VOL. IX. OCTOBER, 1898. NO. 1.

THE MONIST

ON THE FOUNDATIONS OF GEOMETRY.[1]

ALTHOUGH I have already had occasion to set forth my views on the foundations of geometry,[2] it will not, perhaps, be unprofitable to revert to the question with new and ampler developments, and seek to clear up certain points which the reader may have found obscure. It is with reference to the definition of the point and the determination of the number of dimensions that new light appears to me most needed; but I deem it opportune, nevertheless, to take up the question from the beginning.

SENSIBLE SPACE.

Our sensations cannot give us the notion of space. That notion is built up by the mind from elements which pre-exist in it, and external experience is simply the occasion for its exercising this power, or at most a means of determining the best mode of exercising it.

Sensations by themselves have no spatial character.

This is evident in the case of isolated sensations—for example, visual sensations. What could a man see who possessed but a single immovable eye? Different images would be cast upon different points of his retina, but would he be led to classify these images as we do our present retinal sensations?

[1] Translated from Professor Poincaré's MS. by T. J. McCormack.

[2] Both in the *Revue Générale des Sciences* and in the *Revue de Métaphysique et de Morale*.

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Suppose images formed at four points A, B, C, D of this immovable retina. What ground would the possessor of this retina have for saying that, for example, the distance AB was equal to the distance CD? We, constituted as we are, have a reason for saying so, because we know that a *slight* movement of the eye is sufficient to bring the image which was at A to C, and the image which was at B to D. But these slight movements of the eye are impossible for our hypothetical man, and if we should ask him whether the distance AB was equal to the distance CD, we should seem to him as ridiculous as would a person appear to us who should ask us whether there was more difference between an olfactory sensation and a visual sensation than between an auditive sensation and a tactual sensation.

But this is not all. Suppose that two points A and B are very near to each other, and that the distance AC is very great. Would our hypothetical man be cognisant of the difference? We perceive it, we who can move our eyes, because a very slight movement is sufficient to cause an image to pass from A to B. But for him the question whether the distance AB was very small as compared with the distance AC would not only be insoluble, but would be devoid of meaning.

The notion of the contiguity of two points, accordingly, would not exist for our hypothetical man. The rubric, or category, under which he would arrange his sensations, if he arranged them at all, would consequently not be the space of the geometer and would probably not even be continuous, since he could not distinguish small distances from large. And even if it were continuous, it could not, as I have abundantly shown elsewhere, be either homogeneous, isotropic, or tridimensional.

It is needless to repeat for the other senses what I have said for sight. Our sensations differ from one another qualitatively, and they can therefore have no common measure, no more than can the gramme and the metre. Even if we compare only the sensations furnished by the same nerve-fibre, considerable effort of the mind is required to recognise that the sensation of to-day is of the same kind as the sensation of yesterday, but greater or smaller; in other words, to classify sensations according to their character, and then to arrange those of the same kind in a sort of scale, according to their intensity. Such a classification cannot be accomplished without the active intervention of the mind, and it is the object of this intervention to refer our sensations to a sort of rubric or category pre-existing in us.

Is this category to be regarded as a "form of our sensibility"? No, not in the sense that our sensations, individually considered, could not exist without it. It becomes necessary to us only for comparing our sensations, for reasoning upon our sensations. It is therefore rather a form of our understanding.

This, then, is the first category to which our sensations are referred. It can be represented as composed of a large number of scales absolutely independent of one another. Further, it simply enables us to compare sensations of the same kind and not to measure them, to perceive that one sensation is greater than another sensation, but not that it is twice as great or three times as great.

How much such a category differs from the space of the geometer! Shall we say that the geometer admits a category of quite the same kind, where he employs three scales such as the three axes of co-ordinates? But in our category we have not three scales only, but as many as there are nerve-fibres. Further, our scales appear to us as so many separate worlds fundamentally distinct, while the three axes of geometry all fulfil the same office and may be interchanged one for another. In fine, the co-ordinates are susceptible of being measured and not simply of being compared. Let us see, therefore, how we can rise from this rough category which we may call sensible space to geometric space.

THE FEELING OF DIRECTION.

It is frequently said that certain of our sensations are always accompanied by a peculiar feeling of direction, which gives to them a geometrical character. Such are visual and muscular sensations. Others on the contrary like the sensations of smell and taste are not accompanied by this feeling, and consequently are void of any geometrical character whatever. On this theory the notion of direction would be pre-existent to all visual and muscular sensations and would be the underlying condition of the same.

I am not of this opinion; and let us first ask if the feeling of direction really forms a constituent part of the sensation. I cannot very well see how there can be anything else *in* the sensation than the sensation itself. And be it further observed that the same sensation may, according to circumstances, excite the feeling of different directions. Whatever be the position of the body, the contraction of the *same* muscle, the biceps of the right arm, for example, will always provoke the *same* muscular sensation; and yet, through being apprised by other concomitant sensations that the position of the body has changed, we also know perfectly well that the direction of the motion has changed.

The feeling of direction, accordingly, is not an integrant part of the sensation, since it can vary without the sensation being varied. All that we can say is that the feeling of direction is associated with certain sensations. But what does this signify? Do we mean by it that the sensation is associated with a certain indescribable something which we can represent to ourselves but which is still not a sensation? No, we mean simply that the various sensations which correspond to the same direction are associated *with one another*, and that one of them calls forth the others in obedience to the ordinary laws of association of ideas. Every association of ideas is a product of habit merely, and it would be necessary for us to discover how the habit was formed.

But we are still far from geometrical space. Our sensations have been classified in a new manner: those which correspond to the same direction are grouped together; those which are isolated and have reference to no direction are not considered. Of the innumerable scales of sensations of which our sensible space was formed some have disappeared, others have been merged into one another. Their number has been diminished.

But the new classification is still not space; it involves no idea of measurement; and, furthermore, the restricted category so reached would not be an isotropic space, that is to say, different directions would not appear to us as fulfilling the same office and

as interchangeable with one another. And so this "feeling of direction" far from explaining space would itself stand in need of explanation.

But will it help us even towards the explanation we seek? No, because the laws of that association of ideas which we call the feeling of direction are extraordinarily complex. As I explained above, the same muscular sensation may correspond to a host of different directions according to the position of the body which is made known to us by other concomitant sensations. Associations so complex can only be the result of an extremely long process. This, therefore, is not the path which will lead us most quickly to our goal. Therefore we will not regard the feeling of direction as something attained but will revert to the "sensible space" with which we started.

REPRESENTATION OF SPACE.

Sensible space has nothing in common with geometrical space. I believe that few persons will be disposed to contest this assertion. It would be possible, perhaps, to refine the category which I set up at the beginning of this article, and to construct something which would more resemble geometrical space. But whatever concession we might make, the space so constructed would be neither infinite, homogeneous, nor isotropic: it could be such only by ceasing to be accessible to our senses.

Seeing that our representations are simply the reproductions of our sensations, therefore we cannot image geometrical space. We cannot represent to ourselves objects in geometrical space, but can merely reason upon them as if they existed in that space.

A painter will struggle in vain to construct an object of three dimensions upon canvas. The image which he traces, like his canvas, will never have more than two. When we endeavor, for example, to represent the sun and the planets in space, the best we can do is to represent the visual sensations which we experience when five or six tiny spheres are set revolving in close proximity.

Geometrical space, therefore, cannot serve as a category for our representations. It is not a form of our sensibility. It can serve us only in our reasonings. It is a form of our understanding.

DISPLACEMENT AND ALTERATION.

We at once perceive that our sensations vary, that our impressions are subject to change. The laws of these variations were the cause of our creating geometry and the notion of geometrical space. If our sensations were not variable, there would be no geometry.

But that is not all. Geometry could not have arisen unless we had been led to distribute into two classes the changes which can arise in our impressions. We say, in one case, that our impressions have changed because the objects causing them have undergone some alterations of character, and again that these impressions have changed because the objects have suffered displacement. What is the foundation of this distinction?

A sphere of which one hemisphere is blue and the other red, is rotating before our eyes and shows first a blue hemisphere and then a red hemisphere. Again, a blue liquid contained in a vase suffers a chemical reaction which causes it to turn red. In both cases the impression of blue has given way to the impression of red. Now why is the first of these changes classed among displacements, and the second among alterations? Evidently because in the first case it is sufficient for me merely to go around the globe to bring myself face to face again with the other hemisphere, and so to receive a second time the impression of blue.

