784.

PRESIDENTIAL ADDRESS TO THE BRITISH ASSOCIATION,
SEPTEMBER 1883.

[From the *Report of the British Association for the Advancement of Science*, (1883),
pp. 3—37.]

SINCE our last meeting we have been deprived of three of our most distinguished members. The loss by the death of Professor Henry John Stephen Smith is a very grievous one to those who knew and admired and loved him, to his University, and to mathematical science, which he cultivated with such ardour and success. I need hardly recall that the branch of mathematics to which he had specially devoted himself was that most interesting and difficult one, the Theory of Numbers. The immense range of this subject, connected with and ramifying into so many others, is nowhere so well seen as in the series of reports on the progress thereof, brought up unfortunately only to the year 1865, contributed by him to the Reports of the Association; but it will still better appear when to these are united (as will be done in the collected works in course of publication by the Clarendon Press) his other mathematical writings, many of them containing his own further developments of theories referred to in the reports. There have been recently or are being published many such collected editions—Abel, Cauchy, Clifford, Gauss, Green, Jacobi, Lagrange, Maxwell, Riemann, Steiner. Among these the works of Henry Smith will occupy a worthy position.

More recently, General Sir Edward Sabine, K.C.B., for twenty-one years general secretary of the Association, and a trustee, President of the meeting at Belfast in the year 1852, and for many years treasurer and afterwards President of the Royal Society, has been taken from us, at an age exceeding the ordinary age of man. Born October 1788, he entered the Royal Artillery in 1803, and commanded batteries at the siege of Fort Erie in 1814; made magnetic and other observations in Ross and Parry's North Polar exploration in 1818–19, and in a series of other voyages. He

contributed to the Association reports on Magnetic Forces in 1836–7–8, and about forty papers to the *Philosophical Transactions*; originated the system of Magnetic Observatories, and otherwise signally promoted the science of Terrestrial Magnetism.

There is yet a very great loss: another late President and trustee of the Association, one who has done for it so much, and has so often attended the meetings, whose presence among us at this meeting we might have hoped for—the President of the Royal Society, William Spottiswoode. It is unnecessary to say anything of his various merits: the place of his burial, the crowd of sorrowing friends who were present in the Abbey, bear witness to the esteem in which he was held.

I take the opportunity of mentioning the completion of a work promoted by the Association: the determination by Mr James Glaisher of the least factors of the missing three out of the first nine million numbers: the volume containing the sixth million is now published.

I wish to speak to you to-night upon Mathematics. I am quite aware of the difficulty arising from the abstract nature of my subject; and if, as I fear, many or some of you, recalling the Presidential Addresses at former meetings—for instance, the *résumé* and survey which we had at York of the progress, during the half century of the lifetime of the Association, of a whole circle of sciences—Biology, Palæontology, Geology, Astronomy, Chemistry—so much more familiar to you, and in which there was so much to tell of the fairy-tales of science; or at Southampton, the discourse of my friend who has in such kind terms introduced me to you, on the wondrous practical applications of science to electric lighting, telegraphy, the St Gothard Tunnel and the Suez Canal, gun-cotton, and a host of other purposes, and with the grand concluding speculation on the conservation of solar energy: if, I say, recalling these or any earlier Addresses, you should wish that you were now about to have, from a different President, a discourse on a different subject, I can very well sympathise with you in the feeling.

But be this as it may, I think it is more respectful to you that I should speak to you upon and do my best to interest you in the subject which has occupied me, and in which I am myself most interested. And in another point of view, I think it is right that the Address of a President should be on his own subject, and that different subjects should be thus brought in turn before the meetings. So much the worse, it may be, for a particular meeting; but the meeting is the individual, which on evolution principles must be sacrificed for the development of the race.

Mathematics connect themselves on the one side with common life and the physical sciences; on the other side with philosophy, in regard to our notions of space and time, and in the questions which have arisen as to the universality and necessity of the truths of mathematics, and the foundation of our knowledge of them. I would remark here that the connexion (if it exists) of arithmetic and algebra with the notion of time is far less obvious than that of geometry with the notion of space.

As to the former side, I am not making before you a defence of mathematics, but if I were I should desire to do it—in such manner as in the *Republic* Socrates was required to defend justice, quite irrespectively of the worldly advantages which

may accompany a life of virtue and justice, and to show that, independently of all these, justice was a thing desirable in itself and for its own sake—*not* by speaking to you of the utility of mathematics in any of the questions of common life or of physical science. Still less would I speak of this utility before, I trust, a friendly audience, interested or willing to appreciate an interest in mathematics in itself and for its own sake. I would, on the contrary, rather consider the obligations of mathematics to these different subjects as the sources of mathematical theories now as remote from them, and in as different a region of thought—for instance, geometry from the measurement of land, or the Theory of Numbers from arithmetic—as a river at its mouth is from its mountain source.

On the other side, the general opinion has been and is that it is indeed by experience that we arrive at the truths of mathematics, but that experience is not their proper foundation: the mind itself contributes something. This is involved in the Platonic theory of reminiscence; looking at two things, trees or stones or anything else, which seem to us more or less equal, we arrive at the idea of equality: but we must have had this idea of equality before the time when first seeing the two things we were led to regard them as coming up more or less perfectly to this idea of equality; and the like as regards our idea of the beautiful, and in other cases.

The same view is expressed in the answer of Leibnitz, the *nisi intellectus ipse*, to the scholastic dictum, *nihil in intellectu quod non prius in sensu*: there is nothing in the intellect which was not first in sensation, except (said Leibnitz) the intellect itself. And so again in the *Critick of Pure Reason*, Kant's view is that while there is no doubt but that all our cognition begins with experience, we are nevertheless in possession of cognitions *a priori*, independent, not of this or that experience, but absolutely so of all experience, and in particular that the axioms of mathematics furnish an example of such cognitions *a priori*. Kant holds further that space is no empirical conception which has been derived from external experiences, but that in order that sensations may be referred to something external, the representation of space must already lie at the foundation; and that the external experience is itself first only possible by this representation of space. And in like manner time is no empirical conception which can be deduced from an experience, but it is a necessary representation lying at the foundation of all intuitions.

And so in regard to mathematics, Sir W. R. Hamilton, in an Introductory Lecture on Astronomy (1836), observes: "These purely mathematical sciences of algebra and geometry are sciences of the pure reason, deriving no weight and no assistance from experiment, and isolated or at least isolable from all outward and accidental phenomena. The idea of order with its subordinate ideas of number and figure, we must not indeed call innate ideas, if that phrase be defined to imply that all men must possess them with equal clearness and fulness: they are, however, ideas which seem to be so far born with us that the possession of them in any conceivable degree is only the development of our original powers, the unfolding of our proper humanity."

The general question of the ideas of space and time, the axioms and definitions of geometry, the axioms relating to number, and the nature of mathematical reasoning, are

fully and ably discussed in Whewell's *Philosophy of the Inductive Sciences* (1840), which may be regarded as containing an exposition of the whole theory.

But it is maintained by John Stuart Mill that the truths of mathematics, in particular those of geometry, rest on experience; and as regards geometry, the same view is on very different grounds maintained by the mathematician Riemann.

It is not so easy as at first sight it appears to make out how far the views taken by Mill in his *System of Logic Ratiocinative and Inductive* (9th ed. 1879) are absolutely contradictory to those which have been spoken of; they profess to be so; there are most definite assertions (supported by argument), for instance, p. 263 :—"It remains to enquire what is the ground of our belief in axioms, what is the evidence on which they rest. I answer, they are experimental truths, generalisations from experience. The proposition 'Two straight lines cannot enclose a space,' or, in other words, two straight lines which have once met cannot meet again, is an induction from the evidence of our senses." But I cannot help considering a previous argument (p. 259) as very materially modifying this absolute contradiction. After enquiring "Why are mathematics by almost all philosophers . . . considered to be independent of the evidence of experience and observation, and characterised as systems of necessary truth?" Mill proceeds (I quote the whole passage) as follows:—"The answer I conceive to be that this character of necessity ascribed to the truths of mathematics, and even (with some reservations to be hereafter made) the peculiar certainty ascribed to them, is a delusion, in order to sustain which it is necessary to suppose that those truths relate to and express the properties of purely imaginary objects. It is acknowledged that the conclusions of geometry are derived partly at least from the so-called definitions, and that these definitions are assumed to be correct representations, as far as they go, of the objects with which geometry is conversant. Now, we have pointed out that, from a definition as such, no proposition unless it be one concerning the meaning of a word can ever follow, and that what apparently follows from a definition, follows in reality from an implied assumption that there exists a real thing conformable thereto. This assumption in the case of the definitions of geometry is not strictly true: there exist no real things exactly conformable to the definitions. There exist no real points without magnitude, no lines without breadth, nor perfectly straight, no circles with all their radii exactly equal, nor squares with all their angles perfectly right. It will be said that the assumption does not extend to the actual but only to the possible existence of such things. I answer that according to every test we have of possibility they are not even possible. Their existence, so far as we can form any judgment, would seem to be inconsistent with the physical constitution of our planet at least, if not of the universal [*sic*]. To get rid of this difficulty and at the same time to save the credit of the supposed system of necessary truth, it is customary to say that the points, lines, circles and squares which are the subjects of geometry exist in our conceptions merely and are parts of our minds; which minds by working on their own materials construct an *a priori* science, the evidence of which is purely mental and has nothing to do with outward experience. By howsoever high authority this doctrine has been sanctioned, it appears to me psychologically incorrect. The points, lines and squares which anyone has in his mind are (as I apprehend) simply copies

of the points, lines and squares which he has known in his experience. Our idea of a point I apprehend to be simply our idea of the *minimum visibile,* the small portion of surface which we can see. We can reason about a line as if it had no breadth, because we have a power which we can exercise over the operations of our minds : the power, when a perception is present to our senses or a conception to our intellects, of *attending* to a part only of that perception or conception instead of the whole. But we cannot *conceive* a line without breadth : we can form no mental picture of such a line ; all the lines which we have in our mind are lines possessing breadth. If anyone doubt this, we may refer him to his own experience. I much question if anyone who fancies that he can conceive of a mathematical line thinks so from the evidence of his own consciousness. I suspect it is rather because he supposes that, unless such a perception be possible, mathematics could not exist as a science : a supposition which there will be no difficulty in showing to be groundless."

