THE additions to the Zoological Society's Gardens during the past week include a Sykes's Monkey (*Cercopithecus albigularis* ♀) from East Africa, presented by Mr. G. N. Wylie; a Beatrix Antelope (*Oryx beatrix* ♀), an Indian Gazelle (*Gazella bennetti*) from Arabia, presented by Lieut.-Colonel Talbot; a Goshawk (*Astur palumbarius*), European, presented by Captain Noble; a Common Quail (*Coturnix communis*), European, presented by W. K. Purnell; a Hybrid Goose (between *Anser cinereus* and *A. brachyrhynchus*), captured in Holland, presented by Mr. F. E. Blaauw, C.M.Z.S.; a Gould's Monitor (*Varanus gouldi*), a Stump-tailed Lizard (*Trachydosaurus rugosus*) from New South Wales, presented by Mr. T. Hellberg; a Chub (*Leuciscus cephalus*), British fresh waters, presented by Mr. H. E. Young; two Yaks (*Poëphagus grunniens* ♂ ♀) from Tibet, three Gigantic Salamanders (*Megalobatrachus maximus*) from Japan, deposited; an Azara's Agouti (*Dasyprocta azaræ*), a Pucheran's Hawk (*Asturina pucherani*), a Sulphury Tyrant (*Pitangus sulphuratus*), two Short-winged Tyrants (*Machetornis rixosa*) a Brown Milvago (*Milvago chimango*), an Orange-billed Coot (*Fulica leucoptera*), a Cayenne Lapwing (*Vanellus cayennensis*), six Rosy-billed Ducks (*Metopiana peposaca* 3 ♂ 3 ♀) from South America, purchased; an American Bison (*Bison americanus* ♂) from North America, received in exchange; a Gayal (*Bibos frontalis* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE SOLAR DISTURBANCE OF 1891, JUNE 17.—In the October number of the *Observatory* Mr. H. H. Turner publishes an article on the luminous outburst on the sun observed by M. Trouvelot on June 17, and recorded in these columns on July 9. The disturbance was of such an unusual character that M. Trouvelot hazarded the suggestion that it was possibly accompanied by perturbations of the magnetic elements. Mr. Whipple was good enough to look over the Kew curves to see if they showed any such variations, and a negative result was obtained. Mr. Turner, however, after an examination of the Greenwich records has succeeded in finding "a very minute, though unmistakable, disturbance at almost precisely the time noted by Trouvelot. . . . The disturbance is smaller than many others on the same day, although the day itself was very quiet: but it differs from others in its abruptness, which is clearly shown in all three curves. The change in declination is only about 1′, and in H.F. 0·0005 of the whole H.F." Diagrams illustrating these fluctuations accompanied Mr. Turner's paper. It seemed strange that the Kew and the Greenwich records should differ in their indications, so a further enquiry was sent to Mr. Whipple, who replied as follows:—"I have again referred to the curves of June 17, 1891, and fail to find any trace of what can by any means be termed to be a magnetic disturbance at the time in question—accepting Sabine's interpretation of a magnetic disturbance (see Phil. Trans, vol. cliii., p. 274), and so avoiding loose expressions. According to the *Observatory*, October 1891, Father Sidgreaves is quite of our opinion as to the case in point." The evidence in favour of a magnetic disturbance simultaneously with Trouvelot's observation is thus not very strong.

PHOTOGRAPHY OF SOLAR PROMINENCES.—In a communication to the Paris Academy on February 8, M. Deslandres described some new results obtained by him in the photography of solar prominences. The object of the research was to photograph the spectra of prominences further into the ultra-violet than had previously been done. In July of last year, M. Deslandres, following Prof. Hale, succeeded in photographing the spectra to λ 380. He has now been able to obtain negatives upon which the spectrum extends from λ 410 to λ 350. In order to obtain this result, a siderostat with a mirror 8 inches in diameter has been employed to project the sun's image, a Rowland grating has been used to produce the spectra, and the lenses of the observing telescope have been made of quartz. The photographs show eight bright lines of the ultra-violet hydrogen series, and it is believed that observations made from an elevated station would lead to the detection of the remaining two. The line a little more refrangible than hydrogen α (λ 388), is also recorded upon the plates. Photographs have been taken of the spectra of spots and faculæ. The calcium lines at H and K often appear bright upon them, and are always stronger than the hydrogen lines. But no new facts appear to have been discovered in this direction of work.

