

[Extract from a paper presented to the Texas Academy of Science,
December 22, 1897.]

THE INTRODUCTION TO LOBACHÉVSKI'S NEW ELEMENTS OF GEOMETRY.

TRANSLATED FROM THE RUSSIAN BY GEORGE BRUCE HALSTED.

TRANSLATOR'S PREFACE.

No part of Lobachévski's largest work, "New Elements," has ever before been published in any language but the original Russian.

I gave an account of it in 1893 at the Mathematical Congress of the World's Columbian Exposition, and promised then the publication of my translation. (See Mathematical Papers of Chicago Congress, pp. 92-95.)

This promise was delayed for a personal visit to Kazan, the home of Lobachévski, and Maros-Vásárhely, the home of Bolyai.

The volume of *Ucheniya zapiski* in which the publication of *Noviya nachala* was begun is dated 1835, but Lobachévski himself gives as the dates 1836, 1837, 1838, in his "Geometrical Researches on the Theory of Parallels" (1840), the little book by which alone his ideas have heretofore been accessible to the world in general.

But it is preëminently in his "New Elements" that the great Russian allows free expression to his profound philosophic insight, which on the one hand shatters forever Kant's doctrine of our absolute *a priori* knowledge of all fundamental spatial properties, while on the other hand emphasizing the essential relativity of space.

Lobachévski's position is still after sixty years the final philosophy.

No one has gone beyond it.

Many still exhibit, like Professor Newcomb in his beautiful address on the Philosophy of Hyper Space (SCIENCE, January 7, 1898), a marked naïveté. He says: "Certain fundamental axioms are derived from experience, not alone individual experience, perhaps, but the experience of the race." On the contrary, the hereditary geometry, the Euclidean, is underivable from real ex-

perience alone, and can not be even proved by experience. Its adequacy as a subjective form for experience has not yet been disproved, but might in future be disproved. It can never be proved.

The realities which with the aid of our subjective space form we understand under motion and position, may, with the coming of more accurate experience, refuse to fit in that form. Our mathematical reason may decide that they would be fitted better by a non-Euclidean space form. But we are, and will be, helpless to get such a space form from any experience whatever.

Space is presupposed in all human notions of motion or position. We may drop out such specifications from our space form as render it specifically Euclidean, but we can not replace them by non-Euclidean. Euclidean space is a creation of that part of mind which has worked and works yet unconsciously.

It is not the shape of the straight lines which makes the angle-sum of a rectilinear triangle a straight angle. With straight lines of precisely such shape but in a non-Euclidean space, this sum may be greater or less. In non-Euclidean spaces, if one edge of a flat ruler is a straight line the other edge is a curve, if the ruler be everywhere equally broad. In any sense in which it can be properly said that we live in space, it is probable that we really live in such a space. What becomes of the dogma that fundamental axioms are derived from experience alone?

GEORGE BRUCE HALSTED.

INTRODUCTION.

Every one knows, that in Geometry the theory of parallels has remained, even to the present day, incomplete.

The futility of the efforts which have been made since Euclid's time during the lapse of two thousand years to perfect it awoke in me the suspicion that the ideas employed might not contain the truth sought to be demonstrated, and whose verification, as with other natural laws, could be helped only by experiments, as for example astronomic observations.

When finally I had convinced myself of the correctness of my supposition, and believed myself to have completely solved the difficult question, I wrote a paper on it in the year 1826.*

* Exposition succincte des principes de la Géométrie, avec une démonstration rigoureuse du théorème des parallèles, read February 12, 1826, in the séance of the physico-mathematic Faculty of the University of Kazan, but not printed

Application of the new theory to analysis is to be found in memoirs which I printed in the *Kazan Courier* of the years 1829 and 1830 under the title *On the Foundations of Geometry*.

The principal result at which I have arrived under the assumption that lines are dependent upon angles, is the possibility of the existence of Geometry in a wider sense than that in which Euclid first expounded it to us.

In this broadened form I gave to the science the name *Imaginary Geometry*, in which as a special case the *Customary Geometry* is contained under limitations in the general hypotheses recommended by practical measuring.

