. I need not dwell on the case in which the motion of the axes is in a straight line and not uniform. The paradox does not bear a moment's examination. If I am in a railway carriage, and if the train, striking against any obstacle whatever, is suddenly stopped, I shall be projected on to the opposite side, although I have not been directly acted upon by any force. There is nothing mysterious in that, and if I have not been subject to the action of any external force, the train has experienced an external impact. There can be

nothing paradoxical in the relative motion of two bodies being dis-
turbed when the motion of one or the other is modified by an external
cause. Nor need I dwell on the case of relative motion referring to
axes which rotate uniformly. If the sky were forever covered with
clouds, and if we had no means of observing the stars, we might,
nevertheless, conclude that the earth turns round. We should be
warned of this fact by the flattening at the poles, or by the experiment
of Foucault's pendulum. And yet, would there in this case be any
meaning in saying that the earth turns round? If there is no abso-
lute space, can a thing turn without turning with respect to some-
thing; and, on the other hand, how can we admit Newton's conclu-
sion and believe in absolute space? But it is not sufficient to state
that all possible solutions are equally unpleasant to us. We must
analyze in each case the reason of our dislike, in order to make our
choice with the knowledge of the cause. The long discussion which
follows must, therefore, be excused.

Let us resume our imaginary story. Thick clouds hide the stars
from men who cannot observe them, and even are ignorant of their
existence. How will those men know that the earth turns round? No
doubt, for a longer period than did our ancestors, they will regard the
soil on which they stand as fixed and immovable. They will wait a
much longer time than we did for the coming of a Copernicus; but
this Copernicus will come at last. How will he come? In the first
place, the mechanical school of this world would not run their heads
against an absolute contradiction. In the theory of relative motion we
observe, besides real forces, two imaginary forces, which we call ordi-
nary centrifugal force and compounded centrifugal force. Our imag-
inary scientists can thus explain everything by looking upon these two
forces as real, and they would not see in this a contradiction of the
generalized principle of inertia, for these forces would depend, the one
on the relative positions of the different parts of the system, such as
real attractions, and the other on their relative velocities, as in the
case of real frictions. Many difficulties, however, would before long
awaken their attention. If they succeeded in realizing an isolated
system, the centre of gravity of this system would not have an ap-
proximately rectilinear path. They could invoke, to explain this fact,
the centrifugal forces which they would regard as real, and which,
no doubt, they would attribute to the mutual actions of the bodies —
only they would not see these forces vanish at great distances — that is
to say, in proportion as the isolation is better realized. Far from it.
Centrifugal force increases indefinitely with distance. Already this
difficulty would seem to them sufficiently serious, but it would not
detain them for long. They would soon imagine some very subtle
medium analogous to our ether, in which all bodies would be bathed,
and which would exercise on them a repulsive action. But that is not

all. Space is symmetrical — yet the laws of motion would present
no symmetry. They should be able to distinguish between right and
left. They would see, for instance, that cyclones always turn in the
same direction, while for reasons of symmetry they should turn in-
differently in any direction. If our scientists were able by dint of
much hard work to make their universe perfectly symmetrical, this
symmetry would not subsist, although there is no apparent reason
why it should be disturbed in one direction more than in another.
They would extract this from the situation no doubt — they would
invent something which would not be more extraordinary than the
glass spheres of Ptolemy, and would thus go on accumulating compli-
cations until the long-expected Copernicus would sweep them all
away with a single blow, saying it is much more simple to admit that
the earth turns round. Just as our Copernicus said to us: "It is
more convenient to suppose that the earth turns round, because the
laws of astronomy are thus expressed in a more simple language," so
he would say to them: "It is more convenient to suppose that the
earth turns round, because the laws of mechanics are thus expressed in
much more simple language." That does not prevent absolute space —
that is to say, the point to which we must refer the earth to know
if it really does turn round — from having no objective existence.
And hence this affirmation: "the earth turns round," has no mean-
ing, since it cannot be verified by experiment; since such an experi-
ment not only cannot be realized or even dreamed of by the most
daring Jules Verne, but cannot even be conceived of without contra-
diction; or, in other words, these two propositions, "the earth turns
round," and "it is more convenient to suppose that the earth turns
round," have one and the same meaning. There is nothing more in
one than in the other. Perhaps they will not be content with this,
and may find it surprising that among all the hypotheses, or rather
all the conventions, that can be made on this subject there is one
which is more convenient than the rest. But if we have admitted it
without difficulty when it is a question of the laws of astronomy, why
should we object when it is a question of the laws of mechanics? We
have seen that the co-ordinates of bodies are determined by differ-
ential equations of the second order, and that so are the differences
of these co-ordinates. This is what we have called the generalized
principle of inertia, and the principle of relative motion. If the dis-
tances of these bodies were determined in the same way by equations
of the second order, it seems that the mind should be entirely satisfied.
How far does the mind receive this satisfaction, and why is it not
content with it? To explain this we had better take a simple example.
I assume a system analogous to our solar system, but in which fixed
stars foreign to this system cannot be perceived, so that astronomers
can only observe the mutual distances of planets and the sun, and not

the absolute longitudes of the planets. If we deduce directly from Newton's law the differential equations which define the variation of these distances, these equations will not be of the second order. I mean that if, outside Newton's law, we knew the initial values of these distances and of their derivatives with respect to time — that would not be sufficient to determine the values of these same distances at an ulterior moment. A datum would be still lacking, and this datum might be, for example, what astronomers call the area-constant. But here we may look at it from two different points of view. We may consider two kinds of constants. In the eyes of the physicist the world reduces to a series of phenomena depending, on the one hand, solely on initial phenomena, and, on the other hand, on the laws connecting consequence and antecedent. If observation then teaches us that a certain quantity is a constant, we shall have a choice of two ways of looking at it. So let us admit that there is a law which requires that this quantity shall not vary, but that by chance it has been found to have had in the beginning of time this value rather than that, a value that it has kept ever since. This quantity might then be called an *accidental* constant. Or again, let us admit on the contrary that there is a law of nature which imposes on this quantity this value and not that. We shall then have what may be called an *essential* constant. For example, in virtue of the laws of Newton the duration of the revolution of the earth must be constant. But if it is 366 and something sidereal days, and not 300 or 400, it is because of some initial chance or other. It is an *accidental* constant. If, on the other hand, the exponent of the distance which figures in the expression of the attractive force is equal to -2 and not to -3, it is not by chance, but because it is required by Newton's law. It is an *essential* constant. I do not know if this manner of giving to chance its share is legitimate in itself, and if there is not some artificiality about this distinction; but it is certain at least that in proportion as Nature has secrets, she will be strictly arbitrary and always uncertain in their application. As far as the area-constant is concerned, we are accustomed to look upon it as accidental. Is it certain that our imaginary astronomers would do the same? If they were able to compare two different solar systems, they would get the idea that this constant may assume several different values. But I supposed at the outset, as I was entitled to do, that their system would appear isolated, and that they would see no star which was foreign to their system. Under these conditions they could only detect a single constant, which would have an absolutely invariable, unique value. They would be led no doubt to look upon it as an essential constant.

One word in passing to forestall an objection. The inhabitants of this imaginary world could neither observe nor define the area-constant as we do, because absolute longitudes escape their notice;

but that would not prevent them from being rapidly led to remark a certain constant which would be naturally introduced into their equations, and which would be nothing but what we call the area-constant. But then what would happen? If the area-constant is regarded as essential, as dependent upon a law of nature, then in order to calculate the distances of the planets at any given moment it would be sufficient to know the initial values of these distances and those of their first derivatives. From this new point of view, distances will be determined by differential equations of the second order. Would this completely satisfy the minds of these astronomers? I think not. In the first place, they would very soon see that in differentiating their equations so as to raise them to a higher order, these equations would become much more simple, and they would be especially struck by the difficulty which arises from symmetry. They would have to admit different laws, according as the aggregate of the planets presented the figure of a certain polyhedron or rather of a regular polyhedron, and these consequences can only be escaped by regarding the area-constant as accidental. I have taken this particular example, because I have imagined astronomers who would not be in the least concerned with terrestrial mechanics and whose vision would be bounded by the solar system. But our conclusions apply in all cases. Our universe is more extended than theirs, since we have fixed stars; but it, too, is very limited, so we might reason on the whole of our universe just as these astronomers do on their solar system. We thus see that we should be definitively led to conclude that the equations which define distances are of an order higher than the second. Why should this alarm us — why do we find it perfectly natural that the sequence of phenomena depends on initial values of the first derivatives of these distances, while we hesitate to admit that they may depend on the initial values of the second derivatives? It can only be because of mental habits created in us by the constant study of the generalized principle of inertia and of its consequences. The values of the distances at any given moment depend upon their initial values, on that of their first derivatives, and something else. What is that *something else?* If we do not want it to be merely one of the second derivatives, we have only the choice of hypotheses. Suppose, as is usually done, that this something else is the absolute orientation of the universe in space, or the rapidity with which this orientation varies; this may be, it certainly is, the most convenient solution for the geometer. But it is not the most satisfactory for the philosopher, because this orientation does not exist. We may assume that this something else is the position or the velocity of some invisible body, and this is what is done by certain persons, who have even called the body Alpha, although we are destined to never know anything about this body except its name. This is an artifice en-

tirely analogous to that of which I spoke at the end of the paragraph
containing my reflections on the principle of inertia. But as a matter
of fact the difficulty is artificial. Provided that the future indica-
tions of our instruments can only depend on the indications which
they have given us, or that they might have formerly given us, such is
all we want, and with these conditions we may rest satisfied.

Energy and Thermo-Dynamics

Energetics. — The difficulties raised by the classical mechanics have
led certain minds to prefer a new system which they call Energetics.
Energetics took its rise in consequence of the discovery of the prin-
ciple of the conservation of energy. Helmholtz gave it its definite
form. We begin by defining two quantities which play a fundamental
part in this theory. They are *kinetic energy,* or *vis viva,* and *potential
energy.* Every change that the bodies of nature can undergo is regu-
lated by two experimental laws. First, the sum of the kinetic and
potential energies is constant. This is the principle of the conserva-
tion of energy. Second, if a system of bodies is at A at the time t_0, and
at B at the time t_1, it always passes from the first position to the
second by such a path that the *mean* value of the difference between
the two kinds of energy in the interval of time which separates the
two epochs, t_0 and t_1 is a minimum. This is Hamilton's principle, and
is one of the forms of the principle of least action. The energetic
theory has the following advantages over the classical. First, it is
less incomplete — that is to say, the principles of the conservation of
energy and of Hamilton teach us more than the fundamental princi-
ples of the classical theory, and exclude certain motions which do not
occur in nature and which would be compatible with the classical
theory. Second, it frees us from the hypothesis of atoms, which it
was almost impossible to avoid with the classical theory. But in its
turn it raises fresh difficulties. The definitions of the two kinds of
energy would raise difficulties almost as great as those of force and
mass in the first system. However, we can get out of these difficulties
more easily, at any rate in the simplest cases. Assume an isolated
system formed of a certain number of material points. Assume that
these points are acted upon by forces depending only on their relative
position and their distances apart, and independent of their velocities.
In virtue of the principle of the conservation of energy there must
be a function of forces. In this simple case the enunciation of the
principle of the conservation of energy is of extreme simplicity. A
certain quantity, which may be determined by experiment, must re-
main constant. This quantity is the sum of two terms. The first
depends only on the position of the material points, and is inde-
pendent of their velocities; the second is proportional to the squares

of these velocities. This decomposition can only take place in one way. The first of these terms, which I shall call U, will be potential energy; the second, which I shall call T, will be kinetic energy. It is true that if T+U is constant, so is any function of T+U, ϕ (T+U). But this function ϕ (T+U) will not be the sum of two terms, the one independent of the velocities, and the other proportional to the square of the velocities. Among the functions which remain constant there is only one which enjoys this property. It is T+U (or a linear function of T+U), it matters not which, since this linear function may always be reduced to T+U by a change of unit and of origin. This, then, is what we call energy. The first term we shall call potential energy, and the second kinetic energy. The definition of the two kinds of energy may therefore be carried through without any ambiguity.

So it is with the definition of mass. Kinetic energy, or *vis viva*, is expressed very simply by the aid of the masses, and of the relative velocities of all the material points with reference to one of them. These relative velocities may be observed, and when we have the expression of the kinetic energy as a function of these relative velocities, the co-efficients of this expression will give us the masses. So in this simple case the fundamental ideas can be defined without difficulty. But the difficulties reappear in the more complicated cases if the forces, instead of depending solely on the distances, depend also on the velocities. For example, Weber supposes the mutual action of two electric molecules to depend not only on their distance but on their velocity and on their acceleration. If material points attracted each other according to an analogous law, U would depend on the velocity, and it might contain a term proportional to the square of the velocity. How can we detect among such terms those that arise from T or U? and how, therefore, can we distinguish the two parts of the energy? But there is more than this. How can we define energy itself? We have no reason to take as our definition T + U rather than any other function of T+U, when the property which characterized T+U has disappeared — namely, that of being the sum of two terms of a particular form. But that is not all. We must take account, not only of mechanical energy properly so called, but of the other forms of energy — heat, chemical energy, electrical energy, etc. The principle of the conservation of energy must be written T+U+Q=a constant, where T is the sensible kinetic energy, U the potential energy of position, depending only on the position of the bodies, Q the internal molecular energy under the thermal, chemical, or electrical form. This would be all right if the three terms were absolutely distinct; if T were proportional to the square of the velocities, U independent of these velocities and of the state of the bodies, Q independent of the velocities and of the positions of the bodies, and depending only on

their internal state. The expression for the energy could be decomposed in one way only into three terms of this form. But this is not the case. Let us consider electrified bodies. The electro-static energy due to their mutual action will evidently depend on their charge — i. e., on their state; but it will equally depend on their position. If these bodies are in motion, they will act electro-dynamically on one another, and the electro-dynamic energy will depend not only on their state and their position, but on their velocities. We have therefore no means of making the selection of the terms which should form part of T, and U, and Q, and of separating the three parts of the energy. If T+U+Q is constant, the same is true of any function whatever, ϕ (T+U+Q).

If T+U+Q were of the particular form that I have suggested above, no ambiguity would ensue. Among the functions ϕ (T+U+Q) which remain constant, there is only one that would be of this particular form, namely the one which I would agree to call energy. But I have said this is not rigorously the case. Among the functions that remain constant there is not one which can rigorously be placed in this particular form. How then can we choose from among them that which should be called energy? We have no longer any guide in our choice.

Of the principle of the conservation of energy there is nothing left then but an enunciation: — *There is something which remains constant.* In this form it, in its turn, is outside the bounds of experiment and reduced to a kind of tautology. It is clear that if the world is governed by laws there will be quantities which remain constant. Like Newton's laws, and for an analogous reason, the principle of the conservation of energy being based on experiment, can no longer be invalidated by it.

This discussion shows that, in passing from the classical system to the energetic, an advance has been made, but it shows, at the same time, that we have not advanced far enough.

Another objection seems to be still more serious. The principle of least action is applicable to reversible phenomena, but it is by no means satisfactory as far as irreversible phenomena are concerned. Helmholtz attempted to extend it to this class of phenomena, but he did not and could not succeed. So far as this is concerned all has yet to be done. The very enunciation of the principle of least action is objectionable. To move from one point to another, a material molecule, acted upon by no force, but compelled to move on a surface, will take as its path the geodesic line — i.e., the shortest path. This molecule seems to know the point to which we want to take it, to foresee the time that it will take it to reach it by such a path, and then to know how to choose the most convenient path. The enunciation of the principle presents it to us, so to speak, as a living and free entity.

It is clear that it would be better to replace it by a less objectionable enunciation, one in which, as philosophers would say, final effects do not seem to be substituted for acting causes.