ON THE VARIATION OF LATITUDE.—Dr. S. C. Chandler has published a series of papers on the variation of latitude, in the *Astronomical Journal* from No. 248 to No. 251. The general result of a wide discussion indicates a revolution of the earth's axis of inertia about that of rotation from west to east, with a radius of 30 feet measured at the earth's surface, in a period of 427 days.

NON-EUCLIDIAN GEOMETRY.[1]

EVERY conclusion supposes premisses; these premisses themselves are either self-evident and have no need of demonstration, or can only be established by assuming other propositions; and as we cannot continue this process to infinity, every deductive science, and especially geometry, must rest on a certain number of axioms which cannot be demonstrated. All treatises on geometry therefore commence with the enunciation of these axioms. But a distinction must be made between them: some—such as this for example, "Two quantities that are equal to a third quantity are equal to one another"—are not geometrical propositions, but are analytical ones. I regard them as analytical *a priori* judgments, and as such I will not discuss them. But I must insist on other axioms which are special to geometry. Text-books for the most part state them very explicitly:—

(1) Only one straight line can be drawn between two points.
(2) A straight line is the shortest distance between two points.
(3) Only one straight line can be drawn through a point parallel to a given straight line.

Although the demonstration of the second of these axioms is generally dispensed with, it would be possible to deduce it from the other two, and from those, of which the number is more considerable, that we admit explicitly without stating them, as I shall explain in the sequel.

Efforts have also for a long time been made without success to demonstrate the third axiom, known under the name of the *postulatum d'Euclide*. The amount of trouble that has been taken in that chimerical hope is truly beyond imagination. Finally, at the commencement of the century, and almost simultaneously, Lowatchewski and Bolyai, two men of science, a Russian and Hungarian respectively, established, in an irrefutable manner, that such a demonstration was impossible; they have very nearly rid us of the inventors of geometries without postulates: since their time the Academy of Sciences only receives annually one or two new demonstrations.

The question was still not settled; soon a great step was made by the publication of the celebrated memoir of Riemann, entitled "Ueber die Hypothesen welche der Geometrie zum Grunde liegen." This small treatise has inspired the majority of recent works, of which I will make mention subsequently, and among which must be mentioned those of Beltrami and Helmholtz.

The Geometry of Lowatchewski.—If it were possible to deduce the *postulatum d'Euclide* from the other axioms, it would evidently happen that in denying the postulate and admitting the axioms, we should be led to contradictory results; it would then be impossible to base a coherent geometry on such premisses.

But this is precisely what Lowatchewski has done. He supposes in the first place that—

"*Several straight lines can be drawn through a point parallel to a given straight line.*"

And he moreover retains all the other axioms of Euclid. From these hypotheses he deduces a series of theorems among which it is impossible to detect any contradiction, and he constructs a geometry the faultless logic of which is not inferior to that of the Euclidian geometry.

The theorems are, certainly, very different from those to which we are accustomed, and they disconcert us a little at first.

Thus, the sum of the angles of a triangle is always less than two right angles; and the difference between this sum and two right angles is proportional to the surface of the triangle.

[1] Translation of an article that appeared in the *Revue Générale des Sciences*, No. 23, by M. H. Poincaré.

It is impossible to construct a figure similar to a given figure, but of different dimensions.

If a circle be divided into n equal parts, and tangents be drawn to the points of division, these n tangents will meet and form a polygon, provided that the radius of the circle be small enough; but if this radius is sufficiently large, they will not meet. It is useless to multiply these examples; the propositions of Lowatchewski have no longer any connection with those of Euclid, but they are not less logically connected together.