An object is displaced before my eye, and its image which was first formed on the centre of the retina is now brought to the edge of the retina. The sensation which was carried to me by a nerve-fibre proceeding from the centre of the retina is succeeded by another which is carried to me by a fibre proceeding from the edge. These sensations are conducted to me by two different nerves. They ought to appear to me different in character, and if they did not, how could I distinguish them?

Why, then, do I come to conclude that the *same* image has been displaced? Is it because one of these sensations frequently succeeds the other? But similar successions are frequent. These it is that produce all our associations of ideas, and we do not ordinarily conclude that they are due to displacement of an object which is invariable in character.

But what happens in this case is that we can *follow the object with the eye*, and by a displacement of our eye which is generally voluntary and accompanied by muscular sensations, we can bring the image back to the centre of the retina and so *re-establish the primitive sensation*. The following, therefore, is my conclusion.

Among the changes which our impressions undergo, we distinguish two classes:

(1) The first are independent of our will and not accompanied by muscular sensations. These are *external changes* so called.

(2) The others are voluntary and accompanied by muscular sensations. We may call these *internal changes*.

We observe next that in certain cases when an external change has modified our impressions, we can, by voluntarily provoking an internal change, re-establish our primitive impressions. The external change, accordingly, can be *corrected* by an internal change. External changes may consequently be subdivided into the two following classes:

1. Changes which are susceptible of being corrected by an internal change. These are *displacements*.

2. Changes which are not so susceptible. These are *alterations*.

An immovable being would be incapable of making this distinction. *Such a being, therefore, could never create geometry*,—even if his sensations were variable, and even if the objects surrounding him were movable.

CLASSIFICATION OF DISPLACEMENTS.

A sphere of which one hemisphere is blue and the other red, is rotating before me and presents to me first its blue side and then its red side. I regard this external change as a displacement because I can correct it by an internal change, namely, by going around the sphere. Let us repeat the experiment with another sphere, of which one hemisphere is green and the other yellow.

The impression of the yellow hemisphere will succeed that of the green, as before that of the red succeeded that of the blue. For the same reason I shall regard this new external change as a displacement.

But this is not all. I also say that these two external changes are due to the *same* displacement, that is to say, to a rotation. Yet there is no connexion between the impression of the yellow hemisphere and that of the red, any more than there is between that of the blue and that of the green, and I have no reason for saying that the same relation exists between the yellow and the green as exists between the red and the blue. No, I say that these two external changes are due to the same displacement because I have "corrected" them by the same internal change. But how am I to know that the two internal changes by which I corrected first the external change from the blue to the red, then that from the green to the yellow, are to be considered identical? Simply because they have provoked the *same* muscular sensations; and for this it is not necessary for me to know geometry in advance and to represent to myself the movements of my body in geometric space.

Thus several external changes which in themselves have no common relation may be corrected by the same internal change. I collect these into the same class and consider them as the same displacement.

An analogous classification may be made with respect to internal changes. All internal changes are not capable of correcting an external change. Only those which are may be called displacements. On the other hand the same external change may be corrected by several different internal changes. A person knowing geometry might express this idea by saying that my body can go from the position A to the position B by several different paths. Each of these paths corresponds to a series of muscular sensations; and at present I am cognisant of nothing but these muscular sensations. No two of these series have a common resemblance, and if I consider them nevertheless as representing the *same* displacement, it is because they are capable of correcting the same external change.

The foregoing classification suggests two reflexions:

1. The classification is not a crude datum of experience, because the aforementioned compensation of the two changes, the one internal and the other external, is never exactly realised. It is, therefore, an active operation of the mind, which endeavors to insert the crude results of experience into a pre-existing form, into a category. This operation consists in identifying two changes because they possess a common character, and in spite of their not possessing it exactly. Nevertheless, the very fact of the mind's having occasion to perform this operation is due to experience, for experience alone can teach it that the compensation has approximately been effected.

2. The classification further brings us to recognise that two displacements are identical, and it hence results that a displacement can be *repeated* twice or several times. It is this circumstance that introduces number, and that permits measurement where formerly pure quality alone held sway.

INTRODUCTION OF THE NOTION OF GROUP.

That we are able to go farther is due to the following fact, the importance of which is cardinal.

It is obvious that if we consider a change A, and cause it to be followed by another change B, we are at liberty to regard the *ensemble* of the two changes A followed by B as a single change which may be written $A+B$ and may be called the resultant change. (It goes without saying that $A+B$ is not necessarily identical with $B+A$.) The conclusion is then stated that if the two changes A and B are displacements, the change $A+B$ also is a displacement. Mathematicians express this by saying that *the ensemble, or aggregate, of displacements is a group.* If such were not the case there would be no geometry.

But how do we know that the *ensemble* of displacements is a group? Is it by reasoning *a priori?* Is it by experience? One is tempted to reason *a priori* and to say: if the external change A is corrected by the internal change A', and the external change B by the internal change B', the resulting external change $A+B$ will be corrected by the resulting internal change $B'+A'$. Hence this resulting change is by definition a displacement, which is to say that the *ensemble* of displacements forms a group.

But this reasoning is open to several objections. It is obvious that the changes A and A' compensate each other; that is to say, that if these two changes are made in succession, I shall find again my original impressions,—a result which I might write as follows:

$$A+A'=0.$$

I also see that $B+B'=0$. These are hypotheses which I made at the outset and which served me in defining the changes A, A', B, and B'. But is it certain that we shall still have $B+B'=0$,—*after* the two changes A and A'? Is it certain that these two changes compensate in such a manner that not only shall I recover my original impressions, but that the changes B and B' shall recover all their original properties, and in particular that of mutual compensation? If we admit this, we may conclude from it that I shall recover my primitive impressions when the four changes follow in the order

$$A, A', B, B';$$

but not that the same will still be the case when they succeed in the order

$$A, B, B', A'.$$

Nor is this all. If two external changes α and α' are regarded as identical on the basis of the convention adopted above, or in other words, are susceptible of being corrected by the same internal change A; if, on the other hand, two other external changes β and β' can be corrected by the same internal change B, and consequently may also be regarded as identical, have we the right to conclude that the two changes $\alpha+\beta$ and $\alpha'+\beta'$ are susceptible of being corrected by the same internal change, and are consequently identical? Such a proposition is in no wise evident, and if it be true it cannot be the result of *a priori* reasoning.

Accordingly, this set of propositions, which I recapitulate by saying that displacements form a group, is not given us by *a priori* reasoning. Are they then a result of experience? One is inclined to admit that they are; and yet one has a feeling of real misgiving

in so doing. May not more precise experience prove some day that the law above enunciated is only approximate? What, then, will become of geometry?

But we may rest assured on this point. Geometry is safe from all revision; no experience, however precise, can overthrow it. If it could have done it, it would have done so long ago. We have long known that all the so-called experimental laws are approximations, and rough approximations at that.

What, then, is to be done? When experience teaches us that a certain phenomenon does not correspond *at all* to these laws, we strike it from the list of displacements. When it teaches us that a certain change obeys them *only approximately*, we consider the change, *by an artificial convention*, as the resultant of two other component changes. The first component is regarded as a displacement *rigorously* satisfying the laws of which I have just spoken, while the second component, which is small, is regarded as a qualitative alteration. Thus we say that natural solids undergo not only great changes of position but also small flexions and small thermal dilatations.

By an external change α we pass, for example, from the *ensemble* of impressions A to the *ensemble* B. We correct this change by a voluntary internal change β and are carried back to the *ensemble* A. A new external change α' causes us to pass again from the *ensemble* A to the *ensemble* B. We ought to expect then that this change α' could in its turn be corrected by another voluntary internal change β' which would provoke the same muscular sensations as β and which would call forth again the *ensemble* of impressions A. If experience does not confirm this prediction, we shall not be embarrassed. We say that the change α', although like α it has been the cause of my passing from the *ensemble* A to the *ensemble* B, is nevertheless not identical with the change α. If our prediction is confirmed only approximately we say that the change α' is a displacement identical with the displacement α but accompanied by a slight qualitative alteration.

In fine, these laws are not imposed by nature upon us but are imposed by us upon nature. But if we impose them upon nature, it is because she suffers us to do so. If she offered too much resistance, we should seek in our arsenal for another form which would be more acceptable to her.