I think it may be at once conceded that the truths of geometry are truths precisely because they relate to and express the properties of what Mill calls "purely imaginary objects"; that these objects do not exist in Mill's sense, that they do not exist in nature, may also be granted ; that they are "not even possible," if this means not possible in an existing nature, may also be granted. That we cannot "conceive" them depends on the meaning which we attach to the word conceive. I would myself say that the purely imaginary objects are the only realities, the ὄντως ὄντα, in regard to which the corresponding physical objects are as the shadows in the cave ; and it is only by means of them that we are able to deny the existence of a corresponding physical object ; if there is no conception of straightness, then it is meaningless to deny the existence of a perfectly straight line.

But at any rate the objects of geometrical truth are the so-called imaginary objects of Mill, and the truths of geometry are only true, and *a fortiori* are only necessarily true, in regard to these so-called imaginary objects ; and these objects, points, lines, circles, &c., in the mathematical sense of the terms, have a likeness to and are represented more or less imperfectly, and from a geometer's point of view no matter how imperfectly, by corresponding physical points, lines, circles, &c. I shall have to return to geometry, and will then speak of Riemann, but I will first refer to another passage of the Logic.

Speaking of the truths of arithmetic, Mill says (p. 297) that even here there is one hypothetical element : "In all propositions concerning numbers a condition is implied without which none of them would be true, and that condition is an assumption which may be false. The condition is that $1 = 1$: that all the numbers are numbers of the same or of equal units." Here at least the assumption may be absolutely true ; one shilling = one shilling in purchasing power, although they may not be absolutely of the same weight and fineness : but it is hardly necessary ; one coin + one coin = two coins, even if the one be a shilling and the other a half-crown. In fact, whatever difficulty be raisable as to geometry, it seems to me that no similar difficulty applies to arithmetic ; mathematician or not, we have each of us, in its most abstract form, the idea of a number ; we can each of us appreciate the truth of a proposition in regard to numbers ; and we cannot but see that a truth in regard to numbers is something different in kind from an

experimental truth generalised from experience. Compare, for instance, the proposition that the sun, having already risen so many times, will rise to-morrow, and the next day, and the day after that, and so on; and the proposition that even and odd numbers succeed each other alternately *ad infinitum*: the latter at least seems to have the characters of universality and necessity. Or again, suppose a proposition observed to hold good for a long series of numbers, one thousand numbers, two thousand numbers, as the case may be: this is not only no proof, but it is absolutely no evidence, that the proposition is a true proposition, holding good for all numbers whatever; there are in the Theory of Numbers very remarkable instances of propositions observed to hold good for very long series of numbers and which are nevertheless untrue.

I pass in review certain mathematical theories.

In arithmetic and algebra, or say in analysis, the numbers or magnitudes which we represent by symbols are in the first instance ordinary (that is, positive) numbers or magnitudes. We have also in analysis and in analytical geometry *negative* magnitudes; there has been in regard to these plenty of philosophical discussion, and I might refer to Kant's paper, *Ueber die negativen Grössen in die Weltweisheit* (1763), but the notion of a negative magnitude has become quite a familiar one, and has extended itself into common phraseology. I may remark that it is used in a very refined manner in bookkeeping by double entry.

But it is far otherwise with the notion which is really the fundamental one (and I cannot too strongly emphasise the assertion) underlying and pervading the whole of modern analysis and geometry, that of imaginary magnitude in analysis and of imaginary space (or space as a *locus in quo* of imaginary points and figures) in geometry: I use in each case the word imaginary as including real. This has not been, so far as I am aware, a subject of philosophical discussion or enquiry. As regards the older metaphysical writers this would be quite accounted for by saying that they knew nothing, and were not bound to know anything, about it; but at present, and, considering the prominent position which the notion occupies—say even that the conclusion were that the notion belongs to mere technical mathematics, or has reference to nonentities in regard to which no science is possible, still it seems to me that (as a subject of philosophical discussion) the notion ought not to be thus ignored; it should at least be shown that there is a right to ignore it.

Although in logical order I should perhaps now speak of the notion just referred to, it will be convenient to speak first of some other quasi-geometrical notions; those of more-than-three-dimensional space, and of non-Euclidian two- and three-dimensional space, and also of the generalised notion of distance. It is in connexion with these that Riemann considered that our notion of space is founded on experience, or rather that it is only by experience that we know that our space is Euclidian space.

It is well known that Euclid's twelfth axiom, even in Playfair's form of it, has been considered as needing demonstration; and that Lobatschewsky constructed a perfectly consistent theory, wherein this axiom was assumed not to hold good, or say a system of non-Euclidian plane geometry. There is a like system of non-Euclidian

solid geometry. My own view is that Euclid's twelfth axiom in Playfair's form of it does not need demonstration, but is part of our notion of space, of the physical space of our experience—the space, that is, which we become acquainted with by experience, but which is the representation lying at the foundation of all external experience. Riemann's view before referred to may I think be said to be that, having *in intellectu* a more general notion of space (in fact a notion of non-Euclidian space), we learn by experience that space (the physical space of our experience) is, if not exactly, at least to the highest degree of approximation, Euclidian space.

But suppose the physical space of our experience to be thus only approximately Euclidian space, what is the consequence which follows? *Not* that the propositions of geometry are only approximately true, but that they remain absolutely true in regard to that Euclidian space which has been so long regarded as being the physical space of our experience.

It is interesting to consider two different ways in which, without any modification at all of our notion of space, we can arrive at a system of non-Euclidian (plane or two-dimensional) geometry; and the doing so will, I think, throw some light on the whole question.

First, imagine the earth a perfectly smooth sphere; understand by a plane the surface of the earth, and by a line the apparently straight line (in fact, an arc of great circle) drawn on the surface; what experience would in the first instance teach would be Euclidian geometry; there would be intersecting lines which produced a few miles or so would seem to go on diverging: and apparently parallel lines which would exhibit no tendency to approach each other; and the inhabitants might very well conceive that they had by experience established the axiom that two straight lines cannot enclose a space, and the axiom as to parallel lines. A more extended experience and more accurate measurements would teach them that the axioms were each of them false; and that any two lines if produced far enough each way, would meet in two points: they would in fact arrive at a spherical geometry, accurately representing the properties of the two-dimensional space of their experience. But their original Euclidian geometry would not the less be a true system: only it would apply to an ideal space, not the space of their experience.

Secondly consider an ordinary, indefinitely extended plane; and let us modify only the notion of distance. We measure distance, say, by a yard measure or a foot rule, anything which is short enough to make the fractions of it of no consequence (in mathematical language, by an infinitesimal element of length); imagine, then, the length of this rule constantly changing (as it might do by an alteration of temperature), but under the condition that its actual length shall depend only on its situation on the plane and on its direction: viz. if for a given situation and direction it has a certain length, then whenever it comes back to the same situation and direction it must have the same length. The distance along a given straight or curved line between any two points could then be measured in the ordinary manner with this rule, and would have a perfectly determinate value: it could be measured over and over again, and would always be the same; but of course it would be the distance, not in the ordinary

acceptation of the term, but in quite a different acceptation. Or in a somewhat different way: if the rate of progress from a given point in a given direction be conceived as depending only on the configuration of the ground, and the distance along a given path between any two points thereof be measured by the time required for traversing it, then in this way also the distance would have a perfectly determinate value; but it would be a distance, not in the ordinary acceptation of the term, but in quite a different acceptation. And corresponding to the new notion of distance we should have a new non-Euclidian system of plane geometry; all theorems involving the notion of distance would be altered.

We may proceed further. Suppose that as the rule moves away from a fixed central point of the plane it becomes shorter and shorter; if this shortening takes place with sufficient rapidity, it may very well be that a distance which in the ordinary sense of the word is finite will in the new sense be infinite; no number of repetitions of the length of the ever-shortening rule will be sufficient to cover it. There will be surrounding the central point a certain finite area such that (in the new acceptation of the term distance) each point of the boundary thereof will be at an infinite distance from the central point; the points outside this area you cannot by any means arrive at with your rule; they will form a *terra incognita*, or rather an unknowable land: in mathematical language, an imaginary or impossible space: and the plane space of the theory will be that within the finite area—that is, it will be finite instead of infinite.

We thus with a proper law of shortening arrive at a system of non-Euclidian geometry which is essentially that of Lobatschewsky. But in so obtaining it we put out of sight its relation to spherical geometry: the three geometries (spherical, Euclidian, and Lobatschewsky's) should be regarded as members of a system: viz. they are the geometries of a plane (two-dimensional) space of constant positive curvature, zero curvature, and constant negative curvature respectively; or again, they are the plane geometries corresponding to three different notions of distance; in this point of view they are Klein's elliptic, parabolic, and hyperbolic geometries respectively.

Next as regards solid geometry: we can by a modification of the notion of distance (such as has just been explained in regard to Lobatschewsky's system) pass from our present system to a non-Euclidian system; for the other mode of passing to a non-Euclidian system, it would be necessary to regard our space as a flat three-dimensional space existing in a space of four dimensions (i.e., as the analogue of a plane existing in ordinary space); and to substitute for such flat three-dimensional space a curved three-dimensional space, say of constant positive or negative curvature. In regarding the physical space of our experience as possibly non-Euclidian, Riemann's idea seems to be that of modifying the notion of distance, not that of treating it as a locus in four-dimensional space.

I have just come to speak of four-dimensional space. What meaning do we attach to it? Or can we attach to it any meaning? It may be at once admitted that we cannot conceive of a fourth dimension of space; that space as we conceive of it, and the physical space of our experience, are alike three-dimensional; but we can, I think, conceive of space as being two- or even one-dimensional; we can imagine rational

beings living in a one-dimensional space (a line) or in a two-dimensional space (a surface), and conceiving of space accordingly, and to whom, therefore, a two-dimensional space, or (as the case may be) a three-dimensional space would be as inconceivable as a four-dimensional space is to us. And very curious speculative questions arise. Suppose the one-dimensional space a right line, and that it afterwards becomes a curved line: would there be any indication of the change? Or, if originally a curved line, would there be anything to suggest to them that it was not a right line? Probably not, for a one-dimensional geometry hardly exists. But let the space be two-dimensional, and imagine it originally a plane, and afterwards bent or converted into a curved surface (converted, that is, into some form of developable surface): or imagine it originally a developable or curved surface. In the former case there should be an indication of the change, for the geometry originally applicable to the space of their experience (our own Euclidian geometry) would cease to be applicable; but the change could not be apprehended by them as a bending or deformation of the plane, for this would imply the notion of a three-dimensional space in which this bending or deformation could take place. In the latter case their geometry would be that appropriate to the developable or curved surface which is their space: viz. this would be their Euclidian geometry: would they ever have arrived at our own more simple system? But take the case where the two-dimensional space is a plane, and imagine the beings of such a space familiar with our own Euclidian plane geometry; if, a third dimension being still inconceivable by them, they were by their geometry or otherwise led to the notion of it, there would be nothing to prevent them from forming a science such as our own science of three-dimensional geometry.