The Geometry of Riemann.—Let us imagine a world peopled only with beings deprived of thickness; and let us suppose that these animals, "infinitely flat," are all in one plane, and are not able to get out of it. Let us admit, further, that this world is removed sufficiently from others to be free from their influence. As we are making these assumptions, we may as well endow these beings both with reasoning powers and the capacity of founding a geometry. In this case they would certainly attribute to space only two dimensions.

But let us suppose, however, that these imaginary animals, all still devoid of thickness, have the form of a portion of a spherical figure, and not of a plane one, and are all on one and the same sphere without being able to leave it. What geometry would they construct? It is clear at once that they would only attribute to space two dimensions: that which will play for them the part of the straight line will be the shortest distance between two points on the sphere—that is to say, an arc of a great circle; in a word, their geometry would be spherical geometry.

What they will call space will be this sphere which they cannot leave, and on which occur all the phenomena of which they can have any knowledge. Their space then will be *without limits*, since on a sphere one can always go forward, without ever coming to an end, and nevertheless it will be *finite*—one will never find the limit, but one can make the circuit of it.

In fact, the geometry of Riemann is spherical geometry extended to three dimensions. To construct it, the German mathematician had to throw overboard not only the postulates of Euclid, but even the first axiom: *Only one straight line can be drawn between two points.*

On a sphere only one great circle in general can be drawn through two given points (which, as we have just seen, would play the part of the straight line to our imaginary beings); but to this there is an exception; for, if the two given points are diametrically opposed, an infinite number of great circles can be made to pass through them.

In the same way, in the geometry of Riemann, only one straight line in general can be drawn between two points; but there are exceptional cases where an infinite number of straight lines can be drawn between them.

There is a kind of opposition between the geometry of Riemann and that of Lowatchewski.

Thus, the sum of the angles of a triangle is—

Equal to two right angles in Euclid's geometry.
Less than two right angles in that of Lowatchewski.
Greater than two right angles in that of Riemann.

The number of parallels that can be drawn to a given straight line through a given point is equal—

To one in the geometry of Euclid.
To zero in that of Riemann.
To infinity in that of Lowatchewski.

Let us add that the space of Riemann is finite although without limit, in the sense already given to these two words.

Surfaces of Constant Curvature.—There was, however, one possible objection. The theorems of Lowatchewski and of Riemann present no contradiction, but, however numerous the consequences which these two geometers have drawn from their hypotheses, they were compelled to stop before they had exhausted all of them, for the number would be infinite: who can say, therefore, that, if they had carried their deductions further, they would not finally have found such contradictions?

This difficulty does not exist for the geometry of Riemann, provided that it is limited to two dimensions; for, in fact, the geometry of Riemann for two dimensions does not differ, as we have seen, from spherical geometry, which is only a branch of ordinary geometry, and consequently outside all discussion.

M. Beltrami, in considering in the same way the two-dimensional geometry of Lowatchewski to be only a branch of ordinary geometry, has equally refuted the objection in this case.

This he has done this in the following manner:—Consider on a surface any figure. Imagine this figure, traced on a flexible and inextensible cloth, to be laid on this surface, in such a way that when the cloth is moved and changes its shape, the various lines of this figure can change form without altering their length. In general this flexible and inextensible figure cannot leave its place without quitting the surface; but there are certain particular surfaces for which a similar movement would be possible; these are the surfaces with constant curvature.

If we resume the comparison that we previously made, and imagine beings without thickness living on one of these surfaces, they will regard the movement of a figure all of whose lines preserve a constant length as possible. A like movement, on the other hand, would appear absurd to animals without thickness living on a surface whose curvature was variable.

These surfaces of constant curvature are of two kinds:—

Some are of *positive curvature*, and can be so deformed as to be laid on a sphere. The geometry of these surfaces becomes then spherical geometry, which is that of Riemann.

Others are of *negative curvature*. M. Beltrami has shown that the geometry of these surfaces is none other than that of Lowatchewski. The two-dimensional geometries of Riemann and Lowatchewski are thus found to be re-attached to Euclidian geometry.

