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PLURALISM IN GEOMETRY

PAVEL ARAZIM
Dept. of Logic, Faculty of Arts, Charles University in Prague

ABSTRACT
This article is concerned with the topics stemming from the problems with interpreting the devolopment of geometry in the nineteenth and early twentieth century. The emerging of hyperbolic and eliptic systems as alternatives to the classical Euclid's one posed a serious challenge to the opinions about geometry of that time, particularly to the stances of Kant, who ascribed to the whole body of mathematics the status of synthetic apriori. I want to discuss to what degree the Kantian position may be defended and what should be changed in it. The relation with the development of modern logic is also hinted, particularly when dealing with the debate between Frege and Hilbert. Thus the problems of philosophy of geometry flow into broader issues about logic and mathematic knowledge and ultimately about the possibility and character of a priori knowledge in general.
Keywords: geometry, pluralism, structuralism, holism, experience, logic.

1. Introduction

It has always been one of the main aims of philosophy to clarify how we, as human beings, gain our knowledge and also to judge to what degree various kinds of knowledge may be regarded as valid. Such enquiries naturally required certain classifications of human knowledge as well as a classification of the faculties which we use to cognize the world. One of the most momentous distinctions was that between the a priori and a posteriori knowledge. It is well known that in order to get the latter, we must acquire some data from our experience, while getting the former doesn't entail anything else than pure thinking. Presented after the mentioned manner this distinction appears to be quite intelligible, but because of its rather preliminary and pre-theoretical character it leaves lots of issues open, so it is but hardly surprising that in the concrete systems of concrete theoreticians it was spelled out in many different ways. However we will try to render the definitions of the two types of knowledge more accurate and precise, it will be in pursuit of localizing the border which divides these two opposite spheres as exactly as possible. There is then some knowledge which all philosophers, notwithstanding their rather rationalist or rather empiricist inclinations would without much hesitation mark as either a priori or a posteriori. Yet it is a particularly challenging to deal with that knowledge of which it is far from clear to which side of the border between the two realms they should be regarded as belonging. An excellent example of such problematic cases is the knowledge provided by geometry.

Being a part of mathematics geometry had for centuries been regarded as a science which is infallible in its assertions and specifically Euclid's Elements served as a role model of intellectual thoroughness not only for geometers, but in fact for all mathematicians, the book had founded the position and prestige of mathematics. As late as in the nineteenth century they were used as study book and it wasn't until the end of that century that people like Hilbert started to doubt the exactness of Euclid's proofs. But for such a radical change of viewpoint to be undergone, there had to be already some development preceding it and in this case it was putting the assumption that Euclid's geometry is the only correct one into question. Till the time we're talking about it was considered obvious that Euclid's system provides incontestable truths and any alternative system whatsoever must be necessarily wrong. Euclid established his theorems by proofs which had been held for absolutely firm and his proceedings were in accordance with the spirit of Aristotle's concept of a scientific (axiomatic) theory, which also meant that his system contained some propositions that could not be proved though they were regarded as true and the whole system and all the theorems were in fact founded on them. Ultimately it was exactly in these propositions, i.e. in the axioms, that the problematic features of the theory lurked which eventually led to the great change of viewpoint. The very fact that not all the propositions can be proven didn't worry the Greeks, nor did it trouble the next generations, otherwise an infinite regress could hardly be prevented, but one of the axioms still seemed to be less obvious than the other ones and it was regarded as desirable (and was constantly sought for) to prove it from the remaining axioms. It was the axiom regarding parallel lines, which stated that two parallel lines (and no other than parallel, i.e. two right angles enclosing lines) do not meet but in the infinite, so that they can be prolonged as much as we please without getting their meeting at a common point (actually, this is only an equivalent restatement of the original one, but that is of little importance now). Trustworthy though it may seem, it is scarcely surprising that this axiom, originally called the Fifth postulate, should be perceived as troublesome, since it appeals to something which is closed to human intuition because of our finiteness, so that it cannot claim to be as obvious as e.g. the postulate stating that between two points exactly one line can be drawn. As Peter Vopěnka notes, the Fifth postulated used to be called the stain on the body of Euclid's Elements. Proving its redundancy (i.e. that it can be omitted from the theory without losing any theorems) thus became one the chief and, as turned out later, most vain ambitions of the mathematicians.