Evidently all the foregoing questions present themselves in regard to ourselves, and to three-dimensional space as we conceive of it, and as the physical space of our experience. And I need hardly say that the first step is the difficulty, and that granting a fourth dimension we may assume as many more dimensions as we please. But whatever answer be given to them, we have, as a branch of mathematics, potentially, if not actually, an analytical geometry of n-dimensional space. I shall have to speak again upon this.

Coming now to the fundamental notion already referred to, that of imaginary magnitude in analysis and imaginary space in geometry: I connect this with two great discoveries in mathematics made in the first half of the seventeenth century, Harriot's representation of an equation in the form $f(x) = 0$, and the consequent notion of the roots of an equation as derived from the linear factors of $f(x)$, (Harriot, 1560—1621: his *Algebra*, published after his death, has the date 1631), and Descartes' method of coordinates, as given in the *Géométrie*, forming a short supplement to his *Traité de la Méthode, etc.*, (Leyden, 1637).

Taking the coefficients of an equation to be real magnitudes, it at once follows from Harriot's form of an equation that an equation of the order n ought to have n roots. But it is by no means true that there are always n real roots. In particular, an equation of the second order, or quadric equation, may have no real root; but if we assume the existence of a root i of the quadric equation $x^2 + 1 = 0$, then the

other root is $= -i$; and it is easily seen that every quadric equation (with real coefficients as before) has two roots, $a \pm bi$, where a and b are real magnitudes. We are thus led to the conception of an imaginary magnitude, $a + bi$, where a and b are real magnitudes, each susceptible of any positive or negative value, zero included. The general theorem is that, taking the coefficients of the equation to be imaginary magnitudes, then an equation of the order n has always n roots, each of them an imaginary magnitude, and it thus appears that the foregoing form $a + bi$ of imaginary magnitude is the only one that presents itself. Such imaginary magnitudes may be added or multiplied together or dealt with in any manner; the result is always a like imaginary magnitude. They are thus the magnitudes which are considered in analysis, and analysis is the science of such magnitudes. Observe the leading character that the imaginary magnitude $a + bi$ is a magnitude composed of the two real magnitudes a and b (in the case $b = 0$ it is the real magnitude a, and in the case $a = 0$ it is the pure imaginary magnitude bi). The idea is that of considering, in place of real magnitudes, these imaginary or complex magnitudes $a + bi$.

In the Cartesian geometry a curve is determined by means of the equation existing between the coordinates (x, y) of any point thereof. In the case of a right line, this equation is linear; in the case of a circle, or more generally of a conic, the equation is of the second order; and generally, when the equation is of the order n, the curve which it represents is said to be a curve of the order n. In the case of two given curves, there are thus two equations satisfied by the coordinates (x, y) of the several points of intersection, and these give rise to an equation of a certain order for the coordinate x or y of a point of intersection. In the case of a straight line and a circle, this is a quadric equation; it has two roots, real or imaginary. There are thus two values, say of x, and to each of these corresponds a single value of y. There are therefore two points of intersection—viz. a straight line and a circle intersect *always* in two points, real or imaginary. It is in this way that we are led analytically to the notion of imaginary points in geometry. The conclusion as to the two points of intersection cannot be contradicted by experience: take a sheet of paper and draw on it the straight line and circle, and try. But you might say, or at least be strongly tempted to say, that it is meaningless. The question of course arises, What is the meaning of an imaginary point? and further, In what manner can the notion be arrived at geometrically?

There is a well-known construction in perspective for drawing lines through the intersection of two lines, which are so nearly parallel as not to meet within the limits of the sheet of paper. You have two given lines which do not meet, and you draw a third line, which, when the lines are all of them produced, is found to pass through the intersection of the given lines. If instead of lines we have two circular arcs not meeting each other, then we can, by means of these arcs, construct a line; and if on completing the circles it is found that the circles intersect each other in two real points, then it will be found that the line passes through these two points: if the circles appear not to intersect, then the line will appear not to intersect either of the circles. But the geometrical construction being in each case the same, we say that in the second case also the line passes through the two intersections of the circles.

Of course it may be said in reply that the conclusion is a very natural one, provided we assume the existence of imaginary points; and that, this assumption not being made, then, if the circles do not intersect, it is meaningless to assert that the line passes through their points of intersection. The difficulty is not got over by the analytical method before referred to, for this introduces difficulties of its own: is there in a plane a point the coordinates of which have given imaginary values? As a matter of fact, we do consider in plane geometry imaginary points introduced into the theory analytically or geometrically as above.

The like considerations apply to solid geometry, and we thus arrive at the notion of imaginary space as a *locus in quo* of imaginary points and figures.

I have used the word imaginary rather than complex, and I repeat that the word has been used as including real. But, this once understood, the word becomes in many cases superfluous, and the use of it would even be misleading. Thus, "a problem has so many solutions": this means, so many imaginary (including real) solutions. But if it were said that the problem had "so many imaginary solutions," the word "imaginary" would here be understood to be used in opposition to real. I give this explanation the better to point out how wide the application of the notion of the imaginary is—viz. (unless expressly or by implication excluded), it is a notion implied and presupposed in all the conclusions of modern analysis and geometry. It is, as I have said, the fundamental notion underlying and pervading the whole of these branches of mathematical science.

I shall speak later on of the great extension which is thereby given to geometry, but I wish now to consider the effect as regards the theory of a function. In the original point of view, and for the original purposes, a function, algebraic or transcendental, such as $\sqrt{x}$, $\sin x$, or $\log x$, was considered as known, when the value was known for every real value (positive or negative) of the argument; or if for any such values the value of the function became imaginary, then it was enough to know that for such values of the argument there was no real value of the function. But now this is not enough, and to know the function means to know its value—of course, in general, an imaginary value $X + iY$,—for every imaginary value $x + iy$ whatever of the argument.

And this leads naturally to the question of the geometrical representation of an imaginary variable. We represent the imaginary variable $x + iy$ by means of a point in a plane, the coordinates of which are (x, y). This idea, due to Gauss, dates from about the year 1831. We thus picture to ourselves the succession of values of the imaginary variable $x + iy$ by means of the motion of the representative point: for instance, the succession of values corresponding to the motion of the point along a closed curve to its original position. The value $X + iY$ of the function can of course be represented by means of a point (taken for greater convenience in a different plane), the coordinates of which are X, Y.

We may consider in general two points, moving each in its own plane, so that the position of one of them determines the position of the other, and consequently

the motion of the one determines the motion of the other: for instance, the two points may be the tracing-point and the pencil of a pentagraph. You may with the first point draw any figure you please, there will be a corresponding figure drawn by the second point: for a good pentagraph, a copy on a different scale (it may be); for a badly-adjusted pentagraph, a distorted copy: but the one figure will always be a sort of copy of the first, so that to each point of the one figure there will correspond a point of the other figure.

In the case above referred to, where one point represents the value $x + iy$ of the imaginary variable and the other the value $X + iY$ of some function $\phi(x + iy)$ of that variable, there is a remarkable relation between the two figures: this is the relation of orthomorphic projection, the same which presents itself between a portion of the earth's surface, and the representation thereof by a map on the stereographic projection or on Mercator's projection—viz. any indefinitely small area of the one figure is represented in the other figure by an indefinitely small area of the same shape. There will possibly be for different parts of the figure great variations of scale, but the shape will be unaltered; if for the one area the boundary is a circle, then for the other area the boundary will be a circle; if for one it is an equilateral triangle, then for the other it will be an equilateral triangle.

I have for simplicity assumed that to each point of either figure there corresponds one, and only one, point of the other figure; but the general case is that to each point of either figure there corresponds a determinate number of points in the other figure; and we have thence arising new and very complicated relations which I must just refer to. Suppose that to each point of the first figure there correspond in the second figure two points: say one of them is a red point, the other a blue point; so that, speaking roughly, the second figure consists of two copies of the first figure, a red copy and a blue copy, the one superimposed on the other. But the difficulty is that the two copies cannot be kept distinct from each other. If we consider in the first figure a closed curve of any kind—say, for shortness, an oval—this will be in the second figure represented in some cases by a red oval and a blue oval, but in other cases by an oval half red and half blue; or, what comes to the same thing, if in the first figure we consider a point which moves continuously in any manner, at last returning to its original position, and attempt to follow the corresponding points in the second figure, then it may very well happen that, for the corresponding point of either colour, there will be abrupt changes of position, or say jumps, from one position to another; so that, to obtain in the second figure a continuous path, we must at intervals allow the point to change from red to blue, or from blue to red. There are in the first figure certain critical points called branch-points (*Verzweigungspunkte*), and a system of lines connecting these, by means of which the colours in the second figure are determined; but it is not possible for me to go further into the theory at present. The notion of colour has of course been introduced only for facility of expression; it may be proper to add that in speaking of the two figures I have been following Briot and Bouquet rather than Riemann, whose representation of the function of an imaginary variable is a different one.

I have been speaking of an imaginary variable $(x + iy)$, and of a function $\phi(x + iy) = X + iY$ of that variable, but the theory may equally well be stated in

regard to a plane curve: in fact, the $x + iy$ and the $X + iY$ are two imaginary variables connected by an equation; say their values are u and v, connected by an equation $F(u, v) = 0$; then, regarding u, v as the coordinates of a point *in plano*, this will be a point on the curve represented by the equation. The curve, in the widest sense of the expression, is the whole series of points, real or imaginary, the coordinates of which satisfy the equation, and these are exhibited by the foregoing corresponding figures in two planes; but in the ordinary sense the curve is the series of real points, with coordinates u, v, which satisfy the equation.

In geometry it is the curve, whether defined by means of its equation, or in any other manner, which is the subject for contemplation and study. But we also use the curve as a representation of its equation—that is, of the relation existing between two magnitudes x, y, which are taken as the coordinates of a point on the curve. Such employment of a curve for all sorts of purposes—the fluctuations of the barometer, the Cambridge boat races, or the Funds—is familiar to most of you. It is in like manner convenient in analysis, for exhibiting the relations between any three magnitudes x, y, z, to regard them as the coordinates of a point in space; and, on the like ground, we should at least wish to regard any four or more magnitudes as the coordinates of a point in space of a corresponding number of dimensions. Starting with the hypothesis of such a space, and of points therein each determined by means of its coordinates, it is found possible to establish a system of n-dimensional geometry analogous in every respect to our two- and three-dimensional geometries, and to a very considerable extent serving to exhibit the relations of the variables. To quote from my memoir "On Abstract Geometry" (1869), [413]: "The science presents itself in two ways: as a legitimate extension of the ordinary two- and three-dimensional geometries, and as a need in these geometries and in analysis generally. In fact, whenever we are concerned with quantities connected in any manner, and which are considered as variable or determinable, then the nature of the connexion between the quantities is frequently rendered more intelligible by regarding them (if two or three in number) as the coordinates of a point in a plane or in space. For more than three quantities there is, from the greater complexity of the case, the greater need of such a representation; but this can only be obtained by means of the notion of a space of the proper dimensionality; and to use such representation we require a corresponding geometry. An important instance in plane geometry has already presented itself in the question of the number of curves which satisfy given conditions; the conditions imply relations between the coefficients in the equation of the curve; and for the better understanding of these relations it was expedient to consider the coefficients as the coordinates of a point in a space of the proper dimensionality."