Thermo-dynamics. — The rôle of the two fundamental principles of thermo-dynamics becomes daily more important in all branches of natural philosophy. Abandoning the ambitious theories of forty years ago, encumbered as they were with molecular hypotheses, we now try to rest on thermo-dynamics alone the entire edifice of mathematical physics. Will the two principles of Mayer and of Clausius assure to it foundations solid enough to last for some time? We all feel it, but whence does our confidence arise? An eminent physicist said to me one day, *àpropos* of the law of errors: — every one stoutly believes it, because mathematicians imagine that it is an effect of observation, and observers imagine that it is a mathematical theorem. And this was for a long time the case with the principle of the conservation of energy. It is no longer the same now. There is no one who does not know that it is an experimental fact. But then who gives us the right of attributing to the principle itself more generality and more precision than to the experiments which have served to demonstrate it? This is asking, if it is legitimate to generalize, as we do every day, empiric data, and I shall not be so foolhardy as to discuss this question, after so many philosophers have vainly tried to solve it. One thing alone is certain. If this permission were refused to us, science could not exist; or at least would be reduced to a kind of inventory, to the ascertaining of isolated facts. It would no longer be to us of any value, since it could not satisfy our need of order and harmony, and because it would be at the same time incapable of prediction. As the circumstances which have preceded any fact whatever will never again, in all probability, be simultaneously reproduced, we already require a first generalization to predict whether the fact will be renewed as soon as the least of these circumstances is changed. But every proposition may be generalized in an infinite number of ways. Among all possible generalizations we must choose, and we cannot but choose the simplest. We are therefore led to adopt the same course as if a simple law were, other things being equal, more probable than a complex law. A century ago it was frankly confessed and proclaimed abroad that Nature loves simplicity; but Nature has proved the contrary since then on more than one occasion. We no longer confess this tendency, and we only keep of it what is indispensable, so that science may not become impossible. In formulating a general, simple, and formal law, based on a comparatively small number of not altogether consistent experiments, we have only obeyed a necessity from which the human mind cannot free itself. But there is something more, and that is why I dwell on this topic. No one doubts that Mayer's principle is not called upon to survive all the particular

laws from which it was deduced, in the same way that Newton's law has survived the laws of Kepler from which it was derived, and which are no longer anything but approximations, if we take perturbations into account. Now why does this principle thus occupy a kind of privileged position among physical laws? There are many reasons for that. At the outset we think that we cannot reject it, or even doubt, its absolute rigor, without admitting the possibility of perpetual motion; we certainly feel distrust at such a prospect, and we believe ourselves less rash in affirming it than in denying it. That perhaps is not quite accurate. The impossibility of perpetual motion only implies the conservation of energy for reversible phenomena. The imposing simplicity of Mayer's principle equally contributes to strengthen our faith. In a law immediately deduced from experiments, such as Mariotte's law, this simplicity would rather appear to us a reason for distrust; but here this is no longer the case. We take elements which at the first glance are unconnected; these arrange themselves in an unexpected order, and form a harmonious whole. We cannot believe that this unexpected harmony is a mere result of chance. Our conquest appears to be valuable to us in proportion to the efforts it has cost, and we feel the more certain of having snatched its true secret from Nature in proportion as Nature has appeared more jealous of our attempts to discover it. But these are only small reasons. Before we raise Mayer's law to the dignity of an absolute principle, a deeper discussion is necessary. But if we embark on this discussion we see that this absolute principle is not even easy to enunciate. In every particular case we clearly see what energy is, and we can give it at least a provisory definition; but it is impossible to find a general definition of it. If we wish to enunciate the principle in all its generality and apply it to the universe, we see it vanish, so to speak, and nothing is left but this — there is something which remains constant. But has this a meaning? In the determinist hypothesis the state of the universe is determined by an extremely large number n of parameters, which I shall call $x_1, x_2, x_3 \ldots xn$. As soon as we know at a given moment the values of these n parameters, we also know their derivatives with respect to time, and we can therefore calculate the values of these same parameters at an anterior or ulterior moment. In other words, these n parameters specify n differential equations of the first order. These equations have $n-1$ integrals, and therefore there are $n-1$ functions of $x_1, x_2, x_3, \ldots xn$, which remain constant. If we say then, *there is something which remains constant,* we are only enunciating a tautology. We would be even embarrassed to decide which among all our integrals is that which should retain the name of energy. Besides, it is not in this sense that Mayer's principle is understood when it is applied to a limited system. We admit, then, that p of our n parameters vary independently so that we have only

n—p relations, generally linear, between our n parameters and their derivatives. Suppose, for the sake of simplicity, that the sum of the work done by the external forces is zero, as well as that of all the quantities of heat given off from the interior: what will then be the meaning of our principle? *There is a combination of these n—p relations, of which the first member is an exact differential;* and then this differential vanishing in virtue of our n—p relations, its integral is a constant, and it is this integral which we call energy. But how can it be that there are several parameters whose variations are independent? That can only take place in the case of external forces (although we have supposed, for the sake of simplicity, that the algebraical sum of all the work done by these forces has vanished). If, in fact, the system were completely isolated from all external action, the values of our n parameters at a given moment would suffice to determine the state of the system at any ulterior moment whatever, provided that we still clung to the determinist hypothesis. We should therefore fall back on the same difficulty as before. If the future state of the system is not entirely determined by its present state, it is because it further depends on the state of bodies external to the system. But then, is it likely that there exist among the parameters x which define the state of the system of equations independent of this state of the external bodies? and if in certain cases we think we can find them, is it not only because of our ignorance, and because the influence of these bodies is too weak for our experiment to be able to detect it? If the system is not regarded as completely isolated, it is probable that the rigorously exact expression of its internal energy will depend upon the state of the external bodies. Again, I have supposed above that the sum of all the external work is zero, and if we wish to be free from this rather artificial restriction the enunciation becomes still more difficult. To formulate Mayer's principle by giving it an absolute meaning, we must extend it to the whole universe, and then we find ourselves face to face with the very difficulty we have endeavored to avoid. To sum up, and to use ordinary language, the law of the conservation of energy can have only one significance, because there is in it a property common to all possible properties; but in the determinist hypothesis there is only one possible, and then the law has no meaning. In the indeterminist hypothesis, on the other hand, it would have a meaning even if we wished to regard it in an absolute sense. It would appear as a limitation imposed on freedom.

But this word warns me that I am wandering from the subject, and that I am leaving the domain of mathematics and physics. I check myself, therefore, and I wish to retain only one impression of the whole of this discussion, and that is, that Mayer's law is a form subtle enough for us to be able to put into it almost anything we like. I do not mean by that that it corresponds to no objective reality, nor that it

is reduced to mere tautology; since, in each particular case, and provided we do not wish to extend it to the absolute, it has a perfectly clear meaning. This subtlety is a reason for believing that it will last long; and as, on the other hand, it will only disappear to be blended in a higher harmony, we may work with confidence and utilize it, certain beforehand that our work will not be lost.

Almost everything that I have just said applies to the principle of Clausius. What distinguishes it is, that it is expressed by an inequality. It will be said perhaps that it is the same with all physical laws, since their precision is always limited by errors of observation. But they at least claim to be first approximations, and we hope to replace them little by little by more exact laws. If, on the other hand, the principle of Clausius reduces to an inequality, this is not caused by the imperfection of our means of observation, but by the very nature of the question.

General Conclusions on Part III. — The principles of mechanics are therefore presented to us under two different aspects. On the one hand, there are truths founded on experiment, and verified approximately as far as almost isolated systems are concerned; on the other hand, there are postulates applicable to the whole of the universe and regarded as rigorously true. If these postulates possess a generality and a certainty which falsify the experimental truths from which they were deduced, it is because they reduce in final analysis to a simple convention that we have a right to make, because we are certain beforehand that no experiment can contradict it. This convention, however, is not absolutely arbitrary; it is not the child of our caprice. We admit it because certain experiments have shown us that it will be convenient, and thus is explained how experiment has built up the principles of mechanics, and why, moreover, it cannot reverse them. Take a comparison with geometry. The fundamental propositions of geometry, for instance, Euclid's postulate, are only conventions, and it is quite as unreasonable to ask if they are true or false as to ask if the metric system is true or false. Only, these conventions are convenient, and there are certain experiments which prove it to us. At the first glance, the analogy is complete, the rôle of experiment seems the same. We shall therefore be tempted to say, either mechanics must be looked upon as experimental science and then it should be the same with geometry; or, on the contrary, geometry is a deductive science, and then we can say the same of mechanics. Such a conclusion would be illegitimate. The experiments which have led us to adopt as more convenient the fundamental conventions of geometry refer to bodies which have nothing in common with those that are studied by geometry. They refer to the properties of solid bodies and to the propagation of light in a straight line. These are mechanical, optical experiments. In no way can they be regarded as geometrical experiments.

And even the probable reason why our geometry seems convenient to us is, that our bodies, our hands, and our limbs enjoy the properties of solid bodies. Our fundamental experiments are pre-eminently physiological experiments which refer, not to the space which is the object that geometry must study, but to our body — that is to say, to the instrument which we use for that study. On the other hand, the fundamental conventions of mechanics and the experiments which prove to us that they are convenient, certainly refer to the same objects or to analogous objects. Conventional and general principles are the natural and direct generalizations of experimental and particular principles. Let it not be said that I am thus tracing artificial frontiers between the sciences; that I am separating by a barrier geometry properly so called from the study of solid bodies. I might just as well raise a barrier between experimental mechanics and the conventional mechanics of general principles. Who does not see, in fact, that by separating these two sciences we mutilate both, and that what will remain of the conventional mechanics when it is isolated will be but very little, and can in no way be compared with that grand body of doctrine which is called geometry.

We now understand why the teaching of mechanics should remain experimental. Thus only can we be made to understand the genesis of the science, and that is indispensable for a complete knowledge of the science itself. Besides, if we study mechanics, it is in order to apply it; and we can only apply it if it remains objective. Now, as we have seen, when principles gain in generality and certainty they lose in objectivity. It is therefore especially with the objective side of principles that we must be early familiarized, and this can only be by passing from the particular to the general, instead of from the general to the particular.

Principles are conventions and definitions in disguise. They are, however, deduced from experimental laws, and these laws have, so to speak, been erected into principles to which our mind attributes an absolute value. Some philosophers have generalized far too much. They have thought that the principles were the whole of science, and therefore that the whole of science was conventional. This paradoxical doctrine, which is called Nominalism, cannot stand examination. How can a law become a principle? It expressed a relation between two real terms, A and B; but it was not rigorously true, it was only approximate. We introduce arbitrarily an intermediate term, C, more or less imaginary, and C is *by definition* that which has with A *exactly* the relation expressed by the law. So our law is decomposed into an absolute and rigorous principle which expresses the relation of A to C, and an approximate experimental and revisable law which expresses the relation of C to B. But it is clear that however far this decomposition may be carried, laws will always remain. We shall now enter into the domain of laws properly so called.

PART IV. — NATURE

Hypotheses in Physics

The Rôle of Experiment and Generalization. — Experiment is the sole source of truth. It alone can teach us something new; it alone can give us certainty. These are two points that cannot be questioned. But then, if experiment is everything, what place is left for mathematical physics? What can experimental physics do with such an auxiliary — an auxiliary, moreover, which seems useless, and even may be dangerous?

However, mathematical physics exists. It has rendered undeniable service, and that is a fact which has to be explained. It is not sufficient merely to observe; we must use our observations, and for that purpose we must generalize. This is what has always been done, only as the recollection of past errors has made man more and more circumspect, he has observed more and more and generalized less and less. Every age has scoffed at its predecessor, accusing it of having generalizet too boldly and too naïvely. Descartes used to commiserate the Ionians. Descartes in his turn makes us smile, and no doubt some day our children will laugh at us. Is there no way of getting at once to the gist of the matter, and thereby escaping the raillery which we foresee? Cannot we be content with experiment alone? No, that is impossible; that would be a complete misunderstanding of the true character of science. The man of science must work with method. Science is built up of facts, as a house is built of stones; but an accumulation of facts is no more a science than a heap of stones is a house. Most important of all, the man of science must exhibit foresight. Carlyle has written somewhere something after this fashion. " Nothing but facts are of importance. John Lackland passed by here. Here is something that is admirable. Here is a reality for which I would give all the theories in the world." [1] Carlyle was a compatriot of Bacon, and, like him, he wished to proclaim his worship of *the God of Things as they are.*

But Bacon would not have said that. That is the language of the historian. The physicist would most likely have said: " John Lack-

[1] V. *Past and Present,* end of Chapter I., Book II.— [TR.]

land passed by here. It is all the same to me, for he will not pass
this way again."

We all know that there are good and bad experiments. The latter
accumulate in vain. Whether there are a hundred or a thousand,
one single piece of work by a real master — by a Pasteur, for example
— will be sufficient to sweep them into oblivion. Bacon would have
thoroughly understood that, for he invented the phrase *experimentum
crucis;* but Carlyle would not have understood it. A fact is a fact.
A student has read such and such a number on his thermometer. He
has taken no precautions. It does not matter; he has read it, and if
it is only the fact which counts, this is a reality that is as much
entitled to be called a reality as the peregrinations of King John
Lackland. What, then, is a good experiment? It is that which
teaches us something more than an isolated fact. It is that which
enables us to predict, and to generalize. Without generalization, pre-
diction is impossible. The circumstances under which one has op-
erated will never again be reproduced simultaneously. The fact ob-
served will never be repeated. All that can be affirmed is that under
analogous circumstances an analogous fact will be produced. To pre-
dict it, we must therefore invoke the aid of analogy — that is to say,
even at this stage, we must generalize. However timid we may be,
there must be interpolation. Experiment only gives us a certain num-
ber of isolated points. They must be connected by a continuous line,
and this is a true generalization. But more is done. The curve thus
traced will pass between and near the points observed; it will not
pass through the points themselves. Thus we are not restricted to
generalizing our experiment, we correct it; and the physicist who
would abstain from these corrections, and really content himself with
experiment pure and simple, would be compelled to enunciate very
extraordinary laws indeed. Detached facts cannot therefore satisfy us,
and that is why our science must be ordered, or, better still, general-
ized.

It is often said that experiments should be made without precon-
ceived ideas. That is impossible. Not only would it make every
experiment fruitless, but even if we wished to do so, it could not be
done. Every man has his own conception of the world, and this he
cannot so easily lay aside. We must, for example, use language, and
our language is necessarily steeped in preconceived ideas. Only they
are unconscious preconceived ideas, which are a thousand times the
most dangerous of all. Shall we say, that if we cause others to inter-
vene of which we are fully conscious, that we shall only aggravate the
evil? I do not think so. I am inclined to think that they will serve
as ample counterpoises — I was almost going to say antidotes. They
will generally disagree, they will enter into conflict one with another,
and *ipso facto,* they will force us to look at things under different

aspects. This is enough to free us. He is no longer a slave who can choose his master.

Thus, by generalization, every fact observed enables us to predict a large number of others; only we ought not to forget that the first alone is certain, and that all the others are merely probable. However solidly founded a prediction may appear to us, we are never *absolutely* sure that experiment will not prove it to be baseless if we set to work to verify it. But the probability of its accuracy is often so great that practically we may be content with it. It is far better to predict without certainty, than never to have predicted at all. We should never, therefore, disdain to verify when the opportunity presents itself. But every experiment is long and difficult, and the laborers are few, and the number of facts which we require to predict is enormous; and besides this mass, the number of direct verifications that we can make will never be more than a negligible quantity. Of this little that we can directly attain we must choose the best. Every experiment must enable us to make a maximum number of predictions having the highest possible degree of probability. The problem is, so to speak, to increase the output of the scientific machine. I may be permitted to compare science to a library which must go on increasing indefinitely; the librarian has limited funds for his purchases, and he must, therefore, strain every nerve not to waste them. Experimental physics has to make the purchases, and experimental physics alone can enrich the library. As for mathematical physics, her duty is to draw up the catalogue. If the catalogue is well done the library is none the richer for it; but the reader will be enabled to utilize its riches; and also by showing the librarian the gaps in his collection, it will help him to make a judicious use of his funds, which is all the more important, inasmuch as those funds are entirely inadequate. That is the rôle of mathematical physics. It must direct generalization, so as to increase what I called just now the output of science. By what means it does this, and how it may do it without danger, is what we have now to examine.

The Unity of Nature. — Let us first of all observe that every generalization supposes in a certain measure a belief in the unity and simplicity of Nature. As far as the unity is concerned, there can be no difficulty. If the different parts of the universe were not as the organs of the same body, they would not re-act one upon the other; they would mutually ignore each other, and we in particular should only know one part. We need not, therefore, ask if Nature is one, but how she is one.

As for the second point, that is not so clear. It is not certain that Nature is simple. Can we without danger act as if she were?

There was a time when the simplicity of Mariotte's law was an argument in favor of its accuracy: when Fresnel himself, after having

said in a conversation with Laplace that Nature cares naught for analytical difficulties, was compelled to explain his words so as not to give offence to current opinion. Nowadays, ideas have changed considerably; but those who do not believe that natural laws must be simple, are still often obliged to act as if they did believe it. They cannot entirely dispense with this necessity without making all generalization, and therefore all science, impossible. It is clear that any fact can be generalized in an infinite number of ways, and it is a question of choice. The choice can only be guided by considerations of simplicity. Let us take the most ordinary case, that of interpolation. We draw a continuous line as regularly as possible between the points given by observation. Why do we avoid angular points and inflections that are too sharp? Why do we not make our curve describe the most capricious zigzags? It is because we know beforehand, or think we know, that the law we have to express cannot be so complicated as all that. The mass of Jupiter may be deduced either from the movements of his satellites, or from the perturbations of the major planets, or from those of the minor planets. If we take the mean of the determinations obtained by these three methods, we find three numbers very close together, but not quite identical. This result might be interpreted by supposing that the gravitation constant is not the same in the three cases; the observations would be certainly much better represented. Why do we reject this interpretation? Not because it is absurd, but because it is uselessly complicated. We shall only accept it when we are forced to, and it is not imposed upon us yet. To sum up, in most cases every law is held to be simple until the contrary is proved.

This custom is imposed upon physicists by the reasons that I have indicated, but how can it be justified in the presence of discoveries which daily show us fresh details, richer and more complex? How can we even reconcile it with the unity of nature? For if all things are interdependent, the relations in which so many different objects intervene can no longer be simple.

If we study the history of science we see produced two phenomena which are, so to speak, each the inverse of the other. Sometimes it is simplicity which is hidden under what is apparently complex; sometimes, on the contrary, it is simplicity which is apparent, and which conceals extremely complex realities. What is there more complicated than the disturbed motions of the planets, and what more simple than Newton's law? There, as Fresnel said, Nature playing with analytical difficulties, only uses simple means, and creates by their combination I know not what tangled skein. Here it is the hidden simplicity which must be disentangled. Examples to the contrary abound. In the kinetic theory of gases, molecules of tremendous velocity are discussed, whose paths, deformed by incessant impacts, have the most

capricious shapes, and plough their way through space in every direction. The result observable is Mariotte's simple law. Each individual fact was complicated. The law of great numbers has re-established simplicity in the mean. Here the simplicity is only apparent, and the coarseness of our senses alone prevents us from seeing the complexity.

