GAUSS AND NON-EUCLIDEAN GEOMETRY

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Introduction

The claim, made on Gauss's behalf, that he was a, or even the, discoverer of non-Euclidean geometry is very hard to decide because the evidence is so slight. It is nonetheless implicit in the excellent commentaries of Stäckel [23] and Dombrowski [7], as it is in Reichardt's book [21] and the broader but slighter survey by Coxeter [6]. Proponents of this view, with Dunnington, are happy to tie documents written in the late 1820s and 1830s to cryptic claims made by Gauss for early achievements, and to equally elusive passages from the 1810s. In fact, the evidence points in another direction. It suggests that Gauss was aware that much needed to be done to Euclid's *Elements* to make them rigorous, and that the geometrical nature of physical Space was regarded by Gauss as more and more likely to be an empirical matter, but in this his instincts and insights on this occasion were those of a scientist, not a mathematician.

I shall also argue that the whole question of what it is to discover a new geometry of space requires much careful thought, and if definitive agreement on the matter cannot be reached – and perhaps it cannot – then at least positions can profitably be made explicit. This has not usually been done when discussing the discovery of non-Euclidean geometry.

1. The evidence

Gauss was 22 when he confided to Wolfgang Bolyai that he was doubtful of the truth of geometry. He had already found too many mistakes in other people's arguments in defence of the parallel postulate to be so confident any longer in their conclusion. He had begun to consider the fundamental assumptions of geometry at least two years earlier, in July

27, 1797, when he wrote in his Mathematical Diary only too cryptically that he had 'demonstrated the possibility of a plane'. It is tempting to connect this with fragments of arguments dating from 1828 to 1832 in which Gauss investigated whether the locus of a line perpendicular to a fixed line and rotating about that fixed line has all the properties of a plane, because, in a famous letter to Bessel of January 1829, where Gauss claims to have harboured these thoughts for almost 40 years, he wrote that

"apart from the well-known gap in Euclid's geometry, there is another that, to my knowledge no-one has noticed and which is in no way easy to alleviate (although possible). This is the definition of a plane as a surface that contains the line joining any two of its points. This definition contains more than is necessary for the determination of the surface, and tacitly involves a theorem which must first be proved..." [8, Gauss to Bessel, VIII, p. 200].

One knows from the later history of geometry, most clearly from the remarks of Pasch ([20]) that trying to spell out what exactly elementary Euclidean geometry is about is extremely difficult.

By 1808 Gauss was aware that in the hypothetical non-Euclidean geometry similar triangles are congruent, and therefore there is an absolute measure of length. But at this stage, according to Schumacher, he found this conclusion absurd, and therefore held that the matter was still unclear. As he put it in 1813: "In the theory of parallels we are no further than Euclid was. This is the shameful part of mathematics, that sooner or later must be put in quite another form". Evidently he did not then feel confident in a non-Euclidean geometry. By April 1816 he had shifted his opinion

"It seems to be something of a paradox that a constant line can at the same time be given a priori, but I find nothing self-contradictory in this. It would be remarkable if Euclid's geometry were not true, because then we would have a general a priori measure [of length], for example one could take as the spatial unit the length of the side of an equilateral triangle with angle $59°; 59'59.99999''$ [8, Gauss to Gerling, VIII, pp. 168-169].

As Dunnington correctly observed, being remarkable is consistent with being attractive. But still there is no evidence that Gauss deduced anything specific about the new geometry.

In 1816 we do get a glimpse of what Gauss knew as reported by his former student Wachter. On a certain (unspecified) hypothesis, Wachter wrote to Gauss, the opposite of Euclidean geometry would apparently be true, which would involve us with an undetermined constant, a sphere of infinite radius which nonetheless lacks some properties of the plane, and the use of a transcendent trigonometry that probably generalises or underpins spherical trigonometry. Gauss now, as he wrote to Olbers in April 1817, was coming

"ever more to the opinion that the necessity of our geometry cannot be proved, at least not with human understanding. Perhaps in another life. ...but for now geometry must stand, not with arithmetic which is pure a priori, but with mechanics." [8, Gauss to Olbers, VIII, pp. 177].