It is to be borne in mind that the space, whatever its dimensionality may be, must always be regarded as an imaginary or complex space such as the two- or three-dimensional space of ordinary geometry; the advantages of the representation would otherwise altogether fail to be obtained.

I have spoken throughout of Cartesian coordinates; instead of these, it is in plane geometry not unusual to employ trilinear coordinates, and these may be regarded as absolutely undetermined in their magnitude—viz. we may take x, y, z to be, not equal,

but only proportional to the distances of a point from three given lines; the ratios of the coordinates (x, y, z) determine the point; and so in one-dimensional geometry, we may have a point determined by the ratio of its two coordinates x, y, these coordinates being proportional to the distances of the point from two fixed points; and generally in n-dimensional geometry a point will be determined by the ratios of the $(n+1)$ coordinates (x, y, z, ...). The corresponding analytical change is in the expression of the original magnitudes as fractions with a common denominator; we thus, in place of rational and integral non-homogeneous functions of the original variables, introduce rational and integral homogeneous functions (quantics) of the next succeeding number of variables— viz. we have binary quantics corresponding to one-dimensional geometry, ternary to two-dimensional geometry, and so on.

It is a digression, but I wish to speak of the representation of points or figures in space upon a plane. In perspective, we represent a point in space by means of the intersection with the plane of the picture (suppose a pane of glass) of the line drawn from the point to the eye, and doing this for each point of the object we obtain a representation or picture of the object. But such representation is an imperfect one, as not determining the object: we cannot by means of the picture alone find out the form of the object; in fact, for a given point of the picture the corresponding point of the object is not a determinate point, but it is a point anywhere in the line joining the eye with the point of the picture. To determine the object we need two pictures, such as we have in a plan and elevation, or, what is the same thing, in a representation on the system of Monge's descriptive geometry. But it is theoretically more simple to consider two projections on the same plane, with different positions of the eye: the point in space is here represented on the plane by means of two points which are such that the line joining them passes through a fixed point of the plane (this point is in fact the intersection with the plane of the picture of the line joining the two positions of the eye); the figure in space is thus represented on the plane by two figures, which are such that the lines joining corresponding points of the two figures pass always through the fixed point. And such two figures completely replace the figure in space; we can by means of them perform on the plane any constructions which could be performed on the figure in space, and employ them in the demonstration of properties relating to such figure. A curious extension has recently been made: two figures in space such that the lines joining corresponding points pass through a fixed point have been regarded by the Italian geometer Veronese as representations of a figure in four-dimensional space, and have been used for the demonstration of properties of such figure.

I referred to the connexion of Mathematics with the notions of space and time, but I have hardly spoken of time. It is, I believe, usually considered that the notion of number is derived from that of time; thus Whewell in the work referred to, p. xx, says number is a modification of the conception of repetition, which belongs to that of *time*. I cannot recognise that this is so: it seems to me that we have (independently, I should say, of space or time, and in any case not more depending on time than on space) the notion of plurality; we think of, say, the letters a, b, c, &c., and thence in the case

of a finite set—for instance *a, b, c, d, e*—we arrive at the notion of number; coordinating them one by one with any other set of things, or, suppose, with the words first, second, &c., we find that the last of them goes with the word fifth, and we say that the number of things is = five : the notion of cardinal number would thus appear to be derived from that of ordinal number.

Questions of combination and arrangement present themselves, and it might be possible from the mere notion of plurality to develope a branch of mathematical science; this, however, would apparently be of a very limited extent, and it is difficult *not* to introduce into it the notion of number; in fact, in the case of a finite set of things, to avoid asking the question, How many ? If we do this, we have a large enough subject, including the partition of numbers, which Sylvester has called Tactic.

From the notion thus arrived at of an integer number, we pass to that of a fractional number, and we see how by means of these the ratio of any two concrete magnitudes of the same kind can be expressed, not with absolute accuracy, but with any degree of accuracy we please : for instance, a length is so many feet, tenths of a foot, hundredths, thousandths, &c.; subdivide as you please, *non constat* that the length can be expressed accurately, we have in fact incommensurables ; as to the part which these play in the Theory of Numbers, I shall have to speak presently : for the moment I am only concerned with them in so far as they show that we cannot from the notion of number pass to that which is required in analysis, the notion of an abstract (real and positive) magnitude susceptible of continuous variation. The difficulty is got over by a Postulate. We consider an abstract (real and positive) magnitude, and regard it as susceptible of continuous variation, without in anywise concerning ourselves about the actual expression of the magnitude by a numerical fraction or otherwise.

There is an interesting paper by Sir W. R. Hamilton, "Theory of Conjugate Functions, or Algebraical Couples : with a preliminary and elementary Essay on Algebra as the Science of Pure Time," 1833—35 (*Trans. R. I. Acad.* t. XVII.), in which, as appears by the title, he purposes to show that algebra is the science of pure time. He states there, in the General Introductory Remarks, his conclusions : first, that the notion of time is connected with existing algebra ; second, that this notion or intuition of time may be unfolded into an independent pure science ; and, third, that the science of pure time thus unfolded is coextensive and identical with algebra, so far as algebra itself is a science ; and to sustain his first conclusion he remarks that "the history of algebraic science shows that the most remarkable discoveries in it have been made either expressly through the notion of *time*, or through the closely connected (and in some sort coincident) notion of continuous progression. It is the genius of algebra to consider what it reasons upon as *flowing*, as it was the genius of geometry to consider what it reasoned on as *fixed*. . . . And generally the revolution which Newton made in the higher parts of both pure and applied algebra was founded mainly on the notion of *fluxion*, which involves the notion of *time*." Hamilton uses the term algebra in a very wide sense, but whatever else he includes under it, he includes all that in contra-distinction to the Differential Calculus would be called algebra. Using the word in this restricted sense, I cannot myself recognise the connexion of algebra with the notion of time : granting that the notion of continuous progression presents itself, and is of

importance, I do not see that it is in anywise the fundamental notion of the science. And still less can I appreciate the manner in which the author connects with the notion of time his algebraical couple, or imaginary magnitude $a + bi$ ($a + b\sqrt{-1}$, as written in the memoir).

I would go further: the notion of continuous variation is a very fundamental one, made a foundation in the Calculus of Fluxions (if not always so in the Differential Calculus) and presenting itself or implied throughout in mathematics: and it may be said that a change of any kind takes place only in time; it seems to me, however, that the changes which we consider in mathematics are for the most part considered quite irrespectively of time.

It appears to me that we do not have in Mathematics the notion of time until we bring it there: and that even in kinematics (the science of motion) we have very little to do with it; the motion is a hypothetical one; if the system be regarded as actually moving, the rate of motion is altogether undetermined and immaterial. The relative rates of motion of the different points of the system are nothing else than the ratios of purely geometrical quantities, the indefinitely short distances simultaneously described, or which might be simultaneously described, by these points respectively. But whether the notion of time does or does not sooner enter into mathematics, we at any rate have the notion in Mechanics, and along with it several other new notions.

Regarding Mechanics as divided into Statics and Dynamics, we have in dynamics the notion of time, and in connexion with it that of velocity: we have in statics and dynamics the notion of force; and also a notion which in its most general form I would call that of corpus: viz. this may be, the material point or particle, the flexible inextensible string or surface, or the rigid body, of ordinary mechanics; the incompressible perfect fluid of hydrostatics and hydrodynamics; the ether of any undulatory theory; or any other imaginable corpus; for instance, one really deserving of consideration in any general treatise of mechanics is a developable or skew surface with absolutely rigid generating lines, but which can be bent about these generating lines, so that the element of surface between two consecutive lines rotates as a whole about one of them. We have besides, in dynamics necessarily, the notion of mass or inertia.

We seem to be thus passing out of pure mathematics into physical science; but it is difficult to draw the line of separation, or to say of large portions of the *Principia*, and the *Mécanique céleste*, or of the whole of the *Mécanique analytique*, that they are not pure mathematics. It may be contended that we first come to physics when we attempt to make out the character of the corpus as it exists in nature. I do not at present speak of any physical theories which cannot be brought under the foregoing conception of mechanics.

I must return to the Theory of Numbers; the fundamental idea is here integer number: in the first instance positive integer number, but which may be extended to include negative integer number and zero. We have the notion of a product, and that of a prime number, which is not a product of other numbers; and thence also that of a number as the product of a determinate system of prime factors. We have here the

elements of a theory in many respects analogous to algebra: an equation is to be solved—that is, we have to find the integer values (if any) which satisfy the equation; and so in other cases: the congruence notation, although of the very highest importance, does not affect the character of the theory.

But as already noticed we have incommensurables, and the consideration of these gives rise to a new universe of theory. We may take into consideration any surd number such as $\sqrt{2}$, and so consider numbers of the form $a + b\sqrt{2}$, (a and b any positive or negative integer numbers not excluding zero); calling these integer numbers, every problem which before presented itself in regard to integer numbers in the original and ordinary sense of the word presents itself equally in regard to integer numbers in this new sense of the word; of course all definitions must be altered accordingly: an ordinary integer, which is in the ordinary sense of the word a prime number, may very well be the product of two integers of the form $a + b\sqrt{2}$, and consequently not a prime number in the new sense of the word. Among the incommensurables which can be thus introduced into the Theory of Numbers (and which was in fact *first* so introduced) we have the imaginary i of ordinary analysis: viz. we may consider numbers $a + bi$ (a and b ordinary positive or negative integers, not excluding zero), and, calling these integer numbers, establish in regard to them a theory analogous to that which exists for ordinary real integers. The point which I wish to bring out is that the imaginary i does not in the Theory of Numbers occupy a unique position, such as it does in analysis and geometry; it is in the Theory of Numbers one out of an indefinite multitude of incommensurables.