Many phenomena obey a law of proportionality. But why? Because in these phenomena there is something which is very small. The simple law observed is only the translation of the general analytical rule by which the infinitely small increment of a function is proportional to the increment of the variable. As in reality our increments are not infinitely small, but only very small, the law of proportionality is only approximate, and simplicity is only apparent. What I have just said applies to the law of the superposition of small movements, which is so fruitful in its applications and which is the foundation of optics.

And Newton's law itself? Its simplicity, so long undetected, is perhaps only apparent. Who knows if it be not due to some complicated mechanism, to the impact of some subtle matter animated by irregular movements, and if it has not become simple merely through the play of averages and large numbers? In any case, it is difficult not to suppose that the true law contains complementary terms which may become sensible at small distances. If in astronomy they are negligible, and if the law thus regains its simplicity, it is solely on account of the enormous distances of the celestial bodies. No doubt, if our means of investigation became more and more penetrating, we should discover the simple beneath the complex, and then the complex from the simple, and then again the simple beneath the complex, and so on, without ever being able to predict what the last term will be. We must stop somewhere, and for science to be possible we must stop where we have found simplicity. That is the only ground on which we can erect the edifice of our generalizations. But, this simplicity being only apparent, will the ground be solid enough? That is what we have now to discover.

For this purpose let us see what part is played in our generalizations by the belief in simplicity. We have verified a simple law in a considerable number of particular cases. We refuse to admit that this coincidence, so often repeated, is a result of mere chance, and we conclude that the law must be true in the general case.

Kepler remarks that the positions of a planet observed by Tycho are all on the same ellipse. Not for one moment does he think that, by a singular freak of chance, Tycho had never looked at the heavens except at the very moment when the path of the planet happened to cut that ellipse. What does it matter then if the simplicity be real or if it hide a complex truth? Whether it be due to the influence of great numbers which reduces individual differences to a level, or to the

greatness or the smallness of certain quantities which allow of certain terms to be neglected — in no case is it due to chance. This simplicity, real or apparent, has always a cause. We shall therefore always be able to reason in the same fashion, and if a simple law has been observed in several particular cases, we may legitimately suppose that it still will be true in analogous cases. To refuse to admit this would be to attribute an inadmissible rôle to chance. However, there is a difference. If the simplicity were real and profound it would bear the test of the increasing precision of our methods of measurement. If, then, we believe Nature to be profoundly simple, we must conclude that it is an approximate and not a rigorous simplicity. This is what was formerly done, but it is what we have no longer the right to do. The simplicity of Kepler's laws, for instance, is only apparent; but that does not prevent them from being applied to almost all systems analogous to the solar system, though that prevents them from being rigorously exact.

Rôle of Hypothesis. — Every generalization is a hypothesis. Hypothesis therefore plays a necessary rôle, which no one has ever contested. Only, it should always be as soon as possible submitted to verification. It goes without saying that, if it cannot stand this test, it must be abandoned without any hesitation. This is, indeed, what is generally done; but sometimes with a certain impatience. Ah well! this impatience is not justified. The physicist who has just given up one of his hypotheses should, on the contrary, rejoice, for he found an unexpected opportunity of discovery. His hypothesis, I imagine, had not been lightly adopted. It took into account all the known factors which seem capable of intervention in the phenomenon. If it is not verified it is because there is something unexpected and extraordinary about it, because we are on the point of finding something unknown and new. Has the hypothesis thus rejected been sterile? Far from it. It may be even said that it has rendered more service than a true hypothesis. Not only has it been the occasion of a decisive experiment, but if this experiment had been made by chance, without the hypothesis, no conclusion could have been drawn; nothing extraordinary would have been seen; and only one fact the more would have been catalogued, without deducing from it the remotest consequence.

Now, under what conditions is the use of hypothesis without danger? The proposal to submit all to experiment is not sufficient. Some hypotheses are dangerous, — first and foremost those which are tacit and unconscious. And since we make them without knowing them, we cannot get rid of them. Here again, there is a service that mathematical physics may render us. By the precision which is its characteristic, we are compelled to formulate all the hypotheses that we would unhesitatingly make without its aid. Let us also notice that it is

important not to multiply hypotheses indefinitely. If we construct a theory based upon multiple hypotheses, and if experiment condemns it, which of the premises must be changed? It is impossible to tell. Conversely, if the experiment succeeds, must we suppose that it has verified all these hypotheses at once? Can several unknowns be determined from a single equation?

We must also take care to distinguish between the different kinds of hypotheses. First of all, there are those which are quite natural and necessary. It is difficult not to suppose that the influence of very distant bodies is quite negligible, that small movements obey a linear law, and that effect is a continuous function of its cause. I will say as much for the conditions imposed by symmetry. All these hypotheses affirm, so to speak, the common basis of all the theories of mathematical physics. They are the last that should be abandoned. There is a second category of hypotheses which I shall qualify as indifferent. In most questions the analyst assumes, at the beginning of his calculations, either that matter is continuous, or the reverse, that it is formed of atoms. In either case, his results would have been the same. On the atomic supposition he has a little more difficulty in obtaining them — that is all. If, then, experiment confirms his conclusions, will he suppose that he has proved, for example, the real existence of atoms?

In optical theories two vectors are introduced, one of which we consider as a velocity and the other as a vortex. This again is an indifferent hypothesis, since we should have arrived at the same conclusions by assuming the former to be a vortex and the latter to be a velocity. The success of the experiment cannot prove, therefore, that the first vector is really a velocity. It only proves one thing — namely, that it is a vector; and that is the only hypothesis that has really been introduced into the premisses. To give it the concrete appearance that the fallibility of our minds demands, it was necessary to consider it either as a velocity or as a vortex. In the same way, it was necessary to represent it by an x or a y, but the result will not prove that we were right or wrong in regarding it as a velocity; nor will it prove we are right or wrong in calling it x and not y.

These indifferent hypotheses are never dangerous provided their characters are not misunderstood. They may be useful, either as artifices for calculation, or to assist our understanding by concrete images, to fix the ideas, as we say. They need not therefore be rejected. The hypotheses of the third category are real generalizations. They must be confirmed or invalidated by experiment. Whether verified or condemned, they will always be fruitful; but, for the reasons I have given, they will only be so if they are not too numerous.

Origin of Mathematical Physics. — Let us go further and study more closely the conditions which have assisted the development of

mathematical physics. We recognize at the outset that the efforts of men of science have always tended to resolve the complex phenomenon given directly by experiment into a very large number of elementary phenomena, and that in three different ways.

First, with respect to time. Instead of embracing in its entirety the progressive development of a phenomenon, we simply try to connect each moment with the one immediately preceding. We admit that the present state of the world only depends on the immediate past, without being directly influenced, so to speak, by the recollection of a more distant past. Thanks to this postulate, instead of studying directly the whole succession of phenomena, we may confine ourselves to writing down its *differential equation;* for the laws of Kepler we substitute the law of Newton.

Next, we try to decompose the phenomena in space. What experiment gives us is a confused aggregate of facts spread over a scene of considerable extent. We must try to deduce the elementary phenomenon, which will still be localized in a very small region of space.

A few examples perhaps will make my meaning clearer. If we wished to study in all its complexity the distribution of temperature in a cooling solid, we could never do so. This is simply because, if we only reflect that a point in the solid can directly impart some of its heat to a neighboring point, it will immediately impart that heat only to the nearest points, and it is but gradually that the flow of heat will reach other portions of the solid. The elementary phenomenon is the interchange of heat between two contiguous points. It is strictly localized and relatively simple if, as is natural, we admit that it is not influenced by the temperature of the molecules whose distance apart is small.

I bend a rod: it takes a very complicated form, the direct investigation of which would be impossible. But I can attack the problem, however, if I notice that its flexure is only the resultant of the deformations of the very small elements of the rod, and that the deformation of each of these elements only depends on the forces which are directly applied to it, and not in the least on those which may be acting on the other elements.

In all these examples, which may be increased without difficulty, it is admitted that there is no action at a distance or at great distances. That is an hypothesis. It is not always true, as the law of gravitation proves. It must therefore be verified. If it is confirmed, even approximately, it is valuable, for it helps us to use mathematical physics, at any rate by successive approximations. If it does not stand the test, we must seek something else that is analogous, for there are other means of arriving at the elementary phenomenon. If several bodies act simultaneously, it may happen that their actions are independent, and may be added one to the other, either as vectors or as

scalar quantities. The elementary phenomenon is then the action of an isolated body. Or suppose, again, it is a question of small movements, or more generally of small variations which obey the well-known law of mutual or relative independence. The movement observed will then be decomposed into simple movements — for example, sound into its harmonics, and white light into its monochromatic components. When we have discovered in which direction to seek for the elementary phenomenon, by what means may we reach it? First, it will often happen that in order to predict it, or rather in order to predict what is useful to us, it will not be necessary to know its mechanism. The law of great numbers will suffice. Take for example the propagation of heat. Each molecule radiates towards its neighbor — we need not inquire according to what law; and if we make any supposition in this respect, it will be an indifferent hypothesis, and therefore useless and unverifiable. In fact, by the action of averages and thanks to the symmetry of the medium, all differences are levelled, and, whatever the hypothesis may be, the result is always the same.

The same feature is presented in the theory of elasticity, and in that of capillarity. The neighboring molecules attract and repel each other, we need not inquire by what law. It is enough for us that this attraction is sensible at small distances only, and that the molecules are very numerous, that the medium is symmetrical, and we have only to let the law of great numbers come into play.

Here again the simplicity of the elementary phenomenon is hidden beneath the complexity of the observable resultant phenomenon; but in its turn this simplicity was only apparent and disguised a very complex mechanism. Evidently the best means of reaching the elementary phenomenon would be experiment. It would be necessary by experimental artifices to dissociate the complex system which nature offers for our investigations and carefully to study the elements as dissociated as possible; for example, natural white light would be decomposed into monochromatic lights by the aid of the prism, and into polarized lights by the aid of the polarizer. Unfortunately, that is neither always possible nor always sufficient, and sometimes the mind must run ahead of experiment. I shall only give one example which has always struck me rather forcibly. If I decompose white light, I shall be able to isolate a portion of the spectrum, but however small it may be, it will always be a certain width. In the same way the natural lights which are called *monochromatic* give us a very fine a ray, but a ray which is not, however, infinitely fine. It might be supposed that in the experimental study of the properties of these natural lights, by operating with finer and finer rays, and passing on at last to the limit, so to speak, we should eventually obtain the properties of a rigorously monochromatic light. That would not be accurate. I assume that two rays emanate from the same source, that they are first polar-

ized in planes at right angles, that they are then brought back again to the same plane of polarization, and that we try to obtain interference. If the light were *rigorously* monochromatic, there would be interference; but with our nearly monochromatic lights, there will be no interference, and that, however narrow the ray may be. For it to be otherwise, the ray would have to be several million times finer than the finest known rays.

Here then we should be led astray by proceeding to the limit. The mind has to run ahead of the experiment, and if it has done so with success, it is because it has allowed itself to be guided by the instinct of simplicity. The knowledge of the elementary fact enables us to state the problem in the form of an equation. It only remains to deduce from it by combination the observable and verifiable complex fact. That is what we call *integration,* and it is the province of the mathematician. It might be asked, why in physical science generalization so readily takes the mathematical form. The reason is now easy to see. It is not only because we have to express numerical laws; it is because the observable phenomenon is due to the superposition of a large number of elementary phenomena which are *all similar to each other;* and in this way differential equations are quite naturally introduced. It is not enough that each elementary phenomenon should obey simple laws: all those that we have to combine must obey the same law; then only is the intervention of mathematics of any use. Mathematics teaches us, in fact, to combine like with like. Its object is to define the result of a combination without having to reconstruct that combination element by element. If we have to repeat the same operation several times, mathematics enables us to avoid this repetition by telling the result beforehand by a kind of induction. This I have explained before in the chapter on mathematical reasoning. But for that purpose all these operations must be similar; in the contrary case we must evidently make up our minds to working them out in full one after the other, and mathematics will be useless. It is therefore, thanks to the approximate homogeneity of the matter studied by physicists, that mathematical physics came into existence. In the natural sciences the following conditions are no longer to be found: — homogeneity, relative independence of remote parts, simplicity of the elementary fact; and that is why the student of natural science is compelled to have recourse to other modes of generalization.

The Theories of Modern Physics

Significance of Physical Theories. — The ephemeral nature of scientific theories takes by surprise the man of the world. Their brief period of prosperity ended, he sees them abandoned one after another; he sees ruins piled upon ruins; he predicts that the theories in fashion

to-day will in a short time succumb in their turn, and he concludes that they are absolutely in vain. This is what he calls the *bankruptcy of science.*

His skepticism is superficial; he does not take into account the object of scientific theories and the part they play, or he would understand that the ruins may be still good for something. No theory seemed established on firmer ground than Fresnel's, which attributed light to the movements of the ether. Then if Maxwell's theory is to-day preferred, does that mean that Fresnel's work was in vain? No; for Fresnel's object was not to know whether there really is an ether, if it is or is not formed of atoms, if these atoms really move in this way or that; his object was to predict optical phenomena.

This Fresnel's theory enables us to do to-day as well as it did before Maxwell's time. The differential equations are always true, they may be always integrated by the same methods, and the results of this integration still preserve their value. It cannot be said that this is reducing physical theories to simple practical recipes; these equations express relations, and if the equations remain true, it is because the relations preserve their reality. They teach us now, as they did then, that there is such and such a relation between this thing and that; only, the something which we then called *motion,* we now call *electric current.* But these are merely names of the images we substituted for the real objects which Nature will hide forever from our eyes. The true relations between these real objects are the only reality we can attain, and the sole condition is that the same relations shall exist between these objects as between the images we are forced to put in their place. If the relations are known to us, what does it matter if we think it convenient to replace one image by another?

That a given periodic phenomenon (an electric oscillation, for instance) is really due to the vibration of a given atom, which, behaving like a pendulum, is really displaced in this manner or that, all this is neither certain nor essential. But that there is between the electric oscillation, the movement of the pendulum, and all periodic phenomena an intimate relationship which corresponds to a profound reality; that this relationship, this similarity, or rather this parallelism, is continued in the details; that it is a consequence of more general principles such as that of the conservation of energy, and that of least action; this we may affirm; this is the truth which will ever remain the same in whatever garb we may see fit to clothe it.

Many theories of dispersion have been proposed. The first were imperfect, and contained but little truth. Then came that of Helmholtz, and this in its turn was modified in different ways; its author himself conceived another theory, founded on Maxwell's principles. But the remarkable thing is, that all the scientists who followed Helmholtz obtain the same equations, although their starting-points were

to all appearance widely separated. I venture to say that these theories are all simultaneously true; not merely because they express a true relation — that between absorption and abnormal dispersion. In the premisses of these theories the part that is true is the part common to all: it is the affirmation of this or that relation between certain things, which some call by one name and some by another.

The kinetic theory of gases has given rise to many objections, to which it would be difficult to find an answer were it claimed that the theory is absolutely true. But all these objections do not alter the fact that it has been useful, particularly in revealing to us one true relation which would otherwise have remained profoundly hidden — the relation between gaseous and osmotic pressures. In this sense, then, it may be said to be true.

When a physicist finds a contradiction between two theories which are equally dear to him, he sometimes says: " Let us not be troubled, but let us hold fast to the two ends of the chain, lest we lose the intermediate links." This argument of the embarrassed theologian would be ridiculous if we were to attribute to physical theories the interpretation given them by the man of the world. In case of contradiction one of them at least should be considered false. But this is no longer the case if we only seek in them what should be sought. It is quite possible that they both express true relations, and that the contradictions only exist in the images we have formed to ourselves of reality. To those who feel that we are going too far in our limitations of the domain accessible to the scientist, I reply: These questions which we forbid you to investigate, and which you so regret, are not only insoluble, they are illusory and devoid of meaning.

Such a philosopher claims that all physics can be explained by the mutual impact of atoms. If he simply means that the same relations obtain between physical phenomena as between the mutual impact of a large number of billiard balls — well and good! this is verifiable, and perhaps is true. But he means something more, and we think we understand him, because we think we know what an impact is. Why? Simply because we have often watched a game of billiards. Are we to understand that God experiences the same sensations in the contemplation of His work that we do in watching a game of billiards? If it is not our intention to give his assertion this fantastic meaning, and if we do not wish to give it the more restricted meaning I have already mentioned, which is the sound meaning, then it has no meaning at all. Hypotheses of this kind have therefore only a metaphorical sense. The scientist should no more banish them than a poet banishes metaphor; but he ought to know what they are worth. They may be useful to give satisfaction to the mind, and they will do no harm as long as they are only indifferent hypotheses.

These considerations explain to us why certain theories, that were

thought to be abandoned and definitively condemned by experiment, are suddenly revived from their ashes and begin a new life. It is because they expressed true relations, and had not ceased to do so when for some reason or other we felt it necessary to enunciate the same relations in another language. Their life had been latent, as it were.

Barely fifteen years ago, was there anything more ridiculous, more quaintly old-fashioned, than the fluids of Coulomb? And yet, here they are re-appearing under the name of *electrons*. In what do these permanently electrified molecules differ from the electric molecules of Coulomb? It is true that in the electrons the electricity is supported by a little, a very little matter; in other words, they have mass. Yet Coulomb did not deny mass to his fluids, or if he did, it was with reluctance. It would be rash to affirm that the belief in electrons will not also undergo an eclipse, but it was none the less curious to note this unexpected renaissance.