That the new foundations are sufficient, I have undertaken to show in a paper recently published in the *Scientific Memoirs* of the University of Kazan.*

In the endeavor to attain this aim, not so much directly as rather by the shortest inverse way, I chose then, going over from certain assumed foundations, to arrive at equations for all relations and at expressions for all magnitudes of geometry.

Even should my discovery have produced no other advantage than the filling of the gap in the elements, yet at least the interest which this subject has always aroused obliges me now to treat it with detail. I will begin with an examination of the earlier theories.

It is easy to show that two straights making equal angles with a third never meet, since they are then perpendicular to a certain straight.

Euclid assumed inversely, that two straights unequally inclined to a third always meet.

To demonstrate this latter assumption, recourse has been had to many different procedures; such as trying first to find the sum of the angles of a triangle, or comparing infinite areas comprised between the sides of an angle and between perpendiculars to a straight, or supposing that the angles depend only on the ratio of the sides, or finally attributing to the straight new properties, in order to complete its definition.

All these demonstrations, some ingenious, are without exception false, defective in their foundations and without the necessary rigor of deduction.

There is not even one of them sufficiently simple and sufficiently specious to present usefully to beginners.

In 1800 Legendre, in the third edition of his geometry, set up

*In Book I of the *Scientific Memoirs*, for the year 1835, under the title *Imaginary Geometry*.

the proposition that the sum of the angles of a triangle can not be greater than π , than two right angles.

There also he wished to prove that this sum could not be $< \pi$. But here he has not noticed that just then when the value of the sum deduced in another way shows some contradiction, the lines possibly no longer make a triangle.

I do not think it necessary to insist here in more detail on this error, which Legendre himself conceded later, when he declared, although the principle used as foundation was subject to no doubt, yet he found difficulties which he could not overcome.*

In the *Mémoires* of the Academy of Science of Paris for 1833, he has also published the theorem, that the sum of the angles in every rectilinear triangle must equal π , if in any one it has this value.

I had to prove the same proposition in the theory I wrote in 1826. I even find that Legendre often came upon the way which I have chosen with such success. But probably his prejudice in favor of the proposition assumed by all caused him, at each attempt, either to be hasty in the deduction or to fill it out with things which under the new theory are no longer admissible.

Let us analyse all he has printed on this subject in the *Mémoires* of the French Académie for the year 1833.

In the triangle ABC (Fig. 1) draw from A through the midpoint I of the side BC the straight [sect] $AC' = AB$. Prolong AB to make $AB' = 2AI$. Thus we get a triangle $AB'C'$ in which $B'C' = AC$, and the sum of the angles S is the same as in the first triangle ABC, of which the angle CAB passes into the $\triangle AB'C'$, dividing into the two at the points A and B'.

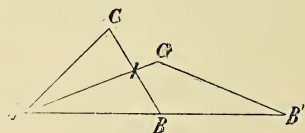


Fig. 1.

Suppose now AB the largest side of the triangle ABC or anyhow not less than the others, and also $BC \leq AC$. Then is $AC' \geq C'B'$, and the angle opposite the side $B'C'$ in $\triangle AB'C'$ is at most half as great as the angle CAB. Continuing thus, we will arrive at a triangle where two of the angles will be as small as we choose, while the sum of all three has the same value as in the first triangle ABC.

Legendre would from this conclude that in diminishing the two angles the approach of the opposite sides toward the third side would finally convert the remaining angle into two right angles,

*Legendre's own words are: "Nous devons avouer que cette seconde proposition, quoique le principe de la démonstration fût bien connu, nous a présenté des difficultés que nous n'avons pu entièrement résoudre." (*Mémoires de l'Acad. des Sc. de l'Inst. de France*, Tome XII. 1833, p. 371.)

and that therefore in the original and consequently in every triangle $S = \pi$. (*Réflexions sur la théorie des parallèles. Mémoires de l'Acad. des Sc. Tome XII. 1833, p. 390.*)

However, this reasoning is not correct, because the sides of the triangle here increase without limit and therefore also we can assume the limit of approach so that the angle $AC'B' = S < \pi$.

Let A, B, C be the angles of $\triangle ABC$ at the points marked by these letters; let $A'B'C'$ be the angles of $\triangle AB'C'$ at the points A, B', C' ; finally let h be the perpendicular let fall from C' on AB' .

