IS SPACE FLAT? NINETEENTH-CENTURY ASTRONOMY AND NON-EUCLIDEAN GEOMETRY

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Abstract: The geometrical structure of space entered astronomy in the second half of the nineteenth century, but slowly and hesitantly. Although in this period non-Euclidean geometry became a very important branch of mathematics, it aroused little interest among the astronomers. Nonetheless, there were more contributors to 'non-Euclidean astronomy' than usually supposed, and their attempts to forge links between the new geometries and the astronomical sciences merit attention. While some astronomers, such as R.S. Ball and K. Schwarzschild, discussed the observational evidence for curved space, in one case the hypothesis was used to solve a cosmological problem, namely, Olbers' Paradox. This paper reviews developments from N.I. Lobachevsky in 1829 to P. Harzer in 1908.

Keywords: space, non-Euclidean geometry, N.I. Lobachevsky, C.S. Peirce, K. Schwarzschild.

1 INTRODUCTION

In the early part of the nineteenth century it was recognized that Euclid's parallel postulate is not true by necessity and that there exist other geometries than the Euclidean system. A small group of mathematicians argued that the geometrical structure of physical space can be determined only by empirical means such as astronomical measurements. One might believe that astronomers eagerly took up the challenge, but this is not what happened. By and large, curved space was a non-subject in nineteenth-century astronomy. Only with Einstein's General Theory of Relativity did the curvature of space (or space-time) enter significantly into to the physical and astronomical sciences.

Although non-Euclidean geometry only played a very limited role in astronomy before Einstein, it was not completely ignored. A handful of astronomers investigated the possibility that space might be curved, a hypothesis that in the first decade of the twentieth century was well known and had permeated even into the more popular literature. For example, the recognized and widely-read *Newcomb-Engelsmanns Populäre Astronomie* included a brief account of finite, positively-curved space (Kempf, 1911: 664). A review of the development from about 1830 to 1910 reveals a history that is richer and more interesting than what can be found in most histories of either astronomy or mathematics.

2 FROM GAUSS TO LOBACHEVSKY

"Maybe in another life we shall attain insights into the essence of space which are now beyond our reach. Until then we should class geometry not with arithmetic, which stands purely a priori, but, say, with mechanics …" (Gauss, 1900: 177; my English translation). Thus wrote Karl Friedrich Gauss (1777–1855) in a letter of 28 April 1817 to the Bremen astronomer Heinrich Wilhelm Olbers (1758–1840), thereby indicating that ordinary Euclidean geometry was not true by necessity. The following year, while serving as Director of the Göttingen Observatory, Gauss was requested to undertake a major cartographic survey project with the purpose of mapping the state of Hanover (to which Göttingen belonged) by means of triangulation. As part of this project he made geodetic measurements of unprecedented precision of a triangle extending between three mountain peaks. The sides of the Brocken-Hohenhagen-Inselsberg triangle were

approximately 69, 85 and 107 km. For a long time it was generally believed that the theoretical purpose of these measurements was to test the assumption of Euclidean geometry, namely, to establish whether or not the sum of the angles in the triangle deviated from 180°. This is a myth that can still be found in the mathematical and astronomical literature. However, historians of science agree that Gauss' work had nothing to do with the possibility of physical space being non-Euclidean (Breitenberger, 1984; Miller, 1972).

Figure 1: The Russian stamp of 1956 commemorating the centenary of Lobachevsky's death.

The non-Euclidean geometry anticipated by Gauss was discovered independently 1829–1831 by János Bolyai (1802–1860) in Hungary and Nikolai Ivanovich Lobachevsky (1792–1856) in Russia. While both of the two mathematicians reached the insight that the truth of Euclidean geometry was a question to be determined empirically, it was only the ten years older Lobachevsky who contemplated the problem within an astronomical perspective and further developed it (Figure 1). He suspected that the truth of geometry "… can only be verified, like all other laws of nature, by experiment, such as astronomical observations …" (Engel, 1898: 67; my English translation; cf. Vucinich,

1962). As a young student at Kasan University, Lobachevsky had studied astronomy under the Austrian Johann Joseph Littrow (1781–1840), who in 1810 had established an Astronomy Department at the University and later became Director of the Vienna Observatory. Recognizing the outstanding mathematical abilities of his student, Littrow made some astronomical observations with him. For example, in the summer of 1811 they observed a large comet. From 1819, Lobachevsky served as the Director of the Kasan University Observatory. Although a mathematician, he was thus well acquainted with astronomical theory and practice.

Already in his 1829 memoir in the *Kasan Messenger* "On the Principles of Geometry" Lobachevsky suggested an astronomically testable consequence of his 'imaginary' (or hyperbolic) geometry. It can be shown that for any triangle the difference of the angle sum ($\alpha + \beta + \gamma$) from 180° is given by the product of the space curvature K and the area δ of the triangle:

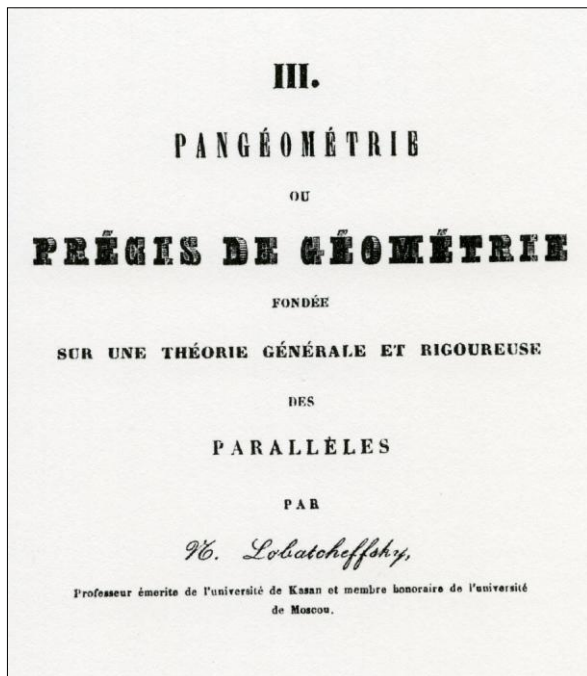$$\alpha + \beta + \gamma - \pi = K\delta \qquad (1)$$

Figure 2: The title page of the French edition of Lobachevsky's *Pangeometry*.

In the case of hyperbolic or Lobachevskian space, the curvature is negative. As Lobachevsky pointed out, this implies that the angle sum of a triangle is always less than 180° and the more so the bigger the triangle becomes. He reasoned that this prediction might be checked by considering the parallax of stars such as 29 Eridani, Rigel and Sirius. For the last-mentioned star he quoted a parallax value of 1.24″ recently published by the French amateur astronomer François-Clément D'Assa-Montardier (1828; 1769–1840).

Lobachevsky concluded that the angle sum of the triangle spanning the Sun, the Earth and Sirius deviated from the Euclidean value of 180° by at most 0.000372″. As was only recognized much later, due to some mistake or misprint, the value he gave in 1829 was too large, as it should have been only 0.00000372″ (Brylevskaya, 2008: 132). At any rate, the tiny deviation strongly suggested that space was Euclidean, and

yet Lobachevsky refrained from drawing this conclusion in firm terms (Bonola, 1955: 94-96; Daniels, 1975). Realizing that while it could in principle be proved that astronomical space is non-Euclidean, it could never be proved to be Euclidean, so he tended to see his calculations as inconclusive. In any case, at the time no reliable determination of a stellar parallax had been made. Only in 1838 did Friedrich Wilhelm Bessel (1784–1846) succeed in finding an annual parallax of 0.3136″ for the star 61 Cygni, corresponding to a distance from the Earth of 657,000 AU. The modern value of the parallax of Sirius is 0.37″, less than a third of the value adopted by Lobachevsky. In another line of reasoning, Lobachevsky showed that, if the world geometry is hyperbolic, the radius of curvature must be greater than 3×10^5 AU.

Lobachevsky also discussed the relevance of his new geometry to astronomical space in later publications, such as his *Pangeometry*, which was published in Russian in 1855 and translated into French in 1856, the year of his death (Figure 2). He wrote: "The distances between the celestial bodies provide us with a means for observing the angles of triangles whose edges are very large …" (Lobachevsky, 2010: 76). Consider a triangle spanned by a star and the two positions of the Earth half a year apart in its orbit around the Sun. Let the angle at the star be denoted α and the two angles at the positions of the Earth be β and γ. Then the parallax angle, p, can be expressed as

$$p = \pi - (\beta + \gamma) = \alpha - K\beta \qquad (2)$$

While in Euclidean space ($K = 0$) the parallax tends toward zero as the distance increases toward infinity ($\alpha = 0$), Lobachevsky realized that there must be a minimum parallax for all stars irrespective of their distances from the Earth. His general conclusion was that since the deviation from flat space was smaller than the errors of observation, Euclidean geometry was a perfect approximation for all practical purposes.

3 RIEMANNIAN SPACE

The ideas of non-Euclidean geometry circulated slowly in the mathematical community and only became generally known about 1870, chiefly through the works of Eugenio Beltrami (1835–1899), Hermann von Helmholtz (1821–1894) and Felix Klein (1849–1925). By that time it was realized that there are three possible geometries of constant curvature K, a quantity that has the dimension of an inverse area. It relates to the radius of curvature R by

$$R^2 = \frac{k}{K} . \qquad (3)$$

The curvature constant, k, distinguishes between flat or Euclidean space ($k = 0$), spherical space ($k = +1$) and hyperbolic space ($k = -1$). The possibility of a positively-curved space was recognized by the German mathematician and physicist Bernhard Riemann (1826–1866) in a famous address of 1854 in which he put the concept of curvature as an intrinsic property of space on a firmer basis and effectively founded differential geometry. Of relevance here is that Riemann (1873: 36) was the first to point out that, in the case of constant positive curvature, the traditional identification of a finite three-dimensional space with a bounded space is unwarranted. Infinity does not follow from

space being unbounded, he said, for

> … if we assume independence of bodies from position, and therefore ascribe to space constant curvature, it must necessarily be finite provided this curvature has ever so small a positive value. (ibid.).

It is only in retrospect that Riemann's address, which remained unpublished until 1867, has become a classic of non-Euclidean geometry. In fact, although he may have known of Lobachevsky's work, he did not refer to it and also did not mention the contributions of Bolyai. He only alluded in passing to astronomy:

> If we suppose that bodies exist independently of position, the curvature is everywhere constant, and it then results from astronomical measurements that it cannot be different from zero; or at any rate its reciprocal must be an area in comparison with which the range of our telescopes may be neglected. (Riemann, 1873: 36).

