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Lobachevsky: Some Anticipations of Later Views on the Relation between Geometry and Physics

By *Norman Daniels**

I

CONTEMPORARY DISCUSSIONS of the relationship between geometry and physics sometimes begin with a brief nod of respect in the direction of Carl Friedrich Gauss or Nicolai Ivanovich Lobachevsky. The respect is usually limited to crediting them with the first awareness that the geometry of space is an empirical question, to be determined by actual measurements.[1] Carnap, Reichenbach, and Hempel all mention the legend that Gauss measured a triangle of mountain tops to see if there was any deviation from 180°.[2] Similarly, Aleksandrov, Kolmogorov, and Lavrent'ev quote Lobachevsky's remark that the geometry of space will have to be "verified like other physical laws by experiments, such as astronomical observations."[3] And Max Jammer's classic *Concepts of Space* notes that Lobachevsky tried to use parallax measurements

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[1] Thomas Reid, who discovered the non-Euclidean (doubly elliptical) "geometry of visibles" in 1764, seemed unaware of the important implications of his discovery for science. See Norman Daniels, "Thomas Reid's Discovery of a Non-Euclidean Geometry," *Philosophy of Science*, 1972, *39*:219–234; also Norman Daniels, *Thomas Reid's 'Inquiry': The Geometry of Visibles and the Case for Realism* (New York: Lenox Hill, 1974), Ch. 1.

[2] Rudolf Carnap, *Philosophical Foundations of Physics: An Introduction to the Philosophy of Science*, Martin Gardner, ed. (New York: Basic Books, 1966), p. 135. Hans Reichenbach, *Rise of Scientific Philosophy* (Berkeley: University of California Press, 1957), pp. 129 ff. Carl Hempel, "Geometry and Empirical Science," in Herbert Feigl and Wilfred Sellars, *Readings in Philosophical Analysis* (New York: Appleton-Century-Crofts, 1949), p. 246.

The legend, however, appears to be the result of a confusion: Gauss' use of such geodetic data was for a different purpose and had nothing to do with testing the Euclidicity of space. See Arthur Miller, "The Myth of Gauss' Experiment on the Euclidean Nature of Physical Space," *Isis*, 1972, *63*: 345–348.

[3] A. D. Aleksandrov, A. N. Kolmogorov, M. A. Lavrent'ev, eds., *Mathematics: Its Content, Methods, and Meaning*, S. H. Gould and T. Bartha, trans. (Cambridge, Mass.: MIT Press, 1963), Vol. III, p. 101.

of the star Sirius to test for the true structure of space and suggests (falsely, I believe) that Lobachevsky considered the matter settled in favor of Euclidean geometry.[4] Even V. F. Kagan's fine study of Lobachevsky's contribution to science stops short of a full discussion of his views on the relation between geometry and physics.[5]

In the brief remarks that follow—which do not purport to be a comprehensive study of Lobachevsky's views—I shall argue that Lobachevsky deserves more than such a cursory nod. I shall try to show that he thought deeply about the implications for physics of his discovery of non-Euclidean geometry and anticipated key questions that are found in later discussions. In particular I shall take up in turn his remarks on the following points: (1) the difficulty of determining the geometry of space simply through astronomical measurement; (2) the need to withhold our intuitive judgments about the likelihood of the new geometry being true of physical space until we develop a completely new physics—through integrating the new, non-Euclidean geometry into a modified mechanics; and (3) the possibility that if it were developed, alternative physics might be radically different from what is known, even involving more than one geometry at the same time.