Interpretation of Non-Euclidian Geometries.—Thus the objection disappears as regards geometries of two dimensions.

It would be easy to extend M. Beltrami's reasoning to geometries of three dimensions. The minds which space of four dimensions does not repel will see here no difficulty; but they are few. I prefer, then, to proceed otherwise.

Let us consider a particular plane that we will call fundamental, and construct a kind of dictionary, making a double series of words, written in the two columns, correspond each to each, in the same way that the words of two languages, having the same signification correspond in ordinary dictionaries:—

Space	Portion of space situated above the fundamental plane.
Plane	Sphere cutting orthogonally the fundamental plane.
Right line	Circle cutting orthogonally the fundamental plane.
Sphere	Sphere.
Circle	Circle.
Angle	Angle.
Distance between two points ...	Logarithm of the anharmonic ratio of these two points and the intersections of the fundamental plane with a circle passing through these two points and cutting it orthogonally.
&c.,	&c.

Let us take, then, the theorems of Lowatchewski, and translate them by means of this dictionary, as we should translate a German text with the aid of a German-French dictionary. *We shall obtain then the theorems of ordinary geometry.*

For example, this theorem of Lowatchewski—"The sum of the angles of a triangle is less than two right angles"—is translated thus: "If a curvilinear triangle has for its sides the arcs of a circle which if prolonged would cut orthogonally the fundamental plane, the sum of the angles of this curvilinear triangle will be less than two right angles." Thus, however far one pushes the results of the hypotheses of Lowatchewski, one will never be led to a contradiction. Indeed, if two of Lowatchewski's theorems were contradictory, the translations of these two theorems, made with the help of our dictionary, would also be contradictory; but these translations are theorems of ordinary geometry, and everyone agrees that ordinary geometry is free from contradictions. Whence comes this certainty, and is it justified? This is a question that I cannot treat here, but which is very interesting, and, as I believe, soluble. The objection that I have formulated above no longer then exists.

But this is not all. The geometry of Lowatchewski, susceptible of a concrete interpretation, ceases to be a frivolous logical exercise, and is capable of application: I have not the time to mention here either these applications or the use that M. Klein and myself had made of them for the integration of linear equations.

This interpretation, moreover, is not unique, and one could construct several dictionaries analogous to that given above, and by which we could by a simple "translation" transform the theorems of Lowatchewski into theorems of ordinary geometry

Implicit Axioms.—Are then the axioms explicitly enunciated in treatises the only foundations of geometry? One can be assured to the contrary when one sees that, after having successively abandoned them, there still remain some propositions common to theorems of Euclid, Lowatchewski, and Riemann. These propositions ought to rest on some premisses, as geometers admit, although they do not state them. It is interesting to try to liberate them from classical demonstrations.

Stuart Mill has made the assertion that every definition contains an axiom, since, in defining it, the existence of the object defined is implicitly affirmed. This is going too far: it is seldom that one gives a definition in mathematics without following it by the demonstration of the existence of the object defined, and when it is omitted, it is generally because the reader can easily supply it. It must not be forgotten that the word existence has not the same sense when it is the question of a mathematical creation as when we have to do with a material object. A mathematical creation exists, provided that its definition involves no contradiction either in itself or with the properties previously admitted.

But if Stuart Mill's remark cannot be applied to all definitions, it is none the less true for some of them.

A plane is sometimes defined in the following manner:—The plane is a surface such that the straight line which joins any two points in it lies altogether in the surface.

This definition manifestly hides a new axiom: we could, it is true, alter it, and that would be better, but then it would be necessary to enunciate the axiom more explicitly.

Other definitions give place to reflections no less important. Such is, for example, that of the equality of two figures: two figures are equal when they can be superposed; to superpose them it is necessary to displace one until it coincides with the other; but how must it be displaced? If we ask, we should be answered that it ought to be done without changing its shape, and in the manner of an invariable solid. The "reasoning in a circle" would then be evident.

