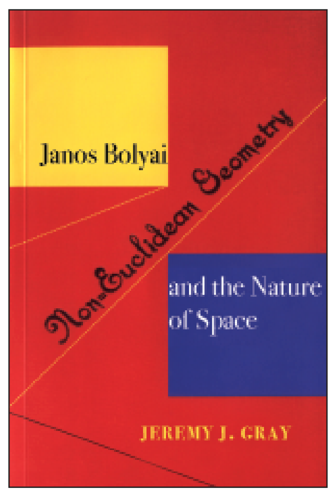
János Bolyai, Non-Euclidean Geometry, and the Nature of Space

Reviewed by Robert Osserman

János Bolyai, Non-Euclidean Geometry, and the Nature of Space
Jeremy J. Gray
MIT Press, 2004
Paperback, 256 pages, $20.00
ISBN 0-262-57174-9

This attractive little volume consists of two major components, together with a number of shorter sections. The major components are labeled "Introduction" and "Appendix" respectively, both designations grossly understating their content. The "Appendix" consists of Bolyai's revolutionary tract, with the subtitle "THE SCIENCE ABSOLUTE OF SPACE *Independent of the Truth or Falsity of Euclid's Axiom XI (which can never be decided* a priori)". It appears here both as a facsimile in the original Latin and also in Halstead's 1896 English translation. It was indeed originally published as an appendix to his father's two-volume treatise on mathematics, whence its name. Jeremy Gray's "Introduction"—or to give it its full name, "Bolyai's *Appendix*: An Introduction"—is in fact a 122-page historical survey of Euclidean and non-Euclidean geometry that focuses on Bolyai and his appendix but goes far beyond that. Among other things, it includes a number of fascinating historical remarks on the debates over the course of many centuries about how to teach Euclidean geometry in school—a debate



that is still very much alive today and whose contemporary shape might profit from a deeper historical perspective.

Gray starts by discussing the way philosophers and scientists, as well as humankind in general, pictured the physical space we inhabit—the shape of the universe as a whole—and the way that such pictures influenced and were influenced by the mathematical constructs that became known as "geometry". In particular, the use made by Newton of the formalism of Euclidean geometry, together with the overwhelming success of his approach to physics and astronomy based on the model of Euclidean space, enshrined Euclidean geometry and cloaked it in a certainty and inevitability that made questioning it appear to be a sign of mental instability. And indeed, that was the reaction by the Russian mathematical establishment, the only ones to read Lobachevskii's first articles on what was to become non-Euclidean geometry, published starting in 1829 in the obscure Russian journal the *Kazan Messenger*. The news of this work apparently did not reach the Bolyais until 1844, which is

Robert Osserman is the special projects director of the Mathematical Sciences Research Institute in Berkeley. His email address is `ro@msri.org`.

astonishing on many grounds, not the least of which involves the shameful role played by Gauss in the sad story of János Bolyai's brilliant beginnings and bitter end.

The story of Gauss and the Bolyais starts back before Gray picks it up, with the close friendship between Gauss and János's father, Farkas (also known by his German name of Wolfgang), dating back to their student days together in Göttingen. The family obsession with Euclid's parallel postulate apparently dates to that time, and indeed, father Bolyai published his first work on the subject, *The Theory of Parallels*, in 1804. He was only one of many who tried to prove that the parallel postulate follows from Euclid's other axioms. Gray describes in some detail the history of such attempts throughout the eighteenth and nineteenth centuries, even well after their futility had been fully demonstrated. Among the prominent mathematicians who fell into the trap were Legendre, who persisted in publishing false proofs over many years, and none other than Lagrange, who did not go so far as publishing any but did have the embarrassment of presenting one at the prestigious Institute of France. As for Farkas Bolyai, it was his friend Gauss who pointed out the error in his argument, but he persisted for at least ten more years before giving up in despair. No wonder then, when his son, János, who had turned out to be something of a mathematical prodigy, appeared as a teenager to have already been bitten by the parallel-postulate bug, Farkas wrote him the often-quoted feverish admonishment to profit from his own example and guard against this will-o-the-wisp: "I have traversed this bottomless night, which extinguished all light and joy in my life. I entreat you, leave the science of parallels alone."