II

In a striking remark Lobachevsky stated his overall perspective on the consequences of his discovery:

> It is well known that in geometry the theory of parallels has so far remained incomplete. The futile efforts from Euclid's time on throughout two thousand years have compelled me to suspect that the concepts themselves do not contain the truth which we have wished to prove, but that it can only be verified like all other physical laws by experiment, such as astronomical observations.[6]

Lobachevsky does not here say whether or not he feels all geometrical propositions, including those independent of the parallels postulates, are also to be treated as physical laws. Nevertheless, verification by experiment is now considered necessary for some propositions previously thought to have been true *a priori*. Lobachevsky appears to be slightly more firm than Gauss was in calling certain geometrical theorems "physical laws," though they are in basic agreement on the point.[7]

[4] Max Jammer, *Concepts of Space: The History and Theories of Space in Physics* (2nd ed., Cambridge, Mass.: Harvard University Press, 1969), p. 150.

[5] V. F. Kagan, *Lobachevsky and His Contribution to Science* (Moscow: Foreign Languages Publishing House, 1957), pp. 82 ff. See n. 20 below.

[6] N. I. Lobachevsky, *Zwei geometrische Abhandlungen: Ueber die Anfangsgrüde der Geometrie (1829)* and *Neue Anfangsgründe der Geometrie (1835)*, Friedrich Engel, trans. (Leipzig: B. G. Teubner, 1898), p. 67.

[7] In a letter to Wilhelm Olbers (Apr. 28, 1817) Gauss comments,

> I come even more to the conviction that the necessity of our geometry cannot be proven, at least by human understanding for human comprehension. Perhaps we shall come in another life to other insights into the essence of space, which are now unrealizable for us. Until then,

It might be noted, incidentally, that Lobachevsky (and Gauss and Bolyai) had in mind an obvious, intended interpretation of the geometrical sentences used in his arguments, namely, the same physical interpretation normally associated with Euclidean geometry. That is why he was so quick to think that the discovery of a new geometry meant that there were new physical laws to be tested.[8] The tendency in some twentieth-century writings to construe Lobachevsky's work in a more formalistic manner which emphasizes that he negated the parallels postulate and used it in rigorous proofs, as one might an "uninterpreted sentence," is not incompatible (whether or not it clarifies what was done) with my remark that there was an intended interpretation. Further, my claiming there was an intended interpretation is also compatible with claiming the parallels postulate and its negation will contain nonlogical terms with the same meaning (on some plausible theory of meaning).

Lobachevsky, apparently unlike Gauss, actually attempted to analyze some existing astronomical measurements in order to test which geometry best accounted for the data. The chief test was to determine if there were serious deviations from 180° in the sum of the angles of a straight-lined triangle. To this end, parallax measurements for several stars are evaluated in "Ueber die Anfangsgründe," Lobachevsky's first published work on the new geometry. He concluded that in a triangle whose vertices are the sun, the earth, and a fixed star (Sirius), the angle sum cannot differ from two right angles by more than 0.00000372″.[9] Since his calculations show a deviation of well under one hundredth of a second, less than the experimental accuracy of protractors of the period,[10] Lobachevsky concluded "the exactitude [of the customary geometry] is very far reaching."[11]

Lobachevsky was well aware, however, that there were problems facing the use of such measurements. He remarks, in commenting on the kind of experiment just described, "one must prefer a triangle whose sides are very

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one must place geometry not with arithmetic, which stands purely *a priori,* but rather somewhat in the same rank with mechanics.

Gauss does not explain whether or not his whimsical remark means that mechanics too could be known *a priori* "in another life," or whether it means that arithmetic could be of the same rank as mechanics in yet another, or whether it means that some sciences can be known in different ways in different "lives" but that others are known in only one way in all "lives." Carl Friedrich Gauss, *Werke* (Gottingen: B. G. Teubner, 1966), Vol. VIII, p. 187.

[8] Discovering a non-Euclidean geometry does not automatically lead to the thought that there are new physical laws to be tested. Thomas Reid (see n. 1 above) saw no such implication. For Reid, Euclidean geometry is synthetic *a priori* for tactual space and the geometry of visibles is synthetic *a priori* for visual space.

