similar disk in a similar telephone at a distant station, causing it to vibrate in an identical manner, and therefore to emit identical sounds." Here we have Reis spoken of as inventing '*an* imperfect telephone,' while Bell invented '*the* articulating telephone.' Reis's instrument was a 'contact-breaker,' and conveyed 'musical tones.' Reis's instrument transmitted speech 'very imperfectly,' and there is not the slightest suggestion of microphonic action in the transmitter. Yet two years later we have statements diametrically opposed to these.

The least that can be said of such varying and contradictory evidence is, that it totally destroys the credibility of the witness, and nullifies his claim to be accepted as a scientific authority, unless good reason is shown for the different opinion. The documents quoted in the book give no substantial reason for this change of ground, as they add very little of any importance to what was already generally known. The motive for the later opinions may be more intelligibly traced in the following items, which will be found in the *Telegraphic journal and electrical review*, vol. xii. p. 72, Jan., 1883, and p. 317, April 14, 1883, in the list of English patents: — "2578. Telephonic instruments. SYLVANUS P. THOMPSON. Dated May 31. 6d. This invention relates to telephonic instruments, and chiefly to improvements in receivers of a well-known form or type, invented by Phillip Reis." "3803. Improvements in telephonic apparatus. SYLVANUS P. THOMPSON. Dated August 9. 6d. Relates to telephonic transmitters based upon the principle discovered by Philipp Reis in 1861, namely that of employing current-regulators actuated, either directly or indirectly, by the sound-waves produced by the voice. By the term 'current-regulator,' the inventor means a device similar to that employed by Reis, wherein a loose contact between two parts of a circuit (in which are included a battery and a telephonic receiver) offers greater or less resistance to the flow of the electric current, the degree of intimacy of contact between the conducting-pieces being altered by the vibrations of the voice."

For a contrast of colors, we may put side by side with these sentences the following, from the preface to the book now under consideration: "To set forth the history of this long-neglected inventor and of his instrument, and to establish upon its own merits, without special pleading, and without partiality, the nature of that much-misunderstood and much-abused invention, has been the aim of the writer. . . . He has nothing to gain by making Reis's invention appear either better or worse than it really was."

Further comment upon the value of such testimony as is contained in this book is surperfluous. What Reis accomplished, and what he failed to do, are now familiar matters of history. His well-earned fame can only suffer from such misstatement of facts, and the unjust exaggeration of his actual achievements.

OBLIGATIONS OF MATHEMATICS TO PHILOSOPHY, AND TO QUESTIONS OF COMMON LIFE.[1] — I.

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 ardor 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, Gen. 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, 1837, and 1838, 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 presi-

[1] Inaugural address by ARTHUR CAYLEY, M.A., D.C.L LL.D., F.R.S., Sadlerian professor of pure mathematics in the University of Cambridge, president of the British association for the advancement of science, for the Southport meeting. From advance proofs kindly furnished by the editors of *Nature*.

dent 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 any thing 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, paleontology, 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 sympathize 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 connection (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 any thing 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 seeingt he 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' (ninth edition, 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 inquire what is the ground of our belief in axioms, what is the evidence on which they rest. I answer, they are experimental truths, generalizations 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 inquiring, "Why are mathematics by almost all philosophers . . . considered to be independent of the evidence of experience and observation, and characterized 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 truths, 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 any one 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 any one doubt this, we may refer him to his own experience. I much question if any one 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, etc., 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, etc. 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 generalized 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 book-keeping by double entry.

But it is far otherwise with the notion which is really the fundamental one (and I cannot too strongly emphasize 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 inquiry. As regards the older metaphysical writers, this would be quite accounted for by saying that they knew nothing, and were not bound to know any thing, 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 generalized notion of distance. It is in connection 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, with which we become acquainted 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, any thing 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 farther. Suppose that as the rule moves away from a fixed central point of the plane it becomes shorter and shorter: if this shortening take 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 any thing 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 (converted, that is, into some form of developable surface), or converted into a curved 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 co-ordinates, as given in the 'Géometrie' forming a short supplement to his 'Traité de la méthode,' etc. (Leyden, 1637).

I show how by these we are led analytically to the notion of imaginary points in geometry. For instance: we arrive at the theorem that a straight line and circle in the same plane intersect *always* in two points, real or imaginary. 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 co-ordinates 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 consider 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 co-ordinates 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 co-ordinates 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 scale different, 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 in 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 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 co-ordinates 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 co-ordinates 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 co-ordinates 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 co-ordinates 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 co-ordinates of a point in space; and, on the like ground, we should at least wish to regard any four or more magnitudes as the co-ordinates 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 co-ordinates, 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.

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 omit some farther developments in regard to geometry, and all that I have written as to the connection of mathematics with the notion of time.

(*To be continued.*)

INTELLIGENCE FROM AMERICAN SCIENTIFIC STATIONS.

STATE INSTITUTIONS.

Illinois state laboratory of natural history, Normal, Ill.

Experiments with diseased caterpillars. — Prof. S. A. Forbes is making a special study of 'schlaffsucht,' or some very similar disease, among our native caterpillars. He has so far proven that the disease is characterized by an enormous development of bacteria in the alimentary canal, the same forms appearing in the blood before death; that it is contagious by way of the food ingested; that the characteristic bacteria may be easily and rapidly cultivated in sterilized beef-broth; and that caterpillars whose food has been moistened with this infected broth, speedily show the bacteria in the alimentary canal, and, later, in the blood, and soon all die of the disease. Other caterpillars of the same lot, receiving the same treatment, except that the food is moistened with distilled water instead of the infected broth, remain unaffected. These bacteria are likewise cultivable in vegetable infusions, but multiply there less freely.

Every step of the investigation is fortified by stained and mounted preparations, which are being submitted to cryptogamists. It has already been determined that the bacterium infesting a brood of Datana ministra in his breeding-cages is identical with the Micrococcus bombycis of the silk-worm; the form, measurements, modes of aggregation, and behavior to reagents, of the two, being the same. Datana Angusii, feeding upon walnut, was also occasionally infested by this M. bombycis, but much more commonly by a spherical species, probably undescribed.

In the cabbage-worm (Pieris rapae) occurs still another species of Micrococcus, very minute ($5\ \mu$ in diameter), globular, and usually either single or in pairs. This is far the most virulent of the insect affections, which is being studied by Forbes, — the