As it turned out, Bolyai Sr. was right, but not for the reasons he thought. János soon concluded that proving the parallel postulate was hopeless, and he gradually became convinced that one could construct a perfectly consistent geometry in which it was not true. By 1825, at the age of twenty-three, János was able to show his achievement to his father but not to convince him that there were no hidden flaws. It was not until 1832 that Farkas agreed to publish his son's work in the form of an appendix to his own book. When Gauss received a copy of the appendix, he wrote back a letter that effectively ended János's brilliant career, as well as, Gray tells us, the relationship between father and son, who did not speak to each other for years afterward. In the letter Gauss gives an infuriatingly mixed message, saying that he had himself carried out much the same program but had written very little of it down, and how pleased he was that it should turn out to be the son of his old friend who had written it down, thereby sparing him the trouble of doing it himself. Had Gauss given his endorsement of the work publicly and brought it to wider attention, it would have changed the course of János's life and career. But he kept his silence,[1] and János not only got no public support but was convinced that his father had betrayed him and revealed what he had been doing to Gauss, who was now claiming it as his own. When Gauss later became aware of Lobachevskii's work, he made no attempt to inform his old friend, Farkas.

Recognition of Bolyai's achievement did not come until too late. Gauss died in 1855, Farkas and Lobachevskii the following year, and János in 1860. As Gauss's correspondence became public, his views on non-Euclidean geometry finally became known. Bolyai's appendix was translated into Italian in 1868, the year and the place that were to be decisive for the future of the new geometry that he had invented.

Between 1832, when it was originally published, and 1868, when it became more widely known, the critical events for the fate of Bolyai's appendix were, as already mentioned, the publication between 1860 and 1865 of the correspondence between Gauss and Schumaker, including a letter Gauss wrote in 1846 praising Lobachevskii's work on non-Euclidean geometry and saying that he had shared the same views for fifty-four years (since 1792, when Gauss was fifteen), and the presentation of Riemann's *Habilitationsvortrag*, entitled "On the hypotheses that lie at the foundation of geometry" in 1854. Not that Riemann refers directly to Bolyai or even to non-Euclidean geometry as it is usually understood. Rather, Riemann proposes a radical rethinking of the entire subject of geometry, not based on axioms in the fashion of Euclid, Bolyai, and Lobachevskii, but on the perhaps shakier but far more flexible foundations of measurement, the calculus, and the whole field of differential geometry as developed by Euler, Gauss, and the French school.

Gray's "Introduction" devotes about twenty-five pages to Bolyai's "Appendix" itself and provides an excellent overview of Bolyai's approach, with its emphasis on an "absolute" geometry of space, with parallel (if one may use the word) results in

[1] *This was not the first time that Gauss's silence on the subject of non-Euclidean geometry had a devastating effect. Another of Gauss's correspondents, named Taurinus, published a brochure in 1826 in which he derived a number of the same trigonometric formulae as Bolyai. In the preface to the brochure he asked Gauss to state his views on the subject, after which Gauss terminated the correspondence. In the book by B. A. Rosenfeld,* A History of Non-Euclidean Geometry: Evolution of the Concept of a Geometric Space, *English translation Springer-Verlag, 1988, p. 219), the author tells us, "Gauss's reaction reduced Taurinus to despair, and he burned all copies of the brochure in his possession."*

both Euclidean and non-Euclidean geometry, but also special results, such as the possibility of "squaring the circle" in the non-Euclidean case, a construction taking up the last ten sections of Bolyai's appendix and described in detail by Gray.

It is in the description of the contributions by Riemann and his successors Beltrami and Poincaré that I find Gray's version of the history of the subject to be somewhat lacking. After noting that Riemann chose not to limit his differential geometry to two-dimensional surfaces, as all previous geometers had done, Gray says (p. 85): "He introduced the idea of *n*-dimensional spaces—rather vaguely, to be sure—as spaces where *n* coordinates were needed to specify the position of a point, and where it was possible to measure lengths along curves. He indicated how the Gaussian idea of intrinsic curvature could be generalized to this new setting. And he mentioned, almost in passing, that there were three two-dimensional geometries where the curvature was constant: the cylinder (curvature zero), the sphere (curvature positive), and surfaces of constant negative curvature (which he only alluded to)."

