Andrei Rodin

Did Lobachevsky have a model of his *Imaginary* geometry?

1) Introduction

The canonical story about discovery of Non-Euclidean geometries goes like this. Since Antiquity people looked at Euclid's Fifth Postulate (P5) with a suspicion because unlike other Postulates and Axioms of Euclid's *Elements* P5 didn't seem self-obvious. For this reason people tried to prove P5 as a theorem (on the basis of the rest of Postulates and Axioms). Typically they tried to prove P5 by *reductio ad absurdum* taking the negation of P5 as a hypothesis and hoping to infer a contradiction out of it (and the rest of the axioms). However the desired contradiction didn't show up. Consequences of non-P5 were unusual but not overtly contradictory. At certain point some people including Gauss, Bolyai and Lobachevsky guessed that non-P5 opens a door into a vast unexplored territory rather then leads to the expected dead end. Lobachevsky was the first one who shared this opinion with public and explored some issues of the new geometry which he called *Imaginary* in his IG (Note 0). However the issue remained highly speculative until Beltrami in (1868) found some models of Lobachevsky's geometry, which proved that Lobachevsky's geometry is consistent and so can be treated on equal footing with Euclidean. Finally Hilbert in his (1899) put things in order by modernising Euclidean axiomatic method and clarifying the logical structure of Non-Euclidean geometries (Note 1).

Obviously the story is oversimplified. However my task now is not to provide it with additional details but question a basic assumption, which this simplified version of history shares with a number of better elaborated ones. This assumption concerns the very notion of mathematical theory. The notion of theory, which goes on a par with the above story is described in the following quote:

"[P]rimitive terms, such as "point", "line" and "plane" are undefined and could just as well be replaced with other terms without affecting the validity of results. ... Despite this change in terms, the proof of all our theorems would still be valid, because correct proofs do not depend on diagrams; they depend only on stated axioms and the rules of logic. Thus, geometry is a purely *formal* exercise in deducing certain conclusions from certain formal premises. Mathematics makes statements of the form "if ... then"; it doesn't say anything about the meaning or truthfulness of the hypotheses." (Greenberg 1974, p.252)

And here is how this modern notion of mathematical theory allegedly relates to the discovery of Non-Euclidean geometry:

"The formalist viewpoint just stated is a radical departure from the older notion that mathematics asserts "absolute truths", a notion that was destroyed once and for all by the discovery of Non-Euclidean geometry. This discovery has had a liberating effect on mathematics, who now feel free to invent any set of axioms they wish and deduce conclusions from them. In fact this freedom may account for the great increase in the scope and generality of modern mathematics. " (ibid.) (Note 2)

The above quote suggests that Lobachevsky was one of those liberators who detached geometrical reasoning from intuition and spatial experience, stopped asking whether or not usual axioms of geometry are true and came to the notion of mathematics as playing with axioms. As we shall shortly see this has nothing to do with the historical reality. Before coming to a more detailed discussion let me point to one question, which Greenberg's story leaves unanswered. Why Lobachevsky and others played with P5 but not another postulate or axiom? The obvious reason for it is that on the *traditional* account P5 (unlike other Euclid's axioms and postulates) looked dubious to begin with; unlike other postulates and axioms it was not self-evident. This triggered the long-term research on the "Problem of parallels", which led to the discovery of Non-Euclidean geometry in 19th century. These facts make it possible to refer to the discovery of Non-Euclidean geometry as an evidence *justifying* the traditional view on geometry. The argument may go like this: since ancient times the old good geometrical intuition showed that P5 is not universally true and finally in19-th century Lobachevsky and others proved this fact rigorously. This shows how much one's favourite interpretation of history of mathematics depends on one's stance concerning the subject-matter of mathematics itself.

As I shall argue the above popular view on the history of geometry of 19th century is deeply misleading. However I shall not blame its overtly anachronistic character for it. For I don't believe that anachronisms can and should be ruled out from a historical study of mathematics on general methodological grounds. As far as a study of history of mathematics is supposed to be something else than making a mere chronology one needs to rely onto some ideas about mathematics to begin with. Otherwise one cannot even say a history of *what* he or she is going to study, and why certain historical sources qualify as mathematical while some other do not. In *this* very general sense every interesting history of mathematics is doomed to be anachronistic. The difference between good and bad anachronisms is more subtle. A historian may extract from given sources everything that fits his or her favourite notion of mathematics and ignore the rest as non-essential peculiarities. So one gets a teleological history showing how mathematics progresses from its early days toward its glorious today's state. I count this method as a bad anachronism even if the fit is well justified. Alternatively the historian may take seriously difficulties of application of today's schemes to older sources and try to revise these schemes (rather than the sources!) aiming at a better fit. This latter method, which I adhere to, has at least two advantages. First, it allows not only for tracing the history of mathematics in the narrow sense (i.e. history of acquiring of existing mathematical knowledge) but also the history of changing notion of mathematics. In other words it allows for a view on history of mathematics as a chapter of the history of ideas. Second, the latter strategy makes the historical research about mathematics interesting for the mathematical research itself. For it eventually helps reviving some old ideas which can turn to be interesting for mathematics and its philosophy today and perhaps even tomorrow.

The rest of the paper is organised as follows. I start with a more precise description of conceptual scheme used in canonical historical reconstructions of geometry of 19th century. Then I stress difficulties arising when one anachronistically applies this scheme to Lobachevsky's work and finally propose a remedy. We shall see that the question of whether or not Lobachevsky had a model of his geometry has two answers none of which is of yes-or-no kind. The first immediate answer is that the question is ill-posed since Lobachevsky didn't have anything like our notion of model in his disposal but worked in an older conceptual framework which combined traditional "synthetic" geometrical methods with certain analytic devices. I shall show that the popular view according to which this traditional approach doesn't work in Non-Euclidean geometry cannot be justified. The second answer is subtler and more interesting. There is in fact an aspect of Lobachevsky's work relevant to our current notion of model. But Lobachevsky's counterpart of this today's notion is nevertheless strikingly different. Allowing for the talk of models in Lobachevsky one discovers something surprising: Lobachevsky didn't have a model for the geometrical theory known by his name (unless one counts as a model some usual intuitions associated with geometrical concepts like in Euclidean case) but he built a non-standard model of *Euclidean plane* in a Non-Euclidean space (sic). We shall see that this construction, which from the today's viewpoint might look bizarre is crucially important for Lobachevsky's project. I shall conclude explaining an approach to building mathematical theories, which makes Lobachevsky's construction to look more natural.

2) Hilbertian scheme

Euclidean geometry and Lobachevskian geometry are two different theories. How exactly these theories are identified and distinguished one from the other? There is a sense in which certain parts of Euclid's *Elements* also can be also qualified as different theories, for example, basic Euclidean Planimetry developed in Books 1-4 and the Theory of Proportions of Book 5. But obviously Euclidean and Lobachevskian geometries are called different theories in a *stronger* sense than that. In which stronger sense exactly? A standard answer relies upon the notion of theory suggested by Hilbert in his (1899) and later elaborated by Veblen, Tarski and others. Here a theory is identified with a list of axioms together with all propositions (theorems) deducible from these axioms. As far as rules of logic governing the deduction are assumed to be the same for the whole of mathematics mathematical theories may be distinguished by their (non-logical) axioms alone. Accordingly Euclidean and Lobachevskian geometries can be distinguished through (an appropriately reformulated version of) P5: Euclidean geometry assumes P5 while Lobachevskian geometry assumes non-P5; the rest of their axioms the two theories share in common. We see that unlike different parts of *Elements* Euclidean and Lobachevskian geometries are logically incompatible. But the notion of incompatibility involved here is also not so simple as it seems. True, combining the two theories into one immediately brings a contradiction. But the method of theory-building applied here and the epistemic scheme associated with this method (which I shall call *Hilbertian* for further references) allow for considering the two geometries on equal footing and doesn't require ruling one of them out in favour of the other. Allowing for such a peaceful co-existence of logically incompatible theories Hilbertian scheme makes them *epistemically* compatible. We have already know from the above Greenberg's quote about the price of this tolerance: the scheme rules out as senseless questions like whether on not P5 is *really* true or false. (Note 3) In 1899 when Hilbert's *Grundlagen* were first published such pluralism about geometrical axioms was not yet common, but on the contrary looked like a strong and very controversial epistemic view about Mathematics.

One may argue that what I call here Hilbertian scheme is commonly known under the name Axiomatic method, and so the new suggested name is useless. I disagree because I think that the neutral title of Axiomatic method too easily becomes misleading, particularly in historical contexts. For it hides essential differences between Hilbert's version of this method (and its more modern versions based on Hilbert's), on the one hand, and more traditional versions of Axiomatic method, on the other hand. Traditionally axioms are understood as "first" self-evident truths, which cannot be possibly proven. Aristotle famously argued that trying to prove everything one gets a risk of loosing the very notion of proof as a way of deriving less obvious truths from more obvious ones. Frege was among those who defended this traditional understanding of Axiomatic method in Hilbert's time. Remark that this traditional version of the method doesn't allow the pluralism about mathematical matters, which has been described above. Hilbertian scheme has some very special features, which make this pluralism possible. Let me now briefly remind them.

Propositions (i.e. axioms and theorems), elements of propositions and theories (systems of propositions linked by deduction) are viewed within Hilbertian scheme in two different ways. First, they are viewed as syntactic constructions having no meaning and truth-value. So conceived theories and propositions are called *formal*. Formal propositions are supposed to be provided with meaning and truth-values through a special procedure of *interpretation*, which assigns to terms of a given proposition some particular mathematical objects (Note 4). This procedure makes formal propositions into "usual" propositions having meaning and certain truth-values; this meaning and these truth-values obviously depend on the aforementioned assignment. This is the second way to conceive of a proposition within Hilbertian scheme. An assignment, which makes all provable (deducible) propositions of a given theory true is called *model* of this theory. A given theory may have multiple models and multiple "would-be-models", in which some formally provable propositions are true but some other turn to be false.

The role of models in Hilbertian scheme is (at least) twofold. First, models provide an intuitive support allowing, for example, for thinking of proposition "given two points there exist an unique straight line going through these points" in the usual way. (Alternatively one can think of points in the way one usually thinks of straight lines and think of straight lines in the way one usually thinks of points. It would make a difference in Euclidean geometry in which there exist lines without common points but not in Projective geometry in which any two straight lines intersect.) Second, models help for proving consistency of theories and independence of some axioms from some other axioms. For proving consistency of a given theory *T* it suffices to find some model *M* of *T*. A naive reasoning behind this claim is this: if some proposition *P* of *T* is true in *M* then proposition *non-P* is not true in *M*, and so *T* contains no contradiction. For it is not possible that *P* and *non-P* are both true "about" one and the same *M* (Note 5). Obviously for establishing that a given M is a model of a given theory *T* it is sufficient to check that axioms of T are true in *M*. Given that the axioms are true and inferences are valid *T* contains no contradiction. Notice that this argument takes us back to the traditional axiomatic method. This is why in the Hilbertian case one needs a refined version of it. The usual refinement goes as follows. Let's first take some mathematical theory, say, arithmetic, for granted. This means that we assume both the corresponding formal theory *A* and some its model *MA*. We shall call such an assumed theory (with its chosen model) a *metatheory*. Take now another formal theory *B* and build its model *MB* from elements of *MA*. Then you are in a position to claim that *if* theory *A* is consistent *then* so is *B*. Instead of consistency *tout court* one gets in this way only a proof of *relative* consistency. This is less than one might desire but still a lot. In particular this method allows for proving the relative consistency of Lobachevskian geometry with respect to Euclidean geometry and relative consistency of both these theories with respect to arithmetic. This provides a sufficient ground for claiming that there is no more reason to expect a contradiction in Lobachevskian geometry than in Euclidean.