But the most striking example is Carnot's principle. Carnot established it, starting from false hypotheses. When it was found that heat was indestructible, and may be converted into work, his ideas were completely abandoned; later, Clausius returned to them, and to him is due their definitive triumph. In its primitive form, Carnot's theory expressed in addition to true relations, other inexact relations, the *débris* of old ideas; but the presence of the latter did not alter the reality of the others. Clausius had only to separate them, just as one lops off dead branches.

The result was the second fundamental law of thermodynamics. The relations were always the same, although they did not hold, at least to all appearance, between the same objects. This was sufficient for the principle to retain its value. Nor have the reasonings of Carnot perished on this account; they were applied to an imperfect conception of matter, but their form — *i.e.*, the essential part of them, remained correct. What I have just said throws some light at the same time on the rôle of general principles, such as those of the principle of least action or of the conservation of energy. These principles are of very great value. They were obtained in the search for what there was in common in the enunciation of numerous physical laws; they thus represent the quintessence of innumerable observations. However, from their very generality results a consequence to which I have called attention in Chapter VIII. — namely, that they are no longer capable of verification. As we cannot give a general definition of energy, the principle of the conservation of energy simply signifies that there is a *something* which remains constant. Whatever fresh notions of the world may be given us by future experiments, we are certain beforehand that there is something which remains constant, and which may be called *energy*. Does this mean that the principle has no meaning and vanishes into

a tautology? Not at all. It means that the different things to which we give the name of *energy* are connected by a true relationship; it affirms between them a real relation. But then, if this principle has a meaning, it may be false; it may be that we have no right to extend indefinitely its applications, and yet it is certain beforehand to be verified in the strict sense of the word. How, then, shall we know when it has been extended as far as is legitimate? Simply when it ceases to be useful to us — *i.e.,*when we can no longer use it to predict correctly new phenomena. We shall be certain in such a case that the relation affirmed is no longer real, for otherwise it would be fruitful; experiment without directly contradicting a new extension of the principle will nevertheless have condemned it.

Physics and Mechanism. — Most theorists have a constant predilection for explanations borrowed from physics, mechanics, or dynamics. Some would be satisfied if they could account for all phenomena by the motion of molecules attracting one another according to certain laws. Others are more exact; they would suppress attractions acting at a distance; their molecules would follow rectilinear paths, from which they would only be deviated by impacts. Others again, such as Hertz, suppress the forces as well, but suppose their molecules subjected to geometrical connections analogous, for instance, to those of articulated systems; thus, they wish to reduce dynamics to a kind of kinematics. In a word, they all wish to bend nature into a certain form, and unless they can do this they cannot be satisfied. Is Nature flexible enough for this?

We shall examine this question hereafter under the head of Maxwell's Theory. Every time that the principles of least action and energy are satisfied, we shall see that not only is there always a mechanical explanation possible, but that there is an unlimited number of such explanations. By means of a well-known theorem due to Königs, it may be shown that we can explain everything in an unlimited number of ways, by connections after the manner of Hertz, or, again, by central forces. No doubt it may be just as easily demonstrated that everything may be explained by simple impacts. For this, let us bear in mind that it is not enough to be content with the ordinary matter of which we are aware by means of our senses, and the movements of which we observe directly. We may conceive of ordinary matter as either composed of atoms, whose internal movements escape us, our senses being able to estimate only the displacement of the whole; or we may imagine one of those subtle fluids, which under the name of *ether* or other names, have from all time played so important a rôle in physical theories. Often we go further, and regard the ether as the only primitive, or even as the only true matter. The more moderate consider ordinary matter to be condensed ether, and there is nothing startling in this conception; but others only re-

duce its importance still further, and see in matter nothing more than the geometrical locus of singularities in the ether. Lord Kelvin, for instance, holds what we call matter to be only the locus of those points at which the ether is animated by vortex motions. Riemann believes it to be the locus of those points at which ether is constantly destroyed; to Wiechert or Larmor, it is the locus of the points at which the ether has undergone a kind of torsion of a very particular kind. Taking any one of these points of view, I ask by what right do we apply to the ether the mechanical properties observed in ordinary matter, which is but false matter? The ancient fluids, caloric, electricity, etc., were abandoned when it was seen that heat is not indestructible. But they were also laid aside for another reason. In materializing them, their individuality was, so to speak, emphasized — gaps were opened between them; and these gaps had to be filled in when the sentiment of the unity of Nature became stronger, and when the intimate relations which connect all the parts were perceived. In multiplying the fluids, not only did the ancient physicists create unnecessary entities, but they destroyed real ties. It is not enough for a theory not to affirm false relations; it must not conceal true relations.

Does our ether actually exist? We know the origin of our belief in the ether. If light takes several years to reach us from a distant star, it is no longer on the star, nor is it on the earth. It must be somewhere, and supported, so to speak, by some material agency.

The same idea may be expressed in a more mathematical and more abstract form. What we note are the changes undergone by the material molecules. We see, for instance, that the photographic plate experiences the consequences of a phenomenon of which the incandescent mass of a star was the scene several years before. Now, in ordinary mechanics, the state of the system under consideration depends only on its state at the moment immediately preceding; the system therefore satisfies certain differential equations. On the other hand, if we did not believe in the ether, the state of the material universe would depend not only on the state immediately preceding, but also on much older states; the system would satisfy equations of finite differences. The ether was invented to escape this breaking down of the laws of general mechanics.

Still, this would only compel us to fill the interplanetary space with ether, but not to make it penetrate into the midst of the material media. Fizeau's experiment goes further. By the interference of rays which have passed through the air or water in motion, it seems to show us two different media penetrating each other, and yet being displaced with respect to each other. The ether is all but in our grasp. Experiments can be conceived in which we come closer still to it. Assume that Newton's principle of the equality of action and re-action is not true if applied to matter *alone,* and that this can be

proved. The geometrical sum of all the forces applied to all the molecules would no longer be zero. If we did not wish to change the whole of the science of mechanics, we should have to introduce the ether, in order that the action which matter apparently undergoes should be counterbalanced by the re-action of matter on something.

Or again, suppose we discover that optical and electrical phenomena are influenced by the motion of the earth. It would follow that those phenomena might reveal to us not only the relative motion of material bodies, but also what would seem to be their absolute motion. Again, it would be necessary to have an ether in order that these so-called absolute movements should not be their displacements with respect to empty space, but with respect to something concrete.

Will this ever be accomplished? I do not think so, and I shall explain why; and yet, it is not absurd, for others have entertained this view. For instance, if the theory of Lorentz, of which I shall speak in more detail in Chapter XIII., were true, Newton's principle would not apply to matter *alone,* and the difference would not be very far from being within reach of experiment. On the other hand, many experiments have been made on the influence of the motion of the earth. The results have always been negative. But if these experiments have been undertaken, it is because we have not been certain beforehand; and indeed, according to current theories, the compensation would be only approximate, and we might expect to find accurate methods giving positive results. I think that such a hope is illusory; it was none the less interesting to show that a success of this kind would, in a certain sense, open to us a new world.

And now allow me to make a digression; I must explain why I do not believe, in spite of Lorentz, that more exact observations will ever make evident anything else but the relative displacements of material bodies. Experiments have been made that should have disclosed the terms of the first order; the results were nugatory. Could that have been by chance? No one has admitted this; a general explanation was sought, and Lorentz found it. He showed that the terms of the first order should cancel each other, but not the terms of the second order. Then more exact experiments were made, which were also negative; neither could this be the result of chance. An explanation was necessary, and was forthcoming; they always are; hypotheses are what we lack the least. But this is not enough. Who is there who does not think that this leaves to chance far too important a rôle? Would it not also be a chance that this singular concurrence should cause a certain circumstance to destroy the terms of the first order, and that a totally different but very opportune circumstance should cause those of the second order to vanish? No; the same explanation must be found for the two cases, and everything tends to show that this explanation would serve equally well for the terms of

the higher order, and that the mutual destruction of these terms will be rigorous and absolute.

The Present State of Physics. — Two opposite tendencies may be distinguished in the history of the development of physics. On the one hand, new relations are continually being discovered between objects which seemed destined to remain forever unconnected; scattered facts cease to be strangers to each other and tend to be marshalled into an imposing synthesis. The march of science is towards unity and simplicity.

On the other hand, new phenomena are continually being revealed; it will be long before they can be assigned their place — sometimes it may happen that to find them a place a corner of the edifice must be demolished. In the same way, we are continually perceiving details ever more varied in the phenomena we know, where our crude senses used to be unable to detect any lack of unity. What we thought to be simple becomes complex, and the march of science seems to be towards diversity and complication.

Here, then, are two opposing tendencies, each of which seems to triumph in turn. Which will win? If the first wins, science is possible; but nothing proves this *à priori*, and it may be that after unsuccessful efforts to bend Nature to our ideal of unity in spite of herself, we shall be submerged by the ever-rising flood of our new riches and compelled to renounce all idea of classification — to abandon our ideal, and to reduce science to the mere recording of innumerable recipes.

In fact, we can give this question no answer. All that we can do is to observe the science of to-day, and compare it with that of yesterday. No doubt after this examination we shall be in a position to offer a few conjectures.

Half-a-century ago hopes ran high indeed. The unity of force had just been revealed to us by the discovery of the conservation of energy and of its transformation. This discovery also showed that the phenomena of heat could be explained by molecular movements. Although the nature of these movements was not exactly known, no one doubted but that they would be ascertained before long. As for light, the work seemed entirely completed. So far as electricity was concerned, there was not so great an advance. Electricity had just annexed magnetism. This was a considerable and a definitive step towards unity. But how was electricity in its turn to be brought into the general unity, and how was it to be included in the general universal mechanism? No one had the slightest idea. As to the possibility of the inclusion, all were agreed; they had faith. Finally, as far as the molecular properties of material bodies are concerned, the inclusion seemed easier, but the details were very hazy. In a word, hopes were vast and strong, but vague.

To-day, what do we see? In the first place, a step in advance — immense progress. The relations between light and electricity are now known; the three domains of light, electricity, and magnetism, formerly separated, are now one; and this annexation seems definitive.

Nevertheless the conquest has caused us some sacrifices. Optical phenomena become particular cases in electric phenomena; as long as the former remained isolated, it was easy to explain them by movements which were thought to be known in all their details. That was easy enough; but any explanation to be accepted must now cover the whole domain of electricity. This cannot be done without difficulty.

The most satisfactory theory is that of Lorentz; it is unquestionably the theory that best explains the known facts, the one that throws into relief the greatest number of known relations, the one in which we find most traces of definitive construction. That it still possesses a serious fault I have shown above. It is in contradiction with Newton's law that action and re-action are equal and opposite — or rather, this principle according to Lorentz cannot be applicable to matter alone; if it be true, it must take into account the action of the ether on matter, and the re-action of the matter on the ether. Now, in the new order, it is very likely that things do not happen in this way.

However this may be, it is due to Lorentz that the results of Fizeau on the optics of moving bodies, the laws of normal and abnormal dispersion and of absorption are connected with each other and with the other properties of the ether, by bonds which no doubt will not be readily severed. Look at the ease with which the new Zeeman phenomenon found its place, and even aided the classification of Faraday's magnetic rotation, which had defied all Maxwell's efforts. This facility proves that Lorentz's theory is not a mere artificial combination which must eventually find its solvent. It will probably have to be modified, but not destroyed.

The only object of Lorentz was to include in a single whole all the optics and electro-dynamics of moving bodies; he did not claim to give a mechanical explanation. Larmor goes further; keeping the essential part of Lorentz's theory, he grafts upon it, so to speak, MacCullagh's ideas on the direction of the movement of the ether. MacCullagh held that the velocity of the ether is the same in magnitude and direction as the magnetic force. Ingenious as is this attempt, the fault in Lorentz's theory remains, and is even aggravated. According to Lorentz, we do not know what the movements of the ether are; and because we do not know this, we may suppose them to be movements compensating those of matter, and re-affirming that action and re-action are equal and opposite. According to Larmor we know the movements of the ether, and we can prove that the compensation does not take place.

If Larmor has failed, as in my opinion he has, does it necessarily follow that a mechanical explanation is impossible? Far from it. I said above that as long as a phenomenon obeys the two principles of energy and least action, so long it allows of an unlimited number of mechanical explanations. And so with the phenomena of optics and electricity.

But this is not enough. For a mechanical explanation to be good it must be simple; to choose it from among all the explanations that are possible there must be other reasons than the necessity of making a choice. Well, we have no theory as yet which will satisfy this condition and consequently be of any use. Are we then to complain? That would be to forget the end we seek, which is not the mechanism; the true and only aim is unity.

We ought therefore to set some limits to our ambition. Let us not seek to formulate a mechanical explanation; let us be content to show that we can always find one if we wish. In this we have succeeded. The principle of the conservation of energy has always been confirmed, and now it has a fellow in the principle of least action, stated in the form appropriate to physics. This has also been verified, at least as far as concerns the reversible phenomena which obey Lagrange's equations — in other words, which obey the most general laws of physics. The irreversible phenomena are much more difficult to bring into line; but they, too, are being co-ordinated and tend to come into the unity. The light which illuminates them comes from Carnot's principle. For a long time thermo-dynamics was confined to the study of the dilatations of bodies and of their change of state. For some time past it has been growing bolder, and has considerably extended its domain. We owe to it the theories of the voltaic cell and of their thermo-electric phenomena; there is not a corner in physics which it has not explored, and it has even attacked chemistry itself. The same laws hold good; everywhere, disguised in some form or other, we find Carnot's principle; everywhere also appears that eminently abstract concept of entropy which is as universal as the concept of energy, and like it, seems to conceal a reality. It seemed that radiant heat must escape, but recently that, too, has been brought under the same laws.

In this way fresh analogies are revealed which may be often pursued in detail; electric resistance resembles the viscosity of fluids; hysteresis would rather be like the friction of solids.. In all cases friction appears to be the type most imitated by the most diverse irreversible phenomena, and this relationship is real and profound.

A strictly mechanical explanation of these phenomena has also been sought, but, owing to their nature, it is hardly likely that it will be found. To find it, it has been necessary to suppose that the irreversibility is but apparent, that the elementary phenomena are reversible

and obey the known laws of dynamics. But the elements are extremely numerous, and become blended more and more, so that to our crude sight all appears to tend towards uniformity — *i.e.,* all seems to progress in the same direction, and that without hope of return. The apparent irreversibility is therefore but an effect of the law of great numbers. Only a being of infinitely subtle senses, such as Maxwell's demon, could unravel this tangled skein and turn back the course of the universe.

This conception, which is connected with the kinetic theory of gases, has cost great effort and has not, on the whole, been fruitful; it may become so. This is not the place to examine if it leads to contradictions, and if it is in conformity with the true nature of things.

Let us notice, however, the original ideas of M. Gouy on the Brownian movement. According to this scientist, this singular movement does not obey Carnot's principle. The particles which it sets moving would be smaller than the meshes of that tightly drawn net; they would thus be ready to separate them, and thereby to set back the course of the universe. One can almost see Maxwell's demon at work. [1]

To resume, phenomena long known are gradually being better classified, but new phenomena come to claim their place, and most of them, like the Zeeman effect, find it at once. Then we have the cathode rays, the X-rays, uranium and radium rays; in fact, a whole world of which none had suspected the existence. How many unexpected guests to find a place for! No one can yet predict the place they will occupy, but I do not believe they will destroy the general unity; I think that they will rather complete it. On the one hand, indeed, the new radiations seem to be connected with the phenomena of luminosity; not only do they excite fluorescence, but they sometimes come into existence under the same conditions as that property; neither are they unrelated to the cause which produces the electric spark under the action of ultra-violet light. Finally, and most important of all, it is believed that in all these phenomena there exist ions, animated, it is true, with velocities far greater than those of electrolytes. All this is very vague, but it will all become clearer.

Phosphorescence and the action of light on the spark were regions rather isolated, and consequently somewhat neglected by investigators. It is to be hoped that a new path will now be made which will facilitate their communications with the rest of science. Not only do we discover new phenomena, but those we think we know are revealed in unlooked-for aspects. In the free ether the laws preserve their majestic simplicity, but matter properly so called seems more and more

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[1] Clerk-Maxwell imagined some supernatural agency at work, sorting molecules in a gas of uniform temperature into (*a*) those possessing kinetic energy above the average, (*b*) those possessing kinetic energy below the average. —[TR.]

complex; all we can say of it is but approximate, and our formulæ are constantly requiring new terms.

But the ranks are unbroken, the relations that we have discovered between objects we thought simple still hold good between the same objects when their complexity is recognized, and that alone is the important thing. Our equations become, it is true, more and more complicated, so as to embrace more closely the complexity of nature; but nothing is changed in the relations which enable these equations to be derived from each other. In a word, the form of these equations persists. Take for instance the laws of reflection. Fresnel established them by a simple and attractive theory which experiment seemed to confirm. Subsequently, more accurate researches have shown that this verification was but approximate; traces of elliptic polarization were detected everywhere. But it is owing to the first approximation that the cause of these anomalies was found in the existence of a transition layer, and all the essentials of Fresnel's theory have remained. We cannot help reflecting that all these relations would never have been noted if there had been doubt in the first place as to the complexity of the objects they connect. Long ago it was said: If Tycho had had instruments ten times as precise, we would never have had a Kepler, or a Newton, or Astronomy. It is a misfortune for a science to be born too late, when the means of observation have become too perfect. That is what is happening at this moment with respect to physical chemistry; the founders are hampered in their general grasp by third and fourth decimal places; happily they are men of robust faith. As we get to know the properties of matter better we see that continuity reigns. From the work of Andrews and Van der Waals, we see how the transition from the liquid to the gaseous state is made, and that it is not abrupt. Similarly, there is no gap between the liquid and solid states, and in the proceedings of a recent Congress we see memoirs on the rigidity of liquids side by side with papers on the flow of solids.