I am puzzled, in particular, by Gray's rather dismissive reference to Riemann's fairly extended and quite explicit discussion of surfaces of constant curvature. For curvature zero, Riemann cites specifically cylindrical and conical surfaces, and for positive curvature he discusses at length both surfaces of revolution that do not lie on spheres and general surfaces of constant positive curvature, noting that (after making suitable cuts, if necessary) these may all be "developed" onto a sphere—that is to say, in modern terminology, mapped onto a sphere of the same curvature by a map that is a local isometry.

It is true that Riemann is vague on many points in his paper, and he himself says as much. That is not surprising when one considers that the new concepts introduced include such elusive ones as that of an *n*-dimensional differentiable manifold, whose precise definition did not come until the twentieth century. But there is a widespread misconception that he was equally vague about the concept of curvature in higher dimensions, and nothing could be further from the truth. His assumption is that one has a measure of arc length given by a differential expression of a certain form, now known as a Riemannian metric, and he provides two different ways to compute the curvature of the manifold at any point in any two-dimensional direction, one of them algebraic/analytic, the other geometric. The former, though explicit, is hard to follow, since it gives a procedure rather than an actual formula. The geometric definition, however, is both explicit and clear. Riemann tells us to construct the surface formed by all geodesics issuing from a given point that start out along a given two-dimensional section. That surface will have a curvature whose value is given explicitly by a formula due to Gauss. The Gauss curvature of the surface at the given point is by definition the Riemannian (sectional) curvature of the manifold at the point in the given two-dimensional direction. As an illustration, he gives explicit formulae for Riemannian metrics of constant positive, negative, and zero curvature in any number of dimensions. The zero-curvature case is of course the standard Euclidean metric in *n*-dimensions, but the other two cases were to play critical roles in future developments.

Riemann's metric of constant positive curvature leads to what the physicist Max Born later described as "one of the greatest ideas about the nature of the world which ever has been conceived." That is the suggestion that the universe could be finite but without a boundary. As Riemann points out, the value of the curvature could be arbitrarily close to zero, so that there would be no way for us to distinguish it from Euclidean space on the scales at which we could make measurements, but the age-old problem of how to choose between the equally unpalatable alternatives of a universe that extends infinitely far in all directions or one that ends somewhere (and what lies beyond?) would be resolved in one stroke. Einstein later seized on this model of the universe as a 3-sphere and used it in his first attempt at a cosmological model based on his general theory of relativity.

Riemann's metric of constant negative curvature proved equally important and even more seminal for future developments. In the two-dimensional case alone it has provided the model we now know as the "hyperbolic plane", while the metric itself tends to be called the "hyperbolic metric" or the "Poincaré" metric. The subject of hyperbolic geometry arose from two completely independent sources: one was the differential geometry of surfaces (or, more generally, Riemannian metrics) of constant negative curvature, the other the axiomatic approach of Bolyai and Lobachevskii with the parallel postulate replaced by an alternative. It was the Italian geometer Eugenio Beltrami who in 1868 finally clarified once and for all the relationship between these two quite distinct-appearing subjects and who may be said to have originated the field of hyperbolic geometry. Poincaré probed in great detail two different models of two-dimensional hyperbolic geometry: the "Poincaré disk" and the "Poincaré upper half plane". The former is simply the unit disk with Riemann's metric of constant curvature -1, while the metric for the latter had been written down even earlier by Liouville in the course of his study of surfaces of constant negative curvature. However, neither of those authors indicated any link to non-Euclidean geometry.