I mention here all these well-known details (Note 6) because one should distinguish them very clearly before considering how to use Hilbertian scheme for a historical reconstruction. We can now see in which precise sense Greenberg is right claiming that the older notion of "absolute truth" was "destroyed" in modern mathematics: within the new scheme mathematical truths are no longer "absolute" but are "truths in a given model" which may turn into falsities in some other models (or would-be models) (Note 7). However, as we shall shortly see, Greenberg goes too far claiming that this destruction was due to the discovery of Non-Euclidean geometries. True, Hilbertian scheme wouldn't come about in 1899 without the discovery of Non-Euclidean geometries earlier in the same century. But this gives no ground for the claim that in works of Lobachevsky and other pioneers of Non-Euclidean geometry Hilbertian scheme was already inherently present. Let's now have a look at Lobachevsky's writings and see whether there is some trace of Hilbertian scheme there. For the following discussion I take Lobachevsky's STP as the principle reference. For discussing some epistemological issues I shall also refer to FG and NFG. (Note 8)

3)Hyperbolic intuition

STP is written in the classical Euclidean "synthetic" style reinforced by analytic methods described in the next section. As far as the logical structure of presentation is concerned it is apparently not of Lobachevsky's major concern. Lobachevsky presents to the reader a list of propositions without specifying which of them are definitions, which are assumed as axioms and which are assumed as commonly known theorems (independent of P5); among following proved propositions there are theorems known to Lobachevsky from his sources as well as theorems first proven by Lobachevsky himself. From a historical viewpoint these features of Lobachevsky's style are hardly surprising since all of Lobachevsky's predecessors and contemporaries working on the "Problem of parallels" also followed the same traditional line. I stress these features only in order to confront the widespread philosophical myth according to which the invention of Non-Euclidean geometry required an abrupt departure from the "usual" spatial intuition. To see that the myth has neither historical nor serious theoretical ground behind it let's reconsider after Lobachevsky the issue of parallels in its traditional setting. Instead of P5 I shall use after Lobachevsky the following Axiom of Parallels (AP) known to be equivalent to P5 since Antiquity (Note 9):

(AP) Given a line and a point outside this line there is unique other line which is parallel to the given line and passes through the given point.

Here the term "parallels" stands as usual for straight lines having no common points. We'll se shortly how Lobachevsky changes this Euclidean terminology. For a terminological convenience I shall call a given straight line *secant* of another given straight line when the two lines intersect (in a single point). Let's now make the required construction and listen what our intuition says about it. The whole point of the Problem of Parallels traditionally conceived is that the intuition says nothing definite as to whether AP is true or not. However it says us few other important things:

(i) Parallel lines exist (unlike round squares); moreover through a given point P outside a given straight line l passes *at least one* parallel line m. Such construction can be readily made on the basis of Euclid's Postulates without using P5, AP or their equivalents. Drop a perpendicular PS from P to l and then produce another perpendicular m to PS passing through P. The fact that m is parallel to l follows from the theorem about an external angle of triangle, which is a theorem of "absolute geometry", that is, it doesn't depend on P5 or its equivalents. In other words a line, which is lower than a given secant is also a secant. (fig.1, Note 10)

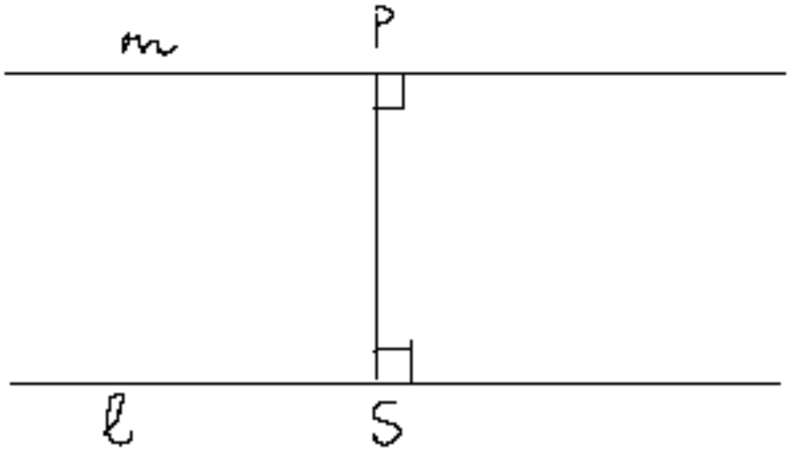


fig.1

(ii) Given a straight line and a point outside this line there exist secants of the given line passing through the given point. To construct a secant take any point of the given line and connect it to the given point outside this line.

(iii) Let *PS* be perpendicular to *l* and *A* be a point of *l*. Consider a straight line *PR* such that angle *SPR* makes a proper part of angle *SPA* (and hence is less than angle *SPA*). Given this I shall call line *PR* *lower* than line *PA* (and call *PA* *upper* than *PR*). Notice that this definition involves the perpendicular *PS*, and so depends on the choice of *P*. Then *PR* intersects *l* in some point *B*, i.e. it is a secant (fig.2, Note 11).

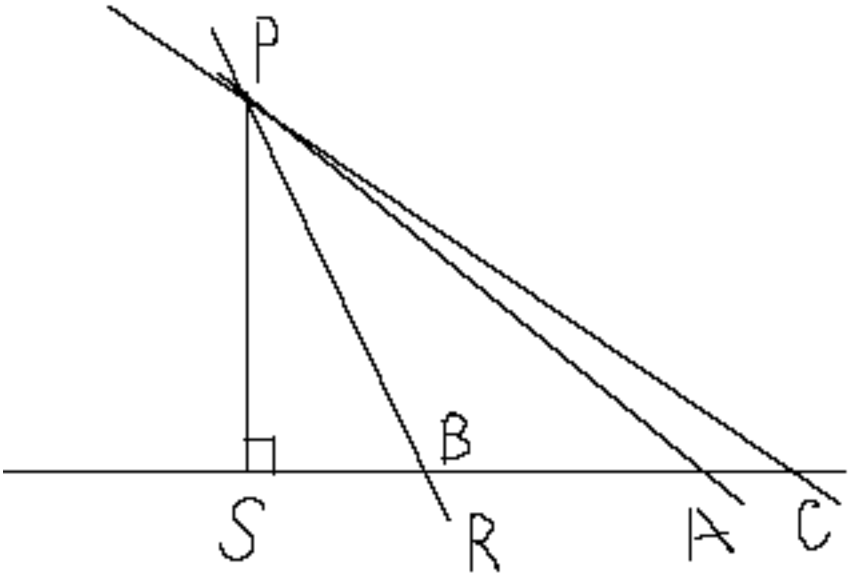


fig.2

(iv) There exist no upper bound for secants of a given line passing through a given point outside this given line. For given some secant *PA* one can always take a further point *C* such that *A* will lay between *S* and *C* and so secant *PC* be upper than the given secant *PA* (fig. 2).

(v) Let *m* be parallel to *l* , which is constructed as in (i). Let *n* be another parallel to *l* passing through the same point *P*. Suppose that *n* is lower than *m* (obviously this condition doesn't restrict the generality). Then any straight line which is upper than *n* and lower than *m* is also parallel to *l* (fig. 3)

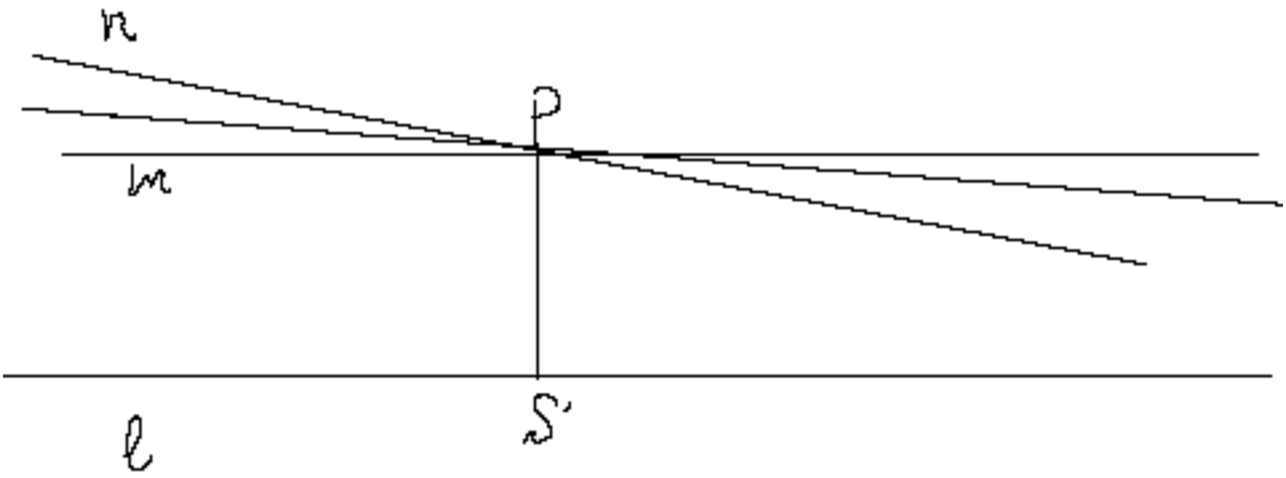


fig.3

(vi) Parallels to a given straight line passing through a given point have a lower bound. To assure it rigorously one needs some continuity principle like one asserting the existence of Dedekind cuts. Then (vi) follows from (iv). Lobachevsky doesn't states such a principle explicitly but endorses (vi) anyway.

(vii) Any straight line *PA* - a secant or a parallel - passing through point *P* as shown at fig. 2 is wholly characterised by its characteristic angle *SPA*. In particular this concerns the lowest parallel mentioned in (vi). Let the measure of *SPA* corresponding to the case of the lowest parallel be α. Now it is clear that by an appropriate choice of *l* and *P* one can make α as close to $\pi/2$ as one wishes. For given any angle *SPA* < $\pi/2$ it is always possible to drop perpendicular *AT* on *PS* (fig.4). Then *PA* is a secant of *AT* and so by (iii) all parallels to *AT* including its lowest parallels are upper than *PA*. Hence the value of α corresponding to straight line *AT* and point *P* outside this line is between *SPA* and $\pi/2$. Since the only variable parameter of the configuration is the distance *d* between the given straight line and the given point outside this line α is wholly determined by this distance.