With help of the Imaginary Geometry, supposing $S < \pi$ and designating by e the base of Napierian logarithms, we find

$$\begin{aligned}\cot A' &= \cot A + \frac{\sin C}{\sin A \sin B} \\ \cot B' &= \cot A + \frac{\sin B}{\sin A \sin C} \\ e^h - e^{-h} &= 4 \sqrt{\frac{\cos \frac{1}{2} S \cos (\frac{1}{2} S - A) \cos (\frac{1}{2} S - B) \cos \frac{1}{2} (S - C)}{\sin^2 B + \sin^2 C + 2 \sin B \sin C \cos A}}.\end{aligned}$$

The first two equations show that A' and B' are always real, and with the transformation of the triangle decrease toward limit zero. The last equation gives always the height h and determines the limit of approach

$$h = \log \cot \frac{1}{4} S,$$

where the logarithm is Napierian.

Although Legendre designates his demonstration as completely rigorous, he without doubt thought otherwise, for he adds the proviso, that a difficulty which one would perhaps still find can always be removed. For this, he has recourse to calculations founded on the first formulas of plane trigonometry, which it would be necessary first to establish, and which just in this case are useless and lead to no result.

To omit no argument in favor of his theorem, Legendre remarks, that congruent triangles placed together with, throughout, different angles by threes at a point, make a ribbon which can be prolonged indefinitely and which then is bounded by two broken lines concave toward one another for $S < \pi$, for $S > \pi$ convex one toward the other. The proved impossibility of the latter case induces also to reject the first, where the lines, turned toward each other like two circle-arcs, would seem necessarily to meet.

It seems superfluous to analyse and judge such an argument, where there is not even the shadow of a rigorous demonstration. I will only add, that lines concave toward each other only approach each other because of the notion assumed in the ordinary geom-

etry, while on the assumption $S < \pi$ nothing prevents supposing their prolongation while remaining equidistant.

Bertrand [of Geneva] and, after his example, Legendre, have wished to compare infinite surfaces in angles and between perpendiculars.

Demonstrations of this kind should be preceded by a determination of the magnitude idea, which in geometry one can only understand in connection with measurement, if besides it is convened beforehand by what characteristic to discriminate greater from lesser.

For example, a piece of a plane enclosed by a curve is considered greater than a polygon comprised entirely within it; on the contrary, smaller, if inversely the curve is entirely within the polygon; and this is so, even if no means be known of measuring these surfaces.

But as to infinite surfaces, it is necessary here, as everywhere else in mathematics, to understand as the ratio of two infinities its limit when its two terms increase indefinitely.

Besides, it is necessary to understand here by geometric magnitude at least such a one as we can approximately determine, estimating by the characteristics of inequality.

In this regard, the demonstration of Bertrand, and all the analogous proofs are far from satisfactory since in them we see no procedure for measuring the surfaces; not to mention, that the surfaces must first be bounded, to increase into the infinite through widening of their boundaries.

Suppose we wish to compare the surface X (Fig. 2), in the opening of the angle DCE, with the surface Y, between the two perpendiculars AB, CD to AC, for $AC = a$.

The ratio of the two surfaces X and Y, even when they increase to the infinite, will turn out different according to the way we convene in the start to limit them.

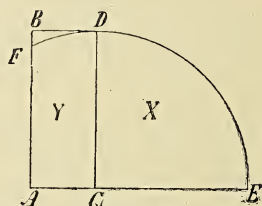


Fig. 2.

Suppose for example that in every triangle the angle-sum $S = \pi$. Make $AB = CD = na$, where n is a whole number.

Then draw the straight DB. On the other hand, limit the surface in the opening of the angle DCE by a circle-arc described from C as center with radius $CD = na$.

We find

$$Y = na^2, \quad X = \frac{1}{4}\pi n^2 a^2;$$

Whence $\frac{Y}{X} = \frac{4}{\pi n}$

a ratio which with increase of the surfaces Y and X, for $n = \infty$ becomes nul, as Bertrand also has assumed.

If, however, instead of assuming $AB = na$, we make $AB = n^2 a$, but CD is left as before, then this time we find the ratio

$$\frac{Y}{X} = \frac{4}{\pi},$$

which is constant for every n and consequently also for $n = \infty$, where both surfaces become infinite.