CONSEQUENCES OF THE EXISTENCE OF THE GROUP.

This first fact, that displacements form a group, contains in germ a host of important consequences. Space must be homogeneous; that is, all its points are capable of playing the same part. Space must be isotropic; that is, all directions which issue from the same point must play the same part.

If a displacement *D* transports me from one point to another, or changes my orientation, I must after such displacement *D* be still capable of the same movements as before the displacement *D*, and these movements must have preserved their fundamental properties, which permitted me to classify them among displacements. If it were not so, the displacement *D* followed by another displacement would not be equivalent to a third displacement; in other words, displacements would not form a group.

Thus the new point to which I have been transported plays the same part as that at which I was originally; my new orientation also plays the same part as the old; space is homogeneous and isotropic.

Being homogeneous, it will be unlimited; for a category that is limited cannot be homogeneous, seeing that the boundaries cannot play the same part as the centre. But this does not say that it is infinite; for the sphere is an unbounded surface, and yet it is finite. All these consequences, accordingly, are germinally contained in the fact which we have just discovered. But we are as yet unable to perceive them, because we do not yet know what a direction is or even what a point is.

PROPERTIES OF THE GROUP.

We have now to study the properties of the group. These properties are purely formal. They are independent of any quality whatever, and in particular of the qualitative character of the phenomena which constitute the change to which we have given the

name displacement. We remarked above that we could regard two changes as representing the same displacement, although the phenomena were quite different in qualitative nature. The properties of this displacement remain the same in the two cases; or rather the only ones which concern us, the only ones which are susceptible of being studied mathematically, are those in which quality is in no wise concerned. A brief digression is necessary here to render my thought comprehensible. What mathematicians call a group is the *ensemble* of a certain number of operations and of all the combinations which can be made of them. In the group which is occupying us our operations are displacements. It sometimes happens that two groups contain operations which are entirely different as to character, and that these operations nevertheless combine according to the same laws. We then say that the two groups are *isomorphic.*

The different permutations of six objects form a group and the properties of this group are independent of the character of the objects. If in place of the six material objects we take six letters, or even the six faces of a cube, we obtain groups which differ as to their component materials, but which are all isomorphic with one another.

The formal properties are those which are common to all isomorphic groups. If I say, for example, that such and such an operation repeated three times is equivalent to such and such an other repeated four times, I have announced a formal property entirely independent of quality. These formal properties are susceptible of being studied mathematically. They should be enunciated, therefore, in *precise* propositions. On the other hand, the experiences which serve to verify them can never be more than approximate; that is to say, the experiences in question can never be the true foundation of these propositions. We have within us, in a potential form, a certain number of models of groups, and experience merely assists us in discovering which of these models departs least from reality.

CONTINUITY.

It is observed first that the group is *continuous.* Let us see what this means, and how the fact can be established.

The same displacement can be repeated twice, three times, etc. We obtain thus different displacements which may be regarded as *multiples* of the first. The multiples of the same displacement D form a group; for the succession of two of these multiples is still a multiple of D. Further, all these multiples are interchangeable (a truth which is expressed by saying that the group which they form is a *sheaf*); that is, it is indifferent whether we repeat D first three times and then four times, or first four times and then three times. This is an analytical judgment *a priori;* an out-and-out tautology. This group of the multiples of D is only a part of the total group. It is what is called a *sub-group.*

Now we soon discover that any displacement whatever can always be divided into two, three, or any number of parts whatever; I mean that we can always find an other displacement which, repeated two, three times will reproduce the given displacement. This divisibility to infinity conducts us naturally to the notion of mathematical continuity; yet things are not so simple as they appear at first sight.

We cannot prove this divisibility to infinity, directly. When a displacement is very small, it is inappreciable for us. When two displacements differ very little, we cannot distinguish them. If a displacement D is extremely small, its consecutive multiples will be indistinguishable. It may happen then that we cannot distinguish $9D$ from $10D$, nor $10D$ from $11D$, but that we can nevertheless distinguish $9D$ from $11D$. If we wanted to translate these crude facts of experience into a formula, we should write

$$9D=10D,\ 10D=11D,\ 9D<11D.$$

Such would be the formula of physical continuity. But such a formula is repugnant to reason. It corresponds to none of the models which we carry about in us. We escape the dilemma by an artifice; and for this physical continuity—or, if you prefer, for

this sensible continuity, which is presented in a form unacceptable to our minds—we substitute mathematical continuity. Severing our sensations from that something which we call their cause, we assume that the something in question conforms to the model which we carry about in us, and that our sensations deviate from it only in consequence of their crudeness.

The same process recurs every time we apply measurement to the data of the senses; it is notably applicable to the study of displacements. From the point which we have now reached, we can render an account of our sensations in several different ways.

(1) We may suppose that each displacement forms part of a sheaf formed of all the multiples of a certain small displacement far too small to be appreciated by us. We should then have a discontinuous sheaf which would give us the illusion of physical continuity because our gross senses would be unable to distinguish any two consecutive elements of the sheaf.

(2) We may suppose that each displacement forms part of a more complex and richer sheaf. All the displacements of which this sheaf is composed would be interchangeable. Any two of them would be multiples of another smaller displacement which likewise formed part of the sheaf and which might be regarded as their greatest common divisor. Finally, any displacement of the sheaf could be divided into two, three, or any number of parts, in the sense which I have given to this word above, and the divisor would still be part of the sheaf. The different displacements of the sheaf would be, so to speak, commensurable with one another. To every one of them would correspond a commensurable number, and *vice versa.* This therefore would be already a sort of mathematical continuity, but this continuity would still be imperfect, for there would be nothing corresponding to incommensurable numbers.

(3) We may suppose, finally, that our sheaf is perfectly continuous. All its displacements are interchangeable. To every commensurable or incommensurable number corresponds a displacement and *vice versa.* The displacement corresponding to the number na is nothing else than the displacement corresponding to the number a repeated n times.

Why has the last of these three solutions been adopted? The reasons for the choice are complicated.

(1) It has been established by experience that displacements which are sufficiently large can be divided by any number whatever; and as the means of measurement increased in precision, this divisibility was demonstrated for displacements much smaller, with respect to which it first seemed doubtful. We have thus been led by induction to suppose that this divisibility is a property of all displacements, however small, and consequently to reject the first solution and to decide in favor of divisibility to infinity.

(2) The first solution, like the second, is incompatible with the other properties of the group which we know from other experience. I shall explain this further on. The third solution, accordingly, is imposed upon us by this fact alone. The contrary might have happened. It might have been that the properties of the group were incompatible with continuity. Then we should undoubtedly have adopted the first solution.

SUB-GROUPS.

The most important of the formal properties of a group is the existence of sub-groups. It must not be supposed that there can be as many sub-groups formed as we like, and that it is sufficient to cut up a group in an arbitrary manner, as one would inert clay, in order to obtain a sub-group. If two displacements be taken at random in a group, it will be necessary, in order to form a subgroup from them, to conjoin with them all their combinations; and in the majority of cases it happens that in combining these two displacements in all possible manners we arrive ultimately at the primitive group again in its original intact form. It may happen thus that a group contains no sub-group.

But groups are distinguished from one another, in a formal point of view, by the number of sub-groups which they contain and by the mutual relations of the sub-groups. A superficial examination of the group of displacements renders it patent that it contains some sub-groups. A more minute examination will disclose them all. We shall see that among these sub-groups there are some that

are: (1) continuous, i. e., have all their displacements divisible to infinity; (2) discontinuous, i. e., have no displacements that are divisible to infinity; (3) mixed, i. e., have displacements divisible to infinity and in addition others that are not so divisible.

From another point of view we distinguish among our subgroups sheaves whose displacements are all interchangeable and those which do not possess this property.

The following is another manner of classing displacements and sub-groups.

Let us consider two displacements D and D'. Let D'' be a third displacement, defined to be the resultant of the displacement D' followed by the displacement D followed itself by the inverse displacement of D'. This displacement D'' is called the *transformation* of D by D'.

From the formal point of view all the transformations of the same displacement are equivalent, so to speak; they play the same part; the Germans say that they are *gleichberechtigt*. Thus (if I may be permitted for an instant to use in advance the ordinary language of geometry which we are supposed not yet to know) two rotations of 60° are *gleichberechtigt*, two helicoidal displacements of the same step and same fraction of spiral are *gleichberechtigt*.