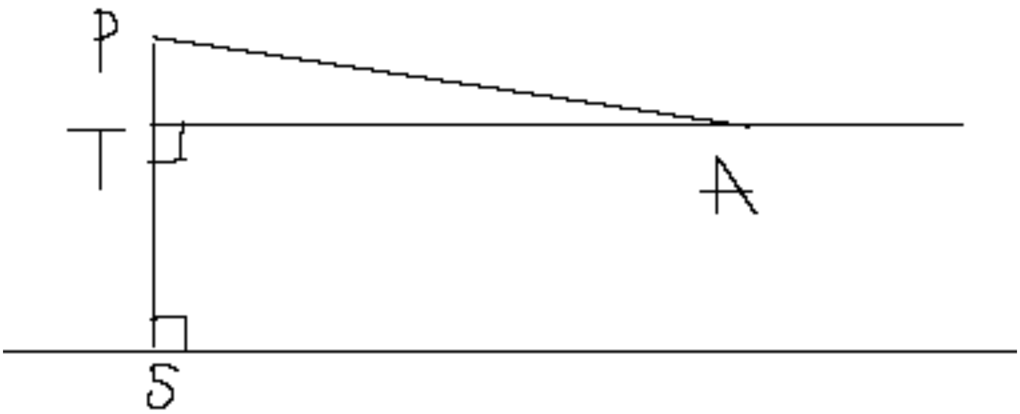


fig.4

(i - vii) provide the intuitive basis for Lobachevsky's *Imaginary* geometry (see STP, propositions 7, 16, 21). He proceeds as follows. First, he makes a terminological change: he calls "parallels" (not just non-intersecting straight lines but) the two boundary lines which separate secants from non-secants (i.e. parallels in the usual terminology) passing through a given point. So in Lobachevsky's terms there exist exactly two parallels to a given straight lines passing through a given point, which may eventually coincide if AP holds (i.e. in the Euclidean case). For further references I shall call these two parallels *right* and *left* (remembering that this assignment is purely conventional). From Lobachevsky's new definition of parallels doesn't immediately follow that parallels form equivalence classes; moreover the definition involves the choice of *P*. So Lobachevsky must show that the property of being parallel (in his new sense) to a given straight line is independent of this choice (STP, proposition 17), and that the relation of being parallel is symmetric and transitive (while reflexivity may be granted by the usual convention) (STP, propositions 18, 25). For the obvious reason transitivity may work here only for parallels of the same orientation, i.e. separately for right and for left parallels. Lobachevsky provides the required proofs making them in the traditional synthetic Euclidean-like manner (Note 12). Then Lobachevsky proves some further properties of parallels, in particular the fact that the angle α characterising a parallel (see vii above) can be made not only however close to $\pi/2$ but also however close to zero (STP, proposition 23). This immediately implies that if P5 doesn't hold then given an angle *ABC*, however small, there always exist a straight line *l* laying wholly inside this angle and intersecting none of its two sides (fig.6).

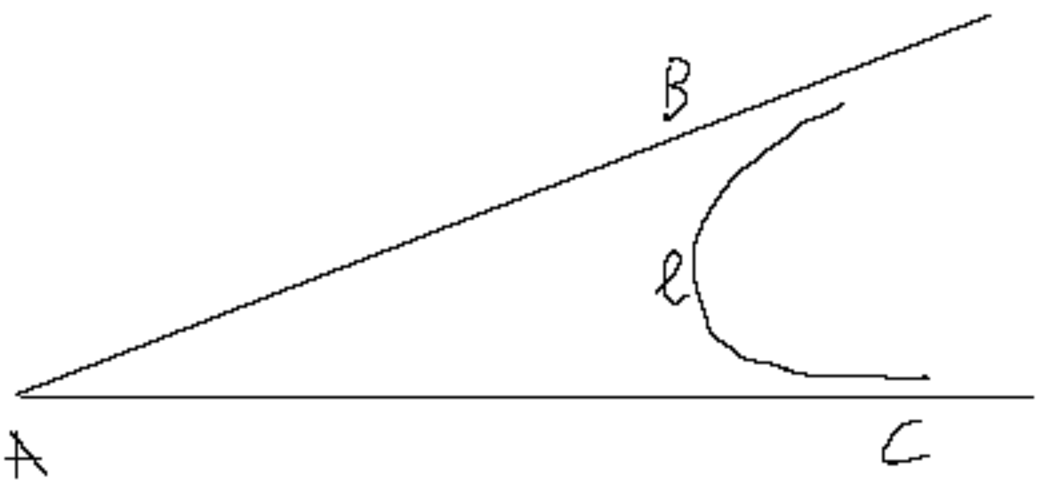


fig.6

This is already by far more counterintuitive than (i-vii) but still not counterintuitive enough to rule out this construction as absurd and on this ground to claim a proof of P5.

So far I have presented only basics of Lobachevsky's theory which were mostly known before Lobachevsky, noticeably to Sacceri and Lambert (Note 13). Nevertheless we are already in a position to make some important epistemological conclusions about Lobachevsky's work. First conclusion concerns the popular view according to which development of Non-Euclidean geometries required a departure from the spatial intuition in favour of a more abstract kind of thinking like one promoted in (Hilbert 1899) or perhaps some other. This view is typically supported by the claim that the common human spatial intuition is inherently Euclidean (Note 14). Lobachevsky's work clearly demonstrates that this view and its supporting claim are ungrounded. True, giving up P5 one enters into a rather unusual world. It looks unusual in eyes of anyone who studied geometry by Euclid's *Elements* or its later replacements, and particularly so if this study didn't involve any discussion of *difficulties* of Euclid's theory including the problem of P5. I claim that the "Euclidean intuition" which allegedly prevents one from conceiving of Lobachevsky's geometry intuitively can be nothing but a result of a very *superficial* study of *Elements* or its later replacements. Experts new about the Problem of Prallels since ancient times and kept their minds open. They didn't need anything like Hilbertian scheme to conceive of Lobachevskian geometry. For the unusual world of this geometry is intuitive just like Euclidean. True, it takes some time and training to feel at home in Lobachevskian spaces. However the required training aims at acquiring some *new* intuitive capacities, not at getting rid of intuition. What one should give up in order to conceive of Lobachevskian geometry intuitively is to give up P5 having a poor intuitive support. The failure of all the attempts to prove P5 on the basis of some intuitively evident statement leaving no room for any reasonable doubt, is perhaps the best evidence *against* the claim that the common spatial intuition is inherently Euclidean.

It is certainly true that after Hilbert one can develop Lobachevskian geometry in an abstract or "formal" way. But this is equally true about Euclidean geometry! Thus both geometries are neutral with respect to the choice between the traditional intuitive and the modern formal treatment. Although the controversy between the "formal" and the "intuitive" approaches to geometry didn't emerge before (Hilbert 1899) a similar controversy was already known in 19th century. I mean the controversy between "analytic" and "synthetic" ways of doing geometry. Here is what Lobachevsky says about it in the Preface to his NFG:

" In Mathematics people use two methods: analysis and synthesis. A specific instrument of analysis are equations, which serve here as the first basis of any judgement and which lead to all conclusions. Synthesis or the method of constructions involves representations immediately connected in our mind with our basic concepts.

<...> Science starts with a pure synthesis; all the rest is produced by jugement which derives new data from the first data given by synthesis and thus broadens our knowledge unlimitedly into all directions. Without any doubt the first data are always acquired in nature through our senses. Mind can and must reduce them to minimum, so they could serve as a solid foundation for science." (Note 15)

The fact that Lobachevsky stresses the importance of synthetic approaches in science in general and in geometry in particular shows that unlike some of his contemporaries he was not at all sympathetic to the idea of replacing intuitive geometrical reasoning by some sort of calculus. He rather believed that spatial intuition and spatial experience are ultimate sources of geometrical truths (Note 16) and that analytic methods serve only for "derivation of new data from first data". This shows that Greenberg's claim according to which the discovery of Non-Euclidean geometry "had a liberating effect on mathematics, who now feel free to invent any set of axioms they wish and deduce conclusions from them" has no historical support whatsoever as far as Lobachevsky is concerned.
A subtler point is this. It may be argued that since one admits that intuition cannot either justify or refute P5 (or any equivalent proposition) one cannot any longer count on intuition as a source of truthfulness of geometrical axioms (even if it can still continue to play some other role). Then, so the argument goes, one is doomed to accept some version of Greenberg's "ifthenism" and refuse from any "absolute" notion of truth in geometry.
I cannot see that the argument is valid. The fact that intuition cannot either justify or refute P5 can be reasonably understood in the sense that nor P5 neither its negation should be considered as plausible axioms. One doesn't need any drastic reconsideration of the role of intuition for it. Actually Lobachevsky never aimed at building a new geometrical theory, which could be taken with Euclidean geometry on equal footing. Like Boliay Lobachevsky aimed at a generalisation of the known geometry, which wouldn't assume dubious P5 or any other dubious principle equivalent to P5. This is how he describes his principle achievement in the *Introduction* to his NFG:

"The principle conclusion, to which I arrived .... was the possibility of Geometry in a broader sense than it has been [earlier] presented by Founder Euclid. This extended notion of this science [=of Geometry] I called *Imaginary Geometry*; *Usual* [=Euclidean] *Geometry* is included in it as a particular case."

To include Euclidean geometry as a special case of Lobachevsky's *Imaginary* geometry it is sufficient to take α in (vii) to be equal to $\pi/2$; in this case Lobachevsky's two parallels to a given straight line coincide. What remains problematic here is the nature of variation of α. (vi) says us nothing about the value of α except that it is positive but don't exceed $\pi/2$. Does this mean that one can stipulate by fiat any value of α from the given interval ? Geometry traditionally makes a sharp distinction between universally valid propositions (axioms and theorems) and particular constructions with their stipulated properties. One is free to build constructions with any desired properties as far as these constructions are doable with Euclid's Postulates or some other assumed constructive principles (Note 17). For example, one is free to produce a right angle, an acute angle or an obtuse angle depending on one's personal taste or specific purpose. But in this traditional setting one is *not* free to stipulate axioms and constructive principles (postulates) in a similar way. This, of course, provides essential constraints on possible choices. One may opt, for example, for constructing with Euclid's Postulates (but without P5) a triangle with a right angle. But then one has only a limited choice of possible values of the two other angles of the triangle: both of them *must* be acute since otherwise the construction is provably impossible. Let's now see how these basic rules of the game apply to the construction shown at fig.4 above.

According to (vii) both Lobachevsky's parallels passing through a given point P are uniquely determined by distance $x = PS$. This means that given line l and point P outside this line angle $APS = \alpha$ has some definite value and cannot be any longer a matter of stipulation. What we can do to learn this value? Since we have no better choice we can make some *hypothetical* reasoning about it. It can be proven that *if* $\alpha = \pi/2$ (the Euclidean case) then the same holds for any other choice of l and point P (compare STP, proposition 20). *If* $\alpha < \pi/2$ the situation becomes more complicated because, as we have already observed in (vii), in this case the value of α depends on distance x. Anticipating what follows I give here Lobachevsky's fundamental equation, which expresses this dependence:

$$\tan(\alpha/2) = a \exp(-x) \quad (1)$$

where a is a positive factor. How to interpret this formula? The factor makes a new trouble. On the one hand, it is clear that the unit used for measuring distance x can be always chosen in such a way that (1) takes this most convenient form:

$$\tan(\alpha/2) = e \exp(-x) \quad (1')$$

were e is the base of natural logarithms. Then α gets determined by x as expected. Lobachevsky makes this move in his STP saying only that it "simplifies calculations". But on the other hand, a different choice of unit brings a different value of α in each particular case. So it turns out that the value of α depends of our arbitrary choice of unit, which is supposed to be a matter of convention having no theoretical significance at all! Turning things the other way round one may also say that the unite of length is uniquely determined here by α, that is, that the usual liberty to choose the unit arbitrarily cannot be any longer granted. This is the way in which this situation was interpreted earlier by Lambert (Note 18).