Thus the ratio Y/X turns out each time different, according as we limit the surfaces in the beginning, and according as they subsequently increase into the infinite.

Now limit both surfaces X and Y by an arc FDE described from C as center with radius $CD = na$.

Under the assumption, that in every triangle the angle-sum $S > \pi$ or $S = \pi$, it is easy to see that the ratio $Y : X$ becomes nul for $n = \infty$.

This means that in both cases Y becomes an infinite of the first order, but X an infinite of the second order, as also Bertrand thought.

On the other hand, with the assumption $S < \pi$, we find the ratio*

$$\frac{Y}{X} = \frac{2(e^{na} + e^{-na}) \arcsin\left(\frac{e^{2a} - 1}{e^{2a} + 1} \cdot \frac{e^{2na} + 1}{e^{2na} - 1}\right) - 4 \arcsin\left(\frac{e^a - e^{-a}}{e^{na} - e^{-na}}\right)}{\pi(e^{na} + e^{-na} - 2)},$$

where the number $e > 1$ is independent of n ; and consequently for $n = \infty$

$$\frac{Y}{X} = \frac{2}{\pi} \arcsin\left(\frac{e^{2a} - 1}{e^{2a} + 1}\right),$$

which is not null so long as $a > 0$, and which can be neglected only when a is infinitesimal.

From another point of view it is easy to see that we can not presume the ratio $Y : X$ for $X = \infty$ to become null in case $S < \pi$.

*Designate by e the base of the Naperian logarithms, and leaving indeterminate the unit for length, put

$$\sin r' = \frac{2}{e^r + e^{-r}}, \quad \sin \phi = \tan r' \cot x',$$

$$\tan r' = \frac{2}{e^r - e^{-r}}, \quad \tan x' = \frac{2}{e^x - e^{-x}}.$$

Then we find as the expression for the piece of the circle comprised between two perpendiculars to the radius r , of which one passes through the center and the other is at distance x from it (Imaginary Geometry)

$$\frac{1}{\sin r'} \arccot(\sin r' \cot \phi) - \phi.$$

Let AB , CD , be perpendiculars on AC (Fig. 3); take arbitrarily $AB = CD$. Assuming $S < \pi$, the angles ABD , CDB , are acute; the perpendiculars BB'' , DD'' , on BD are situated in the interior of the

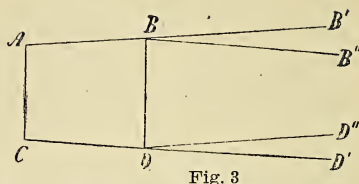


Fig. 3.

figure $B'BDD'$, without ever meeting, and make with BB' and DD' the angles $B'BB''$, $D'DD''$, whose infinite surfaces are smaller than the surface $B'ACD'$, contrary to what Bertrand wished to maintain of all angles without exception.

Bertrand gives another form of his demonstration considering infinite surfaces in angles only.

In the triangle ABC (Fig 4), call A , B , C the angles opposite the sides a , b , c , which prolong: AC through A to A'' , AB through B to B'' , BC through C to C' . The surfaces $X + x$, $Y + y$, Z , in the openings of the exterior angles $\pi - A$, $\pi - B$, $\pi - C$, extended indefinitely, form about the point C toward all sides an unlimited surface, where is omitted the surface

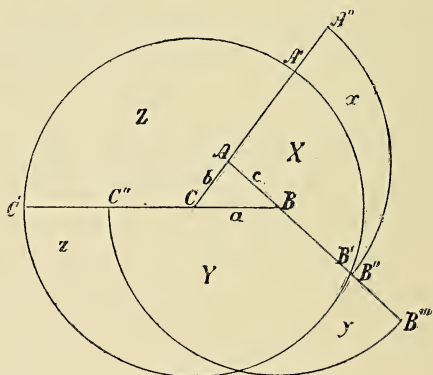


Fig. 4.

ABC , which because of its smallness may be neglected. This signifies that $\pi - A + \pi - B + \pi - C = 2\pi$, whence $A + B + C = \pi$.

Verify now this reasoning, after limiting the surfaces by arcs of circles described from the points A , B , C as centers with radii equal to $CC' = r$.