The transformations of all displacements of a sub-group g by the same displacement D' form a new sub-group which is called the transformation of the sub-group g by the displacement D'. The different transformations of the same sub-group, playing the same part in a formal point of view, are *gleichberechtigt*.

It happens generally that many of the transformations of the same sub-group are identical; it will sometimes even happen that all the transformations of a sub-group are identical with one another and with the primitive sub-group. It is then said this subgroup is *invariant* (which happens, for example, in the case of the sub-group formed of all translations). The existence of an invariant sub-group is a formal property of the highest importance.

ROTATIVE SUB-GROUPS.

The number of sub-groups is infinite; but they may be divided into a rather limited number of classes of which I do not wish to give here a complete enumeration. But these sub-groups are not all perceived with the same facility. Some among them have been only recently discovered. Their existence is not an intuitive truth. Unquestionably it can be deduced from the fundamental properties of the group, from properties which are known to everybody, and which are, so to speak, the common patrimony of all minds. Unquestionably it is contained there in germ; yet those who have demonstrated their existence have justly felt that they had made a discovery and have frequently been obliged to write long memoirs to reach their results.

Other sub-groups, on the contrary, are known to us in much more immediate manner. Without much reflexion every one believes he has a direct intuition of them, and the affirmation of their existence constitutes the axioms of Euclid. Why is it that some sub-groups have directly attracted attention, whilst others have eluded all research for a much longer time? We shall explain it by a few examples.

A solid body having a fixed point is turning before our eyes. Its image is depicted on our retina and each of the fibres of the optic nerve conveys to us an impression; but owing to the motion of the solid body this impression is variable. One of these fibres, however, conveys to us a constant impression. It is that at the extremity of which the image of the fixed point has been formed. We have, thus, a change which causes certain sensations to vary, but leaves others invariable. This is a property of the displacement, but at first blush it does not appear that it is a formal property. It seems to belong to the qualitative character of the sensations experienced. We shall see, however, that we can disengage a formal property from it, and to render my thought clear I shall compare what takes place in this case with what happens in another instance which is apparently analogous.

I suppose that a certain body is moving before my eyes in any manner, but that a certain region of this body is painted in a color sufficiently uniform to leave no shades discernible. Let us say it is red. If the movements are not of too great compass and if the red region is sufficiently large in extent, certain parts of the retina will remain constantly in the image of that region, certain nerve-fibres will convey to us constantly the impression of the red, the displacement will have left certain sensations invariable.

But there is an essential difference between the two cases. Let us go back to the first one. We witnessed there an external change in which certain sensations A did not change, whilst other sensations B did change. We are able to correct this external change by an internal change, and in this correction the sensations A still remain invariable.

But now here is a new solid body which is turning before our eyes and is experiencing the same rotations as the first. This is a new external change which may be different altogether from the first from a qualitative point of view, because the new body which is turning may be painted in new colors, or because we are apprised of its rotation by touch and not by sight. We discover, however, that it is the *same* displacement, because it can be corrected by the same internal change. And we also discover that certain sensations A' in this new external change (totally different perhaps from A) have remained invariable, whilst other sensations B' varied. Thus, this property of conserving certain sensations ultimately appears to us as a formal property independent of the qualitative character of these sensations.

We pass to the second example. We have, first, an external change in which a certain sensation C, a sensation of red, has remained constant. Let us suppose that another solid body, differently painted, undergoes the same displacement. Here is a new external change, and we know that it represents the same displacement because we can correct it by the same internal change. We discover generally that in this new external change certain sensations have not remained constant. Thus the conservation of the sensation C will appear to us as an accidental property only, connected with the qualitative nature of the sensation.

We are thus led to distinguish among displacements those which conserve certain sensations. The *ensemble* of the displacements which thus conserve a given system of sensations, evidently forms a sub-group which we may call a *rotative sub-group.*

Such is the conclusion which we draw from experience. It is needless to point out how crude is the experience and how precise on the other hand is the conclusion. Therefore experience cannot impose the conclusion upon us, but it suffices to suggest it to us. It suffices to show that of all the groups of which the models pre-exist in us, the only ones which we can accept with a view of referring to them our sensations, are those which contain such a sub-group.

By the side of the rotative sub-group, we should consider its transformations, which also may be called rotative sub-groups. (Sub-group of rotations about a fixed point.) By new experiences, always very crude, it is then shown:

(1) That any two rotative sub-groups have common displacements.

(2) That these common displacements, all interchangeable among one another, form a sheaf, which may be called a rotative sheaf. (Rotations about a fixed axis.)

(3) That any rotative sheaf forms part not only of two rotative sub-groups, but of an infinity of them.

Here is the origin of the notion of the straight line, as the rotative sub-group was the origin of the notion of the point.

Let us now look at all the displacements of a rotative sheaf. If we look at any displacement whatever, it will not in general be interchangeable with all the displacements of the sheaf, but we shall discover very soon that there exist displacements which are interchangeable with all those of the rotative sheaf, and that they form a more extensive sub-group which may be called the helicoidal sub-group (combinations of rotations about an axis and of translations parallel to that axis). This will be evident when it is observed that a straight line can slide along itself.

Finally, we derive from the same crude observations such propositions as the following:

Any displacement sufficiently small and forming part of a given rotative sub-group, can always be decomposed into three others belonging respectively to three given rotative sheaves. Every displacement interchangeable with a rotative sub-group forms part of this sub-group.

Any displacement sufficiently small can always be decomposed into two others belonging respectively to two given rotative sub-groups, or to *six given rotative sheaves.*

Later on I shall revert in detail to the origin of these various propositions.

TRANSLATIVE SUB-GROUPS.

With these propositions we have sufficient material, not to construct the geometry of Euclid, but to limit the choice between that of Euclid and the geometries of Lobatchévski and Riemann. In order to go farther, we are in need of a new proposition to take the place of the postulate of parallels. The proposition substituted will be the existence of an *invariant* sub-group, of which all the displacements are interchangeable and which is formed of all translations.

It is this that determines our choice in favor of the geometry of Euclid, because the group that corresponds to the geometry of Lobatchévski does not contain such an invariant sub-group.

NUMBER OF DIMENSIONS.

In the ordinary theory of groups, we distinguish order and degree. Let us suppose the simplest case first, that of a group formed by different permutations between certain objects. The number of the objects is called the degree; the number of the permutations is called the order of the group. Two such groups may be isomorphic and their permutations may combine according to the same laws without their degree being the same. Thus let us consider the different ways in which a cube can be superposed upon itself. The vertices may be interchanged one with another, as may also be the faces and the edges; whence result three groups of permutations which are evidently isomorphic among themselves; but their degree may be either eight, six, or twelve, since there are eight vertices, six faces, and twelve edges.

On the other hand, two mutually isomorphic groups have always the same order. The degree is, so to speak, a material element, and the order a formal element, the importance of which is far greater. The theory of two groups of different degree may be the same so far as its formal properties are concerned; just as the mathematical theory of the addition of three cows and four cows is identical with that of three horses and four horses.

When we pass to continuous groups, the definitions of order and degree must be modified, though without sacrificing their spirit. Mathematicians suppose ordinarily that the object of the operations of the group is an *ensemble* of a certain number n of quantities susceptible of being varied in a continuous manner, which quantities are called *co-ordinates*. On the other hand, every operation of the group may be regarded as forming part of a sheaf analogous to the rotative sheaf and as a multiple of a very high order of an infinitesimal operation belonging to the same sheaf. Then, every infinitesimal operation of the group can be decomposed into k other operations belonging to k given sheaves. The number n of the co-ordinates (or of the dimensions) is then the *degree*, and the number k of the components of an infinitesimal operation is the *order*. Here again two isomorphic groups may have different degrees, but must be of the same order. Here again the degree is an element relatively material and secondary, and the order a formal element. According to the laws established above, our group of displacements is here of the sixth order, but its degree is yet unknown. Is the degree given us immediately?

Displacements, we have seen, correspond to changes in our sensations, and if we distinguish in the present group between form and material, the material can be nothing else than that which the displacements cause to change, viz., our sensations. Even if we suppose that what we have above called sensible space has already been elaborated, the material would then be represented

by as many continuous variables as there are nerve-fibres; the "degree" of our group would then be extremely large. Space would not have three dimensions but as many as there are nerve-fibres. Such is the consequence to which we come if we accept as the material of our group what is immediately given us. How shall we escape the difficulty? Evidently by replacing the group which is given us, together with its form and its material, by another *isomorphic* group, the material of which is simpler.