According to Riemann, the metrical structure of space was likely to be of relevance to microphysics, at the atomic or molecular level, but he did not take an interest in the space of the astronomers. Questions about the global properties of space he cut short as "… idle questions." (Riemann, 1873: 37).

Riemann's emphasis on the possibility of an unbounded yet finite space failed to attract the attention of astronomers. Only in 1872 did the Leipzig astrophysicist Johann Carl Friedrich Zöllner (1834–1882) make astronomical—or rather cosmological—use of Riemann's insight. Primarily known for his pioneering contributions to astrophotometry, Zöllner also carried out important work in spectroscopy, solar physics, stellar evolution and the theory of comets. After 1877 he focused on what he called 'transcendental physics', the study of spiritualist phenomena based on the postulate of a fourth space dimension. As one might expect, this line of work created so much public attention that it damaged his scientific reputation (see Kragh, 2012).

Acquainted with the mathematical literature on non-Euclidean geometry, in his book *Über die Natur der Cometen* Zöllner (1872: 308-314) argued that cosmic space might well be positively curved (Figure 3). He considered Riemann's idea the key that would unravel the secrets of the Universe and dissolve the problems of a materially-finite Universe, for "… it opens up for the deepest and most fruitful speculations concerning the comprehensibility of the world." (Zöllner, 1872: 312; my English translation). According to Zöllner,

> The assumption of a positive value of the spatial curvature measure involves us in no way in contradictions with the phenomena of the experienced world if only its value is taken to be sufficiently small. (Zöllner, 1872: 308; my English translation).

Based on the assumption of a Riemannian Universe with only a finite number of stars, he could explain Olbers' Paradox without having to assume interstellar absorption of starlight or taking recourse to a limitation of either cosmic time or space (see Jaki, 1969: 158-164). Zöllner's aim was not only to demonstrate how an astronomical problem could be solved on the basis of Riemann's hypothesis, but more generally to argue for a closed cosmic space. He suggested that the laws of nature might be derived from the dynamical properties of curved space.

Zöllner's book, *Über die Natur der Cometen*, attracted much attention in Germany and was reprinted in 1883 and 1886. Nonetheless, Zöllner's pioneering contribution to cosmology is not well known, and it was even less well known in the nineteenth century. While it attracted some interest among German philosophers, it was either unknown or ignored by his colleagues in physics and astronomy. For this reason, and also because I have recently described Zöllner's Universe in detail (Kragh, 2012), I shall pass on to other attempts to apply ideas of non-Euclidean geometry in astronomical contexts.

4 ROBERT STAWELL BALL

During the last quarter of the nineteenth century, non-Euclidean geometry became a 'hot topic' in mathematics and philosophy, and was discussed in hundreds of books and scientific papers. On the other hand, the number of astronomers who expressed interest in the topic can be counted on the fingers of one hand. Moreover, the interest rarely went beyond uncommit-
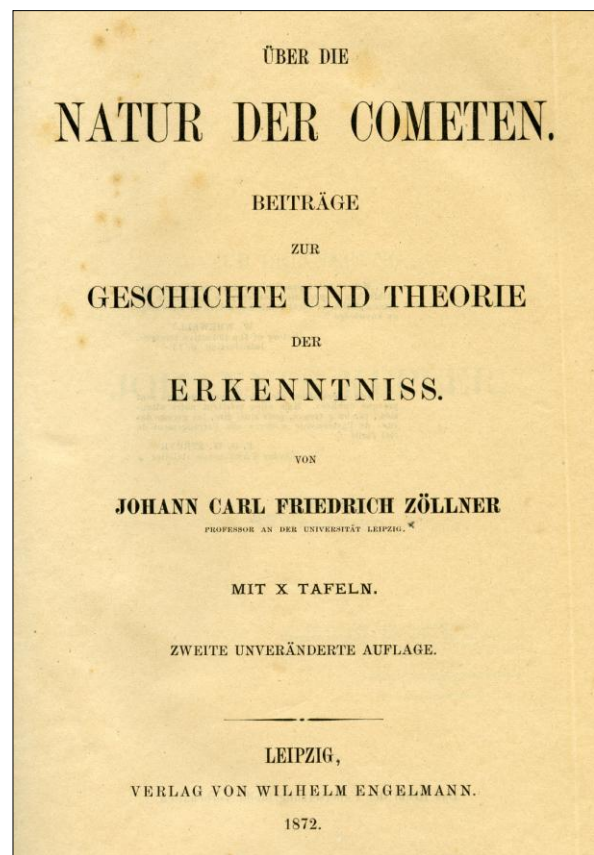

Figure 3: Zöllner's 1872 treatise on the theory of comets, which included a chapter advocating a closed Riemannian Universe.

ted comments. One of those who did express a more substantial interest was the Irish astronomer Robert Stawell Ball (1840–1913), who in 1874 was appointed Royal Astronomer of Ireland and Professor of Astronomy at the University of Dublin, a position that included the Directorship of the Dunsink Observatory. Then from 1892 until his death in 1913 he served as Lowndean Professor of Astronomy and Geometry at Cambridge University, succeeding John Couch Adams (1819–1892) of Neptune fame (Ball, 1915; MacPherson, 1914). Sharing his scientific work between mathematical physics and astronomical observations, he

was also a well-known and much-esteemed author of popular astronomy in the Victorian tradition.