I said that I would speak to you, not of the utility of mathematics in any of the questions of common life or of physical science, but rather of the obligations of mathematics to these different subjects. The consideration which thus presents itself is in a great measure that of the history of the development of the different branches of mathematical science in connexion with the older physical sciences, Astronomy and Mechanics: the mathematical theory is in the first instance suggested by some question of common life or of physical science, is pursued and studied quite independently thereof, and perhaps after a long interval comes in contact with it, or with quite a different question. Geometry and algebra must, I think, be considered as each of them originating in connexion with objects or questions of common life—geometry, notwithstanding its name, hardly in the measurement of land, but rather from the contemplation of such forms as the straight line, the circle, the ball, the top (or sugar-loaf): the Greek geometers appropriated for the geometrical forms corresponding to the last two of these, the words σφαῖρα and κῶνος, our sphere and cone, and they extended the word cone to mean the complete figure obtained by producing the straight lines of the surface both ways indefinitely. And so algebra would seem to have arisen from the sort of easy puzzles in regard to numbers which may be made, either in the picturesque forms of the Bija-Ganita with its maiden with the beautiful locks, and its swarms of bees amid the fragrant blossoms, and the one queen-bee left humming around the lotus flower; or in the more prosaic form in which a student has presented to him in a modern text-book a problem leading to a simple equation.

The Greek geometry may be regarded as beginning with Plato (B.C. 430—347): the notions of geometrical analysis, loci, and the conic sections are attributed to him, and there are in his Dialogues many very interesting allusions to mathematical questions: in particular the passage in the *Theœtetus*, where he affirms the incommensurability of the sides of certain squares. But the earliest extant writings are those of Euclid (B.C. 285): there is hardly anything in mathematics more beautiful than his wondrous fifth book; and he has also in the seventh, eighth, ninth and tenth books fully and ably developed the first principles of the Theory of Numbers, including the theory of incommensurables. We have next Apollonius (about B.C. 247), and Archimedes (B.C. 287—212), both geometers of the highest merit, and the latter of them the founder of the science of statics (including therein hydrostatics): his dictum about the lever, his "Εὕρηκα," and the story of the defence of Syracuse, are well known. Following these we have a worthy series of names, including the astronomers Hipparchus (B.C. 150) and Ptolemy (A.D. 125), and ending, say, with Pappus (A.D. 400), but continued by their Arabian commentators, and the Italian and other European geometers of the sixteenth century and later, who pursued the Greek geometry.

The Greek arithmetic was, from the want of a proper notation, singularly cumbrous and difficult; and it was for astronomical purposes superseded by the sexagesimal arithmetic, attributed to Ptolemy, but probably known before his time. The use of the present so-called Arabic figures became general among Arabian writers on arithmetic and astronomy about the middle of the tenth century, but was not introduced into Europe until about two centuries later. Algebra among the Greeks is represented almost exclusively by the treatise of Diophantus (A.D. 150), in fact a work on the Theory of Numbers containing questions relating to square and cube numbers, and other properties of numbers, with their solutions; this has no historical connexion with the later algebra, introduced into Italy from the East by Leonardi Bonacci of Pisa (A.D. 1202—1208) and successfully cultivated in the fifteenth and sixteenth centuries by Lucas Paciolus, or de Burʒo, Tartaglia, Cardan, and Ferrari. Later on, we have Vieta (1540—1603), Harriot, already referred to, Wallis, and others.

Astronomy is of course intimately connected with geometry; the most simple facts of observation of the heavenly bodies can only be *stated* in geometrical language: for instance, that the stars describe circles about the pole-star, or that the different positions of the sun among the fixed stars in the course of the year form a circle. For astronomical calculations it was found necessary to determine the arc of a circle by means of its chord: the notion is as old as Hipparchus, a work of whom is referred to as consisting of twelve books on the chords of circular arcs; we have (A.D. 125) Ptolemy's *Almagest*, the first book of which contains a table of arcs and chords with the method of construction; and among other theorems on the subject he gives there the theorem afterwards inserted in Euclid (Book VI. Prop. D) relating to the rectangle contained by the diagonals of a quadrilateral inscribed in a circle. The Arabians made the improvement of using in place of the chord of an arc the sine, or half chord, of double the arc; and so brought the theory into the form in which it is used in modern trigonometry: the before-mentioned theorem of Ptolemy, or rather a particular case of it, translated into the notation of sines, gives the expression for the sine of the sum

of two arcs in terms of the sines and cosines of the component arcs; and it is thus the fundamental theorem on the subject. We have in the fifteenth and sixteenth centuries a series of mathematicians who with wonderful enthusiasm and perseverance calculated tables of the trigonometrical or circular functions, Purbach, Müller or Regiomontanus, Copernicus, Reinhold, Maurolycus, Vieta, and many others; the tabulations of the functions tangent and secant are due to Reinhold and Maurolycus respectively.

Logarithms were invented, not exclusively with reference to the calculation of trigonometrical tables, but in order to facilitate numerical calculations generally; the invention is due to John Napier of Merchiston, who died in 1618 at 67 years of age; the notion was based upon refined mathematical reasoning on the comparison of the spaces described by two points, the one moving with a uniform velocity, the other with a velocity varying according to a given law. It is to be observed that Napier's logarithms were nearly but not exactly those which are now called (sometimes Napierian, but more usually) hyperbolic logarithms—those to the base e; and that the change to the base 10 (the great step by which the invention was perfected for the object in view) was indicated by Napier but actually made by Henry Briggs, afterwards Savilian Professor at Oxford (d. 1630). But it is the hyperbolic logarithm which is mathematically important. The direct function e^x or exp. x, which has for its inverse the hyperbolic logarithm, presented itself, but not in a prominent way. Tables were calculated of the logarithms of numbers, and of those of the trigonometrical functions.

The circular functions and the logarithm were thus invented each for a practical purpose, separately and without any proper connexion with each other. The functions are connected through the theory of imaginaries and form together a group of the utmost importance throughout mathematics: but this is mathematical theory; the obligation of mathematics is for the discovery of the functions.

Forms of spirals presented themselves in Greek architecture, and the curves were considered mathematically by Archimedes; the Greek geometers invented some other curves, more or less interesting, but recondite enough in their origin. A curve which might have presented itself to anybody, that described by a point in the circumference of a rolling carriage-wheel, was first noticed by Mersenne in 1615, and is the curve afterwards considered by Roberval, Pascal, and others under the name of the Roulette, otherwise the Cycloid. Pascal (1623—1662) wrote at the age of seventeen his *Essais pour les Coniques* in seven short pages, full of new views on these curves, and in which he gives, in a paragraph of eight lines, his theorem of the inscribed hexagon.

Kepler (1571—1630) by his empirical determination of the laws of planetary motion, brought into connexion with astronomy one of the forms of conic, the ellipse, and established a foundation for the theory of gravitation. Contemporary with him for most of his life, we have Galileo (1564—1642), the founder of the science of dynamics; and closely following upon Galileo we have Isaac Newton (1643—1727): the *Philosophiæ naturalis Principia Mathematica* known as the *Principia* was first published in 1687.

The physical, statical, or dynamical questions which presented themselves before the publication of the *Principia* were of no particular mathematical difficulty, but it

is quite otherwise with the crowd of interesting questions arising out of the theory of gravitation, and which, in becoming the subject of mathematical investigation, have contributed very much to the advance of mathematics. We have the problem of two bodies, or what is the same thing, that of the motion of a particle about a fixed centre of force, for any law of force; we have also the (mathematically very interesting) problem of the motion of a body attracted to two or more fixed centres of force; then, next preceding that of the actual solar system—the problem of three bodies; this has ever been and is far beyond the power of mathematics, and it is in the lunar and planetary theories replaced by what is mathematically a different problem, that of the motion of a body under the action of a principal central force and a disturbing force: or (in one mode of treatment) by the problem of disturbed elliptic motion. I would remark that we have here an instance in which an astronomical fact, the observed slow variation of the orbit of a planet, has directly suggested a mathematical method, applied to other dynamical problems, and which is the basis of very extensive modern investigations in regard to systems of differential equations. Again, immediately arising out of the theory of gravitation, we have the problem of finding the attraction of a solid body of any given form upon a particle, solved by Newton in the case of a homogeneous sphere, but which is far more difficult in the next succeeding cases of the spheroid of revolution (very ably treated by Maclaurin) and of the ellipsoid of three unequal axes: there is perhaps no problem of mathematics which has been treated by as great a variety of methods, or has given rise to so much interesting investigation as this last problem of the attraction of an ellipsoid upon an interior or exterior point. It was a dynamical problem, that of vibrating strings, by which Lagrange was led to the theory of the representation of a function as the sum of a series of multiple sines and cosines; and connected with this we have the expansions in terms of Legendre's functions P_n, suggested to him by the question just referred to of the attraction of an ellipsoid; the subsequent investigations of Laplace on the attractions of bodies differing slightly from the sphere led to the functions of two variables called Laplace's functions. I have been speaking of ellipsoids, but the general theory is that of attractions, which has become a very wide branch of modern mathematics; associated with it we have in particular the names of Gauss, Lejeune-Dirichlet, and Green; and I must not omit to mention that the theory is now one relating to n-dimensional space. Another great problem of celestial mechanics, that of the motion of the earth about its centre of gravity, in the most simple case, that of a body not acted upon by any forces, is a very interesting one in the mathematical point of view.

I may mention a few other instances where a practical or physical question has connected itself with the development of mathematical theory. I have spoken of two map projections—the stereographic, dating from Ptolemy; and Mercator's projection, invented by Edward Wright about the year 1600: each of these, as a particular case of the orthomorphic projection, belongs to the theory of the geometrical representation of an imaginary variable. I have spoken also of perspective, and of the representation of solid figures employed in Monge's descriptive geometry. Monge, it is well known, is the author of the geometrical theory of the curvature of surfaces and of curves of

curvature : he was led to this theory by a problem of earthwork ; from a given area, covered with earth of uniform thickness, to carry the earth and distribute it over an equal given area, with the least amount of cartage. For the solution of the corresponding problem in solid geometry he had to consider the intersecting normals of a surface, and so arrived at the curves of curvature. (See his " Mémoire sur les Déblais et les Remblais," *Mém. de l'Acad.*, 1781.) The normals of a surface are, again, a particular case of a doubly infinite system of lines, and are so connected with the modern theories of congruences and complexes.

The undulatory theory of light led to Fresnel's wave-surface, a surface of the fourth order, by far the most interesting one which had then presented itself. A geometrical property of this surface, that of having tangent planes each touching it along a plane curve (in fact, a circle), gave to Sir W. R. Hamilton the theory of conical refraction. The wave-surface is now regarded in geometry as a particular case of Kummer's quartic surface, with sixteen conical points and sixteen singular tangent planes.