But how is this to be done? It is precisely owing to this circumstance, that the displacements which conserve certain elements are the same as those which conserve certain other elements. Then all those elements which are conserved by the same displacements we agree to replace by a single element which has a purely schematic value only. Whence results a considerable reduction of degree.

For example, I see a solid body rotating about a fixed point. The parts near the fixed point are painted red. Here is a displacement, and within this displacement I perceive that something remains invariable—namely, the sensation of red conveyed to me by a certain optical nerve-fibre. Some time afterward I see an other solid body turning about a fixed point. But the parts near the fixed point are painted green. The sensations experienced are in themselves quite different, but I perceive that it is the same displacement because it can be corrected by the same internal change. Here again something remains invariable; but this something is totally different from the material point of view; it is the sensation of green conveyed by a certain nerve-fibre.

These two things, which materially are so different, I replace schematically by a single thing which I call a point, and I express my thought by saying that in the one case as in the other, a point of the body has remained fixed. Thus every one of our new elements will be what is conserved by all the displacements of a sub-group; to every sub-group there will then correspond an element and *vice versa*.

Let us consider the different transformations of the same sub-group. They are infinite in number and may form a simple, double, triple, continuous infinity. To each one of these transformations an element can be made to correspond; I have then a simple, double, triple, etc., infinity of them, and the degree of our continuous group is 1, 2, 3, ...

Suppose that we choose the different transformations of a rotative sub-group. We have here a triple infinity. The material of our group is accordingly composed of a triple infinity of elements. The degree of the group is three. We have then chosen the point as the element of space and given to space three dimensions.

Suppose we choose the different transformations of a helicoidal sub-group. Here we have a quadruple infinity. The material of our group is composed of a quadruple infinity of elements. Its degree is four. We then have chosen the straight line as the element of space,—which would give to space four dimensions.

Suppose, finally, that we choose the different transformations of a rotative sheaf. The degree would then be five. We have chosen as the element of space the figure formed by a straight line and a point on that straight line. Space would have five dimensions.

Here are three solutions, which are each logically possible. We prefer the first because it is the simplest, and it is the simplest because it is that which gives to space the smallest number of dimensions. But there is another reason which recommends this choice. The rotative sub-group first attracts our attention because it conserves certain sensations. The helicoidal sub-group is known to us only later and more indirectly. The rotative sheaf on the other hand is itself merely a sub-group of the rotative sub-group.

THE NOTION OF POINT.

I feel that I am here touching on the most delicate spot of this discussion, and I am compelled to stop for a moment to justify more completely my previous assertions which some persons may be disposed to doubt. Many persons, indeed, would consider the notion of a point of space as so immediate and so clear that any definition of it is superfluous. But I believe it will be granted me that so subtle a notion as that of the mathematical point, without length, breadth, or thickness, is not immediate, and that it needs to be explained.

But is it the same with the vaguer and less precisely defined, yet more empirical notion, of *place?* Is there any one who does not fancy he knows perfectly well what he is talking about when he says: this object occupies the place which was just occupied by that object. To determine the range of such an assertion, and the conclusions which can be drawn from it, let us seek to analyse its signification. If I have moved neither my body, my head, nor my eye, and if the image of the object *B* affects the same retinal fibres that the image of the object *A* previously affected; if again, although I have moved neither my arm nor my hand, the same sensory fibres which extend to the end of the finger, and which formerly conveyed to me the impression which I attributed to the object *A* now convey to me the impression which I attribute to the object *B;* if both these conditions are fulfilled,—then ordinarily we agree to say that the object *B* occupies the place which previously the object *A* occupied.

Before analysing so complicated a convention as that just stated I shall first make a remark. I have just enunciated two conditions: one relating to sight, and one relating to touch. The first is necessary but not sufficient, for we say in ordinary language that the point on the retina where an image is formed gives us knowledge only of the direction of the visual ray, but that the distance from the eye remains unknown. The second condition is at once necessary and sufficient, because we assume that the action of touch is not exercised at a distance, and that the object *A* like the object *B* cannot act upon the finger except by immediate contact. All this agrees with what experience has taught us; namely, that the first condition can be fulfilled without the second being realised, but that the second cannot be fulfilled without the first. Let it be remarked that we have here something which we could not know *a priori*, that experience alone is able to demonstrate it to us.

Nor is this all. To determine the place of an object I made use only of an eye and a finger. I could have made use of several other means,—for example, of all my other fingers. Having been made aware that the object A has produced upon my first finger a tactual impression, suppose that by a series of movements S my second finger comes into contact with the same object A. My first tactual impression ceases and is replaced by another tactual impression which is conveyed to me by the nerve of the second finger, and which I still attribute to the action of the object A. Some time afterwards, and without my having moved my hand, the same nerve of the second finger conveys to me another tactual impression, which I attribute to the action of another object B. I then say that the object B has taken the place of the object A.

At this moment I make a series of movements S' the inverse of the series S. How do I know that these two series are inverse to one another? Because experience has taught me that when the internal change S that corresponds to certain muscular sensations is followed by an internal change S' which corresponds to other muscular sensations, a compensation is effected and my primitive impressions, originally modified by the change S, are reestablished by the change S'.

I execute the series of movements S'. The effect ought to be to take back my first finger to its initial position and so to put it into contact with the object B, which has taken the place of the object A. I ought, therefore, to expect that the nerve of my first finger should convey to me a tactual sensation attributable to the object B. In fact this is what happens.

But would it therefore be absurd to suppose the contrary? And why would it be absurd? Shall I say that the object B having taken the place of the object A, and my first finger having resumed its original place, it ought to touch the object B just as before it touched the object A? This would be an outright begging of the question. And to show this let us attempt to apply the same reasoning to another example, or rather let us return to the example of sight and touch which I cited at the outset.

The image of the object A has made an impression on one of my retinal fibres. At the same time the nerve of one of my fingers conveys to me a tactual impression which I attribute to the same object. I move neither my eye nor my hand. And a moment after

the image of the object B has impressed the same retinal fibre. By a course of reasoning perfectly similar to that which precedes, I should be tempted to conclude that the object B had taken the place of the object A, and I should expect that the nerve of my finger would convey to me a tactual impression attributable to B. And yet I should be deceived. For the image of B may chance to be formed upon the same point of the retina as the image of A, although the distance to the eye may not be the same in the two cases.

Experience has refuted my reasoning. I extricate myself by saying that it is not sufficient for two bodies to cast their image upon the same retinal fibre in order to justify me in saying that the two bodies are in the same place; and I should extricate myself in a similar manner in the case of the two fingers, if the indications of the second finger had not been in accord with those of the first, and if experience had been at variance with my reasoning. I should still say that two objects A and B can make an impression upon the same finger by means of touch and yet not be in the same place; in other words, I should conclude that touch could be effected at a distance. Or, again, I should agree to consider A and B as being in the same place only on the condition of there being concordance not only between their effects upon the first finger, but also between their effects upon the second finger. One might almost say, in a certain point of view, that one more dimension would be attributed to space in this manner.

To sum up, there are certain laws of *concordance*, which can be revealed to us only by experience, and which are at the basis of the vague notion of place.

But even taking these laws of concordance for granted, can we deduce from them the much more precise notion of point and the notion of number of dimensions? This remains to be examined.

First an observation. We have spoken of two objects A and B, which have cast one after another their image on the same point of the retina. But these two images are not identical; otherwise how could I distinguish them? They differ, for example, in color. The one is red, the other is green. We have, accordingly, two senmade aware that the object A has produced upon my first finger a tactual impression, suppose that by a series of movements S my second finger comes into contact with the same object A. My first tactual impression ceases and is replaced by another tactual impression which is conveyed to me by the nerve of the second finger, and which I still attribute to the action of the object A. Some time afterwards, and without my having moved my hand, the same nerve of the second finger conveys to me another tactual impression, which I attribute to the action of another object B. I then say that the object B has taken the place of the object A.