Yet hardly anybody did seriously doubt the truth of the Fifth postulate, though David Hume might be perhaps seen as a prominent figure of such a highly non-conformist opinion, as in his Treatise he denies geometry the same certainty which algebra and arithmetic may boast, and he is unhappy precisely with the postulates, i.e. with the propositions which are not proven, but taken for obviously true. The Scottish Skeptic refuses to acknowledge as absolutely certain that two lines which are not parallel must necessarily meet at only one point because considering the case when they are almost parallel we can hardly determine whether it is only one point that they share or whether it could be an immensely small line segment. Yet generally the validity of the Fifth postulate wasn't very commonly doubted, actually almost not at all, but it was felt that though true it isn't true so obviously that it may be accepted without further argumentation. Therefore many mathematicians had tried to prove it out of the other axioms. One of the geometers who tried to prove it was the Italian Girolamo Saccheri who proceeded using the *reductio ad absurdum* form of proof. This means that he started to build a system of geometry which would fail to satisfy the Fifth postulate. More specifically, it was a geometry which enabled two lines which enclosed an angle smaller than two right ones never to meet. Petr Vopěnka calls his strategy building a world doomed to fall into ruins. Yet this fall, i.e. discovering the contradiction, still would not occur, not even the mathematicians to come after Saccheri would come to it, which eventually led to refraining from the assumption that such a system must be necessarily contradictory.

2. Kant

Before we shall follow the development of geometry with its denouement in accepting non-Euclidian geometries further, let us now have a little philosophical digression and observe Kant's Critic which brought what for a long time should pass for the most important conception of mathematics and geometry. Alberto Coffa in his book Semantic tradition states that everything that was said or written in the nineteenth geometry on the account of geometry was in fact, for better or worse, an addition to the discussion with Kant. And scarcely may such a statement be deemed exaggerated, since it was indeed Kant who enabled a genuinely thorough debate about the role geometry plays in the whole of the human cognition and he showed how the thesis that Euclid's geometry is the true one while the others are false can still be reasonably asserted. Though he ultimately comes across as a conservative who refused and perhaps slowed down the further non-orthodox progress of geometry, still even his opponents could to some degree thanks to him be capable of formulating their stances with an acceptable amount of clearness and plausibility. Let us now remind ourselves which the place is that geometry occupies in the framework of Kantian epistemology.

The key feature of Kant's view is that geometry, as well the whole mathematics, is a science which is absolutely certain, expressed in his terminology, it achieves apodictic certainty. Yet at the same time Kant did not overlook the fact that it apparently is not just a purely formal science, that it has got quite a specific, concrete content and its results can be applied on the space of our physical world. The first fact prevents us from considering geometry as an empirical science, otherwise we could never arrive at that complete certainty at which we really do, on the other hand the second fact constricts us not to be satisfied with the mere stating that it is a priori, because thus it could not tell us anything genuinely new and it would not be applicable to the real space.

The very realizing of both of these features is that which constitutes Kant's most important contribution to the philosophical treatment of geometry. Unlike his predecessors he did not lack the necessary balance needed not to oversimplify the matter and describe geometry as a posteriori or merely a priori. We may say that he actually conceived a new sphere of human knowledge which he reserved for mathematics and particularly geometry. Such a revolutionary turn was to become a source of both great inspiration and ultimately great problems. His solution of the problem of geometry is closely related to his treatment of the issue of space in general. Basically, the competing predecessors of Kant had come forward with two accounts of space, either it is some sort of a special entity which differs from those located in it or it is not but a theoretical concept of human reason which has invented the space by observation of the reciprocal situation of individual material objects and by abstracting away from their individuality. It was because he was able to find a middle course out of the dilemma of space that he was capable of finding the way out of the dilemma of geometry. The space is according to Kant not something which exists independently of us, yet its depending on us does not straightforwardly imply that it is not but our contrivance which produced it. He argues that it has to be admitted that we are not free to choose the space we exist in; we are forced to perceive everything in the specific spatial form which partakes on our nature. There is of course no necessity regarding what we actually see in the space, but we might be sure that we will see it in the space. Should there be no space as the pure form of our intuition, we could by no means see the objects which fill it.

And geometry is exactly the science about this a priori form of our intuition, i.e. about the space. And thanks to the fact that we cannot choose our space, but there is only one possible space which is given to us (though it is no entity which exists independently on us) geometry can achieve such knowledge that is absolutely sure and cannot be refuted by experience. On the other hand all the specific experience is dependent on the space (simply because it has to be situated in it), therefore the discoveries of geometers are applicable to it. Lots of its theorems might be provable in a purely logical manner by inferences from the axioms or the theorems already proven, but its very foundations, the axioms, are not gained in such a way, but we receive them from our pure intuition, which is an intuition that cannot see the real world surrounding us but merely its form.