It is unfortunate that Gray misses the opportunity here to right a great historical wrong.[2] Everybody quotes the first of two papers published by Beltrami in 1868, entitled "Saggio di Interpretazioine della Geometria Non-euclidea", in which he writes down an explicit Riemannian metric (different from the one given by Riemann) on a disk in the plane and relates it on the one hand to surfaces of constant negative curvature and on the other hand to the formulas and properties given by Lobachevskii for his non-Euclidean geometry. He shows that in this model the straight lines of Lobachevskii's geometry correspond to chords in the circle. Thus, at one stroke, Beltrami provides a model for global non-Euclidean geometry in which one can extend any line infinitely far in each direction and a link between the local properties of Bolyai and Lobachevskii's non-Euclidean geometry and the geometry of surfaces of constant negative curvature such as the pseudosphere.

But it is a second paper of Beltrami's, published in the same year, entitled "Teoria fondamentale degli spazii di curvature costante", that really puts all the pieces in place. What Beltrami does, as his title implies, is make a thorough study of spaces of constant curvature in all dimensions. He analyzes in detail various models of n-dimensional hyperbolic space, two of which reduce in the two-dimensional case to the so-called "Poincaré disk metric" and "Poincaré upper half-plane metric". As John Stillwell has pointed out,[3] they should really be called the "Riemann-Beltrami metric" and "Liouville-Beltrami metric" respectively. Beltrami starts with the latter metric in the upper half-space $z > 0$ in $n+1$ dimensions, where the element of arclength is $d\sigma s = d\sigma / z$, with d equal to the standard Euclidean element of arclength. He shows (by a rather roundabout argument) that this Riemannian metric has constant negative curvature, that its geodesics are semicircles orthogonal to the plane $z = 0$, that the induced metric on the hemispheres orthogonal to the plane $z = 0$ gives a model of n-dimensional hyperbolic space, and that the vertical projection of those hemispheres onto the plane $z = 0$ provides exactly the model of hyperbolic space that he has investigated in detail in his previous paper. Finally, he notes that stereographic projection of the hemisphere leads precisely to hyperbolic space with the metric of constant negative curvature originally written down by Riemann. He further verifies that this metric, which Riemann stated without proof, had constant curvature—indeed does—and he also verifies analytically an important observation made by both Bolyai and Lobachevskii: that for three-dimensional hyperbolic geometry, the two-dimensional subspaces called "horospheres" by Lobachevskii and simply denoted by "F" by Bolyai have the induced metric of the Euclidean plane.

In a section labeled "Taking Stock" (p. 102), Gray points out that the philosophical consequences of having two equally viable geometries at one's disposal were profound. He says, "There cannot, after all, be two incompatible accounts of physical space that are both true. It follows that one of the geometries must be false (and perhaps both, but this radical view was never espoused)." But in fact it was precisely this radical view that Riemann espoused, and doubly so. First of all, Riemann specifically proposed, as noted above, that space might have constant positive curvature and therefore be neither Euclidean nor "non-Euclidean". But even more radically and more fundamentally, he proposed that it be "Riemannian". Which brings us to two points of terminology that have led to much confusion. The first is that one might think that "non-Euclidean" refers to a geometry that is not Euclidean and that it would therefore include "Riemannian" in the sense it is now generally understood, which is: given by a "Riemannian metric" where the element of arc length is expressed by the square root of a quadratic form in the differentials of the coordinates (or in the language of differentiable manifolds: the tangent space at each point is endowed with a positive definite inner product). However, the term "non-Euclidean geometry" is almost universally identified now with "hyperbolic geometry", in which the metric has constant negative curvature. The qualifier "almost" refers to the fact that one sometimes sees two alternatives given to the parallel postulate, in one of which there are two distinct lines through a point parallel to a given line and angles in a triangle add up to less than 180°, and the other in which there are no lines through a point parallel to a given one and the angles add up to more than 180°. If one wishes in the latter case to still have any two lines intersect in a single point, then one can use as a model the projective plane, with metric inherited from the sphere after identifying pairs of antipodal points. The geometry given by this second alternative to the parallel postulate is sometimes termed "elliptic geometry", in contrast to hyperbolic,[4] and sometimes "Riemann's non- Euclidean geometry," only adding

[2] *The reviewer is guilty of the same omission in his book on geometry and cosmology, having only learned of it afterwards.*

[3] Sources of Hyperbolic Geometry, *History of Mathematics, vol. 10, AMS/LMS, 1996, p. 35. This is an excellent reference for the current discussion.*

[4] *The terminology of elliptic, hyperbolic, and parabolic geometries was introduced by Felix Klein in his 1871 paper "On the so-called non-Euclidean geometry", in which he provides a unifying overview by means of projective geometry. (See the book of Stillwell cited above.)*

to the confusion between "Riemannian" and "non-Euclidean" geometry.