While at Dunsink, Ball directed a large-scale observational research programme in determining stellar parallaxes. Among the problems that faced astronomers in this area was the choice of comparison stars for parallax measurements, by taking into account the proper motions of the stars. Ball and his collaborators paid particular attention to the star 61 Cygni that Bessel had originally used in his discovery of the annual parallax. In a lecture given to the Royal Institution in London on 11 February 1881, he discussed the complex questions of comparison stars and proper motions in relation to parallax measurements. At the end of the lecture, he briefly alluded to the nature of space:

> If space be hyperbolic the observed parallax is smaller than the true parallax, while the converse must be the case if space be elliptic. The largest triangle accessible to our measurements has for base a diameter of the earth's orbit, and for vertex a star. If the defect of the sum of the three angles of such a triangle from two right angles be in any case a measurable quantity, it would seem that it can only be elicited by observations of the same kind as those which are made use of in parallax investigations. (Ball, 1881: 92).

What Ball called the "… true parallax …" is the angle under which the radius of the orbiting Earth appears for an observer located at a star; the "… observed parallax …", on the other hand, is half the annual change of the angular distance between the star and some comparison star close to it.

Ball was well acquainted with non-Euclidean geometry, but his remarks in the 1881 address had the character of an afterthought rather than a serious proposal for investigating the geometry of space by astronomical means. He did not return to the subject in his later scientific work, but, characteristically, chose to mention it only in his popular books. One of these was *In the High Heavens*, a book published in 1893. Ball discussed in a general way whether space is finite or infinite, a question which

> … is rather of a metaphysical complexion … [and] depends more on the facts of consciousness than upon those of astronomical observation … (Ball, 1893: 247; cf. Whiting, 2011: 143-158).

Having argued that the number of matter particles in the Universe must be finite, he proceeded to space itself and the possibility of "… a space which is finite in dimensions." (Ball, 1893: 251). With this he did not mean a finite-dimensional space, but rather a three-dimensional spherical space. Although Ball did not explicitly endorse a positively-curved space, he stressed that it was consistent and intuitively acceptable. Indeed, he expressed sympathy for the hypothesis, which

> … provides the needed loophole for escape from illogicalities and contradictions into which our attempted conceptions of [infinite] space otherwise land us. (Ball, 1893: 252).

In this context may be mentioned also the American mathematician James Edward Oliver (1829–1895), Professor at Cornell University, who according to George Halsted (1853–1922) was "… a pronounced believer in the non-Euclidean geometry." (Halsted, 1895: 545). Halsted recalled how Oliver tried to convince him that astronomical evidence pointed to space being closed. On one occasion, Oliver

> … explained a plan for combining stellar spectroscopy with ordinary parallax determinations, and expressed his disbelief that C.S. Pierce [sic] had proved our space to be of Lobachévsky's kind, and his conviction that our universal space is really finite, therein agreeing with Sir Robert Ball. (Halsted, 1895: 545).

It remains unknown what Oliver's ideas were, more precisely, since he never published on the subject.

5 NEWCOMB'S ELLIPTIC SPACE

The distinguished American astronomer Simon Newcomb (1835–1909) took an interest in non-Euclidean geometry, both from a mathematical and an astronomical point of view. As early as 1877, at a time when he had just become Superintendent of the Nautical Almanac Office, he published a mathematical paper on the geometry of space with positive curvature, but without relating his investigation to astronomy (Newcomb, 1877). Newcomb's space was not quite the same as Riemann's, but described by what soon became known as 'elliptic geometry' (and to which Ball referred in the quotation above). While in spherical or Riemannian space all geodesics from a given point intersect again at a distance πR, in elliptic space two geodesics can have only one point in common. In the latter case the largest possible distance between two points is $\frac{1}{2}\pi R$, whereas it is πR in the spherical case. Both spaces are finite, but for the same radius of curvature the volumes differ. Today spherical space is often seen as a special case of the elliptic space.

Newcomb (1877: 299) pointed out that

> … there is nothing within our experience which will justify a denial of the possibility that the space in which we find ourselves may be curved in the manner here described.

On the other hand, he seems to have been reluctant to part with the infinite Euclidean space. On some occasions he mentioned the possibility of curved physical space, but in popular contexts only and without taking it too seriously. In the widely-read *Popular Astronomy*, a book first published in 1878 that over the next twenty years went through many editions (and was translated into German, Russian and Norwegian), he discussed what would happen with the heat of the Sun. Would it forever be lost? Or would it, if space were curved, eventually return to the Sun? He wrote:

> Although this idea of the finitude of space transcends our fundamental conceptions, it does not contradict them and the most that experience can tell us in the matter is that, though space be finite, the whole extent of the visible universe can be but a very small fraction of the sum total of space … (Newcomb, 1878: 505).

But Newcomb did not believe in the possibility of a positively-curved space in which the solar heat would return to its source. On the contrary, he dismissed the hypothesis as "… too purely speculative to admit of discussion." (Newcomb, 1878: 504).

Many years later, in an address given to the American Mathematical Society on 29 December 1897, Newcomb dealt in a general way with what he called the philosophy of 'hyperspace', a concept that included non-Euclidean spaces as well as spaces with more than three dimensions. As he pointed out, the hypoth-

esis of curved space was testable, if more in principle than in practice:

> Unfortunately, we cannot triangulate from star to star; our limits are the two extremes of the earth's orbit. All we can say is that, within those narrow limits, the measures of stellar parallax give no indication that the sum of the angles of a triangle in stellar space differs from two right angles. (Newcomb, 1898: 7).