My imperfect acquaintance as well with the mathematics as the physics prevents me from speaking of the benefits which the theory of Partial Differential Equations has received from the hydrodynamical theory of vortex motion, and from the great physical theories of heat, electricity, magnetism, and energy.

It is difficult to give an idea of the vast extent of modern mathematics. This word " extent " is not the right one : I mean extent crowded with beautiful detail— not an extent of mere uniformity such as an objectless plain, but of a tract of beautiful country seen at first in the distance, but which will bear to be rambled through and studied in every detail of hillside and valley, stream, rock, wood, and flower. But, as for anything else, so for a mathematical theory—beauty can be perceived, but not explained. As for mere extent, I can perhaps best illustrate this by speaking of the dates at which some of the great extensions have been made in several branches of mathematical science.

As regards geometry, I have already spoken of the invention of the Cartesian coordinates (1637). This gave to geometers the whole series of geometric curves of higher order than the conic sections : curves of the third order, or cubic curves ; curves of the fourth order, or quartic curves ; and so on indefinitely. The first fruits of it were Newton's *Enumeratio linearum tertii ordinis*, and the extremely interesting investigations of Maclaurin as to corresponding points on a cubic curve. This was at once enough to show that the new theory of cubic curves was a theory quite as beautiful and far more extensive than that of conics. And I must here refer to Euler's remark in the paper " Sur une contradiction apparente dans la théorie des courbes planes " (Berlin Memoirs, 1748), in regard to the nine points of intersection of two cubic curves (viz. that when eight of the points are given the ninth point is thereby completely determined): this is not only a fundamental theorem in cubic curves (including in itself Pascal's theorem of the hexagon inscribed in a conic), but it introduces into plane geometry a new notion—that of the point-system, or system of the points of intersection of two curves.

A theory derived from the conic, that of polar reciprocals, led to the general notion of geometrical duality—viz. that in plane geometry the point and the line are correlative figures; and founded on this we have Plücker's great work, the *Theorie der algebraischen Curven* (Bonn, 1839), in which he establishes the relation which exists between the order and class of a curve and the number of its different point- and line-singularities (Plücker's six equations). It thus appears that the true division of curves is not a division according to order only, but according to order and class, and that the curves of a given order and class are again to be divided into families according to their singularities: this is not a mere subdivision, but is really a widening of the field of investigation; each such family of curves is in itself a subject as wide as the totality of the curves of a given order might previously have appeared.

We *unite* families by considering together the curves of a given *Geschlecht*, or deficiency; and in reference to what I shall have to say on the Abelian functions, I must speak of this notion introduced into geometry by Riemann in the memoir "Theorie der Abel'schen Functionen," *Crelle*, t. LIV. (1857). For a curve of a given order, reckoning cusps as double points, the deficiency is equal to the greatest number $\frac{1}{2}(n-1)(n-2)$ of the double points which a curve of that order can have, less the number of double points which the curve actually has. Thus a conic, a cubic with one double point, a quartic with three double points, &c., are all curves of the deficiency 0; the general cubic is a curve, and the most simple curve, of the deficiency 1; the general quartic is a curve of deficiency 3; and so on. The deficiency is usually represented by the letter p. Riemann considers the general question of the rational transformation of a plane curve: viz. here the coordinates, assumed to be homogeneous or trilinear, are replaced by any rational and integral functions, homogeneous of the same degree in the new coordinates; the transformed curve is in general a curve of a different order, with its own system of double points; but the deficiency p remains unaltered; and it is on this ground that he unites together and regards as a single class the whole system of curves of a given deficiency p. It must not be supposed that all such curves admit of rational transformation the one into the other: there is the further theorem that any curve of the class depends, in the case of a cubic, upon one parameter, but for $p > 1$ upon $3p - 3$ parameters, each such parameter being unaltered by the rational transformation; it is thus only the curves having the same one parameter, or $3p - 3$ parameters, which can be rationally transformed the one into the other.

Solid geometry is a far wider subject: there are more theories, and each of them is of greater extent. The ratio is not that of the numbers of the dimensions of the spaces considered, or, what is the same thing, of the elementary figures—point and line in the one case; point, line and plane in the other case—belonging to these spaces respectively, but it is a very much higher one. For it is very inadequate to say that in plane geometry we have the curve, and in solid geometry the curve and surface: a more complete statement is required for the comparison. In plane geometry we have the curve, which may be regarded as a singly infinite system of points, and also as a singly infinite system of lines. In solid geometry we have, first, that which under one aspect is the curve, and under another aspect the developable, and which may be

regarded as a singly infinite system of points, of lines, or of planes; secondly, the surface, which may be regarded as a doubly infinite system of points or of planes, and also as a special triply infinite system of lines (viz. the tangent-lines of the surface are a special complex): as distinct particular cases of the former figure, we have the plane curve and the cone; and as a particular case of the latter figure, the ruled surface or singly infinite system of lines; we have *besides* the congruence, or doubly infinite system of lines, and the complex, or triply infinite system of lines. But, even if in solid geometry we attend only to the curve and the surface, there are crowds of theories which have scarcely any analogues in plane geometry. The relation of a curve to the various surfaces which can be drawn through it, or of a surface to the various curves that can be drawn upon it, is different in kind from that which in plane geometry most nearly corresponds to it, the relation of a system of points to the curves through them, or of a curve to the points upon it. In particular, there is nothing in plane geometry corresponding to the theory of the curves of curvature of a surface. To the single theorem of plane geometry, a right line is the shortest distance between two points, there correspond in solid geometry two extensive and difficult theories—that of the geodesic lines upon a given surface, and that of the surface of minimum area for any given boundary. Again, in solid geometry we have the interesting and difficult question of the representation of a curve by means of equations; it is not every curve, but only a curve which is the complete intersection of two surfaces, which can be properly represented by two equations $(x, y, z, w)^m = 0$, $(x, y, z, w)^n = 0$, in quadriplanar coordinates; and in regard to this question, which may also be regarded as that of the classification of curves in space, we have quite recently three elaborate memoirs by Nöther, Halphen, and Valentiner respectively.

In n-dimensional geometry, only isolated questions have been considered. The field is simply too wide; the comparison with each other of the two cases of plane geometry and solid geometry is enough to show how the complexity and difficulty of the theory would increase with each successive dimension.

In Transcendental Analysis, or the Theory of Functions, we have all that has been done in the present century with regard to the general theory of the function of an imaginary variable by Gauss, Cauchy, Puiseux, Briot, Bouquet, Liouville, Riemann, Fuchs, Weierstrass, and others. The fundamental idea of the geometrical representation of an imaginary variable $x + iy$, by means of the point having x, y for its coordinates, belongs, as I mentioned, to Gauss; of this I have already spoken at some length. The notion has been applied to differential equations; in the modern point of view, the problem in regard to a given differential equation is, not so much to reduce the differential equation to quadratures, as to determine from it directly the course of the integrals for all positions of the point representing the independent variable: in particular, the differential equation of the second order leading to the hypergeometric series $F(\alpha, \beta, \gamma, x)$ has been treated in this manner, with the most interesting results; the function so determined for all values of the parameters (α, β, γ) is thus becoming a known function. I would here also refer to the new notion in this part of analysis introduced by Weierstrass—that of the one-valued integer function, as defined by an

infinite series of ascending powers, convergent for all finite values, real or imaginary, of the variable x or $1/x - c$, and so having the one essential singular point $x = \infty$ or $x = c$, as the case may be : the memoir is published in the Berlin *Abhandlungen*, 1876.

But it is not only general theory : I have to speak of the various special functions to which the theory has been applied, or say the various known functions.

For a long time the only known transcendental functions were the circular functions sine, cosine, &c. ; the logarithm—i.e. for analytical purposes the hyperbolic logarithm to the base e ; and, as implied therein, the exponential function e^x. More completely stated, the group comprises the direct circular functions sin, cos, &c. ; the inverse circular functions $\sin^{-1}$ or arc sin, &c. ; the exponential function, exp. ; and the inverse exponential, or logarithmic, function, log.

Passing over the very important Eulerian integral of the second kind or gamma-function, the theory of which has quite recently given rise to some very interesting developments—and omitting to mention at all various functions of minor importance,—we come (1811—1829) to the very wide groups, the elliptic functions and the single theta-functions. I give the interval of date so as to include Legendre's two systematic works, the *Exercices de Calcul Intégral* (1811—1816) and the *Théorie des Fonctions Elliptiques* (1825—1828); also Jacobi's *Fundamenta nova theoriæ Functionum Ellipticarum* (1829), calling to mind that many of Jacobi's results were obtained simultaneously by Abel. I remark that Legendre started from the consideration of the integrals depending on a radical $\sqrt{X}$, the square root of a rational and integral quartic function of a variable x ; for this he substituted a radical $\Delta\phi$, $= \sqrt{1 - k^2 \sin^2 \phi}$, and he arrived at his three kinds of elliptic integrals $F\phi$, $E\phi$, $\Pi\phi$, depending on the argument or amplitude ϕ, the modulus k, and also the last of them on a parameter n ; the function F is properly an inverse function, and in place of it Abel and Jacobi each of them introduced the direct functions corresponding to the circular functions sine and cosine, Abel's functions called by him ϕ, f, F. and Jacobi's functions sinam, cosam, Δam, or as they are also written sn, cn, dn. Jacobi, moreover, in the development of his theory of transformation obtained a multitude of formulæ containing q, a transcendental function of the modulus defined by the equation $q = e^{-\pi K'/K}$, and he was also led by it to consider the two new functions H, Θ, which (taken each separately with two different arguments) are in fact the four functions called elsewhere by him Θ_1, Θ_2, Θ_3, Θ_4 ; these are the so-called theta-functions, or, when the distinction is necessary, the single theta-functions. Finally, Jacobi using the transformation $\sin \phi = $ sinam u, expressed Legendre's integrals of the second and third kinds as integrals depending on the new variable u, denoting them by means of the letters Z, Π, and connecting them with his own functions H and Θ : and the elliptic functions sn, cn, dn are expressed with these, or say with Θ_1, Θ_2, Θ_3, Θ_4, as fractions having a common denominator.

It may be convenient to mention that Hermite in 1858, introducing into the theory in place of q the new variable ω connected with it by the equation $q = e^{i\pi\omega}$ (so that ω is in fact $= iK'/K$), was led to consider the three functions $\phi\omega$, $\psi\omega$, $\chi\omega$, which denote respectively the values of $\sqrt[4]{k}$, $\sqrt[4]{k'}$ and $\sqrt[12]{kk'}$ regarded as functions of ω.