For most Kant's contemporaries this stance was a brilliant coping with the contradictions between the former conception and it appeared to catch the spirit of geometry particularly well. Kant, as most of people at the time did, took for granted that Euclid's geometry, including its very basis, the axioms, must be basically true and thus it seemed that he finally succeeded in finding the philosophical justification which geometry was waiting for so long. Yet precisely what was the main virtue of Kant's account should later turn out to be also its main problem, as the scholars were trying to figure out and explain in an intelligible way, what this pure intuition, as a peculiar human faculty from which our geometry springs, actually is. When endeavouring to prove a certain theorem about the properties of triangle, for instance the Pythagoras' theorem, it is clear that we are not really interested in an individual triangle we might have sketched or even minutely construed on a piece of paper. This physical picture may surely help to understand the proof more swiftly and thoroughly, but we are not actually dealing with this precise triangle that we can see with our very eyes. The validity of our proof would not suffer any diminution should the sketch be but very approximate because of the sides' being somewhat crooked and so on. This means that whenever we are doing the real geometry, the mathematical discipline, we never do so by observing any of the real triangles in the material world. If one is here tempted to adopt the Platonic way of seeing the matters and start to talk about some ideal triangles of which the material ones are but imperfect images, it should be remarked that such a shift should be regarded with suspicion as something conflicting with the whole spirit of Kant's Critique. Kant would surely oppose this way of doubling all the entities. What the geometry is really about are not any specific and mysterious entities, but the forms under which the entities we encounter in experience fall. This makes it seem even more difficult to explain how we can deal with something like that, how we can practice geometry. It was this vagueness which gradually, together with the development occurring in geometry itself, led to the radical revision, if not a straightforward refusal of Kant's position.

Let us ponder yet a little bit on the epistemological status of this conception. How can it refuted and what evindence (of any kind, not necessarily empirical) could warrant it, or at least witness significantly in its favour? This question is in fact aiming at the very heart of the whole issue of geometrical pluralism and therefore we cannot but hint at the possible way of dealing with it, since a more thorough and relevant treatment of it will not be possible until we clarify what the contributions of the new geometries were and why they were so important as to pose a serious challenge to the Kantian account. Let us now realize the difficulty of effectively attacking Kant with the means which are now at hand. What could refute his theory empirically? Might the real objects be shown to disagree with the Euclid's geometry? Imagine two parallel lines were found which would, though possibly after going a very long distance, encounter or a triangle such that the sum of its inner angles would not be equal to the two right angles. As we have observed, for Kant geometry is something that precedes the empirical experience and first enables it and therefore it cannot be judged on the basis of what the experience shows. The possible phenomena we have just pointed to, the disobedient parallels and the disobedient triangle could still be well explained without the need to undergo a significant change of our opinions regarding geometry, as abandoning Kantianism would certainly be. The mentioned parallels could still be disregarded by a Kantian as not being exact lines because they probably do not run perfectly straightforwardly because they are probably, however imperceptibly, curved and so on. And the same would hold for any individual object treated geometrically, the object does not have to correspond to the pure form quite exactly and therefore there is no empirical testing of geometry. Even if the measurements of angles, lengths etc. of physicals object did give us some results significantly differing from what the Euclid's geometry would predict, even if the discrepancies would be visible by mere eye without precise measurements, a Kantian could still maintain that the problem is rather in our inability to measure with the requisite exactness or to construct the figures corresponding to our concepts or even in the imperfections of our very sight. After all, we quite commonly do reckon with the fact that we do not see the objects around us as they really are in themselves, they appear smaller from distance or deformed when observed through certain specific materials etc. To put it shortly, the Hegelian formula "the worse for the facts" can always serve as a refuge and the potential opponents of Kant would have to bring more than mere empirical facts, they would have to insert them in a new, probably complicated theory.

Are there any conceivable ways of attacking Kant, other than just collecting challenging empirical data? As one might expect, this is somewhat more difficult to explain, but however it was indubitably the failure of all the attempts to prove the Fifth postulate what caused still more and more theoreticians to put the Kantian position into question. Yet the mere consistency of non-Euclidian systems, even if it should really be the case, would not suffice as a direct rebuttal of Kant because we have to demand not only consistency, however necessary it is, but also veracity when choosing our geometry. It would not really take Kant by much surprise if more geometrical systems were to be found free of contradiction because he would have still maintained that only the Euclid's one is correct which is a fact we know by the virtue of our pure intuition. Despite the possible vagueness it should be noted that such an assertion did not sound too unclearly or evasively, but quite reasonably to the ears of the late eighteenth and early nineteenth century. Without doubts even nowadays a student which gets to know the issue must be at first pretty perplexed by the strange notions of parallels which meet or on the other hand about lines which are not parallel yet they never encounter. Such ideas can at first sight hardly make different impression than that of an intellectual curiosity. Many decades after Kant's death it was still possible to think of the new geometries as of consistent and in many regards very interesting mathematical theories which nevertheless could hardly be considered as an explication of space. Thus despite the incessantly increasing number of significant contributions to such theories brought by brilliant minds such as those of Gauss or Lobachevsky, it still did not seem that such enterprises could have a substantial motivation and therefore they were rather ignored by the broader audience. Although it holds for all disciplines of mathematics that it is far from easy to determine what they are actually about, the non-Euclidian geometries appeared to be something that lacks any subject matter whatsoever, and thus they were deemed nothing but a queer entertainment of a few peculiar mathematicians who did not want to tackle something more important.