To come back to the main point, the debate as to whether the physical space we inhabit obeys the laws of Euclid or those of Bolyai/Lobachevskii has been superseded for 150 years now by the far more likely alternative of being neither, but rather Riemannian (or even "non-Riemannian") and for almost 100 years by the strong likelihood that it is the three-dimensional space component of a four-dimensional pseudo-Riemannian space-time. Gray devotes a brief section entitled "The Nature of Space", pp. 118–9, to a way in which a model of space-time in the vicinity of a gravitating mass can be related to the geometry of a negatively curved surface (although not constant negative curvature) and hence to something like non-Euclidean geometry.

Let me conclude with a few final remarks.

First, this volume is part of a series of Burndy Library publications, each of which focuses on some book in the library's collection. The library is devoted to the history of science and technology and is currently located on the campus of the Massachusetts Institute of Technology. The facsimile of the copy of Bolyai's appendix is from the Burndy collection, as are the many other illustrations in the book, all done in sepia tones and very handsome.

Second, Gray is an excellent expositor. He covers a wide swath of the history of Euclidean and non-Euclidean geometry, but also goes into some depth in discussing Bolyai's contribution and the ways it resembles and differs from Lobachevskii's approach. Anybody wishing to study exactly what it was that Bolyai did would find this an excellent reference.

Third, there are several additional notes in the volume, among them a biographical sketch of Bolyai's translator, George Bruce Halstead, that I found quite interesting and an announcement from 2002 that for the first time there seems to be a reliable image of Bolyai, which is reproduced here. What is notably missing is an index, an omission that never ceases to puzzle me whenever I encounter it. Why, when so much time and effort is put into making an attractive and useful addition to the literature, does the author (as well as the editor) not take the minimal additional time and effort needed to enhance its usefulness to a reader by adding an index?

Fourth, Gray takes the very sensible approach of not trying to include a comprehensive bibliography but to focus on books and articles to which he makes reference in the text. I would just like to note two references that I have personally found very useful and which are not included in the bibliography: the books by Rosenberg and Stillwell cited in the footnotes.

Finally, Gray's "Introduction" ends with two paragraphs labeled "Conclusions" (pp. 120–21). The first paragraph reviews the ways in which Euclidean geometry has failed to maintain either of its traditional roles as the epitome of rigorous deductive reasoning or as the model for the physical universe we live in. The second paragraph starts with "And yet, and yet." It goes on to relate some of the ways in which Euclidean geometry remains the remarkable achievement that it always was. I would like to add two more "and yet's" of my own. First, although Euclid's reasoning is anything but rigorous to the modern eye, it brought mathematics to a level unlike anything else produced by humanity in the succeeding two thousand years. In particular, I do not know of a single theorem stated by Euclid that has turned out to be false. In each case it was simply a question of polishing up details and filling in gaps. That is quite an astonishing feat.

The other "and yet" was perhaps best stated by John Kelley.[5] I would like to let Kelley have the last word. After describing his background as a true "country boy" in rural America and a "devastating" encounter with high school algebra, he writes:

> The following year I took my last high school mathematics course, geometry. It was a traditional course, very near to Euclid. It talked about axioms and postulates, defined lines and points in utterly confusing ways. The woman who taught us had a chancy disposition and she had been known to throw erasers at inattentive students. It was the loveliest course, the most beautiful stuff that I've ever seen. I thought so then; I think so now.

[5] *On p. 473 of J. L. Kelley, "Once over lightly",* A Century of Mathematics in America, *Part III (Peter Duren, ed.), Amer. Math. Soc., Providence, RI, 1989, pp. 471–93.*