We see that the traditional distinction between universally valid propositions, on the one hand, and arbitrary stipulations concerning particular constructions, on the other hand, in the context of Non-Euclidean geometry is blurred. The old rules of the game don't really apply to the new situation. One expects to have a definite value of α in the construction shown at fig.4 but can make only some hypothetic reasoning about it. But can Greenberg's "ifthenism" making no difference between axioms and mere hypotheses be indeed a remedy? Actually the ifthenist approach to the problem was known long before Hilbert and even before Lobachevsky. Sacceri and after him Lambert both began with the "absolute" geometry (based on Euclidean Axioms and Postulates except P5) and then considered separately the "hypothesis of right angle" equivalent to P5, the "hypothesis of acute angle" and the "hypothesis of obtuse angle" (Note 19). They ruled out the third hypothesis without using P5 but could not do the same with the second hypothesis. To develop two mutually incompatible systems of reasoning one of which grants the first hypothesis while the other grants the second hypothesis doesn't look like an interesting solution in this context even if one provides it with some supporting epistemological arguments. Notice that this move anyway doesn't help to treat the problem of "absolute unit of length" about which Lambert was already aware.

A solution, which later became standard, was found after Beltrami in his (1968-69) identified Lobachevskian spaces as Riemaninan manifolds of constant negative curvature; in this Riemannean setting Lambert's "absolute unit" could be then identified with the radius of curvature. Lobachevsky didn't have yet this solution in hands (Note 20) but he already had a basic idea according to which the smaller are distances the closer *Imaginary* geometry becomes to Euclidean. To see this consider, for example, the following diagram taken from a Schumacher's letter to Gauss (see Gauss 1981, v.8, p.213)

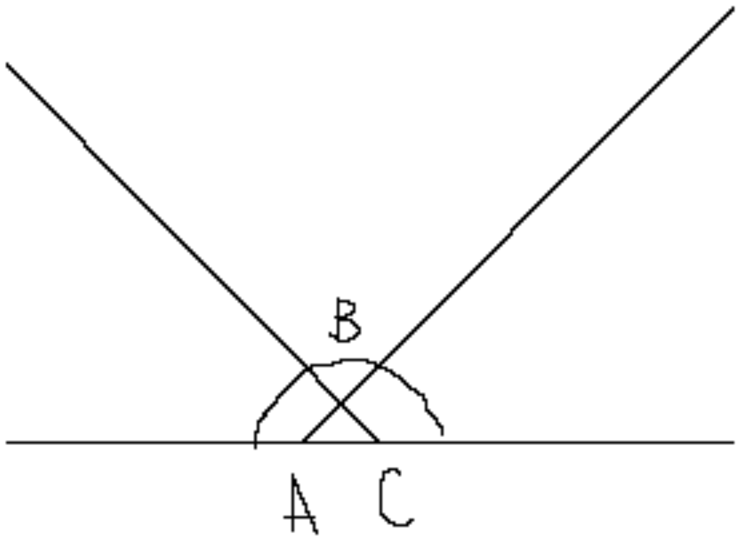


fig.7

It shows (or at least suggests) that if triangle *ABC* is infinitely small then the sum of its internal angles becomes infinitely close to two right angles (notice the semicircle). This fundamental observation gives a sense in which *one and the same* space can be Euclidean and Non-Euclidean at the same time: it can be Euclidean "in the small" and Non-Euclidean "in the large". A logically-minded reader can probably say that this is a sheer contradiction anyway. I shall not go here for a general discussion about logical aspects of infinitesimals but only remark that this logical difficulty is not specific to Non-Euclidean geometry. The aforementioned claim about geometrical spaces is no more contradictory than the claim that any smooth curve line is everywhere straight in the small. This kind of "smooth thinking" was fundamental for the whole of mathematics and physics of 18-19 century. Importantly this view allowed for retaining the older notion of intuition as a truth-maker. The very idea that geometrical properties could depend of "absolute" distances (as distinguished from ratios of distances) was, of course, unusual but it was certainly not counter-intuitive. It allowed for considering the old Euclidean intuition as a "local approximation" of a larger (but not "alternative") geometrical intuition. Thus Greenberg's view that the invention of Non-Euclidean geometry destroyed the older notion of intuition as a truth-maker and left the "inthenism" about mathematical matters as the only available alternative is certainly wrong both historically and theoretically. Although some moderate form of ifthenism can be indeed attributed to Sacceri, Lambert and perhaps to some other geometers it didn't play any significant role in the history of geometry before Hilbert. In particular, Lobachevsky never went for it.

At the same time, it is my impression that Lobachevsky simply didn't have any accomplished theory about how Euclidean and Non-Euclidean geometry relate to each other. In spite of his official view on *Imaginary* geometry as generalisation of Euclidean geometry Lobachevsky often informally speaks about the two geometries as if they were incompatible. Consider, for example this interesting passage from the *Introduction* to NFG where Lobachevsky puts forward a view according to which geometry of physical space is determined by "natural forces":

[T]he assumption according to which some natural forces follow one Geometry while some other forces follow some other specific Geometry, which is their proper Geometry, cannot bring any contradiction into our mind.

Thus it is not completely unreasonable, of course, to look at Lobachevsky as a predecessor of Hilbert, who unlike Hilbert didn't have yet a clear idea about how different geometries can live together. But it is not unreasonable either to say that Lobachevsky anticipated *another* idea of how geometries can live together, namely one based on Riemann's ideas. How the two ideas interacted in history is an interesting question which I cannot touch upon here. But in the last section of this paper I shall provide a sketch of a today's approach to building geometrical theories, which develops the older Riemannean way of reasoning up to the point where it becomes compeatable with Hilbertean scheme.
Taking into consideration what has been told so far about Lobachevsky's work one may come to conclusion that the question "Did Lobachevsky have a model of his geometry?" has no more sense than the question whether or not Euclid has a model of his geometry. Hilbert's scheme described in the beginning of this paper seems to have no more relevance to Lobachevsky than to Euclid. We shall now see that in fact the question allows for a more specific and more interesting answer.

4) Hyperbolic calculus

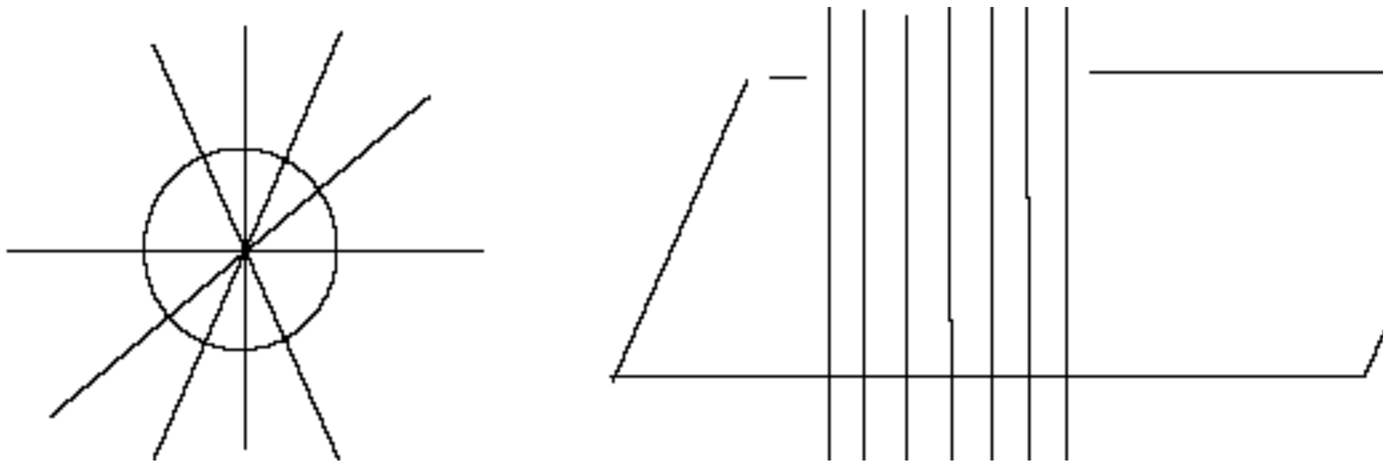
To see that the notion of model is not totally irrelevant to Lobachevsky's work consider the following quote from FG:

"The geometry on the limiting sphere is exactly the same as on the plane. Limiting circles stand for straight lines while angles between planes of these circles stand for angles between straight lines."

Even without knowing the exact sense of Lobachevsky's terms (which I shall shortly explain) one can see here a basic element of Hilbertean scheme, namely the notion that usual geometrical terms like "straight line" and "angle between straight lines" can stand for something else than they usually stand for without producing any essential change in the corresponding theory (in this case - Euclidean geometry). Speaking in today's terms Lobachevsky describes here a non-standard model of Euclidean plane. Why not a model of his new Hyperbolic geometry as the today's reader would most probably expect? Why Lobachevsky translates convenient notions of Euclidean geometry into a new language of Hyperbolic geometry rather than the other way round? Let me now explain why and how.

Basic facts about Hyperbolic geometry proven by synthetic methods and mentioned in the previous section were mostly known before Lobachevsky. However it was Lobachevsky who first publicly claimed the *invention* of Non-Euclidean geometry while earlier workers in the field remained reserved. Actually Lobachevsky's conviction that he indeed invented a new geometry, or more precisely found a far-reaching generalisation of the old geometry, was not without a reason: he first managed to supply a system of synthetic reasoning described in the previous section with an appropriate analytic apparatus. Lobachevsky uses his non-standard model of Euclidean plane for developing this analytic apparatus. I shall now briefly explain how it works referring the reader to STP for further details.

In Euclidean geometry there are two kinds of sheaves of straight lines: (a) sheaves of parallel lines and (b) sheaves of lines passing through the same point. Given a sheaf of either sort consider a line (or surface in 3D case) normal to each line of the given sheaf. So you get (a) either a straight line (plane in 3D case) or (b) a circle (sphere in 3D case) (fig.8 a, b)



a b

fig. 8

In Lobachevskian (Hyperbolic) geometry both configurations shown at fig.7 a, b exist although lines at fig.7b are not parallels in Lobachevsky's sense. But in addition one gets a new specific sort of sheaf, namely that of Lobachevsky's parallels. Correspondingly one gets a new normal line and a new normal surface, which

Lobachevsky calls *limiting circle* (or otherwise *horocircle*) and *limiting sphere* (otherwise *horosphere*) (fig.9):