He continued with an argument that effectively ruled out elliptic space as more than a speculation, at least as seen from the astronomer's perspective:

> If our space is elliptical, then, for every point in it – the position of our sun, for example – there would be, in every direction, an opposite or polar point whose locus is a surface at the greatest possible distance from us. A star in this point would seem to have no parallax. Measures of stellar parallax, photometric determinations and other considerations show conclusively that if there is any such surface it lies far beyond the bounds of our stellar system. (Newcomb, 1898: 7).

6 PEIRCE, A COMMITTED NON-EUCLIDEAN

Newcomb's cautious ideas about non-Euclidean space form an instructive contrast to those of his compatriot and friend, Charles Sanders Peirce (1839–1914; Figure 4). Although today mostly known as a philosopher, as a young man Peirce was primarily recognized as a promising astronomer and chemist. While at Harvard College Observatory he did important work in photometry and spectroscopy, and he was among the first to study the spectrum of an aurora, which he did as early as April 1869. Elected a member of the U.S. National Academy of Sciences in 1877, he spent most of his professional career as a practicing scientist associated with the United States Coast and Geodetic Survey. Contrary to the four years older Newcomb, Peirce was convinced that space is non-Euclidean—indeed *must* be non-Euclidean—a claim he supported with both philosophical and observational arguments (Dipert, 1977).

In letters and manuscripts written between the years 1891 and 1902 Peirce investigated various aspects of the structure of space, which led him to conclude that it was either of the Lobachevskian or the Riemannian kind. In a paper published in *The Monist* of 1891 he discussed the question in terms of stellar parallaxes, although at the time without suggesting a definite answer to the sign of space curvature:

> I think we may feel confident that the parallax of the furthest star lies somewhere between −0.″05 and +0.″15, and within another century our grandchildren will surely know whether the three angles of a triangle are greater or less than 180°, – that they are *exactly* that amount is what nobody ever can be justified in concluding … (Peirce, 1891: 174).

Peirce had a predilection for hyperbolic space, as is evidenced from his manuscripts and correspondence with Newcomb in the early 1890s.

Thus, in one of his manuscripts of 1891 he listed no fewer than fifteen "… methods of investigating the constant of space …" (Peirce, 1891: 229) that included parallax measurements, ideas of stellar evolution, the proper motions of stars, and Doppler shifts in stellar spectra. In addition, Peirce (2010: 230) concluded that "… the relative numbers of stars of different magnitudes depend on the constant of space." In a lengthy

letter to Newcomb he convinced himself—and in vain tried to convince Newcomb—that astronomical data provided support for his "… attempt to make out a negative curvature of space." Although realizing the hypothetical nature of his conclusion, he had no doubt of its significance:

> The discovery that space has a curvature would be more than a striking one; it would be epoch-making. It would do more than anything to break up the belief in the immutable character of mechanical law, and would thus lead to a conception of the universe in which mechanical law should not be the head and centre of the whole. It would contribute to the improving respect paid to American science, were this made out here … In my mind, this is part of a general theory of the universe, of which I have traced many consequences, – some true and others undiscovered, – and of which many more can be deduced; and with one striking success, I trust there would be little difficulty in getting other deductions tested. It is certain that the theory if true is of great moment. (Eisele, 1957: 421-422).

Figure 4: Charles S. Peirce, 1839–1914 (after: http://psychology.wikia.com/wiki/ Charles_Peirce).

Peirce's optimism was short-lived, as indicated in a letter he wrote to Newcomb on 21 December 1891:

> I have for the present given up the idea that anything can be concluded with considerable probability concerning the curvature of space. (Eisele, 1957: 423).

Newcomb welcomed Peirce's more agnostic attitude, which he mistakenly took to be support of his own view, namely, "… that all philosophical and logical discussion is useless." (Eisele, 1957: 424). This was definitely not a view shared by Peirce, who never did quite abandon the matter. Thus, in a manuscript note of 1894 he wrote:

> I made the necessary computations for a selection of stars. The result was markedly in favor of the hyperbolic geometry. (Dipert, 1977: 411).

Peirce's attempt to conceive celestial space as non-Euclidean was the most elaborate and serious one of the few such attempts in the nineteenth century. However, he made no impact at all, primarily because he did not publish his arguments in journals read by most

astronomers and mathematicians. Although his ideas were known to some American scientists, they were not convinced. As Newcomb wrote him in March 1892,

> … the task of getting the scientific world to accept any proof that space is not homoloidal [flat], is hopeless, and you could have no other satisfaction than that of doing a work for posterity … (Eisele, 1957: 424).

When Newcomb died in 1909, and when Peirce died just five years later, observational proof of curved space was still lacking.

7 FRENCH DISCUSSIONS

References to the possible astronomical consequences of non-Euclidean space appeared not only in the contexts of astronomy, but sometimes also in the mathematical and philosophical literature. According to the conventionalist view of Henri Poincaré (1854–1912), one of the most eminent and influential scientists at the turn of the century, the geometry of space could not be determined objectively. According to him, it made no sense to say that one geometry was more true than another, only that it was more convenient. For example, if the sum of angles in a celestial triangle were found by astronomical measurements to be, say, 185° ± 1°, one might assume the physics of light propagation to be correct and change to a spherical geometry; but one might also choose to maintain Euclidean geometry by changing the theory of how light propagates through space. Because Poincaré (1892) found Euclidean geometry to be the most simple and convenient system, he saw no reason to consider other candidates for the structure of space.