In truth, this definition implies nothing. It would have no meaning for a being who lived in a world where there were only fluids. If it seems clear to us, it is that we are accustomed to the properties of natural solids that do not differ greatly from those of ideal solids whose dimensions are all invariable.

Meanwhile, however imperfect it may be, this definition implies an axiom.

The possibility of the movement of an invariable figure is not a truth evident by itself, or at least it is only one in the same way as the *postulatum d'Euclide*, and not as an analytical *a priori* judgment would be.

Moreover, in studying the definitions and the demonstrations of geometry, we see that one is obliged to admit, without demonstrating it, not only the possibility of this movement, but even some of its properties.

This results, first of all, from the definition of the straight line. Many defective definitions have been given, but the true one is that which is understood in all the demonstrations where the straight line is in question:

"It may happen that the movement of a constant figure is such that all points of a line belonging to this figure remain immovable while all the points situated outside this line are displaced. Such a line will be called a straight line."

We have in this enunciation purposely separated the definition from the axiom that it implies.

Several proofs, such as those relating to the equality of triangles which depend on the possibility of letting fall a perpendicular from a point on a line, assume propositions that are not enunciated, since we must admit that it is possible to carry a figure from one place to another in a certain manner.

The Fourth Geometry.—Among these implicit axioms, there is one which seems to me worth mentioning, not only because it has given rise to a recent discussion,[1] but because in abandoning it, one can construct a fourth geometry, as coherent as those of Euclid, Lowatchewski, and Riemann.

To demonstrate that we can always raise from a point, A, a perpendicular to a straight line, AB, a straight line, AC, is considered movable round the point A, and in the first instance coinciding with the fixed line AB; and it is made to turn round the point A until it lies in the prolongation of AB.

[1] See MM. Renouvier, Léchalas, Calinon, *Revue Philosophique*, June 1889; *Critique Philosophique*, September 30 and November 30, 1889; *Revue Philosophique*, 1890, p. 158. See especially the discussion on the "postulate of perpendicularity."

We thus assume two propositions: first, that such a rotation is possible, and then that it can be continued until the two lines are in one straight line.

If the first point be admitted, and the second rejected, we are led to a series of theorems still more curious than those of Lowatchewski and Riemann, but equally free from contradiction.

I will quote only one of them, and that not the most singular: *A true straight line can be perpendicular to itself.*

The Theorem of Lie.—The number of implicit axioms introduced in classical demonstrations is greater than it need be, and it would be interesting to reduce them to a minimum. We can ask ourselves, in the first place, if this reduction is possible, if the number of necessary axioms, and imaginable geometries is not infinite.

M. Sophus Lie's theorem dominates all this discussion: it can be thus stated:—

Let us suppose that the following premisses are admitted:—

(1) Space has n dimensions.

(2) The movement of an invariable figure is possible.

(3) To determine the position of this figure in space, p conditions are necessary.

The number of geometries compatible with these premisses will be limited.

I can even add that, if n be given, a higher limit to p can be assigned.

If, then, the possibility of movement be admitted, only a finite number (and that a restricted one) of geometries can be invented.

The Geometries of Riemann.—However, this result seems to be contradicted by Riemann, because this investigator constructed an infinite number of different geometries, and the one which generally bears his name is only a particular case.

Everything depends, he says, on the way in which we define the length of a curve. But there are an infinite number of ways of defining this length, and each of these can become the starting point of a new geometry.

That is perfectly true; but most of these definitions are incompatible with the movement of an invariable figure, which is supposed possible in Lie's theorem. These geometries of Riemann, so interesting on many grounds, can only then remain purely analytical, and do not lend themselves to demonstrations analogous to those of Euclid.

The Nature of Axioms.—Most mathematicians regard the geometry of Lowatchewski only as a simple logical curiosity; some of them, however, have gone further. Since several geometries are possible, is it certain that ours is the true one? Experience, doubtless, teaches us that the sum of the angles of a triangle is equal to two right angles; but this is only because we operate on too small triangles; the difference, according to Lowatchewski, is proportional to the surface of the triangle; will it not become sensible if we work with larger triangles, or if our means of measurement grow more accurate? Euclidian geometry would only then be a provisional geometry.