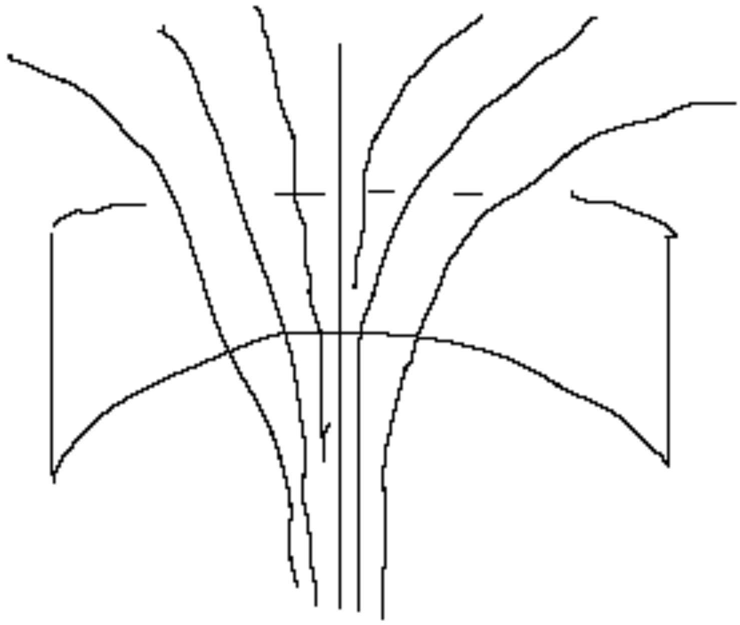


fig.9

To see that horocircles on given horosphere verify AP (and in fact the rest of axioms of Euclidean geometry) observe the following. Call (as usual) a given straight line l parallel to a given plane α just in case l is parallel (in Lobachevsky's sense) to its orthogonal projection m onto α. It can be then easily shown by usual synthetic methods (I leave it as an exercise) that given l and α as before there exist a unique plane β having no common point with α (that is, parallel to α in the usual sense) such that l lays in β. This lemma, which resembles AP in a way, doesn't depend on AP. Notice that any horocircle laying on a given horosphere can be obtained as an intersection of the horosphere with a plane parallel to the sheaf of Lobachevsky's parallels corresponding to this horosphere. This immediately implies that the non-standard interpretation of Euclidean geometry suggested by Lobachevsky verifies AP (the horocircles are called here parallel in the usual Euclidean sense of having no shared point) (fig. 10):

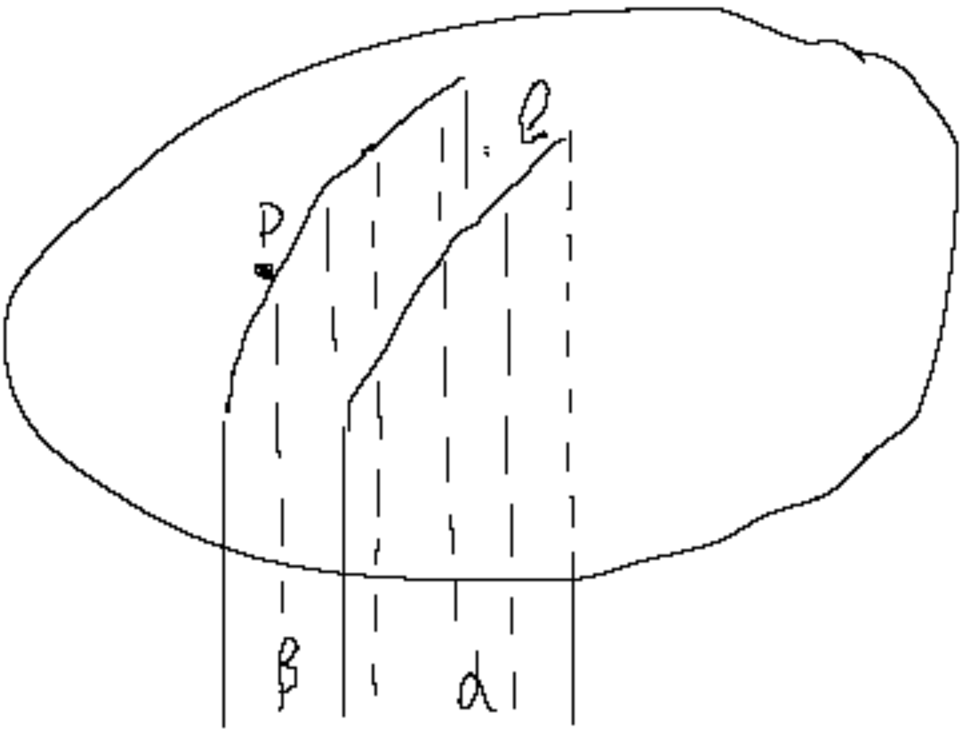


fig.10

Lobachevsky himself uses a slightly different lemma (STP, proposition 28) for the same purpose . Let me quote only his conclusion (STP, proposition 34), which shows more precisely the way in which Lobachevsky anticipated Hilbertean scheme:

"On the limiting surface [i.e. on the horosphere] sides and angles of triangles hold the same relations as in the Usual [i.e. Euclidean] geometry".

This crucial observation allowed Lobachevsky to develop (what we call today) Hyperbolic trigonometry on the basis of the usual (Euclidean) trigonometry. He writes down basics of this new calculus in the form of four (eqational) identites (STP, proposition 37, formula 8). In FG and NFG Lobachevsky applies this calculus to a large class of geometrical problems and in AIG - to calculation of certain integrals, which earlier were not given any geometrical sense. On the top of that Lobachevsky puts forward in FG the following general argument purporting to show that the new calculus guarantees consistency of his *Imaginary* geometry:

"[1] As far as we are found the equations which represent relations between sides and angles of triangle ... Geometry turns into Analytics, where calculations are necessarily coherent and one cannot discover anything what is not already present in the basic equations. [2] It is then impossible to arrive at contradiction, which would oblige us to refute first principles, unless this contradiction is hidden in those basic equations themselves. [3] But one observes that the replacement of sides *a*, *b*, *c* by *ai*, *bi*, *ci* turn these [basic hyperbolic] equations into equations of Spherical Trigonometry. [4] Since relations between lines in the Usual [i.e. Euclidean] and Spherical geometry are always the same, [5] the new [i.e. *Imaginary*] geometry and [Hyperbolic] Trigonometry will be always in accordance with each other." (FG , *i* stands here for the square root from minus one)

Let's analyse this complicated argument step by step. First ([1]) Lobachevsky claims that trigonometric relations valid for an arbitrary triangle allow one to translate the whole of geometry from synthetic to analytic language. He takes this claim for granted in case of Euclidean geometry and then says that it equally holds in a more general case of *Imaginary* geometry. In [2] Lobachevsky apparently assumes that algebraic transformations are better controllable than synthetic constructive procedures. The transparency of the " analytic" procedures guarantees that *if* basic equations contain no hidden contradiction) so does the rest. One cannot claim the same for constructive synthetic procedures since such procedures can bring a contradiction at any step of reasoning but not only at the initial step of laying out basic principles. So the analytic means help to reduce the question about consistency of *Imaginary* geometry to that concerning only foundations ("basic equations") of this geometry. [3] is a crucial observation (first made by Lambert), which allows for a profound analogy between Spherical and Hyperbolic geometries. Lobachevsky didn't understand the precise sense of this analogy but rather took it as purely formal (see again Note 20). His argument, as far as I understand it, is the following. Spherical geometry (including spherical trigonometry) is a well-established part of Euclidean geometry ([4]) and so there is no reason to expect any contradiction in it. The two parts of Spherical geometry - synthetic and analytic - match each other just like in case of Plane Euclidean geometry. Hence Spherical trigonometry is consistent. Since the formal substitution *a*-->*ai* turns every equation of Spherical trigonometry into an equation of Hyperbolic trigonometry Hyperbolic trigonometry is consistent if Spherical trigonometry is consistent. Hence Hyperbolic trigonometry is consistent. But ([5]) the match between the analytic part of *Imaginary* geometry (that is, Hyperbolic trigonometry) and the synthetic part of *Imaginary* geometry can be assured just like in Spherical case. Hence *Imaginary* geometry (including its synthetic part) is consistent in general. The line of the argument can be pictured with the following diagram (fig.11):

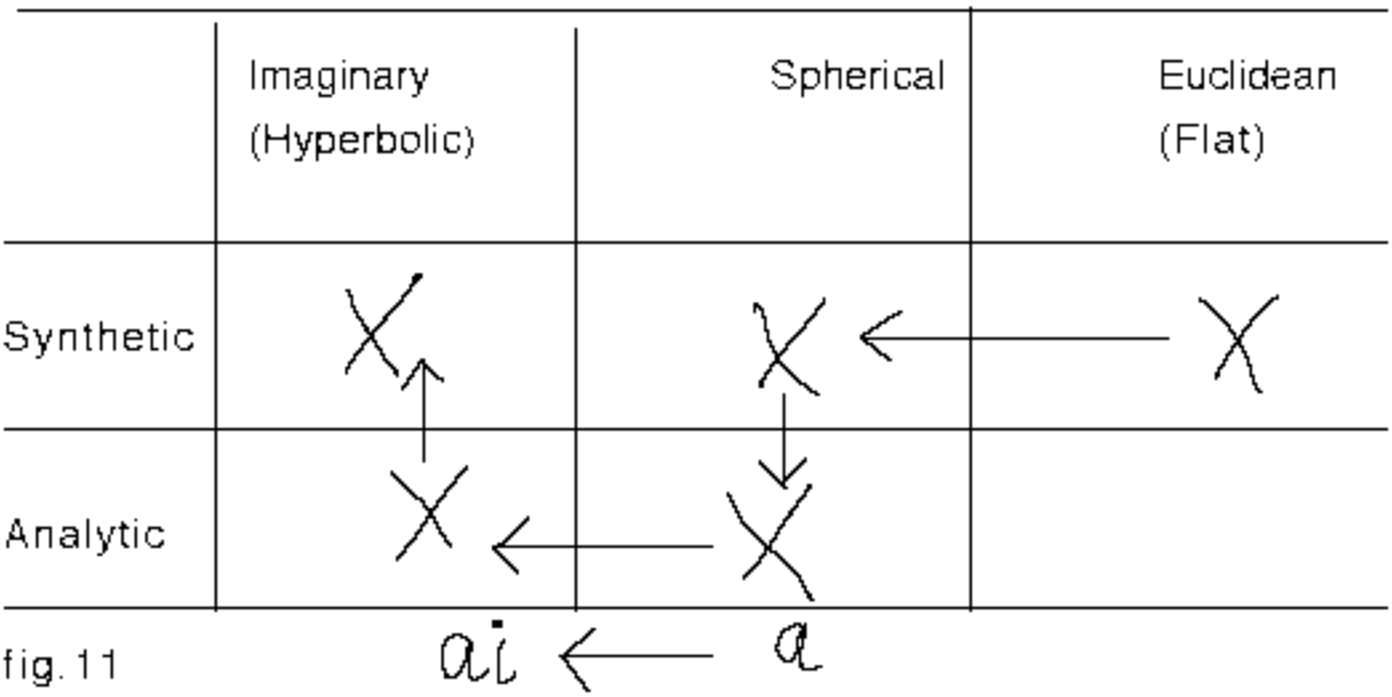


fig.11

The match between synthetic and analytic parts guarantees that if one of them is consistent then so is the other. This works both in Hyperbolic and Spherical cases. The formal substitution *a-->ai* allowing for the switch between the two cases on their analytic sides supposedly preserves consistency. The consistency of Spherical geometry is guaranteed by the fact that this geometry makes part of Euclidean geometry (actually of Euclidean stereometry). Thus the consistency of Hyperbolic geometry is ultimately implied by that of Euclidean geometry.