[9] Lobachevsky, *Zwei geometrische Abhandlungen,* p. 23. The data on which Lobachevsky made his calculations were in error, however, since the parallax of Sirius is 0.40″, less than a third of the 1.24″ Lobachevsky thought it was (see Jammer, *Concepts of Space,* p. 150). Lobachevsky in "Neue Anfangsgründe" refers mistakenly to his own calculations in "Ueber die Anfangsgründe." He says that the maximum deviation is less than 0.000372″ rather than his earlier figure of 0.00000372″ (Lobachevsky, *Zwei geometrische Abhandlungen,* p. 78). Kagan seems to miss this error and also cites Lobachevsky as having calculated (erroneously) a maximum deviation of 0.000372″. (Kagan, *Lobachevsky,* p. 44.)

[10] Kagan, *Lobachevsky,* p. 42.

[11] N. I. Lobachevsky, *Geometrical Researches on the Theory of Parallels* (1840), G. B. Halsted, trans. (Austin: University of Texas Press, 1891), p. 44.

large, since according to pangeometry, the difference between two right angles and the sum of a straight-lined triangle will be proportionally greater the greater the sides are."[12] But this raises a serious problem: how do we know that the distance from the earth to the sun or to Sirius is sufficiently large to reveal any deviations from Euclidicity? After all, "nature itself points out distances to us compared with which even the distance from the earth to the fixed stars disappears to insignificance."[13]

Consequently, in spite of the apparent accuracy and adequacy of Euclidean geometry for current practical purposes, Lobachevsky thinks it still remains

> . . . impossible to assert that the assumption that the measurement of lines is independent from the angles, an assumption which many geometers will have taken as an absolute truth which requires no proof, that this assumption could prove itself perceptibly false possibly even before we step over the bounds of the visible universe.[14]

A few years later, in "Neue Anfangsgründe," Lobachevsky continues to argue that the apparent accuracy of Euclidean geometry can mean one of two things, "either that this system is found in Nature by chance, or else that in it all distances accessible to us are still infinitesimal."[15] So, Lobachevsky seems to leave the question open—in fact suggesting that it may never be settled as long as we fail to detect significant deviations from Euclidicity and as long as we have no way of knowing whether we are measuring a significantly large area of space.

III

The fact that existing astronomical observations appear to establish the "far-reaching exactitude" of Euclidean geometry does not, then, settle the question, since we have no way of knowing how significant such measurements are. Nevertheless, Lobachevsky was moved for reasons other than the calculations he made to hypothesize that his new geometry was not likely to be true for physical space. Immediately following his discussion in "Ueber die Anfangsgründe" of the inconclusiveness of existing calculations, Lobachevsky suggests that "*we are not in a position to conceive what kind of relation of things could*

[12] N. I. Lobachevsky, *Pangéométrie* (1856), Heinrich Liebman, trans., Ostwald's Klassiker der exacten Wissenschaften (Leipzig: Wilhelm Engelmann, 1902), p. 76.

[13] Lobachevsky, *Zwei geometrische Abhandlungen*, p. 24.

Gauss was aware of the same problem. In a letter to Taurinus (Nov. 8, 1824) Gauss remarks: "Should the non-Euclidean geometry be the true one, and *that constant proportionality be of such magnitude that it lies in the region of our measurements on earth or in the heavens,* then it can be ascertained *a priori*." (Emphasis added.) The "constant proportionality" Gauss refers to is the "absolute unit" (a function of curvature) mentioned later in Sec. III above. Gauss is saying we can detect the difference between Euclidean geometry and his new geometry only if we happen to be in a space of sufficiently great curvature or, in his terms, if the absolute unit is sufficiently small (*Werke*, p. 187).

[14] Lobachevsky, *Zwei geometrische Abhandlungen*, p. 24. Jammer ignores this passage, which is from the very same page he quotes when he suggests that Lobachevsky more or less unqualifiedly accepted Euclidean geometry as true (see n. 9 above).

[15] *Ibid.*, p. 77.

hold in nature that would bind such different magnitudes as lines and angles. Therefore, it is very probable that the Euclidean theorems are alone true, although this may remain forever unproven."[16] It was not his calculations but the counter-intuitiveness of features of the new geometry that led Lobachevsky to this conclusion.