A theta-function, putting the argument $= 0$, and then regarding it as a function of ω, is what Professor Smith in a valuable memoir, left incomplete by his death, calls an omega-function, and the three functions $\phi\omega$, $\psi\omega$, $\chi\omega$ are his modular functions.

The proper elliptic functions sn, cn, dn form a system very analogous to the circular functions sine and cosine (say they are a sine and two separate cosines), having a like addition-theorem, viz. the form of this theorem is that the sn, cn and dn of $x + y$ are each of them expressible rationally in terms of the sn, cn and dn of x and of the sn, cn and dn of y; and, in fact, reducing itself to the system of the circular functions in the particular case $k = 0$. But there is the important difference of form that the expressions for the sn, cn and dn of $x + y$ are fractional functions having a common denominator: this is a reason for regarding these functions as the ratios of four functions A, B, C, D, the absolute magnitudes of which are and remain indeterminate (the functions sn, cn, dn are in fact quotients $[\Theta_1, \Theta_2, \Theta_3] \div \Theta_4$ of the four theta-functions, but this is a further result in nowise deducible from the addition-equations, and which is intended to be for the moment disregarded; the remark has reference to what is said hereafter as to the Abelian functions). But there is in regard to the functions sn, cn, dn (what has no analogue for the circular functions), the whole theory of transformation of any order n prime or composite, and, as parts thereof, the whole theory of the modular and multiplier equations; and this theory of transformation spreads itself out in various directions, in geometry, in the Theory of Equations, and in the Theory of Numbers. Leaving the theta-functions out of consideration, the theory of the proper elliptic functions sn, cn, dn is at once seen to be a very wide one.

I assign to the Abelian functions the date 1826—1832. Abel gave what is called his theorem in various forms, but in its most general form in the *Mémoire sur une propriété générale d'une classe très-étendue de Fonctions Transcendantes* (1826), presented to the French Academy of Sciences, and crowned by them after the author's death, in the following year. This is in form a theorem of the integral calculus, relating to integrals depending on an irrational function y determined as a function of x by any algebraical equation $F(x, y) = 0$ whatever: the theorem being that a sum of any number of such integrals is expressible by means of the sum of a determinate number p of like integrals, this number p depending on the form of the equation $F(x, y) = 0$ which determines the irrational y (to fix the ideas, remark that considering this equation as representing a curve, then p is really the deficiency of the curve; but as already mentioned, the notion of deficiency dates only from 1857): thus in applying the theorem to the case where y is the square root of a function of the fourth order, we have in effect Legendre's theorem for elliptic integrals $F\phi + F\psi$ expressed by means of a single integral $F\mu$, and not a theorem applying in form to the elliptic functions sn, cn, dn. To be intelligible I must recall that the integrals belonging to the case where y is the square root of a rational and integral function of an order exceeding four are (in distinction from the general case) termed hyperelliptic integrals: viz. if the order be 5 or 6, then these are of the class $p = 2$; if the order be 7 or 8, then they are of the class $p = 3$, and so on; the *general* Abelian integral of the class $p = 2$ is a hyperelliptic integral: but if $p = 3$, or any greater

value, then the hyperelliptic integrals are only a particular case of the Abelian integrals of the same class. The further step was made by Jacobi in the short but very important memoir "Considerationes generales de transcendentibus Abelianis," *Crelle*, t. IX. (1832): viz. he there shows for the hyperelliptic integrals of any class (but the conclusion may be stated generally) that the direct functions to which Abel's theorem has reference are not functions of a single variable, such as the elliptic sn, cn, or dn, but functions of p variables. Thus, in the case $p = 2$, which Jacobi specially considers, it is shown that Abel's theorem has reference to two functions $\lambda(u, v)$, $\lambda_1(u, v)$ each of two variables, and gives in effect an addition-theorem for the expression of the functions $\lambda(u+u', v+v')$, $\lambda_1(u+u', v+v')$ algebraically in terms of the functions $\lambda(u, v)$, $\lambda_1(u, v)$, $\lambda(u', v')$, $\lambda_1(u', v')$.

It is important to remark that Abel's theorem does not directly give, nor does Jacobi assert that it gives, the addition-theorem in a perfect form. Take the case $p = 1$: the result from the theorem is that we have a function $\lambda(u)$, which is such that $\lambda(u+v)$ can be expressed algebraically in terms of $\lambda(u)$ and $\lambda(v)$. This is of course perfectly correct, $\operatorname{sn}(u+v)$ is expressible algebraically in terms of $\operatorname{sn} u$, $\operatorname{sn} v$, but the expression involves the radicals $\sqrt{1 - \operatorname{sn}^2 u}$, $\sqrt{1 - k^2 \operatorname{sn}^2 u}$, $\sqrt{1 - \operatorname{sn}^2 v}$, $\sqrt{1 - k^2 \operatorname{sn}^2 v}$; but it does not give the three functions sn, cn, dn, or in anywise amount to the statement that the sn, cn and dn u of $u+v$ are expressible rationally in terms of the sn, cn and dn of u and of v. In the case $p = 1$, the right number of functions, each of one variable, is 3, but the three functions sn, cn and dn are properly considered as the ratios of 4 functions; and so, in general, the right number of functions, each of p variables, is $4^p - 1$, and these may be considered as the ratios of 4^p functions. But notwithstanding this last remark, it may be considered that the notion of the Abelian functions of p variables is established, and the addition-theorem for these functions in effect given by the memoirs (Abel 1826, Jacobi 1832) last referred to.

We have next for the case $p = 2$, which is hyperelliptic, the two extremely valuable memoirs, Göpel, "Theoria transcendentium Abelianarum primi ordinis adumbratio læva," *Crelle*, t. XXXV. (1847), and Rosenhain, "Mémoire sur les fonctions de deux variables et à quatre périodes qui sont les inverses des intégrales ultra-elliptiques de la première classe" (1846), Paris, *Mém. Savans Étrang.* t. XI. (1851), each of them establishing on the analogy of the single theta-functions the corresponding functions of two variables, or double theta-functions, and in connexion with them the theory of the Abelian functions of two variables. It may be remarked that in order of simplicity the theta-functions certainly precede the Abelian functions.

Passing over some memoirs by Weierstrass which refer to the general hyperelliptic integrals, p any value whatever, we come to Riemann, who died 1866, at the age of forty: collected edition of his works, Leipzig, 1876. His great memoir on the Abelian and theta-functions is the memoir already incidentally referred to, "Theorie der Abel'schen Functionen," *Crelle*, t. LIV. (1857); but intimately connected therewith we have his Inaugural Dissertation (Göttingen, 1851), *Grundlagen für eine allgemeine Theorie der Functionen einer veränderlichen complexen Grösse*: his treatment of the problem of the Abelian functions, and establishment for the purpose of this theory of the multiple theta-functions, are alike founded on his general principles of the

theory of the functions of a variable complex magnitude $x + iy$, and it is this which would have to be gone into for any explanation of his method of dealing with the problem.

Riemann, starting with the integrals of the most general form, and considering the inverse functions corresponding to these integrals—that is, the Abelian functions of p variables—defines a theta-function of p variables, or p-tuple theta-function, as the sum of a p-tuply infinite series of exponentials, the general term of course depending on the p variables; and he shows that the Abelian functions are algebraically connected with theta-functions of the proper arguments. The theory is presented in the broadest form; in particular as regards the theta-functions, the 4^p functions are not even referred to, and there is no development as to the form of the algebraic relations between the two sets of functions.

In the Theory of Equations, the beginning of the century may be regarded as an epoch. Immediately preceding it, we have Lagrange's *Traité des Équations Numériques* (1st ed. 1798), the notes to which exhibit the then position of the theory. Immediately following it, the great work by Gauss, the *Disquisitiones Arithmeticæ* (1801), in which he establishes the theory for the case of a prime exponent n, of the binomial equation $x^n - 1 = 0$: throwing out the factor $x - 1$, the equation becomes an equation of the order $n - 1$, and this is decomposed into equations the orders of which are the prime factors of $n - 1$. In particular, Gauss was thereby led to the remarkable geometrical result that it was possible to construct geometrically—that is, with only the ruler and compass—the regular polygons of 17 sides and 257 sides respectively. We have then (1826—1829) Abel, who, besides his demonstration of the impossibility of the solution of a quintic equation by radicals, and his very important researches on the general question of the algebraic solution of equations, established the theory of the class of equations since called Abelian equations. He applied his methods to the problem of the division of the elliptic functions, to (what is a distinct question) the division of the complete functions, and to the very interesting special case of the lemniscate. But the theory of algebraic solutions in its most complete form was established by Galois (born 1811, killed in a duel 1832), who for this purpose introduced the notion of a group of substitutions; and to him also are due some most valuable results in relation to another set of equations presenting themselves in the theory of elliptic functions—viz. the modular equations. In 1835 we have Jerrard's transformation of the general quintic equation. In 1870 an elaborate work, Jordan's *Traité des Substitutions et des équations algébriques*: a mere inspection of the table of contents of this would serve to illustrate my proposition as to the great extension of this branch of mathematics.

The Theory of Numbers was, at the beginning of the century, represented by Legendre's *Théorie des Nombres* (1st ed. 1798), shortly followed by Gauss' *Disquisitiones Arithmeticæ* (1801). This work by Gauss is, throughout, a theory of ordinary real numbers. It establishes the notion of a congruence; gives a proof of the theorem of reciprocity in regard to quadratic residues; and contains a very complete theory of binary quadratic forms $(a, b, c)(x, y)^2$, of negative and positive determinant, including

the theory, there first given, of the composition of such forms. It gives also the commencement of a like theory of ternary quadratic forms. It contains also the theory already referred to, but which has since influenced in so remarkable a manner the whole theory of numbers—the theory of the solution of the binomial equation $x^n - 1 = 0$: it is, in fact, the roots or periods of roots derived from these equations which form the incommensurables, or unities, of the complex theories which have been chiefly worked at; thus, the i of ordinary analysis presents itself as a root of the equation $x^4 - 1 = 0$. It was Gauss himself who, for the development of a real theory—that of biquadratic residues—found it necessary to use complex numbers of the before-mentioned form, $a + bi$ (a and b positive or negative real integers, including zero), and the theory of these numbers was studied and cultivated by Lejeune-Dirichlet. We have thus a new theory of these complex numbers, side by side with the former theory of real numbers: everything in the real theory reproducing itself, prime numbers, congruences, theories of residues, reciprocity, quadratic forms, &c., but with greater variety and complexity, and increased difficulty of demonstration. But instead of the equation $x^4 - 1 = 0$, we may take the equation $x^3 - 1 = 0$: we have here the complex numbers $a + b\rho$ composed with an imaginary cube root of unity, the theory specially considered by Eisenstein: again a new theory, corresponding to but different from that of the numbers $a + bi$. The general case of any prime value of the exponent n, and with periods of roots, which here present themselves instead of single roots, was first considered by Kummer: viz. if $n - 1 = ef$, and $\eta_1, \eta_2, ..., \eta_e$ are the e periods, each of them a sum of f roots, of the equation $x^n - 1 = 0$, then the complex numbers considered are the numbers of the form $a_1\eta_1 + a_2\eta_2 + ... + a_e\eta_e$ ($a_1, a_2, ..., a_e$ positive or negative ordinary integers, including zero): f may be $= 1$, and the theory for the periods thus includes that for the single roots.