Although many French scientists were influenced by Poincaré's conventionalism, not all agreed that Euclidean geometry was always to be preferred because of its simplicity. Auguste Calinon (1850–1900), a mathematician and philosopher, argued that the different geometrical systems were not physically equivalent. It was, he maintained, legitimate to ask about the particular geometry that is realized in the physical world. And yet, although he spoke of astronomical measurements of celestial triangles as a "… mode of verification …" of Euclidean geometry, he may not have believed that a non-Euclidean structure of space might ever be revealed observationally. Calinon said (1889: 595; my English translation):

> All that can legitimately be concluded, is that the differences which might exist between Euclidean geometry and that realized by the universe are due to experimental error.

In a later paper, Calinon (1893) argued in agreement with Poincaré that astronomical problems might be approached with the kind of geometry most suited to produce a simple solution. The choice of geometry might vary from one problem to another, he suggested, and even from one area of the Universe to another.

A contemporary of Calinon, the mathematician Paul Barbarin (1855–1931), was a prolific writer on non-Euclidean geometry. Contrary to Poincaré, he was an empiricist in the sense that he believed that the geometry of space was a question that could, and could only, be determined observationally. This is what he argued in his book of 1902, *La Géometrie Non-Euclidienne*, which included a chapter on what he

called geometrical physics (Barbarin, 1902: 81-86). According to the French geometer, measurements of very small stellar parallaxes indicated that the radius of curvature exceeded 400,000 AU, which made him conclude that our part of the Universe might possibly be curved. On the other hand, it might just as well be Euclidean, and from a practical point of view there was not as yet any means of distinguishing between the two possibilities. Barbarin derived formulae for celestial triangles that could in principle distinguish between the three geometries associated with the names of Euclid, Lobachevsky and Riemann. However, he had to admit that his formulae were of no practical value as they relied on angle measurements much more precise than 0.01″. Yet he optimistically expressed his belief that the problem would be solved in the near future, thanks to the rapid progress in astronomical observational technology.

The works of French mathematicians such as Poincaré, Calinon and Barbarin were basically geometrical exercises rather than contributions to astronomy. Tellingly, they did not refer to values of stellar parallaxes or other astronomical data. From an astronomical point of view they were barren, doing nothing to change the general opinion of *fin-de-siècle* scientists, such as the mathematician-philosopher Bertrand Russell (1872–1970) summarized it in a dissertation of 1897:

> Though a small space-constant is regarded as empirically possible, it is not usually regarded as probable; and the finite space-constants with which Meta-geometry is equally conversant, are not usually thought even possible, as explanations of empirical fact. (Russell, 1897: 53).

This was indeed the consensus view at the turn of the century, shared by the majority of astronomers and physicists. In his lecture course in Vienna on natural philosophy in 1903-1906 Ludwig Boltzmann (1844–1906) referred several times to the possibility of a positively-curved stellar Universe. He found it fascinating that in principle an answer might be obtained by measurements of heavenly triangles with stars at their vertices:

> The spherical non-Euclidean space is completely closed in itself; it is not infinite, but has some finite size. If we know how large the triangles must be to correspond to a certain deviation from the sum of angles 180°, then we could also construct the size of the entire universe. We would then have a space which ends nowhere and as a whole returns into itself. (Fasol-Boltzmann, 1990: 215; my English translation).

He thought this was a perspective that offered "… enormous logical advantages." (ibid.). But logic is one thing; empirical reality is another. While in one of his lecture notes Boltzmann considered a closed Universe to be not only possible, but even probable, in a later note he held it to be "… not likely, yet it is a possibility that measurements of the stars will prove space to be non-Euclidean." (Fasol-Boltzmann, 1900: 255; my English translation).

8 TWO GERMAN ASTRONOMERS

Contrary to his French contemporaries and most other scientists at the turn of the century, young Karl Schwarzschild (1873–1916) considered curved-space astronomy a possibility that deserved serious attention.

He was a student of the distinguished Munich astronomer Hugo von Seeliger (1849–1924), according to whom non-Euclidean geometry could not possibly be useful in elucidating questions relating to physics, astronomy or cosmology. Space, Seeliger claimed, was nothing but an abstract reference system and devoid of properties of any kind. He consequently warned against

> … the common and therefore very fatal misapprehension that one … [is] able to decide by measurement which geometry is the 'true' one, or even, which space is the one we live in. (Seeliger, 1913: 200; my English translation).

Schwarzschild (Figure 5) disagreed with his former professor.

In an important lecture given on 9 August 1900 to the Astronomical Society in Heidelberg, Schwarzschild discussed from a modern perspective what Lobachevsky had done much earlier, namely, how to determine the geometry of space from observations. As one among other possible observational tests, he mentioned star counts relating the number of stars to their magnitudes:

> I have found that the number grows with magnitude more slowly in pseudospherical [hyperbolic] space, and more quickly in elliptic space, than under the same assumptions in Euclidean space. (Schwarzschild, 1900: 345; my English translation).

However, he focused on the classical case of parallax measurements.