The circle about center C , meeting the sides of the angle $\pi - A$ at A' and B' divides the surface in the opening of $\pi - A$ into two parts; the one X within, the other x without the entire circle with center C .

The same circle, meeting at B' , C' the sides of the angle $\pi - B$, divides the surface in the opening of $\pi - B$ into two parts: Y within, y without the circle; but besides, the piece z of the entire circle does not appertain to the angle $\pi - B$.

That gives all possible cases which our figure can present, pre-supposing $r > a$, $r > b$, $r > c$, $a > c$.

Designating now by Δ the area of the triangle ABC , by R the area of the circle of radius r we find

$$\frac{R}{2\pi} (\pi - A) = X + x.$$

$$\frac{R}{2\pi} (\pi - B) = Y + y.$$

$$\frac{R}{2\pi} (\pi - C) = Z.$$

$$R = X + Y + Z + z + \Delta.$$

Hence we get

$$A + B + C = \pi + \frac{2\pi}{R} (\Delta - x - y + z) \dots \dots \dots (1).$$

It would still remain now to demonstrate, in the hypothesis that the sum of the angles of a triangle $S < \pi$, that $\Delta = x + y - z$ for every r or at least for $r = \infty$; but to undertake this labor would be in vain. On the contrary, for the condition $S < \pi$, we find always $\Delta < x + y - z$, as we will immediately see.

When $CC' = r$ increases, the points B' , B'' , B''' withdraw from the point B in the direction AB , while the lines CB' , CB'' , CB''' approach to a certain limit CD (Fig. 5), which in the new theory I have designated as a *parallel* to AB , and which makes with CA , CB angles $ACD = \pi - A - a$, $BCD = B - \beta$, such that a and β are any positive quantities.

Moreover, by making $CB' = r$ sufficiently great, the angles $B'CD$, $B''CD$, $B'''CD$ become as small as we choose. Calling, then, P the surface of the triangle ACB' , we will have for r very great (Fig. 4) without appreciable error

$$\frac{1}{2\pi} (\pi - A - a) R = \frac{1}{2\pi} (\pi - A) R + P - x.$$

$$\text{Hence } x = P + \frac{1}{2\pi} a R \dots \dots \dots (2).$$

Putting for the moment the angle $C'CD = M$ (Fig. 5) the already given equation $(\pi - B)R = 2\pi(Y + y)$ may be written

$$\frac{1}{2\pi} (\pi - B) R = \frac{1}{2\pi} M R + P - \Delta + y - z \dots \dots \dots (3)$$

In this substituting the value

$$M = A + C + a$$

we have an equation which combined with equation (2) conducts anew to equation (1) and so verifies it.

If we make

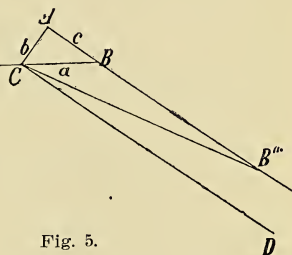


Fig. 5.

$$M = \pi - B - \beta,$$

we deduce from equation (3)

$$\beta = \frac{2\pi}{R}(\Delta - P - y + z),$$

which combined with equations (1) and (2) gives

$$A + B + C = \pi - \alpha - \beta.$$

So in Bertrand's demonstration is already assumed $\alpha = 0$, $\beta = 0$, the very thing which was to be proved.

Just as Bertrand was satisfied with the comparison of infinite surfacee in the angles of the triangle, so Legendre wished to use the infinite surfaces between pairs of perpendiculars, which he called *biangles*.

Really he only demonstrated that the infinite surface CABD (Fig. 6), between the perpendiculars AC, BD, to AB is equal to the infinite surface DEFC obtained from the preceding in cutting off the quadrilateral ABEF by the perpendicular EF to BD. This is sufficiently evident; but Legendre has not noticed here, that EF may possibly not meet AC.

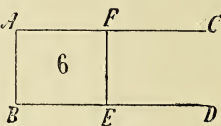


Fig. 6.

To overcome this little difficulty, you have only to suppose that EF is the perpendicular from F on BD; but then how can we conclude therefrom that $FE = AB$ and the angle $EFC = \frac{1}{2}\pi$? It is not possible to mend the false deduction, wherein Legendre's inadvertence was so gross that without remarking this grave error, he considered his demonstration as very simple and perfectly rigorous.