To discuss this question, we ought in the first instance to inquire into the nature of geometrical axioms.

Are they synthetical conclusions *a priori*, as Kant used to say?

They would appeal to us then with such force, that we could not conceive the contrary proposition, nor construct on it a theoretical edifice. There could not be a non-Euclidian geometry.

To convince oneself of it, let us take a true synthetical *a priori* conclusion; for example, the following:—

If an infinite series of positive whole numbers be taken, all different from each other, there will always be one number that is smaller than all the others.

Or this other, which is equivalent to it:—

If a theorem be true for the number 1, *and if it has been shown to be true for* $n + 1$, *provided that it is true for n, then it will be true for all positive whole numbers.*

Let us next try to free ourselves from this conclusion, and, denying these propositions, to invent a false arithmetic analogous to the non-Euclidian geometry. We find that we cannot; we shall be even tempted in the first instance to regard these conclusions as the results of analysis.

Moreover, let us resume our idea of the indefinitely thin animals: surely we can scarcely admit that these beings, if they have minds like ours, would adopt Euclidian geometry, which would be contrary to all their experience.

Ought we, then, to conclude that the axioms of geometry are experimental truths? But we do not experiment on straight lines or ideal circles; only material objects can be dealt with. On what would depend, then, the experiments which would serve to found a geometry? The answer is easy.

We have seen above that one argues constantly as if geometrical figures behaved like solids. That which geometry would borrow from experience is therefore the properties of these bodies.

But a difficulty exists, and it cannot be overcome. If geometry were an experimental science, it would not be an exact science—it would be liable to a continual revision. What do I say? It would from to-day be convicted of error, since we know that a rigorously invariable solid does not exist.

Geometrical axioms, therefore, are neither synthetic a priori conclusions nor experimental facts.

They are *conventions:* our choice, amongst all possible conventions, is *guided* by experimental facts; but it remains *free*, and is only limited by the necessity of avoiding all contradiction. It is thus that the postulates can remain rigorously true, even when the experimental laws which have determined their adoption are only approximate.

In other words, *axioms of geometry* (I do not speak of those of arithmetic) are only definitions in disguise.

This being so, what ought one to think of this question: Is the Euclidian geometry true?

The question is nonsense.

One might just as well ask whether the metric system is true and the old measures false; whether Cartesian co-ordinates are true and polar co-ordinates false; whether one geometry cannot be more true than another—it can only be more convenient.

Now, Euclidian geometry is, and will remain, the most convenient:—

(1) Because it is the simplest; and it is not so simply on account of our habits of thought, or any kind of direct intuition which we may have of Euclidian space; it is the most simple in itself in the same way as a polynomial of the first order is simpler than one of the second.

(2) Because it agrees sufficiently well with the properties of natural solids, those bodies which come nearer to our members and our eye, and with which we make our instruments of measurement.

Geometry and Astronomy.—The above question has also been stated in another way. If the geometry of Lowatchewski is true, the parallax of a very distant star would be finite; if that of Riemann be true, it would be negative. Here we have results which seem subject to experience, and it has been hoped that astronomical observations would have been able to decide between the three geometries.

But what one calls a straight line in astronomy is simply the trajectory of a ray of light. If then, as is impossible, we had discovered negative parallaxes, or shown that all parallaxes are greater up to a certain limit, we should have the choice between two conclusions:—

We could renounce Euclidian geometry, or modify the laws of optics, and admit that light is not propagated strictly in straight lines.

It is useless to add that everyone would regard the latter solution as the more advantageous.

Euclidian geometry, then, has nothing to fear from new experiments.

Let me be pardoned for stating a little paradox in conclusion:—

The beings which had minds like ours, and who had the same senses as we have, but who had not received any previous education, might receive conventionally from an exterior world choices of impressions such that they would be led to construct a geometry different from that of Euclid, and to localize the phenomena of this exterior world in a non-Euclidian space, or even in a space of four dimensions.