If Lobachevsky had let this hypothesis stand unqualified, he would have been yielding, though at a different level, to the kind of argument that had prevented earlier researchers from recognizing the consistency of the new geometry—that is, to an argument based on the counter-intuitive features of the new geometry. Earlier researchers, for example, were particularly upset by the notion that there could be an *absolute* unit of length. Johann Heinrich Lambert first noted in his *Theorie der Parallellinien* (1766) that a geometry derived from the "hypothesis of the acute angle"[17] would have such an absolute unit of length. In ordinary Euclidean geometry the measurement of *lines* is clearly "relative" in the following sense: there are no functions which permit the expression of the length of a segment in terms of certain figures, like straight lines, planes, or pencils, which are taken to be fundamental. On the other hand, we can measure an *angle* in Euclidean geometry by considering its ratio to a complete revolution (the entire pencil), and so angle measurement is "absolute." On the geometry following from the hypothesis of the acute angle Lambert was able to associate a definite angle with every line segment. An appropriate function of the associated angles gives us the needed properties for measuring lines, and in particular we can take as the "absolute" unit of length the segment for which this function takes the value 1. Because Lambert thought such an absolute measure was intuitively impossible,[18] he was inclined to reject the hypothesis of the acute angle, although he never felt he could really demonstrate a contradiction.[19]

[16] *Ibid.*, p. 24; emphasis added.

[17] As his basic construction in investigating the theory of parallels, Lambert used a three-right-angled quadrilateral and made three hypotheses about the nature of the fourth angle. In this *reductio* strategy the goal was to eliminate the obtuse angle hypothesis (equivalent to Riemannian geometry, except for the problem of infinitude) and the acute angle hypothesis (Lobachevskian), leaving only the right angle (Euclidean) hypothesis. The obtuse angle hypothesis was thought to be eliminable because proofs made no distinction between infinite and unbounded planes. For fuller discussion see Roberto Bonola, *Non-Euclidean Geometry: A Critical and Historical Study of Its Development,* H. S. Carslaw, trans. (Chicago: Open Court, 1912), pp. 44–50.

[18] The intuition in question might equivalently be expressed in this way: "it is intuitive that such things as similar triangles exist."

[19] *Ibid.* When Gauss rediscovered this feature of the new geometry, it struck him and his contemporaries as extremely counter-intuitive, as is shown in this fragment from H. C. Shumacher's notebook from Nov. 1808: "Gauss has brought back the theory of parallels to the point that, if the received theory should not be true, then there must be a given line of a fixed length *a priori,* which is absurd. Still he does not yet himself consider this work sufficient" (Gauss, *Werke,* p. 165). Gauss probably never agreed to Schumacher's assessment that the notion of an absolute unit of length is absurd, and he certainly did not agree in 1816: "It appears somewhat paradoxical that there could possibly be a constant line as it were *a priori;* I find, however, nothing contradictory in it" (*ibid.,* p. 169). Consequently Gauss refused to mistake "something appearing unnatural to us with absolute impossibility" in spite of the reassuring "nonsense of the metaphysicians," for he believed we actually know "too little or nothing at all about the true nature of space" (*ibid.,* p. 187).

Lobachevsky certainly did not confuse impossibility or absurdity with counter-intuitiveness and warned others not to think his new geometry was logically inconsistent simply because its features were counter-intuitive. But he does, as we have just seen, try to motivate a *probability* judgment (that Euclidean propositions are alone likely to be true) by appeal to what we can readily comprehend, or, in other words, by appeal to our intuitions. Spelled out more fully, the argument might be something like this: Euclidean geometry (E) is likely to be true and "imaginary" geometry (L) is likely to be false, since L requires a dependency of lines and angles in nature. Such a dependency in theory would require other relationships to hold in nature—relationships which we have not experienced and whose nature we cannot even imagine. The argument rests on a suppressed premise to the effect that a relationship between two things is more likely to exist in nature if we can imagine what it is like than if we cannot.