Again the plan has been thought of in the theory of parallels to use as foundation, that in triangles the angles must depend on the ratios of the sides.

At first such an assumption seems as simple as necessary; but if we seek to probe what idea we have thereof, whence it takes its source, we are forced to designate it as just as arbitrary as all others to which recourse has been had.

We cognize directly in nature only motion, without which sense-impressions are not possible. Consequently all other ideas, for example geometric, are artificial products of our mind, since they are taken from the properties of motion; and consequently space in itself, for itself alone, for us does not exist. Accordingly it can have nothing contradictory for our mind, if we admit that some forces in nature follow the one, others another special geometry.

To illustrate this thought, assume, as many believe, that attractive forces diminish because their action spreads on a sphere. In

the ordinary geometry we find $4\pi r^2$ as magnitude of a sphere of radius r , whence the force must diminish in the squared ratio of the distance.

In the imaginary geometry I have found the surface of the sphere equal to

$$\pi (e^r - e^{-r})^2,$$

and possibly the molecular forces may follow such a geometry, whose whole diversity would depend, consequently, on the number, always very great, e .

Moreover, suppose this only a mere hypothesis, for whose confirmation other more convincing proofs are to seek; yet nevertheless it can not be doubted that forces alone produce all: motion, velocity, time, mass, even distances and angles.

With forces all are in a close connection, which we do not understand in its essence, wherefore also we can not affirm, that in the relation of different kinds of magnitudes to one another only their ratios can enter.

If we admit the dependence on the ratio, why should we not also assume a direct dependence?

Certain circumstances already favor this opinion. For example, the magnitude of attractive force has as expression the mass divided by the square of the distance. For the distance null this expression really represents nothing.

We must begin with some, large or small, but always really present distance, and then first the force manifests itself. Now we may ask how distance produces this force? How a bond between things so essentially different can exist in nature?

That we will probably never understand. But if it is true that forces depend upon distances, just so also may lines be dependent on angles. At least the diversity is alike in the two cases, for the difference lies properly not in the idea, but only in this, that we know the one dependence from experience, but the other, for want of researches, we must mentally assume, either beyond the limits of the visible world, or in the narrow sphere of molecular attractions.

However that may be, yet will the assumption, that merely the ratio of the distances can determine the angles, be a special case, at which we always arrive, if we assume the lines infinitesimal.

The procedure of ordinary geometry therefore always leads to results true but not in so wide a sense as those given by the general geometric system which I have called imaginary geometry.

The difference between the equations of the one and the other comes from the introduction of a new constant, which observations

must determine, but which so obtained, is found such, without noteworthy error, that the geometry assumed by all for practical measurements more than suffices, even if in itself it be not rigorously true. This means either that this system is found in Nature by chance, or else that in it all distances accessible to us are still infinitesimal.

In general must every proposition that the imaginary geometry, if it is applied to lines of great extent, gives about the elements of a magnitude, necessarily lead to the rules of the ordinary geometry, because in this case only the first powers are retained of the numbers which represent the lines, and consequently every where only their ratios enter into the equations. Such for example are, that the distances between two perpendiculars to a straight are everywhere equal; that the perpendicular with its extremity describes a straight line; that the circle with increasing diameter passes over into a straight line.

Of all known theorems of this sort one must give first rank to that which carries with it the dependence of the ratio of lines upon the angles; at least here the simplicity in the idea corresponds to our first impressions; but also that is all that can be said in its defence; every other judgment is either false or superficial.

Again it is not possible to find objectionable, that with the immediate dependence of lines upon angles a magnitude enters which is just as arbitrary as the choice of the unit. To this we may answer, that nothing prevents taking in the equations not the ratios of the lines to one of those therein considered, but the ratios to a magnitude determined in any way in Nature. That I have shown in the Imaginary Geometry by giving equations where all lines appear in relation to a single one to be determined from observations, if these should suffice.

I consider it not necessary to analyze in detail other assumptions, too artificial or too arbitrary.