3. Beltrami's model and the path to the recognition of the new geometries

The described situation started to undergo some changes first at the time around the half of the nineteenth century thanks to the Italian mathematician Eugenio Beltrami who demonstrated that the non-Euclidian geometries can be seen as disciplines about the space after all. It is remarkable that Beltrami was actually still a Kantian and intended his discoveries rather as means to defend Euclid, yet ultimately he, quite on the contrary, opened the way to acknowledging the new geometries as viable alternatives to the traditional one. This scholar showed that those geometries have their models, so that it can be imagined ("seen" in the sense of Kantian pure intuition) what they are about and thus may be considered as dealing with something real. We shall not describe his discovery in much technical detail but rather focus on its very core.

Strange though it may seem, we are in fact capable of imagining something like lines which are parallel, yet they encounter each other, as well as some lines which are not parallel, but never encounter. These of course will not be lines, as we usually understand them. In the first case they are the shortest distances between two points on the surface of a sphere, also called the geodetic lines of the surface. A specific geometry can be developed talking about this kind of lines on the surface of a sphere, which is a type of a curved surface. On the other hand we can also create a geometry dealing with the geodetic lines on a surface which is curved in the opposite way, so that it can be imagined as a sort of a valley which might be extended infinitely, so that, as opposed to the geometry on a sphere (which is called the elliptic geometry), this one will still take place on an infinite surface.

It is clear that any two elliptical lines are finite and that they have to cut each other twice. Furthermore between two points there can be more than only one line (understood in the aforementioned sense as the shortest geodetic distance) because if the two points happen to coincide with the two ends of a diameter, then there are infinitely many shortest lines connecting them. Another fact which was so unthinkable before Beltrami is that there is no similarity between triangles, so that if any two of them differ in the length of their sides, then they necessarily have to differ in their inner angles, as well. The sum of the inner angles, then, is for every triangle greater than the sum of two right angles (for more precise formulations and proofs see [6]).

The length of the lines (which is evidently the same for all the lines on a given sphere) as well the degree of the deviation of the sum of the angles in a triangle from the traditional two right ones depend on how big the sphere is, on which we create our geometry. Looking on the side of the geometry on the oppositely curved surface, which we shall from now on called the hyperbolic geometry, we discover that the opposites of some of the mentioned facts hold here. Thus there are pairs of lines such that they enclose an angle smaller than two right ones, yet they never meet, in fact, through a given point lying outside a given line it is possible to lead infinitely many lines than do not meet the original one. All the lines are, unlike in the previous case, infinite. What regards the triangles, each of them has got the sum of its inner angles smaller than two right angles and there is again no similarity between them. The greater is the negative curvature of the surface we are talking about, the greater are the differences from the Euclid's geometry, thus the sum of the angles in triangle gets smaller, two never-cutting lines can enclose a smaller angle etc.

Yet both these alternative systems share one important feature with the older Euclid's geometry, which is the free mobility of its figures, meaning that for example a triangle can be moved upon the given surface without undergoing any measurable changes, so that both its sides retain the original length as well the angles remain the same. But this not a general property of every surface we can conceive. Imagine a geometry on surface of a form of an egg, there the mentioned law would not hold. Generally this law holds exactly on the surfaces the curvature of which is constant in all their parts such as the Euclidian plain the curvature of which is constantly zero or the sphere surface (elliptic plain) with a constant positive and hyperbolic plain (the "valley") with a constant negative curvature.