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The image of the object A has made an impression on one of my retinal fibres. At the same time the nerve of one of my fingers conveys to me a tactual impression which I attribute to the same object. I move neither my eye nor my hand. And a moment after the image of the object B has impressed the same retinal fibre. By a course of reasoning perfectly similar to that which precedes, I should be tempted to conclude that the object B had taken the place of the object A, and I should expect that the nerve of my finger would convey to me a tactual impression attributable to B. And yet I should be deceived. For the image of B may chance to be formed upon the same point of the retina as the image of A, although the distance to the eye may not be the same in the two cases.

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But even taking these laws of concordance for granted, can we deduce from them the much more precise notion of point and the notion of number of dimensions? This remains to be examined.

First an observation. We have spoken of two objects A and B, which have cast one after another their image on the same point of the retina. But these two images are not identical; otherwise how could I distinguish them? They differ, for example, in color. The one is red, the other is green. We have, accordingly, two sensations which differ in quality and which are doubtless conveyed to me by two different though contiguous nerve-fibres. What have they in common with one another, and why am I led to associate them together? I believe that if the eye were immovable, we should never have thought of this association. It is the movements of the eye that have taught us that there is the same relation on the one hand between the sensation of green at the point *A* of the retina and the sensation of green at the point *B* of the retina, and on the other hand between the sensation of red at the point *A* of the retina and the sensation of red at the point *B* of the retina. We have found, in fact, that the same movements, corresponding to the same muscular sensations, cause us to pass from the first to the second, or from the third to the fourth. Were this not so, these four sensations would appear qualitatively distinct, and we should no more think of establishing a sort of proportion between them than we should between an olfactory, a gustatory, an auditive and a tactual sensation.

Yet whatever be the origin of this association, it is implied in the notion of place, which could not have grown up without it. Let us analyse, therefore, its laws. We can only conceive them under two different forms equally remote from mathematical continuity; namely, under the form of discontinuity or under the form of physical continuity.

Under the first form, our sensations will be divided into a very large number of "families"; all the sensations of one family being associated with one another and not being associated with those of other families. Since to every family there would correspond a place, we should have a finite but very large number of places, and the places would form a discrete aggregate. There would be no reason for classifying them in a table of three dimensions rather than in one of two or four; and we could not deduce from them either the mathematical point or space.

Under the second form, which is more satisfactory, the different families interpenetrate one another. *A*, for example, will be associated with *B*, and *B* with *C*. But *A* will not appear to us as associated with *C*. We shall find that *A* and *C* do not belong to

the same family, although on the one hand *A* and *B*, and on the other hand *B* and *C*, will appear to us as belonging to the same family. Thus we cannot distinguish between a weight of nine grammes and one of ten grammes, or between the latter weight and a weight of eleven grammes. But we can readily tell the difference between the first weight and the third. This is always the formula of physical continuity.

Let us picture to ourselves a series of wafers partially covering one another in such wise that the plane is totally covered; or better, let us picture to ourselves something analogous in a space of three dimensions. If these wafers were to form by their superposition only a sort of one-dimensional ribbon, we should recognise it, because the associations of which I have just been speaking obey a law that may be stated as follows: if *A* is associated at once with *B*, *C*, and *D*, *D* is associated with *B* or with *C*. This law would not be true if our wafers covered by their superposition a plane or a space of more than two dimensions. When I say, therefore, that all possible places constitute an aggregate of one dimension or of more than one dimension, I mean to say simply that this law is true or that it is false. When I say that they constitute an aggregate of two or three dimensions, I simply affirm that certain analogous laws are true.

Such are the foundations on which we may attempt to construct a *static* theory of the number of dimensions. It will be seen how complicated is this manner of defining the number of dimensions, how imperfect it is, and it is useless to remark upon the distance which still separates the physical continuity of three dimensions as thus understood from the real mathematical continuity of three dimensions.

DISCUSSION OF THE PRECEDING THEORY.

Without dwelling upon the multitude of difficult details, let us see in what those associations consist upon which the notion of place rests. We shall see that we are finally led back, after a long detour, to the notion of group, which appeared to us at the outset the best fitted for elucidating the question of the number of dimensions.

By what means are different "places" distinguished from one another? How, for example, are two places occupied successively by the extremity of one of my fingers to be distinguished? Evidently by the movement which my body has made in the interval, movements which are made known to me by a certain series of muscular sensations. These two places correspond to two distinct attitudes and positions of the body which are known solely by the movements which I have had to make in changing a certain initial attitude and a certain initial position; and these movements themselves are known to me only by the muscular sensations which they have provoked.

Two attitudes of the body, or two corresponding places of the finger, appear to me identical if the two movements which I must make to reach them differ so little from each other that I cannot distinguish the corresponding muscular sensations. They will appear to me non-identical, without some new convention, if they correspond to two series of distinguishable muscular sensations.

But in this manner we have engendered not a physical continuity of three dimensions but a physical continuity of a much larger number of dimensions; for I can cause the muscular sensations corresponding to a very large number of muscles to vary, and I do not on the other hand consider a single muscular sensation only, nor even an aggregate of simultaneous sensations, but a series of successive sensations, and I can make the laws by which these sensations succeed one another vary in an arbitrary manner.

Why is the number of dimensions reduced, or, what is the same thing, why do we consider two places as identical when the two corresponding attitudes of the body are different? Why do we say in certain cases that the place occupied by the extremity of a finger has not changed, although the attitude of the body has changed?

It is because we discover that very *frequently*, in the movement which causes the passage from the one to the other of these two attitudes, the tactual sensation attributable to the contact of this finger with an object *A* persists and remains constant. We *agree* then, to say that these two attitudes shall be placed in the same class and that this class shall embrace all attitudes corresponding to the same place occupied by the same finger. We agree that these two attitudes shall still be placed in the same class even when they are accompanied by no tactual sensation, or by variable tactual sensations.

This convention has been evoked by experience, because experience alone informs us that certain tactual sensations are frequently persistent. But in order that conventions of this kind shall be permissible, they must satisfy certain conditions which it now remains for us to analyse.

If I place the attitudes *A* and *B* in the same class, and also the attitudes *B* and *C* in the same class, it follows necessarily that the attitudes *A* and *C* must be regarded as belonging to the same class. If, then, we agree to say that the movements which cause the passage from the attitude *A* to the attitude *B* do not change the place of the finger, and if the same holds true of the movements which cause the passage from the attitude *B* to the attitude *C*, it follows necessarily that the same must again be true of those which cause the passage from the attitude *A* to the attitude *C*. In other words, the aggregate of the movements causing a passage from one attitude to another attitude of the same class constitutes a group. It is only when such a group exists that the convention above laid down is acceptable. To every class of attitudes, and consequently to every place, there will therefore correspond a group, and we are here led back again to the notion of group, without which there would be no geometry.

Nevertheless, there is a difference between the principle here under discussion and the theory which I developed above. Here each place appears to me associated with a certain group which is introduced as the sub-group *S* of the group *G* formed by the movements which can give to the body all possible positions and all possible attitudes, the relative situations of the different parts of the body being allowed to vary in any manner whatsoever. In our other theory, on the contrary, every point was associated with a sub-group *S'* of the group *G'* formed by the displacements of the body viewed as an invariable solid, that is to say, by displacements such that the relative situations of the different parts of the body do not vary.

Which of the two theories is to be preferred? It is evident that G' is a sub-group of G and S' a sub-group of S. Further, G is much simpler than G, and for this reason the theory which I first propounded and which is based upon the consideration of the group G' appears to me simpler and more natural, and consequently I shall hold to it.

But be this as it may, the introduction of a group, more or less complicated, appears to be absolutely necessary. Every purely statical theory of the number of dimensions will give rise to many difficulties, and it will always be necessary to fall back upon a dynamical theory. I am happy to be in accord on this point with the ideas set forth by Professor Newcomb in his *Philosophy of Hyper-space.*

THE REASONING OF EUCLID.

But in order to show that the idea of displacement, and consequently the idea of group, has played a preponderant part in the genesis of geometry, it remains to be shown that this idea dominates all the reasoning of Euclid and of the authors who after him have written upon elementary geometry.

Euclid begins by enunciating a certain number of axioms; but it must not be imagined that the axioms which he enunciates explicitly are the only ones to which he appeals. If we carefully analyse his demonstrations we shall find in them, in a more or less masked form, a certain number of hypotheses which are in reality axioms disguised; and we may say almost as much of some of his definitions.