Only one of them yet merits some attention; the passing over of the circle into a straight line. However, the fault is here visible beforehand in the violation of continuity, when a curve which does not cease to be closed however great it may be transforms itself directly into the most infinite straight, losing in this way an essential property. In this regard the imaginary geometry fills in the interval much better. In it if we increase a circle all of whose diameters come together at a point, we finally attain to a line such that its normals approach each other indefinitely, even though they can no longer cut one another.

This property, however, does not appertain to the straight, but to the curve which in my paper *On the Foundations of Geometry* I have designated as *circle-limit*.

Finally if the difficult problem of parallelism is to be resolved by experiment, Legendre's proposal, to put a radius six times in a circle, must be declared much too insufficient.

In my *Foundations of Geometry* I have shown, using astronomical observations, that in a triangle whose sides are approximately equal to the distance of the earth from the sun, the angle-sum can not differ from two right angles by more than 0.0003 of a second.

This difference increases in geometric ratio with the sides of the triangle, and consequently up to the present, as I remarked before, is the ordinary geometry more than sufficient for the measurements in practice.

We can arrive at this result by the aid of propositions sufficiently simple and conformable to the first notions of the science, though of course the complete theory requires a wholly changed order of exposition and besides the addition of trigonometry.

To the imperfections of the theory of parallels must also be reckoned the definition of parallelism itself. However this imperfection depends nowise, as Legendre supposes, upon a faulty definition of the straight line, nor even on certain faults in primary notions, faults which I propose to indicate here, attempting to correct them so far as I can.

Ordinarily one begins geometry with attributing to bodies three dimensions, to surfaces two, to lines one, while to the point is allowed none.

In calling the three dimensions length, breadth, height, and under these designations actually understanding the three coördinates, we hasten in this way to impart immature ideas by words to which ordinary speech gives already a certain, but indeed for rigorous science still indefinite sense.

In fact, how is it possible to represent clearly to one's self the measurement of length, if we as yet do not know what a straight line is?

How can one speak of breadth and height without previously having said something of perpendiculars, of the plane, and besides how are related perpendiculars in one and in different planes?

Finally, if the point has no extent, what remains over to it then, that it may be the object of a conclusion?

Suppose even that every one represents clearly to himself the straight line, though indeed unable to give an account of his idea; but the question is, how now with help of the straight line to de-

termine one dimension for a curved line, and two for a curved surface?

True, one need not require that length, breadth and height be perpendicular to one another; it suffices, if one has taken for them lines in different directions. Yet we meet also in this case peculiar difficulties.

If we take as principle not to anticipate ideas to be developed later, the question is how then to express the condition that the three dimensions of a solid shall appertain to three straights in different planes?

Further, one must not confuse the different direction of the two parts which go out from a break in the line, with the twofold extension of a surface; finally one must set forth completely what is to be understood precisely by a direction and by an angle.

In short, space, dimension, locus, solid, surface, line, point, direction, angle, are words with which one begins geometry, with which one however never connects a clear idea.

However, we may consider all these things from still another side. It is necessary to observe that here the obscurity in the idea is produced by the abstractness, which is superfluous in the application to actual measurement, and consequently is introduced uselessly into the theory.

Surfaces, lines, points, as geometry defines them, exist only in our imagination; when we treat the measurement of surfaces and lines, we apply thereto solids.

For this reason we need only to speak of surfaces, lines, points, as we must think them in an actual measurement, and then we will hold ourselves only to just the ideas which are immediately connected in our understanding with the representation of bodies, to which our imagination is accustomed and which we can in nature immediately verify, without entering upon others, artificial and strange.

But with these new ideas the science takes even at the beginning another direction, which it follows until it goes over into analysis, where then the procedure in exposition now again takes the ordinary aspect. I will endeavor to explain wherein consists this change.

In mathematics one follows two methods, Analysis and Synthesis.

Equations make the distinctive characteristic of analysis. They serve as first basis for every judgment and in general lead to all conclusions.

Synthesis, or the method of building up, requires exactly the

same representation which in our mind is connected immediately with the primary ideas.

The principal advantage of analysis consists in this, that from the equations one goes always straight to the end proposed.

Synthesis is in general subject to no universal rule, but one must necessarily make the beginning with it, in order finally, after one has found the equations, therewith likewise to reach the limit beyond which now all goes over into the science of numbers.