With this tendency there is no doubt a loss of simplicity. Such and such an effect was represented by straight lines; it is now necessary to connect these lines by more or less complicated curves. On the other hand, unity is gained. Separate categories quieted but did not satisfy the mind.

Finally, a new domain, that of chemistry, has been invaded by the method of physics, and we see the birth of physical chemistry. It is still quite young, but already it has enabled us to connect such phenomena as electrolysis, osmosis, and the movements of ions.

From this cursory exposition what can we conclude? Taking all things into account, we have approached the realization of unity. This has not been done as quickly as was hoped fifty years ago, and

the path predicted has not always been followed; but, on the whole, much ground has been gained.

The Calculus of Probabilities

No doubt the reader will be astonished to find reflections on the calculus of probabilities in such a volume as this. What has that calculus to do with physical science? The questions I shall raise — without, however, giving them a solution — are naturally raised by the philosopher who is examining the problems of physics. So far is this the case, that in the two preceding chapters I have several times used the words " probability " and " chance." " Predicted facts," as I said above, " can only be probable." However solidly founded a prediction may appear to be, we are never absolutely certain that experiment will not prove it false; but the probability is often so great that practically it may be accepted. And a little farther on I added: " See what a part the belief in simplicity plays in our generalizations. We have verified a simple law in a large number of particular cases, and we refuse to admit that this so-often repeated coincidence is a mere effect of chance." Thus, in a multitude of circumstances the physicist is often in the same position as the gambler who reckons up his chances. Every time that he reasons by induction, he more or less consciously requires the calculus of probabilities, and that is why I am obliged to open this chapter parenthetically, and to interrupt our discussion of method in the physical sciences in order to examine a little closer what this calculus is worth, and what dependence we may place upon it. The very name of the calculus of probabilities is a paradox. Probability as opposed to certainty is what one does not know, and how can we calculate the unknown? Yet many eminent scientists have devoted themselves to this calculus, and it cannot be denied that science has drawn therefrom no small advantage. How can we explain this apparent contradiction? Has probability been defined? Can it even be defined? And if it cannot, how can we venture to reason upon it? The definition, it will be said, is very simple. The probability of an event is the ratio of the number of cases favorable to the event to the total number of possible cases. A simple example will show how incomplete this definition is: — I throw two dice. What is the probability that one of the two at least turns up a 6? Each can turn up in six different ways; the number of possible cases is $6 \times 6 = 36$. The number of favorable cases is 11; the probability $\frac{11}{36}$. That is the correct solution. But why cannot we just as well proceed as follows? — The points which turn up on the two dice form $\frac{6 \times 7}{2} = 21$ different combinations. Among these combinations, six are favorable; the probability is $\frac{6}{21}$. Now why is the first method of calculating the number of possible cases more legiti-

mate than the second? In any case it is not the definition that tells us. We are therefore bound to complete the definition by saying, " . . . to the total number of possible cases, provided the cases are equally probable." So we are compelled to define the probable by the probable. How can we know that two possible cases are equally probable? Will it be by a convention? If we insert at the beginning of every problem an explicit convention, well and good! We then have nothing to do but to apply the rules of arithmetic and algebra, and we complete our calculation, when our result cannot be called in question. But if we wish to make the slightest application of this result, we must prove that our convention is legitimate, and we shall find ourselves in the presence of the very difficulty we thought we had avoided. It may be said that common-sense is enough to show us the convention that should be adopted. Alas! M. Bertrand has amused himself by discussing the following simple problem : — " What is the probability that a chord of a circle may be greater than the side of the inscribed equilateral triangle ?" The illustrious geometer successively adopted two conventions which seemed to be equally imperative in the eyes of common-sense, and with one convention he finds 1-2, and with the other 1-3. The conclusion which seems to follow from this is that the calculus of probabilities is a useless science, that the obscure instinct which we call common-sense, and to which we appeal for the legitimization of our conventions, must be distrusted. But to this conclusion we can no longer subscribe. We cannot do without that obscure instinct. Without it, science would be impossible, and without it we could neither discover nor apply a law. Have we any right, for instance, to enunciate Newton's law? No doubt numerous observations are in agreement with it, but is not that a simple fact of chance? and how do we know, besides, that this law which has been true for so many generations will not be untrue in the next? To this objection the only answer you can give is: It is very improbable. But grant the law. By means of it I can calculate the position of Jupiter in a year from now. Yet have I any right to say this? Who can tell if a gigantic mass of enormous velocity is not going to pass near the solar system and produce unforeseen perturbations? Here again the only answer is: It is very improbable. From this point of view all the sciences would only be unconscious applications of the calculus of probabilities. And if this calculus be condemned, then the whole of the sciences must also be condemned. I shall not dwell at length on scientific problems in which the intervention of the calculus of probabilities is more evident. In the forefront of these is the problem of interpolation, in which, knowing a certain number of values of a function, we try to discover the intermediary values. I may also mention the celebrated theory of errors of observation, to which I shall return later; the kinetic theory of

gases, a well-known hypothesis wherein each gaseous molecule is sup-
posed to describe an extremely complicated path, but in which,
through the effect of great numbers, the mean phenomena which are
all we observe obey the simple laws of Mariotte and Gay-Lussac.
All these theories are based upon the laws of great numbers, and the
calculus of probabilities would evidently involve them in its ruin.
It is true that they have only a particular interest, and that, save
as far as interpolation is concerned, they are sacrifices to which we
might readily be resigned. But I have said above, it would not be
these partial sacrifices that would be in question; it would be the
legitimacy of the whole of science that would be challenged. I quite
see that it might be said: We do not know, and yet we must act. As
for action, we have not time to devote ourselves to an inquiry that
will suffice to dispel our ignorance. Besides, such an inquiry would
demand unlimited time. We must therefore make up our minds
without knowing. This must be often done whatever may happen,
and we must follow the rules although we may have but little confi-
dence in them. What I know is, not that such a thing is true, but
that the best course for me is to act as if it were true. The calculus
of probabilities, and therefore science itself, would be no longer of
any practical value.

Unfortunately the difficulty does not thus disappear. A gambler
wants to try a *coup,* and he asks my advice. If I give it him, I use
the calculus of probabilities; but I shall not guarantee success. That
is what I shall call *subjective probability.* In this case we might be
content with the explanation of which I have just given a sketch.
But assume that an observer is present at the play, that he knows of
the *coup,* and that play goes on for a long time, and that he makes
a summary of his notes. He will find that events have taken place
in conformity with the laws of the calculus of probabilities. That
is what I shall call *objective probability,* and it is this phenomenon
which has to be explained. There are numerous Insurance Societies
which apply the rules of calculus of probabilities, and they distribute
to their shareholders dividends, the objective reality of which cannot
be contested. In order to explain them, we must do more than invoke
our ignorance and the necessity of action. Thus, absolute scepti-
cism is not admissible. We may distrust, but we cannot condemn
en bloc. Discussion is necessary.

I. *Classification of the Problems of Probability.* — In order to
classify the problems which are presented to us with reference to
probabilities, we must look at them from different points of view, and
first of all, from that of *generality.* I said above that probability is
the ratio of the number of favorable to the number of possible cases.
What for want of a better term I call generality will increase with the
number of possible cases. This number may be finite, as, for instance,

if we take a throw of the dice in which the number of possible cases is 36. That is the first degree of generality. But if we ask, for instance, what is the probability that a point within a circle is within the inscribed square, there are as many possible cases as there are points in the circle — that is to say, an infinite number. This is the second degree of generality. Generality can be pushed further still. We may ask the probability that a function will satisfy a given condition. There are then as many possible cases as one can imagine different functions. This is the third degree of generality, which we reach, for instance, when we try to find the most probable law after a finite number of observations. Yet we may place ourselves at a quite different point of view. If we were not ignorant there would be no probability, there could only be certainty. But our ignorance cannot be absolute, for then there would be no longer any probability at all. Thus the problems of probability may be classed according to the greater or less depth of this ignorance. In mathematics we may set ourselves problems in probability. What is the probability that the fifth decimal of a logarithm taken at random from a table is a 9? There is no hesitation in answering that this probability is 1-10th. Here we possess all the data of the problem. We can calculate our logarithm without having recourse to the table, but we need not give ourselves the trouble. This is the first degree of ignorance. In the physical sciences our ignorance is already greater. The state of a system at a given moment depends on two things — its initial state, and the law according to which that state varies. If we know both this law and this initial state, we have a simple mathematical problem to solve, and we fall back upon our first degree of ignorance. Then it often happens that we know the law and do not know the initial state. It may be asked, for instance, what is the present distribution of the minor planets? We know that from all time they have obeyed the laws of Kepler, but we do not know what was their initial distribution. In the kinetic theory of gases we assume that the gaseous molecules follow rectilinear paths and obey the laws of impact and elastic bodies; yet as we know nothing of their initial velocities, we know nothing of their present velocities. The calculus of probabilities alone enables us to predict the mean phenomena which will result from a combination of these velocities. This is the second degree of ignorance. Finally it is possible, that not only the initial conditions but the laws themselves are unknown. We then reach the third degree of ignorance, and in general we can no longer affirm anything at all as to the probability of a phenomenon. It often happens that instead of trying to discover an event by means of a more or less imperfect knowledge of the law, the events may be known, and we want to find the law; or that, instead of deducing effects from causes, we wish to deduce the causes from the effects. Now, these problems are

classified as *probability of causes,* and are the most interesting of all from their scientific applications. I play at *écarté* with a gentleman whom I know to be perfectly honest. What is the chance that he turns up the king? It is 1-8. This is a problem of the probability of effects. I play with a gentleman whom I do not know. He has dealt ten times, and he has turned the king up six times. What is the chance that he is a sharper? This is a problem in the probability of causes. It may be said that it is the essential problem of the experimental method. I have observed n values of x and the corresponding values of y. I have found that the ratio of the latter to the former is practically constant. There is the event; what is the cause? Is it probable that there is a general law according to which y would be proportional to x, and that small divergences are due to errors of observation? This is the type of question that we are ever asking, and which we unconsciously solve whenever we are engaged in scientific work. I am now going to pass in review these different categories of problems by discussing in succession what I have called subjective and objective probability.

II. *Probability in Mathematics.* — The impossibility of squaring the circle was shown in 1885, but before that date all geometers considered this impossibility as so "probable" that the Académie des Sciences rejected without examination the, alas! too numerous memoirs on this subject that a few unhappy madmen sent in every year. Was the Académie wrong? Evidently not, and it knew perfectly well that by acting in this manner it did not run the least risk of stifling a discovery of moment. The Académie could not have proved that it was right, but it knew quite well that its instinct did not deceive it. If you had asked the Academicians, they would have answered: "We have compared the probability that an unknown scientist should have found out what has been vainly sought for so long, with the probability that there is one madman the more on the earth, and the latter has appeared to us the greater." These are very good reasons, but there is nothing mathematical about them; they are purely psychological. If you had pressed them further, they would have added: "Why do you expect a particular value of a transcendental function to be an algebraical number; if π be the root of an algebraical equation, why do you expect this root to be a period of the function $\sin 2x$, and why is it not the same with the other roots of the same equation?" To sum up, they would have invoked the principle of sufficient reason in its vaguest form. Yet what information could they draw from it? At most a rule of conduct for the employment of their time, which would be more usefully spent at their ordinary work than in reading a lucubration that inspired in them a legitimate distrust. But what I called above objective probability has nothing in common with this first problem. It is otherwise with the second. Let us consider the

first 10,000 lgarithms that we find in a table. Among these 10,000 logarithms I take one at random. What is the probability that its third decimal is an even number? You will say without any hesitation that the probability is 1-2, and in fact if you pick out in a table the third decimals in these 10,000 numbers you will find nearly as many even digits as odd. Or, if you prefer it, let us write 10,000 numbers corresponding to our 10,000 logarithms, writing down for each of these numbers $+1$ if the third decimal of the corresponding logarithm is even, and -1 if odd; and then let us take the mean of these 10,000 numbers. I do not hesitate to say that the mean of these 10,000 units is probably zero, and if I were to calculate it practically, I would verify that it is extremely small. But this verification is needless. I might have rigorously proved that this mean is smaller than 0.003. To prove this result I should have had to make a rather long calculation for which there is no room here, and for which I may refer the reader to an article that I published in the *Revue générale des Sciences,* April 15th, 1899. The only point to which I wish to draw attention is the following. In this calculation I had occasion to rest my case on only two facts — namely, that the first and second derivatives of the logarithm remain, in the interval considered, between certain limits. Hence our first conclusion is that the property is not only true of the logarithm but of any continuous function whatever, since the derivatives of every continuous function are limited. If I was certain beforehand of the result, it is because I have often observed analogous facts for other continuous functions; and next, it is because I went through in my mind in a more or less unconscious and imperfect manner the reasoning which led me to the preceding inequalities, just as a skilled calculator before finishing his multiplication takes into account what it ought to come to approximately. And besides, since what I call my intuition was only an incomplete summary of a piece of true reasoning, it is clear that observation has confirmed my predictions, and that the objective and subjective probabilities are in agreement. As a third example I shall choose the following: — The number u is taken at random and n is a given very large integer. What is the mean value of sin nu? This problem has no meaning by itself. To give it one, a convention is required — namely, we agree that the probability for the number u to lie between a and $a + da$ is ϕ (a) da; that it is therefore proportional to the infinitely small interval da, and is equal to this multiplied by a function ϕ (a), only depending on a. As for this function I choose it arbitrarily, but I must assume it to be continuous. The value of sin nu remaining the same when u increases by 2π, I may without loss of generality assume that u lies between 0 and 2π, and I shall thus be led to suppose that ϕ (a) is a periodic function whose period is 2π . The mean value that we seek is readily expressed by

a simple integral, and it is easy to show that this integral is smaller than $2 \pi \frac{M_{\kappa}}{_{\iota}{}^{\kappa}}$, M_{κ} being the maximum value of the κth derivative of ϕ (u). We see then that if the κth derivative is finite, our mean value will tend towards zero when n increases indefinitely, and that more rapidly than $\frac{1}{n^{\kappa-1}}$. The mean value of $\sin nu$ when n is very large is therefore zero. To define this value I required a convention, but the result remains the same *whatever that convention may be*. I have imposed upon myself but slight restrictions when I assumed that the function ϕ (a) is continuous and periodic, and these hypotheses are so natural that we may ask ourselves how they can be escaped. Examination of the three preceding examples, so different in all respects, has already given us a glimpse on the one hand of the rôle of what philosophers call the principle of sufficient reason, and on the other hand of the importance of the fact that certain properties are common to all continuous functions. The study of probability in the physical sciences will lead us to the same result.

III. *Probability in the Physical Sciences.* — We now come to the problems which are connected with what I have called the second degree of ignorance — namely, those in which we know the law but do not know the initial state of the system. I could multiply examples, but I shall take only one. What is the probable present distribution of the minor planets on the zodiac? We know they obey the laws of Kepler. We may even, without changing the nature of the problem, suppose that their orbits are circular and situated in the same plane, a plane which we are given. On the other hand, we know absolutely nothing about their initial distribution. However, we do not hesitate to affirm that this distribution is now nearly uniform. Why? Let b be the longitude of a minor planet in the initial epoch — that is to say, the epoch zero. Let a be its mean motion. Its longitude at the present time — *i.e.*, at the time t will be $at + b$. To say that the present distribution is uniform is to say that the mean value of the sines and cosines of multiples of $at+b$ is zero. Why do we assert this? Let us represent our minor planet by a point in a plane — namely, the point whose co-ordinates are a and b. All these representative points will be contained in a certain region of the plane, but as they are very numerous this region will appear dotted with points. We know nothing else about the distribution of the points. Now what do we do when we apply the calculus of probabilities to such a question as this? What is the probability that one or more representative points may be found in a certain portion of the plane? In our ignorance we are compelled to make an arbitrary hypothesis. To explain the nature of this hypothesis I may be allowed to use, instead of a mathematical formula, a crude but concrete image. Let us suppose that over the surface of our plane has been spread imaginary matter, the density of which is

variable, but varies continuously. We shall then agree to say that the probable number of representative points to be found on a certain portion of the plane is proportional to the quantity of this imaginary matter which is found there. If there are, then, two regions of the plane of the same extent, the probabilities that a representative point of one of our minor planets is in one or other of these regions will be as the mean densities of the imaginary matter in one or other of the regions. Here then are two distributions, one real, in which the representative points are very numerous, very close together, but discrete like molecules of matter in atomic hypothesis; the other remote from reality in which our representative points are replaced by imaginary continuous matter. We know that the latter cannot be real, but we are forced to adopt it through our ignorance. If, again, we had some idea of the real distribution of the representative points, we could arrange it so that in a region of some extent the density of this imaginary continuous matter may be nearly proportional to the number of representative points, or if it is preferred, to the number of atoms which are contained in that region. Even that is impossible, and our ignorance is so great that we are forced to choose arbitrarily the function which defines the density of our imaginary matter. We shall be compelled to adopt a hypothesis from which we can hardly get away; we shall suppose that this function is continuous. That is sufficient, as we shall see, to enable us to reach our conclusion.