Lobachevsky himself questions the soundness of this argument. In doing so he withdraws the probability judgment he at first proposed in favor of Euclidean geometry. In a startlingly modern analysis he suggests that such arguments, based on existing intuitions, are not sound because the kinds of intuitions we come to have and to live with depend on what theories we develop and consider to be live alternatives. As early as 1829, in his "Ueber die Anfangsgründe," Lobachevsky remarks:

> It remains for future investigations to determine what kind of modifications will result through the introduction of imaginary geometry into mechanics and whether or not here already recognized and indubitable conceptions about the nature of things are on hand which necessarily circumscribe for us the dependence of lines and angles or do not permit it at all. One can predict that the modification in mechanics resulting from the new elements of geometry will be of the same type as that which Laplace has achieved, since he assumed that dependence of velocity on force. . . .[20]

The mention of Laplace is intended to show us that progress in scientific theories often involves linking things in nature which prior to the development of the new theory could hardly have been any more "intuitive" or comprehensible or expected than linking lines and angles is in Lobachevskian geometry.

This example is more explicitly discussed in "Neue Anfangsgründe." Lobachevsky argues that our standard (Newtonian) physics shows us that many things are linked in nature, such as velocity and force or velocity and distance, which have no more conceptual link to each other and are no more intuitively linked before the emergence of the relevant theory than are lines and angles.

[20] Lobachevsky, *Zwei geometrische Abhandlungen,* p. 66. The only reference I have been able to find that even alludes to Lobachevsky's awareness of the importance of integrating the new geometry into a modified mechanics is Kagan, *Lobachevsky,* pp. 28 ff. Unfortunately, Kagan does not discuss Lobachevsky's views on the subject but only suggests that later researchers who tried to see if the new geometry led to serious problems in mechanics were following a program suggested by Lobachevsky's *discovery* (that is, not a program anticipated by Lobachevsky's *remarks*).

> Ask yourself this, how does distance produce this force? How does it happen that there is a link between two so different things in nature? To be sure we'll never understand that; however, if it is true that forces depend on distances; just so too may lines be dependent on angles. At least the diversity is alike in the two cases, for the difference lies properly not in the idea but rather only in the fact that we recognize the one dependency through experience, the other, however, as a result of the faultiness of observations, we must mentally assume holds either on the far side of the bounds of the visible world or in the narrow spheres of molecular attractions.[21]

Because scientific theories regularly link things in nature in ways we might at one time have thought counter-intuitive, Lobachevsky insists that judgments about the intuitiveness of his geometry be withheld until we see how it can be integrated into mechanics. Before we decide that Euclidean geometry plus Newtonian mechanics ($E + N$) is alone "intuitive," we had better see whether or not the imaginary geometry plus a modified mechanics ($L + M$) is not also "intuitive." At any rate, the question about which combined theory, $E + N$ or $L + M$, is more intuitive cannot be decided *a priori* but only when both alternatives are at hand. Of course, evidence against the probability of L being true might result if the following conditions held: (a) measurements, with ever greater refinements, continued to support $E + N$ with great exactitude, and (b) all attempts, over a suitably long period of time, to produce an $L + M$ that had a ghost of a chance of being true failed. Since Lobachevsky was pointing out that neither (a) nor (b) was satisfied, we can safely suppose that he refused, in the end, to accept his earlier, tentative probability judgment that Euclidean theorems are alone true.