For example, one demonstrates in geometry that two straights perpendicular to a third never meet; that the equality of triangles follows from that of certain of their parts. In vain would one seek to treat analytically propositions of this species, even as all the theory of parallels. One would never succeed, just as one would not be able to do without synthesis for measuring plane rectilineal figures, or solids terminated by plane surfaces.

It is well understood that in synthesis we should use the helps offered by analysis; but it is incontestible that in the beginnings of geometry or mechanics, analysis can not serve as sole method. One may compress the circle of synthesis; but it is impossible to suppress it completely.

It is not even necessary too much to hasten the substitution of analysis for synthesis, and the introduction of functions each time some dependence can be perceived between quantities, without yet knowing in what this dependence consists and how it expresses itself.

With this restriction, analysis is the sole method which, introduced in the science to begin with notions where reasoning alone is thenceforth sufficient, extends the bounds of our knowledge indefinitely in all directions.

Notions acquired in nature by the aid of our senses are without any doubt the primitive data.

The mind can and should as much as possible reduce their number, to make them afterwards the solid bases of the science.

No one, however, uses synthesis from this point of view, observing the rules we have just indicated.

It has generally been chosen to introduce prematurely analysis, and to develop the notions formed naturally in our mind, and to which remains only to give a name, without entering profoundly enough into the explanation, and without considering the exactitude of the definitions.

If facility and simplicity did not require to choose a method of instruction like that I recommend, its superiority in rigor would still make us sometimes employ it to great advantage.

I have made a first trial of it in algebra; I am now to undertake it for geometry.

Pure analysis, without any intermixture of synthesis, ought to be introduced in geometry only after having established equations for every sort of dependence, and given expressions for all geometric magnitudes.

We can not comprehend a geometric magnitude without measuring it, an operation which we can not effect, rigorously speaking, either for curves or for curved surfaces. In fact, however little may be the parts of a curve, they do not cease to be curves, and consequently they can not be compared with a straight; just as parts of a curved surface are not comparable with portions of a plane.

From another side, lines straight or curved, planes and curved surfaces do not exist in nature; we encounter only bodies, so that all the rest, created by our imagination, exist only in theory.

Lagrange admits in principle the proposition of Archimedes, that on a curve one can always take two points so near that the arc between these points may be considered as being greater than its chord, and smaller than the broken line formed by the two tangents touching at its extremities. (*Théorie des fonctions analytiques, par Lagrange.*)

Such a proposition is, in fact, necessary, but it destroys by itself the primitive idea of measuring curves with straights. The same thing happens when one proposes to measure curved surfaces with planes.

Thus the evaluation of the length of a curve represents not at all the rectification of the curvature; but it has for aim the finding of a limit to which the magnitude that would be obtained by a real measuring would approach the more as this measure became more exact. Now measuring is made more exactly as the chain employed has smaller links; it is done altogether exactly when in place of a chain a perfectly flexible thread is used. This is why in geometry one is obliged to prove that as the subdivisions are increased the sum of tangents decreases and that of chords increases, until these two sums differ indefinitely little from the common limit toward which they tend, and which is considered as the length of the curve.

It is then clear that exhaustion by such rules accords so much the more with direct measuring as this is the more exact. From this is also seen on what is founded the proposition of Archimedes.

As we have reasoned on lines, we should reason on curved surfaces, not pretending at all that very small parts of these latter can be planified.

Rigorously speaking, plane curvilinear areas, as also solids bounded by curved surfaces, can not be measured so long as they are compared those to a square, these to a cube. However, if one only proposes to find a limit toward which tend the results of a direct operation, it will suffice, for the attainment of this end, to demonstrate that such a limit exists necessarily in each particular case, and then to explain in what way we ought to understand the operation of measuring, and how to attain the amount of exactitude desired.

To satisfy these conditions recourse must be had to certain auxiliary propositions, admitted under the name of axioms; namely:

1. Two plane figures are equal when they are decomposable into parts identical, though differently arranged.
2. Of two plane figures, that is the smaller which is entirely contained in the interior of the contour of the other, without, however, filling this completely.
3. The magnitude of a triangle decreases indefinitely when one of its sides diminishes indefinitely.

This latter proposition is necessary for the evaluation of plane areas themselves.

It is necessary also to have recourse to analogous axioms when one wishes to measure solids.