While in Euclidean space the parallax, p, of a star infinitely far away is zero, in hyperbolic space there will be a minimal non-zero parallax that decreases with the curvature radius, R, such as shown by Equation (2). Let the radius of the orbit of the Earth be r, then $p \geq r/R$, as shown already by Lobachevsky in 1829. Thus, a measurement of the smallest known parallax imposes a lower limit on R. Schwarzschild estimated $p_{min} = 0.005''$, from which he concluded that $R > 4 \times 10^6$ AU. The bound, corresponding to about 20 parsecs or 60 light years, was an order of magnitude higher than the one estimated by Lobachevsky. Schwarzschild commented:

> Thus the curvature of the hyperbolic space is so insignificant that it cannot be observed by measurements in the planetary system, and because hyperbolic space is infinite, like Euclidean space, no unusual appearances will be observed on looking at the system of fixed stars. (Schwarzschild, 1900: 342; cf. Schemmel, 2005).

With regard to positively-curved space, Schwarzschild argued that the spherical case would lead to physically-unacceptable consequences, and for this reason he discussed only the elliptic possibility. In this case there are no infinite distances, and every parallax, including $p = 0$, corresponds to a finite distance. The relevant formula replacing $p \geq r/R$ is

$$\cot \frac{d}{R} = p \frac{R}{r} \qquad (4)$$

where R is real and d is the distance from the object (star) to the observer along a geodesic. Contrary to the hyperbolic case, "… it is a mistake to believe that a limit for R can be found simply from measurements of the parallax of fixed stars." (Schwarzschild, 1900: 342; my English translation). Therefore, physical considerations were needed to determine the minimal value of

R. Based upon star catalogues, he argued that all stars having a parallax smaller than $0.1''$ were located within a finite volume, and from this, and by assuming a uniform distribution of the stars, he reached the conclusion that $R = 1.6 \times 10^8$ AU = 2500 light years.

Schwarzschild further pointed out that in elliptic space a ray of light will return to its starting point after having traversed the world. We should therefore expect to see an antipodal image of the Sun, a 'counter-Sun', identical to our ordinary image of it but in the opposite direction. Of course, no such second image of the Sun is observed, a problem that Schwarzschild solved, or explained away, by assuming a suitable absorption of light in interstellar space. He summarized his results as follows:

Figure 5: Karl Schwarzschild, 1873–1916 (after Runge, 1916: 545).

> One may, without coming into contradiction with experience, conceive the world to be contained in a hyperbolic (pseudo-spherical) space with a radius of curvature greater than 4 000 000 earth radii, or in a finite elliptic space with a radius of curvature greater than 100 000 000 earth radii, where, in the last case, one assumes an absorption of light circumnavigating the world corresponding to 40 magnitudes. (Schwarzschild, 1900: 345; my English translation).

He saw no way to go further than this rather indefinite conclusion and decide observationally whether space really has a negative or positive curvature, or whether it really is finite or infinite. Nonetheless, from a philosophical point of view he preferred a closed Universe. It would, he said, be "… satisfying to reason …" if we could conceive of

> … space itself as being closed and finite, and filled, more or less completely, by this stellar system. If this were the case, then a time will come when space will have been investigated like the surface of the earth, where macroscopic investigations are complete and only

the microscopic ones need continue. A major part of
the interest for me in the hypothesis of an elliptic space
derives from this far reaching view. (Schwarzschild,
1900: 342; my English translation).

In his systematic discussion of a curved cosmic space
there was one assumption that he, contrary to Zöllner
nearly thirty years earlier, failed to mention, namely,
that the Universe had existed for an eternity of time.
But this was an assumption rarely questioned or even
mentioned at the time, and one that also went unquest-
ioned in the early relativistic models of the Universe.

While Schwarzschild's paper of 1900 is well known,
an interesting paper by Paul Harzer (1857–1932) eight
years later has rarely if ever received mention in the
literature on history of astronomy. The reason may be
that it was published in a mathematical and not an
astronomical journal. It deserves to be better known,
for Harzer, a Professor of Astronomy at the University
of Kiel, went further than Schwarzschild's investiga-
tion by extending it to the distribution of stars. Start-
ing in 1898, Seeliger had developed a model of our
Galaxy by means of an elaborate mathematical analy-
sis of star counts and stellar magnitudes (Paul, 1993).
While Seeliger based his 'statistical cosmology' on the
unstated assumption of Euclidean space, in a lecture of
1908 Harzer transformed the calculations to a space of
constant positive curvature. In this way he arrived at a
modified picture of our Galaxy.

Harzer's stellar Universe was enclosed in a finite
cosmic space with a volume about seventeen times that
of the stellar system. As to this stellar system, it con-
tained the same number of stars but was compressed to
a size approximately one half of what it had in See-
liger's infinite Euclidean space. The size of the entire
Universe was given by the time it took for a ray of
light to circumnavigate it, which Harzer estimated to
be 8,700 years. During its travel round the world the
light would became dimmer because of absorption,
and by taking into account the motion of the Solar
System he arrived at a loss in light intensity corre-
sponding to thirteen magnitudes. This was a more rea-
listic value than Schwarschild's forty magnitudes, yet
it was sufficient to make the problem of the counter-
Sun go away.

Harzer took the model of a closed stellar Universe
no less seriously than Schwarzschild, but of course he
realized that it was hypothetical and lacked the support
of solid observational evidence. Consequently, his
conclusion was cautious:

> This picture includes no features that can be character-
> ized as improbable … But the picture speaks of the
> *possibility* of the finite space only, not of its *reality*, and
> as yet we have no evidence for this reality. (Harzer,
> 1908: 266; his italics; my English translation).