It is possible to see in Lobachevsky's argument against prejudging the likelihood of $L + M$ being true both an anticipation of and an answer to Henri Poincaré's version of conventionalism in *Science and Hypothesis*.[22] First of all, Lobachevsky is suggesting that to determine the geometry of physical space, we do not just test L, but we really are concerned with L as integrated into a body of physics. Lobachevsky and Poincaré agree on this point, albeit with this slight difference: for Lobachevsky at least some of the theorems of geometry are themselves to be treated as physical laws, whereas for Poincaré they are treated as conventions or definitional truths linked to the remainder of the physical theory.[23] Second, Lobachevsky is suggesting that a theory such as $L + M$ could prove to provide an acceptable account of the space we live in. There are no *a priori* arguments that rule out the applicability to physical space of theorems in a theory of the type $L + M$. Here, again, Lobachevsky and Poincaré agree. But third, Lobachevsky seems to leave it a strictly empirical

[21] Lobachevsky, *Zwei geometrische Abhandlungen*, p. 77.

[22] Henri Poincaré, *The Foundations of Science: Science and Hypothesis; The Value of Science; Science and Method*, G. B. Halsted, trans. (New York: Science Press, 1929), p. 65.

[23] Lobachevsky is pretty definitely not a conventionalist, as his remarks above indicate (*cf. Zwei geometrische Abhandlungen*, pp. 24, 67). His scattered epistemological remarks hint more at a positivist, constructivist approach to theoretical concepts such as those used in geometry. But see the discussion in Sec. IV above.

question whether or not we would come to accept a theory of the type $E + P$ or one of the type $L + M$. He insists we must reserve judgment about what is more comprehensible or intuitive *until we see the alternative theories as a whole.* Judgment cannot be made on the basis of comparing the intuitiveness of E and L alone. Here Lobachevsky seems to reject the heart of Poincaré's argument for the conventionality of geometry, since he appears to reject Poincaré's claim that the greater "simplicity" of Euclidean geometry would always lead us to accept (as a matter of convenience) a theory of the type $E + P$ rather than one of the type $L + M$.

I think it is safe to assert at least this much anticipation of a disagreement with Poincaré, though there are some objections to be considered. One objection is that since Lobachevsky was not directly addressing the specific problem raised by Poincaré, we should not construe his remarks as anticipating him. Specifically Lobachevsky was not concerned with the claim that we can always modify the nongeometrical physical theory we use in such a way that if $L + M$ accounts for the facts, then there is a P' such that $E + P'$ accounts for the same facts. Nevertheless, I think Lobachevsky warns us not to prejudge what is intuitively acceptable until we look at the developed alternative theories as a whole and not just at the geometries involved. Presumably, the P' needed to make $E + P'$ account for the facts as well as $L + M$ could be so counter-intuitive or incomprehensible that we would prefer to accept $L + M$ over $E + P'$. Some such argument as this is at the heart of later replies to Poincaré.

A second objection to claiming that Lobachevsky anticipated Poincaré is this: Poincaré's argument rests on saying that a theory containing E is always simpler than one containing, say, L, but Lobachevsky does not talk about simplicity at all and instead concerns himself with the vaguer notions of intuitiveness and comprehensibility. Therefore Lobachevsky cannot be addressing Poincaré's argument. In answer, I would agree that arguments from simplicity need not come to the same thing, but in this case Poincaré's own remarks on simplicity refer centrally to the "consequences of our mental habits" and to our familiarity with E as a tool for dealing with everyday objects of measurement.[24] These are in fact the very kinds of considerations Lobachevsky would have cautioned against relying on, especially since Poincaré at best only establishes that E, taken by itself, is "simpler" than L, not that $E + P'$ is always simpler than $L + M$.

If it is fair to construe Lobachevsky's admittedly brief remarks as I have done, and if my answers to the obvious objections are sound, then Lobachevsky can be seen to hold a position far more like Hempel's than Poincaré's.[25]

In the course of warning us in "Neue Anfangsgründe" against yielding to premature or preconceived judgments about the theories of physical space we may come to accept, Lobachevsky offers the following argument and the rather startling suggestion:

[24] Poincaré, *Foundations of Science,* p. 65.
[25] *Cf.* Hempel, "Geometry and Empirical Science," pp. 247 ff.