As I have already said, Beltrami intended his contribution as a support of Kant in that it showed that the alternative geometries are capable of spatial interpretation only because the Euclid's geometry is capable of it. Thus every geometry which claims its legitimacy has to be in a way transferable to the classical one. The geometry on the elliptic plane is actually an Euclidian geometry on a surface of a sphere as well as the geometry on a hyperbolic plane is an Euclidian geometry on the surface shaped as a valley. But thanks to the German universal genius Hermann von Helmholtz and his interpretation of these discoveries Beltrami's models eventually led to a certain liberalization regarding the new geometries and to forming pluralism in geometry. Both of the models mentioned so far were dealing only with two dimensions, so that the geometries were developed on surface, not in space. It still appeared that the space as it is with its three dimensions can be really treated only by the means of Euclid's system, while the alternatives have to remain constrained to only two dimensions. Helmholtz used one particular feature of Beltrami's work, namely Beltrami had conceived a bijection between two models, between the points of a pseudosphere and points of an Euclidian circle. This map was made in such a way that to each pair of points from the circle it assigned exactly the same distance as their counterparts on the pseudosphere had, the length of the geodetic line segment connecting them. Because the equation for counting the distance on the pseudosphere was already known, Helmholtz (and together with him Riemann) could generalize the equation for three or actually arbitrarily many dimensions. As we have been talking about curved surfaces, Helmholtz dared to start talking about curved spaces, though he cautiously admitted that such a description is much more metaphorical than the two-dimensional one, so that it has to be taken in a much more reserved manner. Thus it might have still seemed that when applied to all the three dimension, the new geometries were nothing but abstract systems without a genuine connection to the real world. But Helmholtz was still to come with another theoretical experiment which would change this opinion once and for all.

4. How Helmholtz used and further developed Beltrami's results

Though the reader might have well started to intuit it, I shall now explicitly state the theoretical orientation of the lecture, in which Helmholtz first made his revolutionary opinions on geometry public. Helmholtz is expressly mentioning Kant and takes his conception for well exposing the double character geometry apparently has, giving us on the one hand perfectly certain (necessary) knowledge, while at the same time it is applicable on the world around us. Shortly put, the knowledge provided by geometry is synthetic and a priori. But Helmholtz wants to challenge this view and thus poses a question, whether some of our empirical knowledge did not happen to creep into our geometrical systems and due to its inconspicuousness and the force of habit had become considered as necessary and a priori (by the way, what a Humean way of putting the matters!). After presenting the elliptic and hyperbolic geometry in two dimensions, he edeavours to convince that these models already suffice as a rebuttal of the position that Euclid's geometry is a posteriori because we can think of people who would live in only two dimensions on the elliptic or hyperbolic surface and would thus with all probability form their geometrical theories quite differently from us. But this example is clearly problematic.

But Helmholtz came with yet another example which, though similar in the spirit, was to become much more important because it demonstrates that it is possible to have an empirical experience in three dimensions which would serve as testimony in favour of the new geometries, i.e. it would naturally induce us to either the elliptic or the hyperbolic geometry. Let us have at least a glance at this example. As we have spoken about the bijection between the points of an Euclidian circle and a (notice the indefinite article, there are infinitely many hyperbolic surfaces depending on the curvature) hyperbolic plane, so we can after the same manner consider a bijection between the points of an Euclidian sphere (including the points inside, not just on the surface) and a hyperbolic space. Let us now imagine somebody who would get transferred from our apparently Euclidian world into a hyperbolic one. What would his experience be like? Because the straight lines would still correspond to the trajectory of the rays, he would feel as if he were inside the mentioned sphere, so that the world would appear finite to him, with all the objects within the distance of the radius of the sphere (supposing the traveler happens to be exactly in the middle of the sphere). But as well as the secants of the circle were infinite (by the bijection), so the secants of this sphere would be actually infinite, in spite of all the appearances. If he then saw two lines which seemed not to be parallel and with all probability cutting somewhere behind the surface of the sphere, he would never be able to get to the point where they would be supposed to meet because no such point would in fact exist. Approaching the objects lying near the seeming surface of the sphere he would find them surprisingly larger than he expected, more in their depth than length. Should he then observe two lines which would seem quite what we are used to call by appearance parallels, following them to the edge of the sphere he would find their distance constantly increasing and not remaining the same as he with all probability had thought. But these actually would not be real parallels because their (hyperbolic) distance would really increase in the direction outside of the sphere. On the other hand the real parallels would seemingly approach themselves as they would run closer to the surface. Yet measuring their distance, the traveler would find it constant. Actually he would feel as if he were getting smaller by approaching the surface of the sphere. Sooner or later he should change his opinion and regard as parallels the lines which indeed are parallel in the hyperbolic sense and would to stop to regard the pairs of lines, which to his Euclidian eye seemed to be of constant distance, as parallel. Thus such a man would, as we have sufficiently proven, develop much rather a hyperbolic than Euclidian geometry because the latter would not correspond to his measurements!