For us, whose education has been formed by our real world, if we were suddenly transported in this new one, we should not have any difficulty in referring the phenomena to our Euclidian space.

Anyone who should dedicate his life to it could, perhaps, eventually imagine the fourth dimension.

I fear that in the last few lines I have not been very clear. I can only be so by introducing new developments; but I have already been too long, and those whom these explanations might interest have read their Helmholtz.

Desiring to be brief, I have affirmed more than I have proved: the reader must pardon me for this. So much has been written on this subject, so many different opinions have been put forward, that the discussion of them would fill a volume.

W. J. L.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 11.—"The *Rôle* played by Sugar in the Animal Economy: Preliminary Note on the Behaviour of Sugar in Blood." By Vaughan Harley, M.D.

This communication was to show that the causes why the whole amount of added sugar can seldom be recovered from blood are threefold. Firstly, the imperfections in the as yet known methods of analysis. Secondly, the different ways in which the albumens of the blood behave themselves while coagulating; some coagulating in the form of firm clots, which retain the saccharine matter in their interstices, rendering it impossible to extract all the sugar from them by washing; others separating as loose, flocculent curds, from which the sugar can be regained with comparative facility. While, thirdly, as bacteria were distinctly ascertained to have nothing to do in the matter, and yet the loss of the sugar added to the blood is in every instance distinctly progressive—according to the period of time the sugar is left in contact with the blood before the analysis is begun—Dr. Vaughan Harley considered himself justified in saying that there must exist in the normal blood itself a sugar-transforming agent. This he described as an enzyme; but refrained from going into any further particulars regarding it until his researches upon the subject are more advanced.

He gave tables of the results of his experiments, and compared them with those recently published by Schenk, Rohmann, and Seegen; showing that while the percentages of the sugar regained by the first observer ranged from 20 to 55 per cent., and those recovered by the two last experimenters fluctuated between 80 and 96 per cent., in his three different series of experiments, where different methods of analysis were employed, the percentages of the added sugar regained ranged respectively between 85 and 100; 92·9 and 99·3; and 94·7 and 99·9 per cent.

Mathematical Society, February 11.—Prof. Greenhill, F.R.S., President, in the chair.—The following communications were made:—On the logical foundations of applied mathematical sciences, by Mr. Dixon. He maintained the importance of distinguishing in all sciences between what is dependent on verbal conventions and what is not. He thus distinguished between that part of the meaning of a term which is laid down as its definition, and the part which remains to be discovered as a consequence of the definition. So also sciences might be divided into purely symbolic sciences, which being based on definitions alone conveyed no real information; subjective sciences, which deal with concepts and objective sciences, which deal with actual things. He then stated the conditions under which a set of assertions might be arbitrarily laid down as the definition of a term; and applied these conditions to show that Newton's three laws of motion could be regarded as a definition of the term force, that if this was done there could no longer be any discussion as to whether or not force alone is sufficient to account for the movements of matter. The anomaly that we are apparently able to determine directions absolutely, though we can determine positions only relatively, was explained, and a formal proof of all the elementary theorems of mechanics, including the principle of virtual work, might be deduced.—Note on the inadmissibility of the usual reasoning by which it appears that the limiting value of the ratio of two infinite functions is the same as the ratio of their first derived, with instances in which the result obtained by it is erroneous, by Mr. Culverwell.—On Saint Venant's theory of the torsion of prisms, by Mr. A. B. Basset, F.R.S.

DUBLIN

Royal Society, January 20.—Prof. W. N. Hartley, F.R.S., in the chair.—Reports on the zoological collections made by Prof. Haddon in Torres Straits, 1888–89: the Hydrocorallinæ, by S. J. Hickson. The specimens described are a female stock of *Stylaster gracilis*, *Distichopora violacea*, and *Millepora Murrayi*. Some of the smaller colonies of *Distichopora* are bright orange in colour, others vandyke brown, and the larger ones are deep purple with pale yellowish tips. The author