What is at the instant t the probable distribution of the minor planets — or rather, what is the mean value of the sine of the longitude at the moment t — i.e., of $\sin (at+b)$? We made at the outset an arbitrary convention, but if we adopt it, this probable value is entirely defined. Let us decompose the plane into elements of surface. Consider the value of $\sin (at+b)$ at the centre of each of these elements. Multiply this value by the surface of the element and by the corresponding density of the imaginary matter. Let us then take the sum for all the elements of the plane. This sum, by definition, will be the probable mean value we seek, which will thus be expressed by a double integral. It may be thought at first that this mean value depends on the choice of the function ϕ which defines the density of the imaginary matter, and as this function ϕ is arbitrary, we can, according to the arbitrary choice which we make, obtain a certain mean value. But this is not the case. A simple calculation shows us that our double integral decreases very rapidly as t increases. Thus, I cannot tell what hypothesis to make as to the probability of this or that initial distribution, but when once the hypothesis is made the result will be the same, and this gets me out of my difficulty. Whatever the function ϕ may be, the mean value tends towards zero as t increases, and as the minor planets have certainly accomplished a very large number of revolutions, I may assert that this mean value is

very small. I may give to ϕ any value I choose, with one restriction: this function must be continuous; and, in fact, from the point of view of subjective probability, the choice of a discontinuous function would have been unreasonable. What reason could I have, for instance, for supposing that the initial longitude might be exactly $o°$, but that it could not lie between $o°$ and $1°$?

The difficulty reappears if we look at it from the point of view of objective probability; if we pass from our imaginary distribution in which the supposititious matter was assumed to be continuous, to the real distribution in which our representative points are formed as discrete atoms. The mean value of $\sin (at + b)$ will be represented quite simply by

$$\tfrac{1}{n} \sum \sin (at+b),$$

n being the number of minor planets. Instead of a double integral referring to a continuous function, we shall have a sum of discrete terms. However, no one will seriously doubt that this mean value is practically very small. Our representative points being very close together, our discrete sum will in general differ very little from an integral. An integral is the limit towards which a sum of terms tends when the number of these terms is indefinitely increased. If the terms are very numerous, the sum will differ very little from its limit — that is to say, from the integral, and what I said of the latter will still be true of the sum itself. But there are exceptions. If, for instance, for all the minor planets $b = \tfrac{\pi}{2} - at$, the longitude of all the planets at the time t would be $\tfrac{\pi}{2}$, and the mean value in question would be evidently unity. For this to be the case at the time o, the minor planets must have all been lying on a kind of spiral of peculiar form, with its spires very close together. All will admit that such an initial distribution is extremely improbable (and even if it were realized, the distribution would not be uniform at the present time — for example, on the 1st January 1900; but it would become so a few years later). Why, then, do we think this initial distribution improbable? This must be explained, for if we are wrong in rejecting as improbable this absurd hypothesis, our inquiry breaks down, and we can no longer affirm anything on the subject of the probability of this or that present distribution. Once more we shall invoke the principle of sufficient reason, to which we must always recur. We might admit that at the beginning the planets were distributed almost in a straight line. We might admit that they were irregularly distributed. But it seems to us that there is no sufficient reason for the unknown cause that gave them birth to have acted along a curve so regular and yet so complicated, which would appear to have been expressly chosen so that the distribution at the present day would not be uniform.

IV. *Rouge et Noir.* — The questions raised by games of chance, such as roulette, are, fundamentally, quite analogous to those we have just treated. For example, a wheel is divided into thirty-seven equal compartments, alternately red and black. A ball is spun round the wheel, and after having moved round a number of times, it stops in front of one of these sub-divisions. The probability that the division is red is obviously 1-2. The needle describes an angle θ, including several complete revolutions. I do not know what is the probability that the ball is spun with such a force that this angle should lie between θ and $\theta + d\theta$, but I can make a convention. I can suppose that this probability is $\phi(\theta)d\theta$. As for the function $\phi(\theta)$, I can choose it in an entirely arbitrary manner. I have nothing to guide me in my choice, but I am naturally induced to suppose the function to be continuous. Let ϵ be a length (measured on the circumference of the circle of radius unity) of each red and black compartment. We have to calculate the integral of $\phi(\theta)d\theta$, extending it on the one hand to all the red, and on the other hand to all the black compartments, and to compare the results. Consider an interval 2ϵ comprising two consecutive red and black compartments. Let M and m be the maximum and minimum values of the function $\phi(\theta)$ in this interval. The integral extended to the red compartments will be smaller than $\sum M\epsilon$; extended to the black it will be greater than $\sum m\epsilon$. The difference will therefore be smaller than $\sum (M - m)\epsilon$. But if the function ϕ is supposed continuous, and if on the other hand the interval ϵ is very small with respect to the total angle described by the needle, the difference M—m will be very small. The difference of the two integrals will be therefore very small, and the probability will be very nearly 1-2. We see that without knowing anything of the function ϕ we must act as if the probability were 1-2. And on the other hand it explains why, from the objective point of view, if I watch a certain number of *coups*, observation will give me almost as many black *coups* as red. All the players know this objective law; but it leads them into a remarkable error, which has often been exposed, but into which they are always falling. When the red has won, for example, six times running, they bet on black, thinking that they are playing an absolutely safe game, because they say it is a very rare thing for the red to win seven times running. In reality their probability of winning is still 1-2. Observation shows, it is true, that the series of seven consecutive reds is very rare, but series of six reds followed by a black are also very rare. They have noticed the rarity of the series of seven reds; if they have not remarked the rarity of six reds and a black, it is only because such series strike the attention less.

V. *The Probability of Causes.* — We now come to the problems of the probability of causes, the most important from the point of view

of scientific applications. Two stars, for instance, are very close together on the celestial sphere. Is this apparent contiguity a mere effect of chance? Are these stars, although almost on the same visual ray, situated at very different distances from the earth, and therefore very far indeed from one another? or does the apparent correspond to a real contiguity? This is a problem on the probability of causes.

First of all, I recall that at the outset of all problems of probability of effects that have occupied our attention up to now, we have had to use a convention which was more or less justified; and if in most cases the result was to a certain extent independent of this convention, it was only the condition of certain hypotheses which enabled us *à priori* to reject discontinuous functions, for example, or certain absurd conventions. We shall again find something analogous to this when we deal with the probability of causes. An effect may be produced by the cause *a* or by the cause *b*. The effect has just been observed. We ask the probability that it is due to the cause *a*. This is an *à posteriori* probability of cause. But I could not calculate it, if a convention more or less justified did not tell me in advance what is the *à priori* probability for the cause *a* to come into play — I mean the probability of this event to some one who had not observed the effect. To make my meaning clearer, I go back to the game of *écarté* mentioned before. My adversary deals for the first time and turns up a king. What is the probability that he is a sharper? The formulæ ordinarily taught give 8-9, a result which is obviously rather surprising. If we look at it closer, we see that the conclusion is arrived at as if, before sitting down at the table, I had considered that there was one chance in two that my adversary was not honest. An absurd hypothesis, because in that case I should certainly not have played with him; and this explains the absurdity of the conclusion. The function on the *à priori* probability was unjustified, and that is why the conclusion of the *à posteriori* probability led me into an inadmissible result. The importance of this preliminary convention is obvious. I shall even add that if none were made, the problem of the *à posteriori* probability would have no meaning. It must be always made either explicitly or tacitly.

Let us pass on to an example of a more scientific character. I require to determine an experimental law; this law, when discovered, can be represented by a curve. I make a certain number of isolated observations, each of which may be represented by a point. When I have obtained these different points, I draw a curve between them as carefully as possible, giving my curve a regular form, avoiding sharp angles, accentuated inflections, and any sudden variation of the radius of curvature. This curve will represent to me the probable law, and not only will it give me the values of the functions intermediary

to those which have been observed, but it also gives me the observed values more accurately than direct observation does; that is why I make the curve pass near the points and not through the points themselves.

Here, then, is a problem in the probability of causes. The effects are the measurements I have recorded; they depend on the combination of two causes — the true law of the phenomenon and errors of observation. Knowing the effects, we have to find the probability that the phenomenon shall obey this law or that, and that the observations have been accompanied by this or that error. The most probable law, therefore, corresponds to the curve we have traced, and the most probable error is represented by the distance of the corresponding point from that curve. But the problem has no meaning if before the observations I had an *à priori* idea of the probability of this law or that, or of the chances of error to which I am exposed. If my instruments are good (and I knew whether this is so or not before beginning the observations), I shall not draw the curve far from the points which represent the rough measurements. If they are inferior, I may draw it a little farther from the points, so that I may get a less sinuous curve; much will be sacrificed to regularity.

Why, then, do I draw a curve without sinuosities? Because I consider *à priori* a law represented by a continuous function (or function the derivatives of which to a high order are small), as more probable than a law not satisfying those conditions. But for this conviction the problem would have no meaning; interpolation would be impossible; no law could be deduced from a finite number of observations; science would cease to exist.

Fifty years ago physicists considered, other things being equal, a simple law as more probable than a complicated law. This principle was even invoked in favor of Mariotte's law as against that of Regnault. But this belief is now repudiated; and yet, how many times are we compelled to act as though we still held it! However that may be, what remains of this tendency is the belief in continuity, and as we have just seen, if the belief in continuity were to disappear, experimental science would become impossible.

VI. *The Theory of Errors.* — We are thus brought to consider the theory of errors which is directly connected with the problem of the probability of causes. Here again we find *effects* — to wit, a certain number of irreconcilable observations, and we try to find the *causes* which are, on the one hand, the true value of the quantity to be measured, and, on the other, the error made in each isolated observation. We must calculate the probable *à posteriori* value of each error, and therefore the probable value of the quantity to be measured. But, as I have just explained, we cannot undertake this calculation unless we admit *à priori*—i.e., before any observations are made —

that there is a law of the probability of errors. Is there a law of errors? The law to which all calculators assent is Gauss's law, that is represented by a certain transcendental curve known as the " bell."

But it is first of all necessary to recall the classic distinction between systematic and accidental errors. If the metre with which we measure a length is too long, the number we get will be too small, and it will be no use to measure several times — that is a systematic error. If we measure with an accurate metre, we may make a mistake, and find the length sometimes too large and sometimes too small, and when we take the mean of a large number of measurements, the error will tend to grow small. These are accidental errors.

It is clear that systematic errors do not satisfy Gauss's law, but do accidental errors satisfy it? Numerous proofs have been attempted, almost all of them crude paralogisms. But starting from the following hypotheses we may prove Gauss's law: the error is the result of a very large number of partial and independent errors; each partial error is very small and obeys any law of probability whatever, provided the probability of a positive error is the same as that of an equal negative error. It is clear that these conditions will be often, but not always, fulfilled, and we may reserve the name of accidental for errors which satisfy them.

We see that the method of least squares is not legitimate in every case; in general, physicists are more distrustful of it than astronomers. This is no doubt because the latter, apart from the systematic errors to which they and the physicists are subject alike, have to contend with an extremely important source of error which is entirely accidental — I mean atmospheric undulations. So it is very curious to hear a discussion between a physicist and an astronomer about a method of observation. The physicist, persuaded that one good measurement is worth more than many bad ones, is pre-eminently concerned with the elimination by means of every precaution of the final systematic errors; the astronomer retorts: " But you can only observe a small number of stars, and accidental errors will not disappear."

What conclusion must we draw? Must we continue to use the method of least squares? We must distinguish. We have eliminated all the systematic errors of which we have any suspicion; we are quite certain that there are others still, but we cannot detect them; and yet we must make up our minds and adopt a definitive value which will be regarded as the probable value; and for that purpose it is clear that the best thing we can do is to apply Gauss's law. We have only applied a practical rule referring to subjective probability. And there is no more to be said.

Yet we want to go farther and say that not only the probable value is so much, but that the probable error in the re-

sult is so much. This is absolutely invalid: it would be true only if we were sure that all the systematic errors were eliminated, and of that we know absolutely nothing. We have two series of observations; by applying the law of least squares we find that the probable error in the first series is twice as small as in the second. The second series may, however, be more accurate than the first, because the first is perhaps affected by a large systematic error. All that we can say is, that the first series is *probably* better than the second because its accidental error is smaller, and that we have no reason for affirming that the systematic error is greater for one of the series than for the other, our ignorance on this point being absolute.

VII. *Conclusion.* — In the preceding lines I have set several problems, and have given no solution. I do not regret this, for perhaps they will invite the reader to reflect on these delicate questions.

However that may be, there are certain points which seem to be well established. To undertake the calculation of any probability, and even for that calculation to have any meaning at all, we must admit, as a point of departure, an hypothesis or convention which has always something arbitrary about it. In the choice of this convention we can be guided only by the principle of sufficient reason. Unfortunately, this principle is very vague and very elastic, and in the cursory examination we have just made we have seen it assume different forms. The form under which we meet it most often is the belief in continuity, a belief which it would be difficult to justify by apodeictic reasoning, but without which all science would be impossible. Finally, the problems to which the calculus of probabilities may be applied with profit are those in which the result is independent of the hypothesis made at the outset, provided only that this hypothesis satisfies the condition of continuity.

Optics and Electricity [1]

Fresnel's Theory. — The best example that can be chosen is the theory of light and its relations to the theory of electricity. It is owing to Fresnel that the science of optics is more advanced than any other branch of physics. The theory called the theory of undulations forms a complete whole, which is satisfying to the mind; but we must not ask from it what it cannot give us. The object of mathematical theories is not to reveal to us the real nature of things; that would be an unreasonable claim. Their only object is to co-ordinate the physical laws with which physical experiment makes us acquainted,

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[1] This section is mainly taken from the prefaces of two of my books — *Théorie Mathématique de la lumière* (Paris: Naud, 1889), and *Electricité et Optique* (Paris: Naud, 1901).

enunciation of which, without the aid of mathematics, we should be unable to effect. Whether the ether exists or not matters little — let us leave that to the metaphysicians; what is essential for us is, that everything happens as if it existed, and that this hypothesis is found to be suitable for the explanation of phenomena. After all, have we any other reason for believing in the existence of material objects? That, too, is only a convenient hypothesis; only, it will never cease to be so, while some day, no doubt, the ether will be thrown aside as useless.

But at the present moment the laws of optics, and the equations which translate them into the language of analysis, hold good — at least as a first approximation. It will therefore be always useful to study a theory which brings these equations into connection.

The undulatory theory is based on a molecular hypothesis; this is an advantage to those who think they can discover the cause under the law. But others find in it a reason for distrust; and this distrust seems to me as unfounded as the illusions of the former. These hypotheses play but a secondary rôle. They may be sacrificed, and the sole reason why this is not generally done is, that it would involve a certain loss of lucidity in the explanation. In fact, if we look at it a little closer we shall see that we borrow from molecular hypotheses but two things — the principle of the conservation of energy, and the linear form of the equations, which is the general law of small movements as of all small variations. This explains why most of the conclusions of Fresnel remain unchanged when we adopt the electro-magnetic theory of light.

Maxwell's Theory. — We all know that it was Maxwell who connected by a slender tie two branches of physics — optics and electricity — until then unsuspected of having anything in common. Thus blended in a larger aggregate, in a higher harmony, Fresnel's theory of optics did not perish. Parts of it are yet alive, and their mutual relations are still the same. Only, the language which we use to express them has changed; and, on the other hand, Maxwell has revealed to us other relations, hitherto unsuspected, between the different branches of optics and the domain of electricity.

The first time a French reader opens Maxwell's book, his admiration is tempered with a feeling of uneasiness, and often of distrust.

It is only after prolonged study, and at the cost of much effort, that this feeling disappears. Some minds of high calibre never lose this feeling. Why is it so difficult for the ideas of this English scientist to become acclimatized among us? No doubt the education received by most enlightened Frenchmen predisposes them to appreciate precision and logic more than any other qualities. In this respect the old theories of mathematical physics gave us complete satisfaction. All our masters, from Laplace to Cauchy, proceeded along the same

lines. Starting with clearly enunciated hypotheses, they deduced from them all their consequences with mathematical rigor, and then compared them with experiment. It seemed to be their aim to give to each of the branches of physics the same precision as to celestial mechanics.

A mind accustomed to admire such models is not easily satisfied with a theory. Not only will it not tolerate the least appearance of contradiction, but it will expect the different parts to be logically connected with one another, and will require the number of hypotheses to be reduced to a minimum.

This is not all; there will be other demands which appear to me to be less reasonable. Behind the matter of which our senses are aware, and which is made known to us by experiment, such a thinker will expect to see another kind of matter — the only true matter in his opinion — which will no longer have anything but purely geometrical qualities, and the atoms of which will be mathematical points subject to the laws of dynamics alone. And yet he will try to represent to himself, by an unconscious contradiction, these invisible and colorless atoms, and therefore to bring them as close as possible to ordinary matter.

Then only will he be thoroughly satisfied, and he will then imagine that he has penetrated the secret of the universe. Even if the satisfaction is fallacious, it is none the less difficult to give it up. Thus, on opening the pages of Maxwell, a Frenchman expects to find a theoretical whole, as logical and as precise as the physical optics that is founded on the hypothesis of the ether. He is thus preparing for himself a disappointment which I should like the reader to avoid; so I will warn him at once of what he will find and what he will not find in Maxwell.