It is tempting to see in this Lobachevsky's argument a proof of relative consistency in Hilbert's sense. Even if such reading is not unreasonable one should keep in mind that, first, this argument is in fact very vaguely formulated and, second, it is produced by Lobachevsky at the absence of any genuine understanding of what is behind the formal correspondence between trigonometric identities in Spherical and Hyperbolic cases. The main source of Lobachevsky's ambiguity here is the lack of any proper distinction between *Imaginary* (Hyperbolic), Spherical and *Usual* (Euclidean) geometries. The context strongly suggests considering them on equal footing as we do it today. But remind that Lobachevsky also considers the *Usual* geometry as a special case of *Imaginary* and Spherical geometry as a part of *Usual*. At least the latter assumption is essential for the argument. When Lobachevsky says that "relations between lines in the Usual and Spherical geometry are always the same" he, in my understanding, looks at a given sphere as an Euclidean object but not intrinsically. Thus he doesn't think about it as a *model* in anything like today's sense. The *formal* character of substitution *a-->ai* is obviously explained by Lobachevsky's lack of understanding of what is behind it. Lobachevsky like Lambert simply noticed the striking analogy between trigonometric identities valid in his *Imaginary* geometry and well-known trigonometric identities for spherical triangles. The analogy suggested considering Plane *Imaginary* geometry as a sort of Spherical geometry on a sphere of *imaginary* radius, for example, of radius equal to *i*. Prima facie this didn't make any geometrical sense. But the analogy also suggested that the new trigonometric calculus could work just as well as Spherical trigonometry, and this in its turn suggested that the synthetic reasoning behind this new calculus was also correct. This is the core of the above argument. But obviously the analogy noticed first by Lambert and later by Lobachevsky was calling for explanation. It is nothing but an irony of history that Lobachevsky's eventual "formalism", which was due to the lack of understanding of one particular mathematical question, could be later seen as an anticipation of Hilbert's deliberate formalism based on serious epistmological considerations.

5) Rethinking Hilbertian Scheme with Lobachevsky

We have seen that although Lobachevsky had some elements of Hilbertian scheme at his disposal he was quite strongly attached to the traditional way of geometrical thinking. Given the historical distance between Lobachevsky and Hilbert this is hardly very surprising. What is more surprising is the unusual way in which Lobachevsky uses these elements of Hilbertian scheme. Remind that Hilbertian scheme comes with the idea according to which models serve (among other purposes) for providing an intuitive support to abstract (formal) theories. But Lobachevsky's non-standard model of the Euclidean plane does *not* serve this purpose. This author has an intuitive support both for Euclidean and his *Imaginary* geometry to begin with and use his non-standard model for building a system of calculus. Remind also that in order to build a theory in a Hilbertain setting one needs first to take a suitable metatheory for granted. But Lobachevsky proceeds in the opposite epistemological order. He naturally grants first Euclidean geometry (in the sense that he assumes its consistency), not his *Imaginary* geometry. But then he uses the *latter* as a metatheory for the former but not the other way round! He builds a new model of well-known Euclidean Planimetry by means of a new non-accomplished theory and this helps him to accomplish the theory in question. This technically clever trick from Hilbertian viewpoint looks weird.
Although we have found in Lobachevsky something strongly resembling the notion of model relevant to Hilbertian scheme the epistemological background behind this scheme doesn't square well with Lobachevsky's work. We didn't find in Lobachevsky any trace of the ifthenist attitude described by Greenberg in the above quote or any particular tendency toward an abstract or formal thinking (as opposed to concrete

intuitive thinking). One obvious moral about this is the need to read historical textes carefully and avoid their unjustified anachronistic interpretations. But another moral is the need of keeping mind open to eventual revisions of currently accepted views about mathematics. I shall now show how one can make a better sense of Lobachevsky's work than just saying that this author anticipated certain elements of Hilbertian scheme but didn't know how to put them rightly together. Elsewhere (Rodin, forthcoming) I presented a general theoretical argument aming to show that Hilbertian scheme is unsatisfactory. Here I summarize and illustrate the argument using Lobachevsky's work as an appropriate historical example. I shall start with a more precise analysis of tensions between Hilbertian scheme and Lobachevsky's work.
Hilbertian scheme introduces a twofold relativity in mathematical (and more generally - in theoretical) thinking. Using Fregean terminology I shall call the first kind of relativity *relativity of sense* and the second kind - *relativity of reference* or *semantical relativity* (Note 21). The relativity of sense amounts to saying that meaning of basic theoretical terms like "plane", "curve", "between", etc. are context-dependent, i.e. that the terms mean different things in different theories (for example, in Euclidean and in Lobachevskian geometries). This equally applies to derived terms like "triangle", which are defined through basic ones. Semantical relativity in its turn has to do with the assumption according to which mathematical terms just like words in natural languages not only *mean* something (i.e. have a sense) but also *refer* to something. Within Hilbertian scheme *reference* of mathematical terms is obtained through *interpretation* of a given formal theory, i.e. through picking up one (or more) of its *models*. These features of Hilbertian scheme support an anti-essentialist view about mathematical objects according to which basic geometrical notions like that of point or straight line have no mathematical content outside a given theory (and this content always changes when one changes a theory).
The semantical account just given involves an obvious infinite regress since to get a model M for a given theory T one needs to repeat the whole reasoning once again, namely, build another theory T' and specify its model M' which can allow for building the desired model M of T. The usual way to stop this regress is this: one assignes to pair (T', M'') a special epistemic status of *metatheory*, which implies that in the given context M' and T' are taken for granted.
Since Lobachevsky recognised horocircles on a horosphere in a hyperbolic space as Euclidean straight lines one may argue that Lobachevsky already well understood Hilbertian semantical relativity. However this argument is not conclusive as we shall now see. To show this I shall not try to reveal Lobachevsky's hidden assumptions but rather suggest an alternative understanding of his explicite geometrical construction, which will allow us to avoid the absurd conclusion that Lobachevsky used his Imaginary

geometry as a metatheory for developing Euclidean geometry in a unusual way.
A Hilbertian analysis of the Lobachevsky's finding goes like this. Since Euclidean plane and straight lines on this plane can be equally represented by the usual intuitive concepts (supported by traditional drawings) and by a horosphere with horocircles on it the two representation share in common a conceptual core of these notions and in addition have certain specific features, which are theoretically superfluous. This conceptual core can be identified by Hilbertian scheme (using formal relations between relata of "any nature" as a shared *form* (rather than *essence*) of the two things, then abstracted away and given the name of Euclidean *structure*. I claim that this analysis is inconclusive (Note 22) because the two "representations" cannot and shouldn't be taken on equal footing. For on the traditional "standard" side we have here a pure geometrical intuition (whatever this might mean) while on the other side we have a well-elaborated theoretical construct, namely a horosphere, which doesn't make sense outside its ambient hyperbolic space and without its supporting theory of Hyperbolic geometry. I don't believe one can apply to such different things the same notion of abstraction and then talk about about a shared structure allegedly specified by this abstraction. In fact Hilbertian scheme suggests in this case two different procedures. In the case of traditional (standard) representation it suggests simply to give up the usual adherence to intuition and re-orient geometrical thinking toward a more formal mode. But in the case of a non-standard representation it suggests something different: to leave a theory supporting this representation out of consideration by providing it with a special epistemic status of metatheory. Even if this move is supposed to be only temporal it is generally not justified. As we have seen in Lobachevsky's case it leads to a sheer epistemic absurdity.
The above argument becomes particularly clear if one looks at the situation from a Riemanian viewpoint. Euclidan plan E2 and Hyperbolic 3-space H3 are (or at least can be readily conceived of as) Riemanian manifolds. But a horosphere is not! A horosphere is not a manifold of its own but an *embedding* E2-->H3 of one manifold into the other. For *intrinsically* a horosphere is indistinguishable from E2. This is why the intrinsic viewpoint is inappropriate when on talks about a horosphere meaning a specific surface living in Hyperbolic space. In a Riemanian perspective a horosphere cannot be seen as an embodiment of the concept of Euclidean plane standing on equal footing with the "usual" embodiment of this concept because the notion of horosphere doesn't make sense outside a Hyperbolic space and a Hyperbolic space is a manifold of its own, which cannot be left out of consideration in the given context. This shows that the Hilbertian way of thinking about this situation, which amounts to "carving out" a given horosphere from its ambient space and forgetting about its supporting theory (by calling it "metatheory"), is seriously misleading.

I shall use the above Riemanian view on the problem for introducing some general notions important for what follows. Embedding E2-->H3 is a *map* between the two manifolds. It is an *irreversible* map, i.e. it is not an *isomorphism.* In structuralist terms this observation is tantamount to saying that E2 and H3 support different structures. However this map can be also viewed as a *partial* isomorphism between the whole of E2 and a part of H3, namely a horosphere. As I have just argued this latter view is misleading even if not plainly wrong because the separation of parts of H3 (or of any other space) changes properties of these parts dramatically as it happens when one looks at a horosphere intrinsically and it "turns into" an Euclidean plane. However this misleading view is essential for making sense of the notion of Euclidean *structure* in the given example. For given an *isomorphism A<-->B* (e.g. isomorphism between E2 and a horosphere) one can think about objects *A*,*B* "up to isomorphism" and replace both by a new abstract or "formal" object *C* (e.g. Euclidean structure). With a general map (in particular, with an embedding) *A-->B* one cannot do anything similar. To show this I shall use Frege's account of abstraction given in his (1884). Consider a class of individuals with an equivalence relation on it. Frege's abstraction amounts, roughly, to replacement of each equivalence subclass by a particular abstract object (Note 23). The existence of isomorphism is an equivalence relation, so reasoning "up to isomorphism" (at least in simple cases like our) can be understood in terms of Frege's abstraction. But the existence of a general map *A-->B* is *not* an equivalence (since it is not symmetric) and so Frege's abstraction doesn't apply . A general map *A-->B* doesn't make objects *A*,*B* in any reasonable sense the same and doesn't allow for replacement of both by some new object *C*. (Note 24)

Hilbertian scheme and Mathematical Structuralism are usually opposed to various kinds of *essentialism* about mathematical objects. But the alternative approach I am thinking of is different. While Structuralism comes with the slogan "think up to isomorphism" (Note 25) this alternative approach amounts to thinking "up to general morphism" (where *morphism* is another word for *map*). This analogy shouldn't be taken too literally because the mere existence of general morphism between two objects doesn't provide any sense in which these objects could be thought of as the same. Thinking about mathematical objects "up to general morphism" amounts to conceiving of them through their mutual maps. The idea is not a new one. In his (1925) von Neuman first made an attempt of building an axiomatic set theory taking the notion of function (i.e. map between sets) as primitive. Later this idea has been realised in a different setting by Lawvere in (1964) who used for it *Category theory*, which is a general theory of maps (called in this theory *morphisms*). A similar approach (also based on Category theory) to Riemanian geometry brought about *Synthetic Differential* geometry. Instead of making assumptions about how a general manifold looks like one begins here with describing a *category* of manifolds, which comprises the class of "all" manifolds with all available maps (but not only isomorphisms) between them. For this end one first assumes that manifolds form a category (i.e. a class of objects with composable maps between them) and then makes appropriate additional assumption, which specify the given category as the category of Riemanian manifolds (Note 26). For further details and references see (Bell 2005, chapter 10) and for further philosophical discussion see my (2007). Here I shall only show how this categorical approach allows for making a better sense of Lobachevsky's work.

As we have seen Lobachevsky certainly understood how Euclidean Planimetry can be *translated* into terms of his *Imaginary* , i.e. Hyperbolic, geometry. This allowed him to provide Hyperbolic geometry with a powerful analytic apparatus. But he didn't need anything like Hilbert's notion of formal theory for this purpose. Categorical approach to theory-building sketched above takes this notion of translation (under the name of morphism) as primitive and purports to (re)construct further mathematical concepts out of it. So it makes it less surprising that a particular map found by Lobachevsky turns to be a key for the whole of his theory.