For the sake of completeness, let's add that Helmholtz also shows what the elliptic world would be like. In this case the distant objects would appear remoter than they really are and one would need less time to move towards them etc. (this basic explication highlights the way in which the two alternative geometries are opposed to each other, the first being about negatively, the other about positively "curved" spaces). What really matters here is that the Euclidian geometry can now hardly be called the only true one, neither does it precede our experience, at least not in the way Kant and most of the theoreticians of the time pictured it. The sphere (once again – just a seeming sphere in the hyperbolic case) we have been describing right now doesn't have to be so clearly visible, it can be actually as large as we may please, provided the curvature is very low. The example with the traveler who can see the whole sphere he inhabits is possible only with quite a high curvature of the given hyperbolic space.

Should thus the real physical space have a trifling curvature, then discovering its not being Euclidian would indeed be far from easy. Many extremely minute measurements might be needed to arrive at such a surprising discovery.

Does this all imply that we have established that geometry is in fact not a discipline of pure mathematics and should rather be taken for a natural science, if still a very general natural science, perhaps even the first and most general of all? Helmholtz unsurprisingly shows a certain inclination towards such a stance at various points of his lectures, but this still didn't satisfy him and he doubted it, as well. For the time being let's content ourselves with realizing that the whole example may also be understood otherwise than as witnessing for the validity of hyperbolic geometry.

Instead of changing our geometry we may in one way or another change some different parts of our theoretical framework. One route we could take would be that of creating new mechanics and therewith describe the world while still sticking with Euclidian geometry and metric. So we could well assert that the whole space is enclosed within the finitely large sphere and whenever something approaches its surface, then it becomes smaller; thus a new force might be conceived which causes this diminution, perhaps as a property of the sphere. Alternatively we could choose to regard the size as constant, but their velocity changing analogically, i.e. decreasing ad infinitum in the direction outside of the sphere.

The sketch I have now produced might suffice to convince that we cannot confront with the real world our mere geometry. The geometry must be presented together with a much broader theoretical picture. Should we then end up rather as relativists, knowing practically no boundaries of what is admissible as a geometrical theory, instead of having a clear empiricist conception of geometry? I should think that we have arrived precisely at the point of our treatise where philosophy is desperately needed by this mathematical and physical theory. The real philosophical work should take place.

5. Some important philosophical approaches

As I have already mentioned, Helmholtz wasn't fully content with the empiricist account of geometry and he didn't take it for a conclusive solution of the whole issue. He also tried to make his account more just with respect to Kant, to ask whether Kant wasn't in some sense still right. In a response to the polemic against his Heidelberg lecture he begins with remarking that though the Euclid's geometry isn't true a priori as a whole, there still might be some a priori knowledge about the space, even though perhaps only so vague as for example that the objects are placed next to each other etc. So there might be some a priori spatial knowledge, though poorer than Kant thought. Helmholtz doesn't actually say much more about this proposal, yet he says that the results of modern geometry could be perhaps thought of not as rebuttals but rather as specification and purification of Kant's system. Still in the same favourable vein he presents another notable thought.

Liberating himself somewhat from the empiricism he follows the thought that it is not geometry itself but a larger, particularly mechanical theoretical framework that we can reasonably confront with the real space. Therefore it might be perhaps good to isolate geometry somehow from the experience and make it an a priori science in a way. The objects which inhabit the universes of geometric theories such as points, lines triangles and so on could be seen not as something described by the axioms (or, less directly, by the theorems), but rather defined by them. Thus Helmholtz approached the modern methods in mathematical logic quite significantly. The expressions he uses to describe this stance are not at first glance very intelligible, at least he doesn't use the standard modern terminology.

He states that it is possible to see the matters in such a way that the axioms cannot be really tested by empirical measurements because before doing so we would have to check whether the objects to be measured and the measuring tools themselves satisfy the very axiom which are supposed to be tested. If we adopt this view, then the geometric entities become completely determined by certain descriptions, namely by those given by (e.g. Euclid's) axioms. The axioms would then remain a priori, but they would cease to be synthetic and turn analytic, which is a status still radically different from the one ascribed to them by Kant. If the universe of geometry would be determined notionally, then the axioms would be only consequences of such notional determinations, they would actually just spell it out explicitly and vice versa we may say that the universe would be built after the instructions given by the axioms. This however is an utmost change of the traditional conception of a theory because the difference between axioms and definitions virtually disappears. But even though Helmholtz did come up with this option, he didn't argue in favor of it and left it just as one of the hypothetical options (Alberto Coffa describes this as Helmholtz's most illuminated moments). It was up to the next generation of philosophers to formulate this position more clearly and present compelling arguments in favour of it, despite the harshly critical reactions which might have been awaken by that. Let's now observe the further life of this vein of thought, including its potential trouble.