His geometry begins with declaring that two figures are equal if they are superposable. This assumes that they can be displaced and also that among all the changes which they may undergo, we can distinguish those which may be regarded as displacements without deformation. Again, this definition implies that two figures which are equal to a third are equal to each other. And that

is tantamount to saying that if there be a displacement which puts the figure A upon the figure B, and a second displacement which superposes the figure B upon the figure C, there will also be a third, the resultant of the first two, which will superpose the figure A upon the figure C. In other words, it is presupposed that the displacements form a group. The notion of a group, accordingly, is introduced from the outset, and inevitably introduced.

When I pronounce the word "length," a word which we frequently do not think necessary to define, I implicitly assume that the figure formed by two points is not always superposable upon that which is formed by two other points; for otherwise any two lengths whatever would be equal to each other. Now this is an important property of our group.

I implicitly enunciate a similar hypothesis when I pronounce the word "angle."

And how do we proceed in our reasonings? By displacing our figures and causing them to execute certain movements. I wish to show that at a given point in a straight line a perpendicular can always be erected, and to accomplish this I conceive a movable straight line turning about the point in question. But I presuppose here that the movement of this straight line is possible, that it is continuous, and that in so turning it can pass from the position in which it is lying on the given straight line, to the opposite position in which it is lying on its prolongation. Here again is a hypothesis touching the properties of the group.

To demonstrate the cases of the equality of triangles, the figures are displaced so as to be superposed one upon the other.

Finally, what is the method employed in demonstrating that from a given point one and only one perpendicular can always be drawn to a given straight line? The figure is turned 180° around the given straight line, and in this manner the point symmetrical to the given point with respect to the given straight line is obtained. We have here a feature most characteristic, and here appears the part which the straight line most frequently plays in geometrical demonstrations, namely, that of an axis of rotation.

There is implied here the existence of the sub-group which I have called the rotative sheaf. When—which also frequently happens—a straight line is made to slide along itself (for we shall, of course, continue to suppose that it can serve as an axis of rotation), we implicitly take the existence of the helicoidal sub-group for granted. In fine, the principal foundation of Euclid's demonstrations is really the existence of the group and its properties.

Unquestionably he appeals to other axioms which it is more difficult to refer to the notion of group. An axiom of this kind is that which some geometers employ when they define a straight line as the shortest distance between two points. *But it is precisely such axioms that Euclid enunciates.* The others, which are more directly associated with the idea of displacement and with the idea of groups, are the very ones which he implicitly admits, and which he does not deem it even necessary to enunciate. This is tantamount to saying that the former are the fruit of a later experience, that the others were first assimilated by us, and that consequently the notion of group existed prior to all the others.

THE GEOMETRY OF STAUDT.

It is known that Staudt attempted to base geometry upon different principles. Staudt admits the following axioms only:

1. Through two points a straight line can always be drawn.
2. Through three points a plane can always be drawn.
3. Every straight line which has two of its points in a plane lies entirely in that plane.
4. If three planes have one point in common, and one only, any straight line will cut at least one of these three planes.

These axioms are sufficient to establish all the *descriptive* properties relating to the intersections of straight lines and planes. To obtain the metrical properties we begin with *defining* a harmonic pencil of four straight lines, taking as definition the well-known descriptive property. Then the anharmonic ratio of four points is *defined*, and finally, supposing that one of these four points has been relegated to infinity, the ratio of two lengths is *defined*.

This last is the weak point of the foregoing theory, attractive though it be. To arrive at the notion of length by regarding it merely as a particular case of the anharmonic ratio is an artificial and repugnant detour. This evidently is not the manner in which our geometrical notions were formed.

Let us see now whether we can conceive, without the introduction of the notion of group and of movement, how the notions which serve as the foundations of this ingenious geometry have taken their rise. Let us see what experiences might have led us to formulate the axioms enunciated above.

If the straight line is not given as an axis of rotation, it can be given only in one way, namely, as the trajectory of a ray of light. I mean, that the experiences, always more or less crude, which serve us as our point of departure, should all be applicable to the ray of light, and that we must define the straight line as a line for which the simple laws which the ray of light approximately obeys will be rigorously true. The following is the experience which must be made in order to verify the most important of our axioms, namely, the third.

Let two threads be stretched. Let the eye be placed at the extremity of one of these threads. We see that the thread is entirely hidden by its extremity, which teaches us that the thread is rectilinear, that is to say, is the direction of the trajectory of a ray of light. Let the same be done for the second thread. The following is then observed: either there will be no position of the eye for which one of the threads is entirely hidden by the other, or there will be an infinity of such positions.

How is the question of the number of dimensions presented in this order of ideas? Let us consider all the positions of the eye for which one of the strings is hidden by the other. Let us suppose that in one of these positions the point A of the first string is hidden by the point A' of the second, the point B by the point B', the point C by the point C'. We then discover that if the body is so displaced that the point A is always hidden by the point A' and the point B by the point B', that the point C always remains hidden by the point C', and that in general any point whatsoever of the first thread remains hidden by the same point of the second thread by which it was hidden before the body was displaced. We express this fact by saying that although the body is displaced, the position of the eye has not changed.

We see thus that the position of the eye is defined by two conditions, viz., that *A* is hidden by *A'* and *B* by *B'*. We express this fact by saying that the *locus* of the points such that the two threads mutually hide each other has two dimensions.

Similarly, let us suppose that in a certain position of the body four threads *A*, *B*, *C*, *D*, hide four points *A'*, *B'*, *C'*, *D'*; let us suppose that the body is displaced, but in such a manner that *A*, *B*, and *C* continue to hide *A'*, *B'*, and *C'*. We shall then discover that *D* continues to hide *D'*, and we shall again express this fact by saying that the position of the eye has not changed. This position will therefore be defined by three conditions, and this is why we say that space has three dimensions.

It will be remarked that the law as thus experimentally discovered, is only approximately true. But this is not all. It is not even always true, because *D* or *D'* may have moved at the same time that my body was being displaced. We then simply declare that this law is often approximately true.

But we are desirous of arriving at geometrical axioms which are rigorously and always true, and we always escape the dilemma by the same artifice, namely, by saying that we agree to consider the change observed as the resultant of two others, viz., of one which rigorously obeys the law and which we attribute to the displacement of the eye, and of a second one which is generally very small and which we attribute either to qualitative alterations or to the movements of external bodies.

We have not been able to avoid the consideration of movements of the eye and of the body, yet we may say, that from a certain point of view the geometry of Staudt is predominantly a visual geometry, while that of Euclid is predominantly muscular.

Undoubtedly unconscious experiences analogous to those of which I have just spoken may have played a part in the genesis of geometry; but they are not sufficient. If we had proceeded, as the geometry of Staudt supposes us to have done, some Apollonius would have discovered the properties of polars. But it would have

been only long after, that the progress of science would have made clear what a length or an angle is. We should have had to wait for some Newton to discover the various cases of the equality of triangles. And this is evidently not the way that things have come to pass.

THE AXIOM OF LIE.

It is Sophus Lie who has contributed most towards making prominent the importance of the notion of group and laying the foundations of the theory that I have just expounded. It is he, in fact, who gave the present form to the mathematical theory of continuous groups. But to render possible its application to geometry, he regards a new axiom as necessary, which he enunciates by declaring that space is a *Zahlenmannigfaltigkeit;* that is, that to every point of a straight line there corresponds a number and *vice versa*.

Is this axiom absolutely necessary? And could not the other principles which Lie has laid down dispense with it? We have seen above in connexion with continuity, that the best known groups may be distributed from a certain point of view into three classes; all the operations of the group can be divided into sheaves; for "discontinuous" groups the different operations of the same sheaf are only a single operation repeated once, twice, three times, etc.; for "continuous" groups properly so called the different operations of the same sheaf correspond to different whole numbers, commensurable or incommensurable; finally, for groups which may be called "semi-continuous," these operations correspond to different commensurable numbers.

Now it may be demonstrated that no discontinuous or semi-continuous group exists possessing other properties than those which experience has led us to adopt for the fundamental group of geometry, and which I here briefly recall: The group contains an infinity of sub-groups, all *gleichberechtigt*, which I call rotative sub-groups. Two rotative sub-groups have a sheaf in common which I call rotative and which is common not only to two but also to an infinity of rotative sub-groups. Finally, every very small displacement of the group may be regarded as the resultant of six displacements belonging to six given rotative sheaves. A group satisfying these conditions can be neither discontinuous nor semi-continuous.