The Schwarzschild-Harzer suggestion of a closed
space filled with stars had the conceptual advantage
that it did away with the infinite empty space, but it
made almost no impact on mainstream astronomy.
The cosmological problem that moved to the forefront
of astronomy in the 1910s was concerned with the size
of our Galaxy and the question of whether the spiral
nebulae were external objects or belonged to our
Galaxy. This was a problem in which the geometry of
space was considered irrelevant. When it was finally
solved in the mid-1920s it was by observational
means, namely, Edwin Hubble's (1889–1953) famous

discovery of Cepheid variables in the Andromeda
Nebula (Hubble, 1925; cf. Berendzen, Hart, and See-
ley, 1984).

9 CONCLUSION

Whereas non-Euclidean geometry flourished as a
mathematical research field in the last half of the
nineteenth century, its connection to the real space
inhabited by physical objects was much less cultivated.
The large majority of mathematicians did not care
whether real space was Euclidean or not; and those
who did care only dealt with the subject in a general
and often casual way, and avoided dealing seriously
with the possibility of determining a space curvature
different from zero. After all, that was supposed to be
the business of the astronomers. While some mathe-
maticians, following Poincaré, declared the problem
meaningless, others admitted that in principle space
might be curved—but in principle only—and left it at
that.

Most astronomers were well aware of the possibility
of space being non-Euclidean, but it was considered a
remote possibility and not one that would keep them
awake at night. Astronomy and cosmology books in
the early twentieth century usually presented the mat-
erial world as consisting of a huge conglomerate of
stars, essentially our Galaxy, floating in the infinite
Euclidean space. What might be beyond the stellar
system was left to speculation. It might be empty
space or some ethereal medium, in any case it was
regarded as irrelevant from an astronomical point of
view. As the historian and astronomy author Agnes
Mary Clerke (1842–1907) expressed it, "With the pos-
sibilities beyond, science has no concern… " (Clerke,
1890: 368).

Astronomers had their own reasons, different from
those of the mathematicians, to ignore non-Euclidean
geometry. Lack of awareness of the new forms of
geometry or lack of mathematical competence were
not generally among the reasons as many astronomers
had strong backgrounds in mathematics and were con-
versant with the technicalities of non-Euclidean geo-
metry. But while the motion and properties of celestial
bodies were definitely the business of the astronomers,
the space in which the bodies move was not seen as
belonging to the domain of astronomy. It was a kind
of 'nothingness' that philosophers could speak of, and
did speak of. Newcomb (1898: 5) probably spoke for
the majority of his colleagues when he warned against
"… the tendency among both geometers and psycho-
ogists to talk of space as an entity in itself." To arouse
interest in the astronomical community, theories of
non-Euclidean space would have to be observationally
testable or offer opportunities for solving problems of
astronomical relevance. They scored badly on both
counts.

Even though non-Euclidean geometry was thought
to have little or no explanatory force, there was the
possibility that it could be verified by measurements.
While it could never be proved that space was Euclid-
ean, it could conceivably be proved that it was not. As
we have seen, a few astronomers and other scientists—
such as Ball, Newcomb, Peirce, Barbarin and Schwarz-
schild—did take an interest in this line of reasoning,
going back to Lobachevsky. However, while in the
early years of the twentieth century it was realized that

the curvature of space was indeed measurable, it was also realized that the kind of upper bound for the curvature that measurements allowed was ineffective to distinguish curved from flat space. Under these circumstances, it is no wonder that astronomers saw no reason to abandon the intuitively pleasing Euclidean space that had served their science so well in the past. Even should space be curved, the curvature radius would be so large that for all practical purposes it was infinite, that is, space could be considered Euclidean. So why bother? It seems that the main reason for the astronomers' reluctance to consider the consequences of space being non-Euclidean was just this: they had no need for the hypothesis.

10 EINSTEINIAN POSTSCRIPT

Although this review is limited to the pre-relativity era it would not be out of place to recall that the question of curved space entered a wholly new phase with Albert Einstein's (1879–1955) General Theory of Relativity. The observational evidence for curved space that was still missing at the time of Schwarzschild and Harzer first turned up in 1919 with the detection of the bending of starlight in the famous Eddington-Dyson solar eclipse expedition. Of course, this was a local curvature of space caused by the Sun's gravitational field and not a proof that global space is positively curved. Einstein's General Theory of Relativity revolutionized cosmology, but it did not and cannot provide an answer to the old question of whether cosmic space is closed or not, or finite or not. The present consensus view is that we live in a flat infinite space, yet (as Lobachevsky was already aware of) this is a view that can never be proved observationally. Another question that turned up in physical theory in the 1920s was the number of space dimensions, although this question was more discussed in the context of microphysics than in a cosmological context (Wünsch, 2010).

In early 1921 Einstein gave a brilliant address to the Prussian Academy of Sciences in which he reflected on the relationship between mathematics and the physical sciences (Einstein, 1982: 233). He famously stated that "… as far as the propositions of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality." Einstein distinguished between what he called 'practical geometry' and 'purely axiomatic geometry', arguing that while the first version was a natural science, the second was not, and

> The question whether the universe is spatially finite or not seems to me an entirely meaningful question in the sense of practical geometry. I do not even consider it impossible that the question will be answered before long by astronomy. (Einstein, 1982: 239).

Indeed, without this view of geometry, he continued, "I should have been unable to formulate the theory of [general] relativity." (Einstein, 1982: 235).

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