The new geometries had become an important issue for the philosophers by the end of the nineteenth century and Stewart Shapiro describes in his article A Tale of Two Debates the history of passionate exchanges of opinions between Poincaré and Russell on one and Frege and Hilbert on the other hand. Let's now see some particularly interesting points of this history. In both cases it is a conflict between a conservative conception of geometry and mathematics in general with a more modern, much more liberal and relativistic stance. While the two German authors in their polemic concentrate more specifically on the geometrical systems and their logical background, the Brit and the French speak more generally, thus touching more the relation between geometry and the real world and physics. Poincaré succeeded very well in describing the new possibilities opened by the alternative systems and showed how far the pluralism, or even relativism, can actually lead.

According to him the debate whether Euclid's, hyperbolic or elliptic geometry should be called the true one is misguided and doesn't make any sense because all the three competing systems can do the work required from geometry with the same effectiveness and are thus interchangeable. Let's consider the fact that the three geometries do not agree on the sum of the inner angles of a triangle. This disagreement, as Poincaré puts it, is only apparent because each of the three theories denotes a different entity when they all speak about a triangle. Traditionally it is a plain figure constituted by three straight lines, which intersect in pairs and it can be proven that the inner angles are taken together exactly of the greatness of two right angles. The hyperbolic and elliptic geometry then speak about quite a similar figure, but the described lines are somewhat curved, in the first case in the direction inside the figure, in the other outside, so that the discussed sum is naturally smaller or, respectively, greater than in the case of the classical triangle. And such a description is still relative, for the hyperbolic geometry only its lines are straight and the others curved, as well as for the elliptic one. So ultimately deciding between different geometries isn't but deciding about which names to assign to which entities, such as for example the name of the line. According to Poincaré all the attempts to judge the geometries empirically by measurements are in vain because they necessarily end up in a vicious circle. In order to verify with a ruler whether some line segment is really straight, thus really a segment of a line, we would first have to measure the ruler itself, in order to be sure it is really straight. So a new ruler, playing the role of a meta-ruler would be needed, and thus it would progress ad infinitum.

So it depends on nothing else than our mere will and convention which geometry we adopt in our theories. Should we thus remain faithful to Euclid and in our measurements discover some discrepancies between the behavior of light rays and what our geometry tells us to expect from them, then we are free to choose between ceasing to regard the rays as genuine lines or abandoning Euclid's geometry in favour of another one. Perhaps one of the two mentioned options might prove to be more viable or practical, but we cannot say that it's more true.

Russell returned to Poincaré, similarly as Frege to Hilbert, that this means ignoring the real world, which geometry is supposed to describe and is not able to simply make up, as the relativists appear to be trying to persuade us. Both Russell and Frege also persist on strictly distinguishing between definitions and axioms. Though they have the important common feature of needing no proof, we still ought not treat them as interchangeable because the axioms are either true or false, while the definition just determines what a specific theory deals with. There thus must be some means how to decide the competition of geometries and talking about such things as an infinite secant (recall Beltrami's model) is an absurdity. Despite all those attacks, Russell gradually started to accept the new developments in geometry, but still thought that philosophy has to find a reasonable interpretation of them.

As Jaroslav Peregrin notes in his article The Natural and The Formal, Frege and Hilbert perhaps didn't have quite incompatible opinions on our topic but rather each of them over-accentuated different aspects of the plurality of geometries. Both of them shared very much as two of the most important personalities of modern logic in both their goals and achievements in building working formal systems. Hilbert occupied himself a lot with the study of Euclid and fixing the gaps which were gradually found in the traditional proofs. This meant particularly inserting the steps which Euclid deemed obvious, yet which could not be omitted in the eyes of modern logic. Similarly Frege also wanted to make the foundations of mathematics more solid, to give a more firm ground to the truths discovered in the course of history. But Hilbert didn't have a very strict notion of what is actually true in mathematics, neither of its subject matter. He quite explicitly subscribed to the conception presented as possible by Helmholtz that axioms are in fact definition of a certain mathematical discourse. A given set of axioms is then acceptable under the very liberal condition that it's free of contradiction. Thus more axiomatic systems of geometry might be acceptable and Hilbert thus reduces truth into mere consistency. Hilbert also pointed to what has since become a common wisdom, that any theory can determine its universe at most up to the isomorphism. According to Shapiro he thus opened the path leading to structuralism because this means that mathematics cannot talk about any entities, but just about their structural relations.