Maxwell does not give a mechanical explanation of electricity and magnetism; he confines himself to showing that such an explanation is possible. He shows that the phenomena of optics are only a particular case of electro-magnetic phenomena. From the whole theory of electricity a theory of light can be immediately deduced. Unfortunately the converse is not true; it is not always easy to find a complete explanation of electrical phenomena. In particular it is not easy if we take as our starting-point Fresnel's theory; to do so, no doubt, would be impossible; but none the less we must ask ourselves if we are compelled to surrender admirable results which we thought we had definitively acquired. That seems a step backwards, and many sound intellects will not willingly allow of this.

Should the reader consent to set some bounds to his hopes, he will still come across other difficulties. The English scientist does not try to erect a unique, definitive, and well-arranged building; he seems to raise rather a large number of provisional and independent con-

structions, between which communication is difficult and sometimes impossible. Take, for instance, the chapter in which electrostatic attractions are explained by the pressures and tensions of the dielectric medium. This chapter might be suppressed without the rest of the book being thereby less clear or less complete, and yet it contains a theory which is self-sufficient, and which can be understood without reading a word of what precedes or follows. But it is not only independent of the rest of the book; it is difficult to reconcile it with the fundamental ideas of the volume. Maxwell does not even attempt to reconcile it; he merely says: " I have not been able to make the next step — namely, to account by mechanical considerations for these stresses in the dielectric."

This example will be sufficient to show what I mean; I could quote many others. Thus, who would suspect on reading the pages devoted to magnetic rotatory polarization that there is an identity between optical and magnetic phenomena?

We must not flatter ourselves that we have avoided every contradiction, but we ought to make up our minds. Two contradictory theories, provided that they are kept from overlapping, and that we do not look to find in them the explanation of things, may, in fact, be very useful instruments of research; and perhaps the reading of Maxwell would be less suggestive if he had not opened up to us so many new and divergent ways. But the fundamental idea is masked, as it were. So far is this the case, that in most works that are popularized, this idea is the only point which is left completely untouched. To show the importance of this, I think I ought to explain in what this fundamental idea consists; but for that purpose a short digression is necessary.

The Mechanical Explanation of Physical Phenomena. — In every physical phenomenon there is a certain number of parameters which are reached directly by experiment, and which can be measured. I shall call them the parameters q. Observation next teaches us the laws of the variations of these parameters, and these laws can be generally stated in the form of differential equations which connect together the parameters q and time. What can be done to give a mechanical interpretation to such a phenomenon? We may endeavor to explain it, either by the movements of ordinary matter, or by those of one or more hypothetical fluids. These fluids will be considered as formed of a very large number of isolated molecules m. When may we say that we have a complete mechanical explanation of the phenomenon? It will be, on the one hand, when we know the differential equations which are satisfied by the co-ordinates of these hypothetical molecules m, equations which must, in addition, conform to the laws of dynamics; and, on the other hand, when we know the relations which define the co-ordinates of the molecules m as func-

tions of the parameters q, attainable by experiment. These equations, as I have said, should conform to the principles of dynamics, and, in particular, to the principle of the conservation of energy, and to that of least action.

The first of these two principles teaches us that the total energy is constant, and may be divided into two parts:

(1) Kinetic energy, or *vis viva*, which depends on the masses of the hypothetical molecules m, and on their velocities. This I shall call T. (2) The potential energy which depends only on the co-ordinates of these molecules, and this I shall call U. It is the sum of the energies T and U that is constant.

Now what are we taught by the principle of least action? It teaches us that to pass from the initial position occupied at the instant t_0 to the final position occupied at the instant t_1, the system must describe such a path that in the interval of time between the instant t_0 and t_1, the mean value of the action — *i.e.*, the *difference* between the two energies T and U, must be as small as possible. The first of these two principles is, moreover, a consequence of the second. If we know the functions T and U, this second principle is sufficient to determine the equations of motion.

Among the paths which enable us to pass from one position to another, there is clearly one for which the mean value of the action is smaller than for all the others. In addition, there is only such path; and it follows from this, that the principle of least action is sufficient to determine the path followed, and therefore the equations of motion. We thus obtain what are called the equations of Lagrange. In these equations the independent variables are the co-ordinates of the hypothetical molecules m; but I now assume that we take for variables the parameters q, which are directly accessible to experiment.

The two parts of the energy should then be expressed as a function of the parameters q and their derivatives; it is clear that it is under this form that they will appear to the experimenter. The latter will naturally endeavor to define kinetic and potential energy by the aid of quantities he can directly observe.[1] If this be granted, the system will always proceed from one position to another by such a path that the mean value of the action is a minimum. It matters little that T and U are now expressed by the aid of the parameters q and their derivatives; it matters little that it is also by the aid of these parameters that we define the initial and final positions; the principle of least action will always remain true.

Now here again, of the whole of the paths which lead from one

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[1] We may add that U will depend only on the q parameters, that T will depend on them and their derivatives with respect to time, and will be a homogeneous polynomial of the second degree with respect to these derivatives.

position to another, there is one and only one for which the mean action is a minimum. The principle of least action is therefore sufficient for the determination of the differential equations which define the variations of the parameters q. The equations thus obtained are another form of Lagrange's equations.

To form these equations we need not know the relations which connect the parameters q with the co-ordinates of the hypothetical molecules, nor the masses of the molecules, nor the expression of U as a function of the co-ordinates of these molecules. All we need know is the expression of U as a function of the parameters q, and that of T as a function of the parameters q and their derivatives — i.e., the expressions of the kinetic and potential energy in terms of experimental data.

One of two things must now happen. Either for a convenient choice of T and U the Lagrangian equations, constructed as we have indicated, will be identical with the differential equations deduced from experiment, or there will be no functions T and U for which this identity takes place. In the latter case it is clear that no mechanical explanation is possible. The *necessary* condition for a mechanical explanation to be possible is therefore this: that we may choose the functions T and U so as to satisfy the principle of least action, and of the conservation of energy. Besides, this condition is *sufficient*. Suppose, in fact, that we have found a function U of the parameters q, which represents one of the parts of energy, and that the part of the energy which we represent by T is a function of the parameters q and their derivatives; that it is a polynomial of the second degree with respect to its derivatives, and finally that the Lagrangian equations formed by the aid of these two functions T and U are in conformity with the data of the experiment. How can we deduce from this a mechanical explanation? U must be regarded as the potential energy of a system of which T is the kinetic energy. There is no difficulty as far as U is concerned, but can T be regarded as the *vis viva* of a material system?

It is easily shown that this is always possible, and in an unlimited number of ways. I will be content with referring the reader to the pages of the preface of my *Électricité et Optique* for further details. Thus, if the principle of least action cannot be satisfied, no mechanical explanation is possible; if it can be satisfied, there is not only one explanation, but an unlimited number, whence it follows that since there is one there must be an unlimited number.

One more remark. Among the quantities that may be reached by experiment directly we shall consider some as the co-ordinates of our hypothetical molecules, some will be our parameters q, and the rest will be regarded as dependent not only on the co-ordinates but on the velocities — or what comes to the same thing, we look on them

as derivatives of the parameters q, or as combinations of these parameters and their derivatives.

Here then a question occurs: among all these quantities measured experimentally which shall we choose to represent the parameters q? and which shall we prefer to regard as the derivatives of these parameters? This choice remains arbitary to a large extent, but a mechanical explanation will be possible if it is done so as to satisfy the principle of least action.

Next, Maxwell asks: Can this choice and that of the two energies T and U be made so that electric phenomena will satisfy this principle? Experiment shows us that the energy of an electro-magnetic field decomposes into electro-static and electro-dynamic energy. Maxwell recognized that if we regard the former as the potential energy U, and the latter as the kinetic energy T, and that if on the other hand we take the electro-static charges of the conductors as the parameters q, and the intensity of the currents as derivatives of other parameters q — under these conditions, Maxwell has recognized that electric phenomena satisfies the principle of least action. He was then certain of a mechanical explanation. If he had expounded this theory at the beginning of his first volume, instead of relegating it to a corner of the second, it would not have escaped the attention of most readers. If therefore a phenomenon allows of a complete mechanical explanation, it allows of an unlimited number of others, which will equally take into account all the particulars revealed by experiment. And this is confirmed by the history of every branch of physics. In Optics, for instance, Fresnel believed vibration to be perpendicular to the plane of polarization; Neumann holds that it is parallel to that plane. For a long time an *experimentum crucis* was sought for, which would enable us to decide between these two theories, but in vain. In the same way, without going out of the domain of electricity, we find that the theory of two fluids and the single fluid theory equally account in a satisfactory manner for all the laws of electro-statics. All these facts are easily explained, thanks to the properties of the Lagrange equations.

It is easy now to understand Maxwell's fundamental idea. To demonstrate the possibility of a mechanical explanation of electricity we need not trouble to find the explanation itself; we need only know the expression of the two functions T and U, which are the two parts of energy, and to form with these two functions Lagrange's equations, and then to compare these equations with the experimental laws.

How shall we choose from all the possible explanations one in which the help of experiment will be wanting? The day will perhaps come when physicists will no longer concern themselves with questions which are inaccessible to positive methods, and will leave

them to the metaphysicians. That day has not yet come; man does not so easily resign himself to remaining for ever ignorant of the causes of things. Our choice cannot be therefore any longer guided by considerations in which personal appreciation plays too large a part. There are, however, solutions which all will reject because of their fantastic nature, and others which all will prefer because of their simplicity. As far as magnetism and electricity are concerned, Maxwell abstained from making any choice. It is not that he has a systematic contempt for all that positive methods cannot reach, as may be seen from the time he has devoted to the kinetic theory of gases. I may add that if in his *magnum opus* he develops no complete explanation, he has attempted one in an article in the *Philosophical Magazine*. The strangeness and the complexity of the hypotheses he found himself compelled to make, led him afterwards to withdraw it.

The same spirit is found throughout his whole work. He throws into relief the essential — *i.e.,* what is common to all theories; everything that suits only a particular theory is passed over almost in silence. The reader therefore finds himself in the presence of form nearly devoid of matter, which at first he is tempted to take as a fugitive and unassailable phantom. But the efforts he is thus compelled to make force him to think, and eventually he sees that there is often something rather artificial in the theoretical " aggregates " which he once admired.

Electro-Dynamics

The history of electro-dynamics is very instructive from our point of view. The title of Ampère's immortal work is, *Thèorie des phénomenès electro-dynamiques, uniquement fondée sur expérience.* He therefore imagined that he had made no hypotheses; but as we shall not be long in recognizing, he was mistaken; only, of these hypotheses he was quite unaware. On the other hand, his successors see them clearly enough, because their attention is attracted by the weak points in Ampère's solution. They made fresh hypotheses, but this time deliberately. How many times they had to change them before they reached the classic system, which is perhaps even now not quite definitive, we shall see.

I. *Ampère's Theory.* — In Ampère's experimental study of the mutual action of currents, he has operated, and he could operate only, with closed currents. This was not because he denied the existence or possibility of open currents. If two conductors are positively and negatively charged and brought into communication by a wire, a current is set up which passes from one to the other until the two potentials are equal. According to the ideas of Ampère's time, this was considered to be an open current; the current was known to pass

from the first conductor to the second, but they did not know it returned from the second to the first. All currents of this kind were therefore considered by Ampère to be open currents — for instance, the currents of discharge of a condenser; he was unable to experiment on them, their duration being too short. Another kind of open current may be imagined. Suppose we have two conductors A and B connected by a wire AMB. Small conducting masses in motion are first of all placed in contact with the conductor B, receive an electric charge, and leaving B are set in motion along a path BNA, carrying their charge with them. On coming into contact with A they lose their charge, which then returns to B along the wire AMB. Now here we have, in a sense, a closed circuit, since the electricity describes the closed circuit BNAMB; but the two parts of the current are quite different. In the wire AMB the electricity is displaced *through* a fixed conductor like a voltaic current, overcoming an ohmic resistance and developing heat; we say that it is displaced by *conduction*. In the part BNA the electricity is *carried* by a moving conductor, and is said to be displaced by *convection*. If therefore the convection current is considered to be perfectly analogous to the conduction current, the circuit BNAMB is closed; if on the contrary the convection current is not a "true current," and, for instance, does not act on the magnet, there is only the conduction current AMB, which is *open*. For example, if we connect by a wire the poles of a Holtz machine, the charged rotating disc transfers the electricity by convection from one pole to the other, and it returns to the first pole by conduction through the wire. But currents of this kind are very difficult to produce with appreciable intensity; in fact, with the means at Ampère's disposal we may almost say it was impossible.

To sum up, Ampère could conceive of the existence of two kinds of open currents, but he could experiment on neither, because they were not strong enough, or because their duration was too short. Experiment therefore could only show him the action of a closed current on a closed current — or more accurately, the action of a closed current on a portion of current, because a current can be made to describe a *closed* circuit, of which part may be in motion and the other part fixed. The displacements of the moving part may be studied under the action of another closed current. On the other hand, Ampère had no means of studying the action of an open current either on a closed or on another open current.

1. *The Case of Closed Currents.* — In the case of the mutual action of two closed currents, experiment revealed to Ampère remarkably simple laws. The following will be useful to us in the sequel: —

(1) *If the intensity of the currents is kept constant,* and if the two circuits, after having undergone any displacements and deformations whatever, return finally to their initial positions, the total work

done by the electro-dynamical actions is zero. In other words, there is an *electro-dynamical* potential of the two circuits proportional to the product of their intensities, and depending on the form and relative positions of the circuits; the work done by the electro-dynamical actions is equal to the change of this potential.

(2) The action of a closed solenoid is zero.

(3) The action of a circuit C on another voltaic circuit C′ depends only on the " magnetic field " developed by the circuit C. At each point in space we can, in fact, define in magnitude and direction a certain force called " magnetic force," which enjoys the following properties : —

(*a*) The force exercised by C on a magnetic pole is applied to that pole, and is equal to the magnetic force multiplied by the magnetic mass of the pole.

(*b*) A very short magnetic needle tends to take the direction of the magnetic force, and the couple to which it tends to reduce is proportional to the product of the magnetic force, the magnetic moment of the needle, and the sine of the dip of the needle.

(*c*) If the circuit C′ is displaced, the amount of the work done by the electro-dynamic action of C on C′ will be equal to the increment of " flow of magnetic force " which passes through the circuit.

2. *Action of a Closed Current on a Portion of Current.* — Ampère being unable to produce the open current properly so called, had only one way of studying the action of a closed current on a portion of current. This was by operating on a circuit C composed of two parts, one movable and the other fixed. The movable part was, for instance, a movable wire αβ, the ends α and β of which could slide along a fixed wire. In one of the positions of the movable wire the end α rested on the point A, and the end β on the point B of the fixed wire. The current ran from α to β — *i.e., from A to B along the movable wire, and then from B to A along the fixed wire. This current was therefore closed.*

In the second position, the movable wire having slipped, the points α and β were respectively at A′ and B′ on the fixed wire. The current ran from α to β — i.e., from A′ to B′ on the movable wire, and returned from B′ to B, and then from B to A, and then from A to A′ — all on the fixed wire. This current was also closed. If a similar circuit be exposed to the action of a closed current C, the movable part will be displaced just as if it were acted on by a force. Ampère *admits* that the force, apparently acting on the movable part A B, representing the action of C on the portion αβ of the current, remains the same whether an open current runs through αβ, stopping at α and β, or whether a closed current runs first to β, and then returns to α through the fixed portion of the circuit. This hypothesis seemed natural enough, and Ampère innocently assumed it ; nevertheless the

hypothesis *is not a necessity,* for we shall presently see that Helmholtz rejected it. However that may be, it enabled Ampère, although he had never produced an open current, to lay down the laws of the action of a closed current on an open current, or even on an element of current. They are simple:

(1) The force acting on an element of current is applied to that element; it is normal to the element and to the magnetic force, and proportional to that component of the magnetic force which is normal to the element.

(2) The action of a closed solenoid on an element of current is zero. But the electro-dynamic potential has disappeared — *i.e.,* when a closed and an open current of constant intensities return to their initial positions, the total work done is not zero.

3. *Continuous Rotations.* — The most remarkable electro-dynamical experiments are those in which continuous rotations are produced, and which are called *unipolar induction* experiments. A magnet may turn about its axis; a current passes first through a fixed wire and then enters the magnet by the pole N, for instance, passes through half the magnet, and emerges by a sliding contact and re-enters the fixed wire. The magnet then begins to rotate continuously. This is Faraday's experiment. How is it possible? If it were a question of two circuits of invariable form, C fixed and C' movable about an axis, the latter would never take up a position of continuous rotation; in fact, there is an electro-dynamical potential; there must therefore be a position of equilibrium when the potential is a maximum. Continuous rotations are therefore possible only when the circuit C' is composed of two parts — one fixed, and the other movable about an axis, as in the case of Faraday's experiment. Here again it is convenient to draw a distinction. The passage from the fixed to the movable part, or *vice versâ,* may take place either by simple contact, the same point of the movable part remaining constantly in contact with the same point of the fixed part, or by sliding contact, the same point of the movable part coming successively into contact with the different points of the fixed part.

It is only in the second case that there can be continuous rotation. This is what then happens: — the system tends to take up a position of equilibrium; but, when at the point of reaching that position, the sliding contact puts the moving part in contact with a fresh point in the fixed part; it changes the connections and therefore the conditions of equilibrium, so that as the position of equilibrium is ever eluding, so to speak, the system which is trying to reach it, rotation may take place indefinitely.