> In Nature we strictly speaking perceive or know only motions, without which our sense impressions are not possible. Consequently, all other concepts, for example, the geometrical ones, are artificially engendered from the properties of motions; and therefore space, in itself, for itself alone, does not exist for us. Accordingly, there can be nothing contradictory for our understanding if we allow that some forces in nature follow one, others another special geometry.[26]

Lobachevsky's remark here is rather brief. It is not elaborated on in his major publications. Consequently I admit the risk of reading too much into what he says. But it can do no harm here to draw out some of the implications of even so brief a remark in view of its special interest. I will begin with the specific suggestion that "some forces in nature [might] follow one, others another special geometry," and then comment briefly on the more philosophical remarks in the passage quoted.

First of all, it should be noted that Lobachevsky once again is asking us to consider geometries which are integrated into comprehensive physical theories, not just geometries taken in isolation. In fact, it is right after he makes this particular suggestion that Lobachevsky comments on the implications of Laplace's work, which we have already discussed. So judging by the context of his remark we are led to a second conclusion: Lobachevsky is concerned with more than just the claim that it is not *contradictory* for us to think of more than one true geometry. Here, too, he is concerned with asking us to set aside our intuitions that such a complicated physics is completely unlikely. He is saying that just as it may turn out that a better overall physical theory emerges if we drop a theory of the type $E + P$ and accept one of the form $L + M$, so too it may turn out that we will have to give up our intuitive idea that a physical theory can employ at most one geometry.

It is too bad that Lobachevsky does not offer any examples to illustrate his suggestion, for it could be implemented in a variety of interesting ways. For example, observations might bring us to conclude that gravitational force and electromagnetic forces behave in accordance with two different geometries. Consider Newton's general equation for gravitational force and Maxwell's equations for electromagnetic attraction. Each involves a function of distance. We might discover that we have a better overall account of "motions" if we develop a theory using distinct metrics M_1 and M_2 to give us distance in each type of equation. We might suppose that M_1 and M_2 agree up to distances less than some very large order of magnitude.

Someone might object to our hypothetical example by insisting that rigid bodies would always define a preferred metric for us. The problem with this objection is that in fact there are no perfectly rigid bodies. The rigid bodies we can use as the basis for such a metric could be compatible with both M_1 and M_2, since they are either not sufficiently large or not sufficiently rigid to allow us to distinguish between M_1 and M_2. Another possible objection to our example is the claim that there is at most one geometry or geometry-plus-

[26] Lobachevsky, *Zwei geometrische Abhandlungen*, p. 76.

physics, in which universal forces disappear. If true, this would seem to rule out our example. Hilary Putnam, however, has shown this claim to be mathematically false.[27] So it appears that these two objections to our hypothetical example of a theory illustrating Lobachevsky's suggestion are not insurmountable.[28]

Whatever kind of example Lobachevsky had in mind, his suggestion seems to have lurking behind it an anticipation of a later view (though Lobachevsky cannot be credited with being explicit about it). He seems to be suggesting—or more accurately his suggestion seems to imply—that introducing different geometries (metricizations) would not involve attributing conflicting properties to bodies or "motions" *considered locally.* That is, the same space of bodies could, as a matter of fact, have different geometries. The contemporary way in which this point has been put is that the individuals—bodies or motions—give rise to one and only one physically significant topology and so to only one space, but this space can be made into a metric space in different ways.[29]

The epistemological and ontological import of Lobachevsky's remark is somewhat unclear: is he, for example, adopting a realist or an anti-realist view of space? He says we directly know or perceive only motions and that all our other concepts are "engendered artificially." Similarly he says space "itself and for itself alone, does not exist for us."[30] This sounds somewhat idealist. On the other hand, he also says that these concepts "engendered artificially" are *inferred* from the properties of motions.