Thus an arbitrary set of entities may be confronted with a given axiomatic theory and if these entities, together with their properties and relations, satisfy all the axioms, then the axiomatic theory can be legitimately said to talk about this set of entities, among other sets. Frege's objection that with Hilbert's system he has no means to decide whether his pocket watch doesn't happen to be a point in the geometrical sense actually goes to the very core of the issue, but it doesn't surprise Hilbert at all and he doesn't regard it as problem. Frege later started to interpret Hilbert as talking not about such entities as points, lines, triangles etc., but rather about the notions (in the sense of Frege's Begriff, a predicate) of a point, line, triangle etc. Frege believed that Hilbert was thus, without knowing it, working in the second-order logic and he then "translated" his results into the second order language.

6. Conclusion

Having thus seen the most important points of the history of new geometries and their philosophical importance, let's now ponder on to what extent does the issue still remain important, which question does it keep posing and whether acceptable answers are at disposition. The key question probably remains to be the one whether and how can geometry be considered an empirical science, the results of which should be verified by measurements. I believe it was already sufficiently shown in this article that, despite all the raised doubts about Kantianism, geometry simply cannot be considered as an empirical science, at least not exclusively empirical. On the other hand if we adopted the Euclid's system and maintained that the sum of the angles in a triangle is equal to two right ones and were confronted with more and more real triangles contradicting this hypothesis, then we would surely have to react in one way or another. And though it might have some relevance to point out to the imprecision of our measurements and the discrepancies between the ideal and real figures, this should not be used as a general excuse under any circumstances whatsoever. If somebody would in such a situation keep advocating the authority of the pure intuition, we would then tend to think of it as of a natural delusion intrinsic to us, as Helmholtz suggests at one point. Anyway, Kant himself talks about various natural delusions which do not achieve any authority by the virtue of their being natural and thus inevitable.

On the other hand the mentioned discrepancies do not have to lead us to one exclusive solution of the problematic situation. We can of course change our geometry, but also preserve it while changing our mechanics (for example) and talking for example about some physical deformation of the real figures etc. Yet such changes may sometimes cost too much and make our theoretical framework too complicated or in a way absurd and thus it might be sometimes the most reasonable option to change the geometry. But how to decide which theoretical frameworks should be still regarded as acceptably reasonable and which not is a problem that I am afraid doesn't really have an explicit solution. I do not believe there is a way to determine something like that in general. On the whole, this account appears to be applicable as a strong argument in favour of Quine's holism, as we can in fact decide which parts of our knowledge should be confronted with the experience more and which less directly (and thus made less liable to revisions). Such decisions are with great probability guided significantly by pragmatic reasons, with some theories being more useful and easier to work with than others.

But pluralism doesn't have to be a complete relativism, not all the geometric systems which may be contrived, may also be good candidates for explaining the space. We have to take Hilbert's expressions that consistency is all to be required from a geometrical theory cum grano salis. He himself would actually not have subscribed to something so plainly absurd, as he still regarded geometry as a science describing our spatial intuition, though this intuition is used only when deciding about the axioms and then is banned from the proofs. When proving the theorems everything should be done in a way that corresponds to Frege's remark about pocket watch or Hilbert's own one to the effect that in his theories the words such as line, point, triangle and so on should always be interchangeable with any other words, such as beer mugs, trains and any others (by a correct substitution, of course). But which systems deserve being called geometry? No simple guide seems to be at hand and some vagueness can hardly be avoided.

Yet let's note that both the hyperbolic and elliptic geometries are in a specific way reducible to the Euclid's one, they may be considered as geometries on a curved Euclidian surface or space. Thus it might be reasonable to demand that every new geometrical system be, despite all its novelty, in a similar relation to the already established ones, otherwise it should not be considered as a possible alternative to them, actually not as geometry at all. Petr Vopěnka in his work Rozpravy s geometrií says in this vein that Kant was wrong when he thought that Euclid's geometry is the only possible one, but his error was not a complete one because every geometry still has to be interpretable in it.

It is of course already a great testimony of Kant's importance that basically whenever we try to cope with the issue of geometrical pluralism, then the issue might be put as deciding whether his stance is acceptable. And it surely isn't without further qualifications, but, as Helmholtz suggested, there are still possibilities of accepting a modified form of Kantianism. It may seem that we can't but decide to what extent we will take geometry for an empirical and to what degree as pure, i.e. a priori science. It is hard to explain why exactly the Euclid's system had seemed so natural for many centuries and why the alternative systems can be deemed natural in their own way, as well. But the choice of geometry should still not be an arbitrary one. In accordance with Vopěnka's expression, we can see that the alternative systems still must have a lot in common with the classical one and perhaps this common ground may be considered as a deeper foundation of this science. A foundation, which may not be reducible to our experience, which also may not be changed because it is simply given to us a part of our cognitive nature. Thus it might be premature to condemn Kant's synthetic knowledge a priori by stating that there is simply no such. Thus Kant wasn't perhaps really wrong, but rather too hasty in his conclusions.

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