Ampère admits that the action of the circuit on the movable part of C' is the same as if the fixed part of C' did not exist, and therefore as if the current passing through the movable part were an open

current. He concluded that the action of a closed on an open current, or *vice versâ*, that of an open current on a fixed current, may give rise to continuous rotation. But this conclusion depends on the hypothesis which I have enunciated, and to which, as I said above, Helmholtz declined to subscribe.

4. *Mutual Action of Two Open Currents.* — As far as the mutual action of two open currents, and in particular that of two elements of current, is concerned, all experiment breaks down. Ampère falls back on hypothesis. He assumes: (1) that the mutual action of two elements reduces to a force acting along their *join;* (2) that the action of two closed currents is the resultant of the mutual actions of their different elements, which are the same as if these elements were isolated.

The remarkable thing is that here again Ampère makes two hypotheses without being aware of it. However that may be, these two hypotheses, together with the experiments on closed currents, suffice to determine completely the law of mutual action of two elements. But then, most of the simple laws we have met in the case of closed currents are no longer true. In the first place, there is no electro-dynamical potential; nor was there any, as we have seen, in the case of a closed current acting on an open current. Next, there is, properly speaking, no magnetic force; and we have above defined this force in three different ways: (1) By the action on a magnetic pole; (2) by the director couple which orientates the magnetic needle; (3) by the action on an element of current.

In the case with which we are immediately concerned, not only are these three definitions not in harmony, but each has lost its meaning :—

(1) A magnetic pole is no longer acted on by a unique force applied to that pole. We have seen, in fact, the action of an element of current on a pole is not applied to the pole but to the element; it may, moreover, be replaced by a force applied to the pole and by a couple.

(2) The couple which acts on the magnetic needle is no longer a simple director couple, for its moment with respect to the axis of the needle is not zero. It decomposes into a director couple, properly so called, and a supplementary couple which tends to produce the continuous rotation of which we have spoken above.

(3) Finally, the force acting on an element of a current is not normal to that element. In other words, *the unity of the magnetic force has disappeared.*

Let us see in what this unity consists. Two systems which exercise the same action on a magnetic pole will also exercise the same action on an indefinitely small magnetic needle, or on an element of current placed at the point in space at which the pole is. Well, this is true if the two systems only contain closed currents, and according

to Ampère it would not be true if the systems contained open currents. It is sufficient to remark, for instance, that if a magnetic pole is placed at A and an element at B, the direction of the element being in AB produced, this element, which will exercise no action on the pole, will exercise an action either on a magnetic needle placed at A, or on an element of current at A.

5. *Induction.* — We know that the discovery of electro-dynamical induction followed not long after the immortal work of Ampère. As long as it is only a question of closed currents there is no difficulty, and Helmholtz has even remarked that the principle of the conservation of energy is sufficient for us to deduce the laws of induction from the electro-dynamical laws of Ampère. But on the condition, as Bertrand has shown, — that we make a certain number of hypotheses.

The same principle again enables this deduction to be made in the case of open currents, although the result cannot be tested by experiment, since such currents cannot be produced.

If we wish to compare this method of analysis with Ampère's theorem on open currents, we get results which are calculated to surprise us. In the first place, induction cannot be deduced from the variation of the magnetic field by the well-known formula of scientists and practical men; in fact, as I have said, properly speaking, there is no magnetic field. But further, if a circuit C is subjected to the induction of a variable voltaic system S, and if this system S be displaced and deformed in any way whatever, so that the intensity of the currents of this system varies according to any law whatever, then so long as after these variations the system eventually returns to its initial position, it seems natural to suppose that the *mean* electromotive force induced in the current C is zero. This is true if the circuit C is closed, and if the system S only contains closed currents. It is no longer true if we accept the theory of Ampère, since there would be open currents. So that not only will induction no longer be the variation of the flow of magnetic force in any of the usual senses of the word, but it cannot be represented by the variation of that force whatever it may be.

II. *Helmholtz's Theory.* — I have dwelt upon the consequences of Ampère's theory and on his method of explaining the action of open currents. It is difficult to disregard the paradoxical and artificial character of the propositions to which we are thus led. We feel bound to think " it cannot be so." We may imagine then that Helmholtz has been led to look for something else. He rejects the fundamental hypothesis of Ampère — namely, that the mutual action of two elements of current reduces to a force along their join. He admits that an element of current is not acted upon by a single force but by a force and a couple, and this is what gave rise to the celebrated polemic

between Bertrand and Helmholtz. Helmholtz replaces Ampère's hypothesis by the following: — Two elements of current always admit of an electro-dynamic potential, depending solely upon their position and orientation; and the work of the forces that they exercise one on the other is equal to the variation of this potential. Thus Helmholtz can no more do without hypothesis than Ampère, but at least he does not do so without explicitly announcing it. In the case of closed currents, which alone are accessible to experiment, the two theories agree; in all other cases they differ. In the first place, contrary to what Ampère supposed, the force which seems to act on the movable portion of a closed current is not the same as that acting on the movable portion if it were isolated and if it constituted an open current. Let us return to the circuit C′, of which we spoke above, and which was formed of a movable wire sliding on a fixed wire. In the only experiment that can be made the movable portion $\alpha\beta$ is not isolated, but is part of a closed circuit. When it passes from AB to A′B′, the total electro-dynamic potential varies for two reasons. First, it has a slight increment because the potential of A′B′ with respect to the circuit C is not the same as that of AB; secondly, it has a second increment because it must be increased by the potentials of the elements AA′ and B′B with respect to C. It is this *double* increment which represents the work of the force acting upon the portion AB. If, on the contrary, $\alpha\beta$ be isolated, the potential would only have the first increment, and this first increment alone would measure the work of the force acting on AB. In the second place, there could be no continuous rotation without sliding contact, and in fact, that, as we have seen in the case of closed currents, is an immediate consequence of the existence of an electro-dynamic potential. In Faraday's experiment, if the magnet is fixed, and if the part of the current external to the magnet runs along a movable wire, that movable wire may undergo continuous rotation. But it does not mean that, if the contacts of the wire with the magnet were suppressed, and an open current were to run along the wire, the wire would still have a movement of continuous rotation. I have just said, in fact, that an isolated element is not acted on in the same way as a movable element making part of a closed circuit. But there is another difference. The action of a solenoid on a closed current is zero according to experiment and according to the two theories. Its action on an open current would be zero according to Ampère, and it would not be zero according to Helmholtz. From this follows an important consequence. We have given above three definitions of the magnetic force. The third has no meaning here, since an element of current is no longer acted upon by a single force. Nor has the first any meaning. What, in fact, is a magnetic pole? It is the extremity of an indefinite linear magnet. This magnet may be replaced by an indefinite solenoid. For

the definition of magnetic force to have any meaning, the action exercised by an open current on an indefinite solenoid would only depend on the position of the extremity of that solenoid—*i.e.*, that the action of a closed solenoid is zero. Now we have just seen that this is not the case. On the other hand, there is nothing to prevent us from adopting the second definition which is founded on the measurement of the director couple which tends to orientate the magnetic needle; but, if it is adopted, neither the effects of induction nor electro-dynamic effects will depend solely on the distribution of the lines of force in this magnetic field.

III. *Difficulties raised by these Theories.* — Helmholtz's theory is an advance on that of Ampère; it is necessary, however, that every difficulty should be removed. In both, the name " magnetic field " has no meaning, or, if we give it one by a more or less artificial convention, the ordinary laws so familiar to electricians no longer apply; and it is thus that the electro-motive force induced in a wire is no longer measured by the number of lines of force met by that wire. And our objections do not proceed only from the fact that it is difficult to give up deeply-rooted habits of language and thought. There is something more. If we do not believe in actions at a distance, electro-dynamic phenomena must be explained by a modification of the medium. And this medium is precisely what we call " magnetic field," and then the electro-magnetic effects should only depend on that field. All these difficulties arise from the hypothesis of open currents.

IV. *Maxwell's Theory.* — Such were the difficulties raised by the current theories, when Maxwell with a stroke of the pen caused them to vanish. To his mind, in fact, all currents are closed currents. Maxwell admits that if in a dielectric, the electric field happens to vary, this dielectric becomes the seat of a particular phenomenon acting on the galvanometer like a current and called the *current of displacement*. If, then, two conductors bearing positive and negative charges are placed in connection by means of a wire, during the discharge there is an open current of conduction in that wire; but there are produced at the same time in the surrounding dielectric currents of displacement which close this current of conduction. We know that Maxwell's theory leads to the explanation of optical phenomena which would be due to extremely rapid electrical oscillations. At that period such a conception was only a daring hypothesis which could be supported by no experiment; but after twenty years Maxwell's ideas received the confirmation of experiment. Hertz succeeded in producing systems of electric oscillations which reproduce all the properties of light, and only differ by the length of their wave — that is to say, as violet differs from red. In some measure he made a synthesis of light. It might be said that Hertz has not directly proved Maxwell's fundamental idea of the action of the current of

displacement on the galvanometer. That is true in a sense. What he has shown directly is that electro-magnetic induction is not instantaneously propagated, as was supposed, but its speed is the speed of light. Yet, to suppose there is no current of displacement, and that induction is with the speed of light; or, rather, to suppose that the currents of displacement produce inductive effects, and that the induction takes place instantaneously — *comes to the same thing.* This cannot be seen at the first glance, but it is proved by an analysis of which I must not even think of giving even a summary here.

V. *Rowland's Experiment.* — But, as I have said above, there are two kinds of open conduction currents. There are first the currents of discharge of a condenser, or of any conductor whatever. There are also cases in which the electric charges describe a closed contour, being displaced by conduction in one part of the circuit and by convection in the other part. The question might be regarded as solved for open currents of the first kind; they were closed by currents of displacement. For open currents of the second kind the solution appeared still more simple.

It seemed that if the current were closed it could only be by the current of convection itself. For that purpose it was sufficient to admit that a " convection current "—*i.e.*, a charged conductor in motion — could act on the galvanometer. But experimental confirmation was lacking. It appeared difficult, in fact, to obtain a sufficient intensity even by increasing as much as possible the charge and the velocity of the conductors. Rowland, an extremely skilful experimentalist, was the first to triumph, or to seem to triumph, over these difficulties. A disc received a strong electrostatic charge and a very high speed of rotation. An astatic magnetic system placed beside the disc underwent deviations. The experiment was made twice by Rowland, once in Berlin and once at Baltimore. It was afterwards repeated by Himstedt. These physicists even believed that they could announce that they had succeeded in making quantitative measurements. For twenty years Rowland's law was admitted without objection by all physicists, and, indeed, everything seemed to confirm it. The spark certainly does produce a magnetic effect, and does it not seem extremely likely that the spark discharged is due to particles taken from one of the electrodes and transferred to the other electrode with their charge? Is not the very spectrum of the spark, in which we recognize the lines of the metal of the electrode, a proof of it? The spark would then be a real current of induction.

On the other hand, it is also admitted that in an electrolyte the electricity is carried by the ions in motion. The current in an electrolyte would therefore also be a current of convection; but it acts on the magnetic needle. And in the same way for cathodic rays; Crooks attributed these rays to very subtle matter charged with negative elec-

tricity and moving with very high velocity. He looked upon them, in other words, as currents of convection. Now, these cathodic rays are deviated by the magnet. In virtue of the principle of action and reaction, they should in their turn deviate the magnetic needle. It is true that Hertz believed he had proved that the cathodic rays do not carry negative electricity, and that they do not act on the magnetic needle; but Hertz was wrong. First of all, Perrin succeeded in collecting the electricity carried by these rays — electricity of which Hertz denied the existence; the German scientist appears to have been deceived by the effects due to the action of the X-rays, which were not yet discovered. Afterwards, and quite recently, the action of the cathodic rays on the magnetic needle has been brought to light. Thus all these phenomena looked upon as currents of convection, electric sparks, electrolytic currents, cathodic rays, act in the same manner on the galvanometer and in conformity to Rowland's law.

VI. *Lorentz's Theory.* — We need not go much further. According to Lorentz's theory, currents of conduction would themselves be true convection currents. Electricity would remain indissolubly connected with certain material particles called *electrons*. The circulation of these electrons through bodies would produce voltaic currents, and what would distinguish conductors from insulators would be that the one could be traversed by these electrons, while the others would check the movement of the electrons. Lorentz's theory is very attractive. It gives a very simple explanation of certain phenomena, which the earlier theories — even Maxwell's in its primitive form — could only deal with in an unsatisfactory manner; for example, the aberration of light, the partial impulse of luminous waves, magnetic polarization, and Zeeman's experiment.

A few objections still remained. The phenomena of an electric system seemed to depend on the absolute velocity of translation of the centre of gravity of this system, which is contrary to the idea that we have of the relativity of space. Supported by M. Crémieu, M. Lippman has presented this objection in a very striking form. Imagine two charged conductors with the same velocity of translation. They are relatively at rest. However, each of them being equivalent to a current of convection, they ought to attract one another, and by measuring this attraction we could measure their absolute velocity. "No!" replied the partisans of Lorentz. "What we could measure in that way is not their absolute velocity, but their relative velocity *with respect to the ether,* so that the principle of relativity is safe." Whatever there may be in these objections, the edifice of electro-dynamics seemed, at any rate in its broad lines, definitively constructed. Everything was presented under the most satisfactory aspect. The theories of Ampère and Helmholtz, which were made for the open currents that no longer existed, seem to have no more than purely historic in-

terest, and the inextricable complications to which these theories led have been almost forgotten. This quiescence has been recently disturbed by the experiments of M. Crémieu, which have contradicted, or at least have seemed to contradict, the results formerly obtained by Rowland. Numerous investigators have endeavored to solve the question, and fresh experiments have been undertaken. What result will they give? I shall take care not to risk a prophecy which might be falsified between the day this treatise is ready for the press and the day on which it is placed before the public.
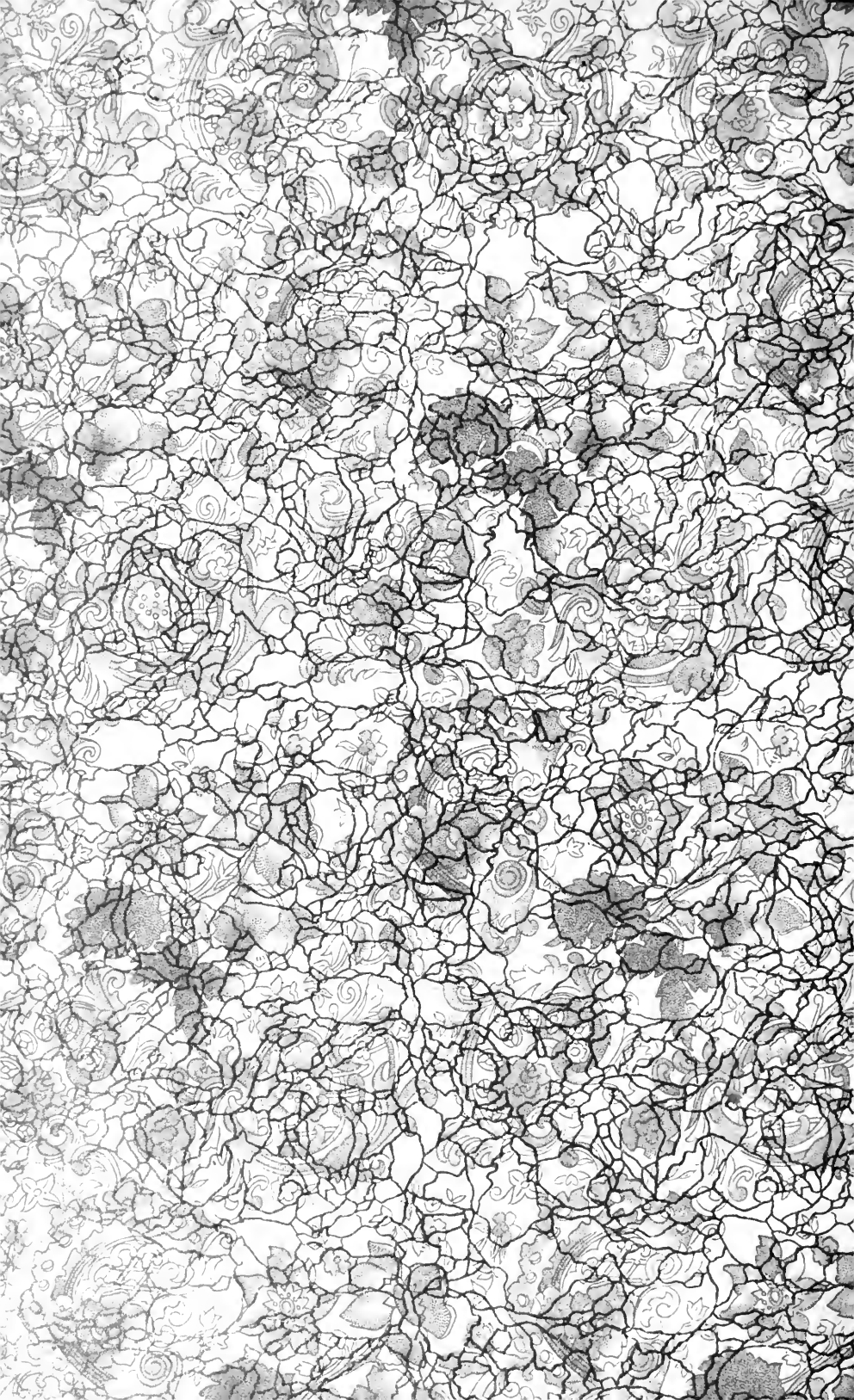
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