We have thus a new and very general theory, including within itself that of the complex numbers $a + bi$ and $a + b\rho$. But a new phenomenon presents itself; for these special forms the properties in regard to prime numbers corresponded precisely with those for real numbers; a non-prime number was in one way only a product of prime factors; the power of a prime number has only factors which are lower powers of the same prime number: for instance, if p be a prime number, then, excluding the obvious decomposition $p \cdot p^2$, we cannot have $p^3 = $ a product of two factors A, B. In the general case this is not so, but the exception first presents itself for the number 23; in the theory of the numbers composed with the 23rd roots of unity, we have prime numbers p, such that $p^3 = AB$. To restore the theorem, it is necessary to establish the notion of ideal numbers; a prime number p is by definition not the product of two actual numbers, but in the example just referred to the number p is the product of two ideal numbers having for their cubes the two actual numbers A, B, respectively, and we thus have $p^3 = AB$. It is, I think, in this way that we most easily get some notion of the meaning of an ideal number, but the mode of treatment (in Kummer's great memoir, "Ueber die Zerlegung der aus Wurzeln der Einheit gebildeten complexen Zahlen in ihre Primfactoren," *Crelle*, t. XXXV. 1847) is a much more refined one; an ideal number, without ever being isolated, is made to manifest itself in the properties of the prime number of which it is a factor, and without reference to the

theorem afterwards arrived at, that there is always some power of the ideal number which is an actual number. In the still later developments of the Theory of Numbers by Dedekind, the units, or incommensurables, are the roots of any irreducible equation having for its coefficients ordinary integer numbers, and with the coefficient unity for the highest power of x. The question arises, What is the analogue of a whole number? thus, for the very simple case of the equation $x^2 + 3 = 0$, we have as a whole number the apparently fractional form $\frac{1}{2}(1 + i\sqrt{3})$ which is the imaginary cube root of unity, the ρ of Eisenstein's theory. We have, moreover, the (as far as appears) wholly distinct complex theory of the numbers composed with the congruence-imaginaries of Galois: viz. these are imaginary numbers assumed to satisfy a congruence which is not satisfied by any real number; for instance, the congruence $x^2 - 2 = 0$ (mod 5) has no real root, but we assume an imaginary root i, the other root is then $= -i$, and we then consider the system of complex numbers $a + bi$ (mod 5), viz. we have thus the 5^2 numbers obtained by giving to each of the numbers a, b, the values 0, 1, 2, 3, 4, successively. And so in general, the consideration of an irreducible congruence $F(x) = 0$ (mod p) of the order n, to any prime modulus p, gives rise to an imaginary congruence root i, and to complex numbers of the form $a + bi + ci^2 + \ldots + ki^{n-1}$, where a, b, k, $\ldots$ &c., are ordinary integers each $= 0$, 1, 2, $\ldots$, $p - 1$.

As regards the theory of forms, we have in the ordinary theory, in addition to the binary and ternary quadratic forms, which have been very thoroughly studied, the quaternary and higher quadratic forms (to these last belong, as very particular cases, the theories of the representation of a number as a sum of four, five or more squares), and also binary cubic and quartic forms, and ternary cubic forms, in regard to all of which something has been done; the binary quadratic forms have been studied in the theory of the complex numbers $a + bi$.

A seemingly isolated question in the Theory of Numbers, the demonstration of Fermat's theorem of the impossibility for any exponent λ greater than 3, of the equation $x^\lambda + y^\lambda = z^\lambda$, has given rise to investigations of very great interest and difficulty.

Outside of ordinary mathematics, we have some theories which must be referred to: algebraical, geometrical, logical. It is, as in many other cases, difficult to draw the line; we do in ordinary mathematics use symbols not denoting quantities, which we nevertheless combine in the way of addition and multiplication, $a + b$, and ab, and which may be such as not to obey the commutative law $ab = ba$: in particular, this is or may be so in regard to symbols of operation; and it could hardly be said that any development whatever of the theory of such symbols of operation did not belong to ordinary algebra. But I do separate from ordinary mathematics the system of multiple algebra or linear associative algebra, developed in the valuable memoir by the late Benjamin Peirce, *Linear Associative Algebra* (1870, reprinted 1881 in the *American Journal of Mathematics*, vol. IV., with notes and addenda by his son, C. S. Peirce); we here consider symbols A, B, &c. which are linear functions of a determinate number of letters or units i, j, k, l, &c., with coefficients which are ordinary analytical magnitudes, real or imaginary, viz. the coefficients are in general of the form $x + iy$, where

58

i is the before-mentioned imaginary or $\sqrt{-1}$ of ordinary analysis. The letters i, j, &c., are such that every binary combination i^2, ij, ji, &c., (the ij being in general not $=ji$), is equal to a linear function of the letters, but under the restriction of satisfying the associative law: viz. for each combination of three letters $ij \cdot k$ is $= i \cdot jk$, so that there is a determinate and unique product of three or more letters; or, what is the same thing, the laws of combination of the units i, j, k, are defined by a multiplication table giving the values of i^2, ij, ji, &c.; the original units may be replaced by linear functions of these units, so as to give rise, for the units finally adopted, to a multiplication table of the most simple form; and it is very remarkable, how frequently in these simplified forms we have nilpotent or idempotent symbols ($i^2 = 0$, or $i^2 = i$, as the case may be), and symbols i, j, such that $ij = ji = 0$; and consequently how simple are the forms of the multiplication tables which define the several systems respectively.

I have spoken of this multiple algebra before referring to various geometrical theories of earlier date, because I consider it as the general analytical basis, and the true basis, of these theories. I do not realise to myself directly the notions of the addition or multiplication of two lines, areas, rotations, forces, or other geometrical, kinematical, or mechanical entities; and I would formulate a general theory as follows: consider any such entity as determined by the proper number of parameters a, b, c (for instance, in the case of a finite line given in magnitude and position, these might be the length, the coordinates of one end, and the direction-cosines of the line considered as drawn from this end); and represent it by or connect it with the linear function $ai + bj + ck + $ &c., formed with these parameters as coefficients, and with a given set of units, i, j, k, &c. Conversely, any such linear function represents an entity of the kind in question. Two given entities are represented by two linear functions; the sum of these is a like linear function representing an entity of the same kind, which may be regarded as the sum of the two entities; and the product of them (taken in a determined order, when the order is material) is an entity of the same kind, which may be regarded as the product (in the same order) of the two entities. We thus establish by definition the notion of the sum of the two entities, and that of the product (in a determinate order, when the order is material) of the two entities. The value of the theory in regard to any kind of entity would of course depend on the choice of a system of units, i, j, k, ..., with such laws of combination as would give a geometrical or kinematical or mechanical significance to the notions of the sum and product as thus defined.

Among the geometrical theories referred to, we have a theory (that of Argand, Warren, and Peacock) of imaginaries in plane geometry; Sir W. R. Hamilton's very valuable and important theory of Quaternions; the theories developed in Grassmann's *Ausdehnungslehre*, 1841 and 1862; Clifford's theory of Biquaternions; and recent extensions of Grassmann's theory to non-Euclidian space, by Mr Homersham Cox. These different theories have of course been developed, not in anywise from the point of view in which I have been considering them, but from the points of view of their several authors respectively.

The literal symbols x, y, &c., used in Boole's *Laws of Thought* (1854) to represent things as subjects of our conceptions, are symbols obeying the laws of algebraic com-

bination (the distributive, commutative, and associative laws) but which are such that for any one of them, say x, we have $x - x^2 = 0$, this equation not implying (as in ordinary algebra it would do) either $x = 0$ or else $x = 1$. In the latter part of the work relating to the Theory of Probabilities, there is a difficulty in making out the precise meaning of the symbols; and the remarkable theory there developed has, it seems to me, passed out of notice, without having been properly discussed. A paper by the same author, "Of Propositions numerically definite" (*Camb. Phil. Trans.* 1869), is also on the border-land of logic and mathematics. It would be out of place to consider other systems of mathematical logic, but I will just mention that Mr C. S. Peirce in his "Algebra of Logic," *American Math. Journal*, vol. III., establishes a notation for relative terms, and that these present themselves in connexion with the systems of units of the linear associative algebra.

Connected with logic, but primarily mathematical and of the highest importance, we have Schubert's *Abzählende Geometrie* (1878). The general question is, How many curves or other figures are there which satisfy given conditions? for example, How many conics are there which touch each of five given conics? The class of questions in regard to the conic was first considered by Chasles, and we have his beautiful theory of the characteristics μ, ν, of the conics which satisfy four given conditions; questions relating to cubics and quartics were afterwards considered by Maillard and Zeuthen; and in the work just referred to the theory has become a very wide one. The noticeable point is that the symbols used by Schubert are in the first instance, not numbers, but mere logical symbols: for example, a letter g denotes the condition that a line shall cut a given line; g^2 that it shall cut each of two given lines; and so in other cases; and these logical symbols are combined together by algebraical laws: they first acquire a numerical signification when the number of conditions becomes equal to the number of parameters upon which the figure in question depends.

In all that I have last said in regard to theories outside of ordinary mathematics, I have been still speaking on the text of the vast extent of modern mathematics. In conclusion I would say that mathematics have steadily advanced from the time of the Greek geometers. Nothing is lost or wasted; the achievements of Euclid, Archimedes, and Apollonius are as admirable now as they were in their own days. Descartes' method of coordinates is a possession for ever. But mathematics have never been cultivated more zealously and diligently, or with greater success, than in this century—in the last half of it, or at the present time: the advances made have been enormous, the actual field is boundless, the future full of hope. In regard to pure mathematics we may most confidently say :—

> Yet I doubt not through the ages one increasing purpose runs,
> And the thoughts of men are widened with the process of the suns.