It is possible to give at least two interpretations, and I shall not try to settle the question of which one was really held by Lobachevsky. We can make Lobachevsky out to be a logical constructionist, and the tone of Lobachevsky's remark is suggestive of such an interpretation. That is, he might be saying that there is nothing real except motions and that all else is a logical (mental) construction to account for these motions.[31] On the other hand, we can make Lobachevsky out to be a realist who says that our other concepts, although "engendered artificially" (i.e., not the direct effect of sense impressions), nevertheless purport to refer to real properties of motions, and consequently we have constantly to revise our theories of these properties as we learn more about the motions. On this last interpretation Lobachevsky could simply be

[27] Hilary Putnam, "An Examination of Grünbaum's Philosophy of Geometry," in *Philosophy of Science,* Bernard Baumrin, ed., The Delaware Seminar (New York: Interscience, 1963), Vol. II, pp. 251 ff.

[28] Consider another example: we might construct a theory in which two basic types of forces or fields—say gravitational and electromagnetic—are each taken to give rise to a distinct geometry or metricization of space-time (or, we might want to say that space-time containing two metrics accounts for the two types of forces, as in J. A. Wheeler's "Curved Empty Space-Time as the Building Material of the Physical World," in Ernest Nagel, Patrick Suppes, and Alfred Tarski, eds., *Logic, Methodology, and the Philosophy of Science,* Stanford: Stanford University Press, 1962, pp. 361–374). Observations might prove such a theory more acceptable than one in which electromagnetic waves are taken to travel in a metrical space-time whose geodesics are defined by freely falling bodies.

[29] See Hans Reichenbach, *The Philosophy of Space and Time* (New York: Dover, 1957), and Adolph Grünbaum, "The Special Theory of Relativity as a Case Study of the Importance of the Philosophy of Science for the History of Science," in *Philosophy of Science,* Baumrin, ed., Vol. II, pp. 171–204.

[30] Lobachevsky, *Zwei geometrische Abhandlungen,* p. 76.

[31] See n. 23 above.

saying that space "in and for itself" is not perceived directly by us however "real" it is. Rather, we know the nature of space only by developing comprehensive theories containing both geometrical and nongeometrical concepts and testing the whole—subject to the constraint that we want the best explanatory account of the motions we do directly perceive.

Whatever the correct interpretation of Lovachevsky's own epistemological views, idealist (constructionist) or realist, it might be noted by way of conclusion that one is not forced into idealism simply by the suggestion that distinct geometries may be needed to account for different forces.[32] That is, if a realist (or anti-conventionalist) account of a theory containing only one geometry is acceptable (an issue I do not want to discuss here), then adding another metric does not by itself force us to give up the realist (or anti-conventionalist) account.

My point, without trying to prove it, is that the arguments that would lead us to view a metric in a single-metric theory as more than just a descriptive convenience and would instead incline us to view the field tensor as something implicitly determined ("inferred" in Lobachevsky's terms) by the whole system of physical and geometrical laws, including correspondence laws, are the same arguments that would also incline us to view the two metrics in a double-metric theory as more than just descriptive conveniences. What I am claiming might be made more plausible if I borrow some terminology from Hartry Field's illuminating discussion "Theory Change and Indeterminacy of Reference."[33] We might say that between a single-metric theory S and a double-metric theory D there has been a double "denotational refinement" of the term "distance." Similarly, we might say that "distance" in S "partially refers" to both of the two metrics M_1 and M_2 that might come to be denoted by the terms "gravitational distance" and "electromagnetic distance" in D. Because "distance" is ambiguous in its reference in the light of later refinements is no reason for us to conclude that any or all of the terms "distance," "gravitational distance," or "electromagnetic distance" are denotationless and do not refer to real quantities. In short, Lobachevsky's suggestion that future physical theory might compel us to adopt more than one geometry as true does not by itself force him into idealism with regard to geometry.

[32] This conclusion would no doubt be welcomed by those Russian commentators on Lobachevsky who have emphasized his "materialist" outlook, such as Kagan, *Lobachevsky*.

[33] Hartry Field, "Theory Change and Interdeterminancy of Reference," *Journal of Philosophy*, 1973, *70*:462–481, Sec. II.