In 19th century people learnt to think about complex numbers as points on Euclidean plane, exchange points for straight lines and vice versa in Projective geometry (projective duality) and make other earlier unknown translations between different parts of mathematics. Hilbert's notion of formal theory and the later notion of formal structure provided a general epistemological account of these findings. But it was in fact only a limited and insufficient account, which didn't fully grasp their potential. For the Hilbertian account has been based on a strong assumption according to which all "good" translations are isomorphisms. When Jordan in 1870 first distinguished between isomorphisms and homomorphisms in Group theory he conceived of the latter as partial isomorphisms (Note 27). However historical examples like Lobachevsky's suggest to abandon this way of thinking about morphisms and treat different kinds of morphisms on equal footing to begin with. Instead of looking at Lobachevsky as one of Hilbert's predecessors and asking how much of the content of (Hilbert 1899) was already present in Lobachevsky's works one should rather recognise that Lobachevsky's project was essentially different and in certain respects more promising.

6) Conclusion

We have seen that Lobachevsky's writings provide no support for the canonical story about early days of Non-Euclidean geometry, which has been produced in order to comply Hilbertian views on mathematics with the earlier history of this discipline. In particular we have found in Lobachevsky no tendency towards a retreat from spatial intuition and spatial experience and no trace of ifthenism. We have also seen that the often repeated claim according to which Non-Euclidean geometry cannot be intuitively conceived in principle is nothing but a philosophical myth designed for supporting a particular view on mathematics. Non-Euclidean geometry extends Euclidean intuition but doesn't throw it away. At least this is a *possible* way to understand it. However important Hilbert's formalist views about mathematics might be (both historically and theoretically) they should not be taken for granted when one makes a historical research.

A more specific conclusion concerns the notion of map and its historical genesis. In today's mathematics this notion has many faces and subsumes in particular, older notions of function and geometrical transformation. The face relevant to the present discussion concerns the notion of map as "translation" from one geometrical framework into another. The idea that familiar geometrical concepts like that of straight line can be rendered in unusual ways (so a straight line in a different framework can "become" a curve) is usually associated with Hilbert's fundamental text (1899), his notions of axiomatic theory and its interpretation, and later developments including Model theory. However, as we have seen, Lobachevsky had the idea of map but conceived of it in a way very different from Hilbert's.

To claim that Lobachevsky anticipated Category theory would be even more absurd than claim that he anticipated Hilbert's Formal axiomatic method. However there is nothing absurd in analysing Lobachevsky's finding in terms of maps rather than in terms of models understood in Hilbertian sense. A careful reading of historical mathematical texts as ever provides a lot of material for reflection on today's hot topics. Lobachevsky's recovering of Euclidean plane in Hyperbolic space through a horosphere allows us for a better understanding of where today's notion of map comes from. Such historical understanding seems me necessary for further development of this new mathematical concept.

Thus the straightforward answer to the question whether or not Lobachevsky had a model of his geometry is that the question is ill-posed. I opted nevertheless for taking it seriously for two reasons. First, because this question naturally arises within the standard view on history of geometry of 19th century. So the question provided me a good opportunity to suggest a revision of this standard view. Second, because this straightforward answer doesn't imply that Lobachevsky's work has nothing to do with today's notion of model. As we have seen Lobachevsky certainly grasped a basic idea behind this notion, namely the possibility to represent objects and relations belonging to a given mathematical theory by objects and relations of another theory. But as I have argued in this paper one can make a better sense of Lobachevsky's work than claim that he was one of Hilbert's predecessors. It is far more interesting, in my view, to find in Lobachevsky and other older writers grains of some conceptual possibilities, which hasn't been yet fully explored, and then try to develop them further. Instead of looking at Lobachevsky's way of translating between different theories as an early incomplete grasp of a later notion of model I suggested a different approach, which takes this notion of "incomplete" translation seriously and puts it into foundations of geometry. Then as it so often happens in history apparent shortcomings of an older work become to look as strokes of genius.

Endnotes:

Note 0:
The shortened titles of Lobachevsky's works used in this paper are explained in the Bibliography, section A, where the reader can find the full references.

Note 1:
Consider, for example, this account provided by Wikipedia (entry "Non-Euclidean Geometry" as for February 20, 2008 http://en.wikipedia.org/wiki/Non-Euclidean_geometry):
"Even after the work of Lobachevsky, Gauss, and Bolyai, the question remained: does such a model exist for hyperbolic geometry? The model for hyperbolic geometry was answered by Eugenio Beltrami, in 1868, who first showed that a surface called the pseudosphere has the appropriate curvature to model a portion of hyperbolic space, and in a second paper in the same year, defined the Klein model, the Poincaré disk model, and the Poincaré half-plane model which model the entirety of hyperbolic space...."
A very different and, in my view, much more satisfactory treatment of this question has been given in 2007 by Roberto Torretti in another public internet resource: Stanford Encyclopedia of Philosophy: plato.stanford.edu/entries/geometry-19th/

Note 2:
(Greenberg 1974) is a geometry textbook of college level containing some historical and philosophical material. It may be argued that it is not appropriate to take historical and philosophical claims contained in this book too seriously and criticise them thoroughly. I disagree. Such books written for younger students often make explicit certain assumptions about history and philosophy of mathematics, which in more serious studies are often taken for granted or hidden behind further details. The fact that on Greenberg's account these assumptions are oversimplified and perhaps even partly confused is a price to be paid for its compactness. Since my aim here is to

reconsider basics rather than elaborate on details (Greenberg 1974) serves me as a perfect reference.

Note 3.
Beware that we are talking about the pure mathematics here, not about a study of physical space. The scheme of theory-building in question usually comes with a strong epistemological thesis according to which these two things are quite distinct and the former has a priority over the latter in the following sense : one may reasonably ask whether a given mathematical construction correctly describes physical phenomena but one is not allowed to use physical arguments in pure mathematics.

Note 4
My presentation of Hilbertian scheme is slightly anachronistic with respect to Hilbert (1899) and based on a later distinction between syntax and semantics. I opt for this anachronism here because my aim here is to make explicit a fairly standard today's understanding of the matter rather than to analyse its genesis.

Note 5:
This is, of course, a strong and highly controversial metaphysical claim, which can be challenged, in particular, via a reference to changing entities: a changing entity *A* has some property *P* before a change and doesn't have this property after the change. This problem is usually (but in my view mistakenly) seen as irrelevant to mathematics since, as people often believe, mathematical objects cannot change.

Note 6:
I provide some further details about Hilbertian scheme in section 5 of the main text.

Note 7:
I don't claim here that Greenberg's "formalist viewpoint" is indeed shared by the majority of living working mathematicians. Actually I think that this is not the case. Nevertheless Hilbertian scheme (liberally understood) remains a fairly standard "official" framework for doing mathematics, and in this sense its identification with "modern mathematics" is justified.

Note 8:
This choice of references needs a justification. I provide it here beginning with a brief description of Lobachevsky's works in Geometry. G is a geometry textbook published only after the author's death, which contains no material related to Non-Euclidean

geometry. FG and NFG are two author's attempts to write a fundamental geometrical treatise covering the whole of the discipline from its foundations to its special chapters. Lobachevsky's project of rebuilding foundations of geometry developed in these two works doesn't reduce to what became known as Lobachevskean geometry but also includes some other new ideas which I cannot discuss here. IG and AIG have, on the contrary, a more limited task of presentation of a new analytic apparatus related to Lobachevskian geometry (the hyperbolic trigonometry) and demonstration of its power. Lobachevsky introduces here this apparatus "by hand" reducing its geometrical background to minimum. STP is another shortened account of the basics of Lobachevskean geometry, which, however, is theoretically complete: it begins with synthetics geometrical considerations and proceeds to analytic methods. STP doesn't cover some more specific issues (like calculation of areas and volumes) treated in FG and NFG but unlike IG includes the foundational synthetic part. PG is the last overview of Lobachevskean geometry written by the author; it is less systematic than STP and fixes some minor technical problems, which Lobachevsky found in STP after its publication. This description makes it clear that STP is the best compact systematic presentation of the topic written by Lobachevsky himself. Importantly Lobachevsky's notion of "Imaginary geometry" remains quite stable across all of these works. This allows me not to refer to any particular period of his work in the present general discussion. English translation of STP is available and referred to in the Bibliography below.

Note 9:
AP is also known under the name of *Playfair's Axiom*.

Note 10:
For suppose that *m* and *l* intersect in *A.* Then the external angle *RPA* is equal to the internal angle *PSA.* This contradicts the theorem about an external angle which implies that *RPA* must be strictly superior to *PSA*.

Note 11:
To prove (iii) rigorously one needs Pasch's axiom which Lobachevsky never mentions but always tacitly takes for granted. This axiom first introduced in (Pasch 1882) says this:

Given a triangle and a straight line intersecting one of the triangle's sides but passing through none of the triangle's apexes the given line intersects one of the two other sides of the given triangle.

To apply this axiom to the given case one needs a simple additional construction, which I leave to the reader. Remind that my point here concerns common intuition but not rigorous proofs: whatever improvement on (iii) can be possibly made it remains intuitively evident.

Note 12:
Consider, for example, the first half of Lobachevsky's proof of the fact that his definition of parallel doesn't depend of the initial choice of point P of this parallel. Let $m = PA$ be the right parallel to the given straight line l passing through the given point P outside l , PS be the perpendicular dropped from P to l, P' be any other point of m laying to the right from P, and finally $P'S'$ be the perpendicular dropped from P' to l (fig. 5). We now show that given any point B inside angle $AP'S'$ line $P'B$ intersects l at certain point D. (This will mean that m is right parallel to l with respect to P'). To see this draw line PB. Since B lays inside angle APS and m is right parallel to l with respect to P line PB intersects l at certain point C. Then draw line $P'B$, denote by E the point of intersection of $P'S'$ and PC, and consider triangle $ES'C$. $P'C$ intersects side EC of this triangle in B and doesn't intersect ES' by the choice of B. Hence $P'B$ intersects the third side $S'C$ (and hence line l) in some point D.