Unquestionably this is an exceedingly recondite property, and not easy to demonstrate. Geometers who were ignorant of it have not the less hit upon its consequences, as for example, when they learned that the ratio of a diagonal to the side of a square is incommensurable. It was for this reason that the introduction of incommensurables into geometry became necessary.

The group, therefore, must be continuous, and it seems as if the axiom of Lie were useless.

Nevertheless, we are obliged to remark that the classification of groups above sketched is not complete; groups may be conceived which are not included in it. We might, therefore, suppose that the group is neither discontinuous, semi-continuous, nor continuous. But this would be a complex hypothesis. We reject it, or rather we never think of it, for the reason that it is not the simplest compatible with the axioms adopted.

The foundation of the axiom of Lie remains to be supplied.

GEOMETRY AND CONTRADICTION.

In following up all the consequences of the different geometrical axioms, are we never led to contradictions? The axioms are not analytical judgments *a priori;* they are conventions. Is it certain that all these conventions are compatible?

These conventions, it is true, have all been suggested to us by experiences, but by crude experiences. We discover that certain laws are approximately verified, and we decompose the observed phenomenon conventionally into two others: a purely geometrical phenomenon which exactly obeys these laws; and a very minute disturbing phenomenon.

Is it certain that this decomposition is always permissible? It is certain that these laws are *approximately* compatible, for experience shows that they are all approximately realised at one and the same time in nature. But is it certain that they would be compatible if they were absolutely rigorous?

For us the question is no longer doubtful. Analytical geometry has been securely established, and *all* the axioms have been introduced into the equations which serve as its point of departure; we could not have written these equations if the axioms had been contradictory. Now that the equations are written, they can be combined in all possible manners; analysis is the guarantee that contradictions shall not be introduced.

But Euclid did not know analytical geometry, and yet he never doubted for a moment that his axioms were compatible. Whence came his confidence? Was he the dupe of an illusion? And did he attribute to our unconscious experiences more value than they really possess? Or perhaps, since the idea of the group was potentially pre-existent in him, did he have some obscure instinct for it, without reaching a distinct notion of it? I shall leave the question undecided although inclined towards the second solution.

THE USE OF FIGURES.

It may be asked why geometry cannot be studied without figures. This is easy to account for. When we commence studying geometry, we have already had in innumerable instances the fundamental experiences which have enabled our notion of space to originate. But they were made without method, without scientific attention and unconsciously, so to speak. We have acquired the ability *to represent to ourselves* familiar geometrical experiences without being obliged to resort to material reproductions of them; but we have not yet deduced from them logical conclusions. How is this to be done? Before enunciating the law, the experience in question is perceptually represented by stripping it as completely as possible of all accessory or disturbing circumstances,—just as a physicist eliminates the sources of systematic error in his experiments. It is here that figures are necessary, but they are an instrument only slightly less crude than the chalk which is employed in drawing them; and, like material objects, it is beyond our power to represent them in the geometrical space which forms the object of our studies; we can only represent them in sensible space. We accordingly do not study material figures, but simply make use of them in studying something which is higher and more subtle.

FORM AND MATTER.

We owe the theory which I have just sketched to Helmholtz and Lie. I differ from them in one point only, but probably the difference is in the mode of expression only and at bottom we are completely in accord.

As I explained above, we must distinguish in a group the form and the matter [material]. For Helmholtz and Lie the matter of the group existed previously to the form, and in geometry the matter is a *Zahlenmannigfaltigkeit* of three dimensions. *The number of dimensions is therefore posited prior to the group.* For me, on the contrary, the form exists before the matter. The different ways in which a cube can be superposed upon itself, and the different ways in which the roots of a certain equation may be interchanged, constitute two isomorphic groups. They differ in matter only. The mathematician should regard this difference as superficial, and he should no more distinguish between these two groups than he should between a cube of glass and a cube of metal. In this view the group exists prior to the number of dimensions.

We escape in this way also an objection which has often been made to Helmholtz and Lie. "But your group," say these critics, "presupposes space; to construct it you are obliged to assume a continuum of three dimensions. You proceed as if you already knew analytical geometry." Perhaps the objection was not altogether just; the continuum of three dimensions which Helmholtz and Lie posited was a sort of non-measurable magnitude analogous to magnitudes concerning which we may say that they have grown larger or smaller, but not that they have become twice or three times as large.

It is only by the introduction of the group, that they made of it a measurable magnitude, that is to say a veritable space. Again, the origin of this non-measurable continuum of three dimensions remains imperfectly explained.

But, it will be said, in order to study a group even in its formal properties, it is necessary to construct it, and it cannot be constructed without matter. One might as well say that one cannot study the geometrical properties of a cube without supposing this cube to be of wood or of iron. The complexus of our sensations has without doubt furnished us with a sort of matter, but there is a striking contrast between the grossness of this matter and the subtle precision of the form of our group. It is impossible that this can be, properly speaking, the matter of such a group. The group of displacements such as it is given us directly by experience, is something more gross in character; it is, we may say, to continuous groups proper what the physical continuum is to the mathematical continuum. We first study its form agreeably to the formula of the physical continuum, and since there is something repugnant to our reason in this formula we reject it and substitute for it that of the continuous group which, potentially, pre-exists in us, but which we originally know only by its form. The gross matter which is furnished us by our sensations was but a crutch for our infirmity, and served only to force us to fix our attention upon the pure idea which we bore about in ourselves previously.

CONCLUSIONS.

Geometry is not an experimental science; experience forms merely the occasion for our reflecting upon the geometrical ideas which pre-exist in us. But the occasion is necessary; if it did not exist we should not reflect; and if our experiences were different, doubtless our reflexions would also be different. Space is not a form of our sensibility; it is an instrument which serves us not to represent things to ourselves, but to reason upon things.

What we call geometry is nothing but the study of formal properties of a certain continuous group; so that we may say, space is a group. The notion of this continuous group exists in our mind prior to all experience; but the assertion is no less true of the notion of many other continuous groups; for example, that which corresponds to the geometry of Lobatchévski. There are, accordingly, several geometries possible, and it remains to be seen how a choice is made between them. Among the continuous mathematical groups which our mind can construct, we choose that which deviates least from that rough group, analogous to the physical continuum, which experience has brought to our knowledge as the group of displacements.

Our choice is therefore not imposed by experience. It is simply guided by experience. But it remains free; we choose this geometry rather than that geometry, not because it is more *true*, but because it is the more *convenient*.

To ask whether the geometry of Euclid is true and that of Lobatchévski is false, is as absurd as to ask whether the metric system is true and that of the yard, foot, and inch, is false. Transported to another world we might undoubtedly have a different geometry, not because our geometry would have ceased to be true, but because it would have become less convenient than another. Have we the right to say that the choice between geometries is imposed by reason, and, for example, that the Euclidean geometry is alone true because the principle of the relativity of magnitudes is inevitably imposed upon our mind? It is absurd, they say, to suppose a length can be equal to an abstract number. But why? Why is it absurd for a length and not absurd for an angle? There is but one answer possible. It appears to us absurd, because it is contrary to our habitual way of thinking. Unquestionably reason has its preferences, but these preferences have not this imperative character. It has its preferences for the simplest because, all other things being equal, the simplest is the most convenient. Thus our experiences would be equally compatible with the geometry of Euclid and with a geometry of Lobatchévski which supposed the curvature of space to be very small. We choose the geometry of Euclid because it is the simplest. If our experiences should be considerably different, the geometry of Euclid would no longer suffice to represent them conveniently, and we should choose a different geometry.

Let it not be said that the reason why we deem the group of Euclid the simplest is because it conforms best to some pre-existing ideal which has already a geometrical character; it is simpler because certain of its displacements are interchangeable with one another, which is not true of the corresponding displacements of the group of Lobatchévski. Translated into analytical language, this means that there are fewer terms in the equations, and it is clear that an algebraist who did not know what space or a straight line was would nevertheless look upon this as a condition of simplicity.

In fine, it is our mind that furnishes a category for nature. But this category is not a bed of Procrustes into which we violently force nature, mutilating her as our needs require. We offer to nature a choice of beds among which we choose the couch best suited to her stature.

H. Poincaré.

Paris, France.