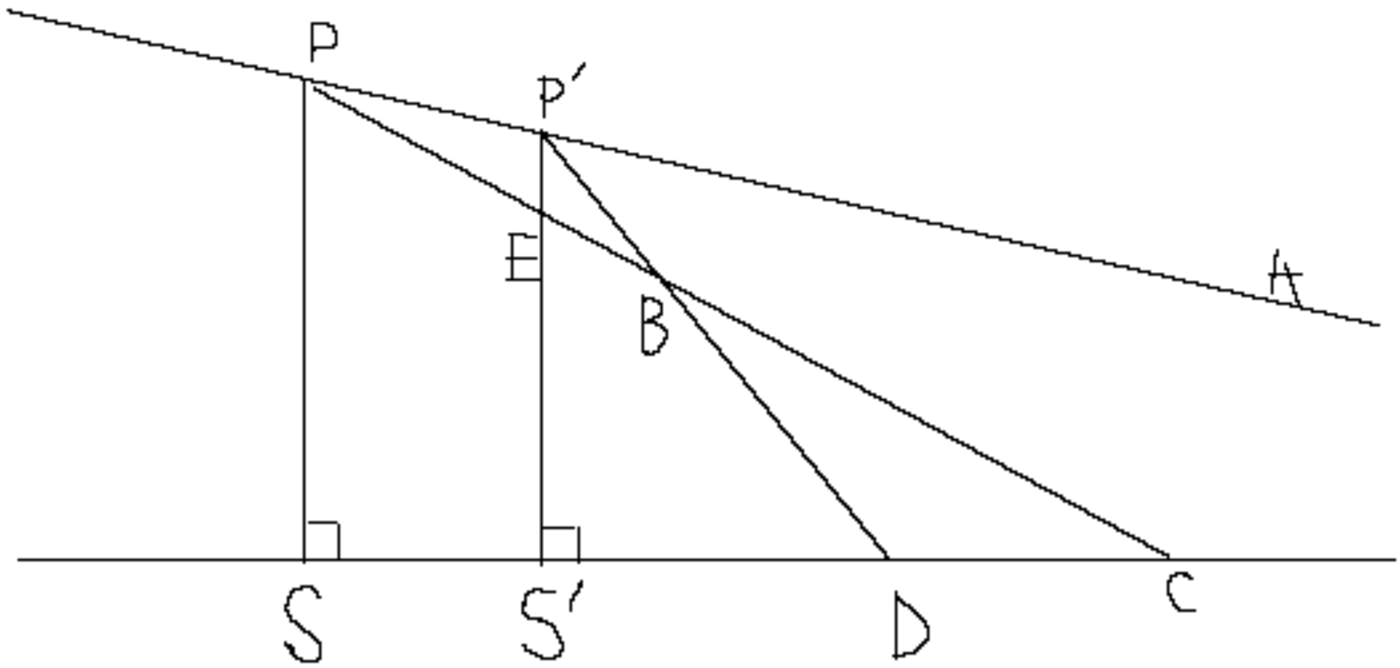


fig. 5

The last step requires Pasch's axiom, see Note 10 above. The other half of the proof corresponding to the case when P' lays to the left of P I leave as an exercise to the reader.

Note 13:
see (Bonola 1955)

Note 14:
In recent philosophical discussions this claim is often identified as Kant's view. For the obvious historical reason Kant never distinguished between Euclidean and Non-Euclidean geometries in his writings but always refers to geometry *tout court*. The only reason to identify geometry mentioned by Kant as Euclidean in today's technical sense of the term is Kant's choice of examples: in particular, in his Critique of Pure Reason (A713/B741) Kant refers to Euclid's proof of the fact that the sum of internal angles of a given triangle is equal to two right angles. If Kant would learn more about the Problem of Parallels (from Lambert or otherwise) he would likely change his examples without changing principle arguments.

Note 15:
Hereafter translations of Lobachevsky's passages from Russian are mine.

Note 16:
Lobachevsky doesn't distinguish clearly between geometrical truth and truth about physical space, even if some of his passages suggest such a distinction. Given his empiricists view on geometry this is hardly surprising.

Note 17:
I have here in mind Euclid's distinction between Axioms and Postulates, which I explain in the end of section 4 of this paper. The question whether a construction with some desired properties is doable or not often have no trivial answer.

Note 18:
See Bonola (1955). Wallis (1616-1703) first realised that P5 is equivalent to any proposition, which grants the existence of similar triangles. Lambert re-interpreted this result in terms of the possibility of *scaling* , i.e. the free choice of the unit of length. In my view there is a sense in which these authors *resolved* the Problem of Parallels. For the scaling property of geometrical spaces can be justified on epistemological and pragmatic grounds and then be adopted as a plausible axiom or postulate. This certainly makes sense if geometry is seen as a theory supporting measuring practices rather than an a priori science of space. It is moreover remarkable that having at hands what looked like a genuine proof of P5 (based on a

new plausible principle) Lambert didn't rule out the possibility of Non-Euclidean geometries.

Note 19:

See (Bonola 1955). Sacceri constructs a quadrilateral with three right angles and then conciders separetely cases when the fourth angle is right, acute and obtuse. Hence the three hypotheses. The third hypothesis can be ruled out without the use of P5 but the second cannot.

Note 20:

In the same year of 1840 when Lobachevsky published his STP Ferdinand Minding (1806-1885) published in the *Crelle Journal* a note (Minding 1840), where he showed that trigonometrical formulae for triangles formed by geodesics on surfaces of constant negative curvature can be obtained from trigonometrical formulae for spherical triangles by replacement of usual trigonometric functions by hyperbolic functions. Lobachevsky in STP makes a similar observation about straight lines of his geometry (see section 3 below in the main text). A communication between the two authors would most probably lead to the discovery made by Beltrami later in 1868! However Lobachevsky apparently didn't read this Minding's paper in spite of the fact that the library of Kazan University had this issue the *Crelle Journal*. At least the preserved list of books and journals borrowed by Lobachevsky from the University Library doesn't include this reference (see □Ł□Ł□□ººŁ□ et al. 1974).

Note 21:

See (Frege 1892). Notice that the two kind of relativity I discuss below build upon the more general *denotational* relativity which allows for replacement of any sign having certain sense and certain reference by another sign.

Note 22:

I don't mean, of course, that there is a simple error in this reasoning. After all it grounded a strategy of mathematical research - I mean the structuralist program broadly conceived - which proved successful in 20th century and brought about a great part of the bulk of today's mathematical knowledge. It is my view, which I cannot fully expose and defend in this paper, that today the structuralist strategy is mostly expired. See my 2007 for further discussion. My critical arguments against mathematical structuralism don't challenge its historical importance.

Note 23:
Frege's example: the equivalence relation of being parallel between Euclidean straight lines and the notion of *direction* abstracted away by the described method. Frege's notion of abstraction allows for more refined interpretations than one just given in the main text but this have no impact on the following argument.

Note 24:
Let me be more precise. The bare existence of morphism between two given objects *A*,*B* (i.e. existence of morphism of the form *A*-->*B* or of the form *B*-->*A*) *is* an equivalence relation (let's call this relation R1). However the existence of morphism *from A to B* (i.e. existence of morphism of the form *A*-->*B*) for a given ordered pair *A*,*B* is not (let's call this latter relation R2). Beware that R1 is *not* the relation of isomorphism (since morphisms in question are, generally, not isomorphisms). Call now objects *A*, *B* *morphic* just in case they hold R1 and then observe that *all* sets, groups and topological spaces are correspondingly morphic (obviously objects are morphic only if they belong to the same category). This shows that the idea of "thinking up to general morphism" by identifying morphic objects like structuralists do this with isomorphic objects is sheerly absurd: R1 is too weak to replace the relation of isomorphism in any reasonable way. R2, which takes the sense of morphisms into account, is stronger but it is no longer an equivalence relation, and so it gives no new meaning to the expression "morphic objects" (which suggests a splitting of a given class of objects into equivalence sub-classes of morphic objects). This shows that one cannot use R2 either in anything like the same way people use the relation of isomorphism in a Hilbertian setting.

Note 25:
Preparing his (1899) Hilbert thought about classes of isomorphic constructions (later called *models*) and their shared forms (structures). He thought that in this way one may distinguish between what is theoretically significant (the form) from what is not (specific features of particular models). Hence his idea of formal mathematics. The fact that formal theories, generally, have also so-called *non-standard* models, which are *not* isomorphic to *standard* models (i.e. the models one begins the Fregean abstraction with) Hilbert first realised and first treated only in his (1900); in the second and later editions of his (1899) Hilbert added a new controversial axiom (Vollstandigkeitsaxiom) supposed to treat the same problem. The property of an axiom system, which consists of the fact that all of its models are isomorphic was called by Veblen in his (1904) *categoricity*. Since then the pursuit of categoricity became a part of programs of formalisation of mathematics through Hilbertian

scheme. It has been realised that the requirement of categoricity cannot be generally met without a price, which in many important cases, like in axiomatic Set theories, turns to be quite high.

Note 26:
A category of manifolds can be also obtained through construing manifolds and their maps by some other methods and *then* putting all these things together. The approach I'm talking about is clearly different.

Note 27:
In Book 2, section 67 of his (1870) Jordan distinguishes between *isomorphisme holoédrique* (literally "complete isomorphism), which is isomorphism in today's sense, and *isomorphisme mériédrique* (literally "partial isomorphism") called today *homomorphism* (see Jordan 1957 and Kline 1972).

Bibliography:

A) Principle source:

Лобачевский Н. И. (Lobachevsky N. I.) 1949-
Полное соб. сочинений п/ ред. В. Ф. Кагана, Москва

This Russian complete edition of Lobachevsky's works contains the following works in Geometry (listed below in the chronological order by dates of their creation or first publication):

1823: *Geometry* (G)

1829-30: *Foundations of Geometry* (FG)

1835-37: *Imaginary Geometry* (IG) (1935 - Russian version and 1937 - the author's French version; the two version are slightly different in their content).

1835-38: *New Foundations of Geometry* (NFG)

1836: *Application of Imaginary Geometry to some Integrals* (AIG)

1840: *Studies in Theory of Parallels* (STP)

1855-56: *Pangeometry* (PG)

First publication of French version of IG:
Lobachevsky, N. I., 1837. "Géométrie imaginaire," : 295-320. *Journal für die reine und angewandte Mathematik* 17

First publication of German version of STP:
Lobachevsky, N. I., 1840. . Berlin: F. Fincke. *Geometrische Untersuchungen zur Theorie der Parallellinien*

English translation of STP by G. B. Halsted is printed as a supplement to (Bonola 1955.)

B) other literature:
Beltrami, E., 1868, "Saggio di interpetrazione della geometria non-euclidea", *Giornale di Matematiche*, v.6, p. 284-312

Beltrami, E., 1868-69, "Teoria fondamentale degli spazii di curvatura constante", *Annali di matematica pura et applicata* (2), 2, p. 232-255

Bonola, R.,1955, *Non-Euclidean Geometry*, Dover Publications, Inc., New York

Frege, G., 1884 (or later editions), *Grundlagen der Arithmetik*, Wilhelm Koebner, Breslau

Frege, G., 1892, "Über Sinn und Bedeutung", *Zeitschrift für Philosophie und philosophische Kritik, NF 100* , p. 25–50.

Gauss, C.F., 1981, *Werke* (herausgegeben von der Königlichen Gesellschaft der Wissenschaften zu Göttingen), Olms v.8,

Greenberg, M.J., 1974, *Euclidean and Non-Euclidean Geometries*, W.H. Freeman and Company, San-Francisco

Hilbert, D. (1899 or later editions), *Grundlagen der Geometrie*, Teubner, Leipzig

Jordan, C., 1870, *Traité des Substitutions et des équations algébriques*, Gauthier-Villars

Kline, M., 1972, *Mathematical Thought From Ancient to Modern Times*, Oxford University Press

Lawvere, W., 1964, "Elementary Theory of the Category of Sets", *Proceedings of the National Academy of Science*, vol. 52, N6, p.1506-1511

Minding E.-F.-A.,1840, "Bemerkung ueber die Abwicklung krummer Linien von Flaechen", *Journal de Crelle*, Berlin, 6, p.159-161

Neumann J. von, 1925, "Ein Axiomatisierung der Mengenlehre", *Journal fiir die reine und angewandte Mathematik*, 154.

Pash, M., 1882, *Vorlesungen ueber neuere Geometrie*, Teubner-Verlag

Rodin, A., 2007, *On Categorical Theory-Building: Beyond the Formal* , arXiv: 0707.3745

Кирилукин А.Г., Лаптев Б.Л. 1974
Что читал Лобачевский? Казань