The 'transcendent trigonometry' is usually taken to be the hyperbolic trigonometry appropriate to non-Euclidean geometry, but there is very little evidence to support any interpretation. Accordingly, when Gauss replied to Schweikart in March 1819 that he could "do all of astral geometry once the constant is given" we cannot be sure what precisely, Gauss had formulae for. The only one dated to this period is the one in his reply to Schweikart for the maximum area of a triangle in terms of Schweikart's Constant (the maximum altitude of an isosceles right-angled triangle).

All that his correspondence with Taurinus reveals is that, by 1824, Gauss was more comfortable than ever with the idea of a new geometry. In November 1824, in the course of explaining his views about non-Euclidean geometry, and his reluctance to be drawn in public, Gauss wrote to Taurinus that

"...the assumption that the angle sum is less than 180° leads to a geometry quite different from Euclid's, logically coherent, and one that I am entirely satisfied with. It depends on a constant, which is not given a priori. The larger the constant, the closer the geometry to Euclid's and when the constant is infinite they agree. The theorems are paradoxical but not self-contradictory or illogical. [...] All my efforts to find a contradiction have failed, the only thing that our understanding finds contradictory is that, if the geometry were to be true, there would be an absolute (if unknown to us) measure of length [...] As a joke I've even wished Euclidean geometry was not true, for then we would have an absolute measure of length a priori." [8, Gauss to Taurinus, VIII, pp. 187].

But, Gauss went on, to reject the geometry on that ground would be to confuse the unnatural with the absolutely impossible.

There is very little evidence of Gaussian contributions to trigonometry in non-Euclidean geometry before the letter to Schumacher of 12 July 1831, where he says that the circumference of a semi-circle is $\frac{1}{2}\pi k\left(e^{r/k} - e^{-r/k}\right)$ where k is a very large constant that is infinite in Euclidean geometry. In particular, there is no evidence that Gauss derived the relevant trigonometric formulae from the profound study of differential geometry that occupied him in the 1820s. What he did say in the *Disquisitiones generales circa superficies curvas* [1828] is summed up in what he regarded as one of the most elegant theorems in the theory of curved surfaces:

"The excess over 180° of the sum of the angles of a triangle formed by shortest lines on a concavo-concave surface, or the deficit from 180° of the sum of the angles of a triangle formed by shortest lines on a concavo-convex surface, is measured by the area of the part of the sphere which corresponds, through the direction of the normals, to that triangle, if the whole surface of the sphere is set equal to 720 degrees." [8, IV, p. 246]

What weight can two trigonometric formulae be made to carry? They are not difficult to obtain and manipulate if, as for example Taurinus did, one assumes that non-Euclidean geometry is described by the formulae of hyperbolic trigonometry – a natural enough assumption. To introduce hyperbolic trigonometry into the study of non-Euclidean geometry properly is, as Bolyai and Lobachevskii found, a considerable labour of which no trace remains in Gauss's work. It is more plausible to imagine that he made the assumption, but did not derive it from basic principles. So, perhaps by 1816, or, at the latest, 1824, Gauss was convinced of ideas like these:

- there could be a non-Euclidean geometry, in which the angle sum of triangles is less than π,
- the area of triangles is proportional to their angular defect and is bounded by a finite amount,
- the trigonometric formulae for this geometry are those of hyperbolic trigonometry, and the analogy with spherical geometry and trigonometry extends to formulae for the circumference and area of circles.

2. The question of the empirical test

Did Gauss, however, as a scientist, make an empirical test of the matter? This is one of the most discussed questions in the whole subject of Gauss and non-Euclidean geometry. Those who believe that he did quote Sartorius von Waltershausen's reminiscence, where on p. 81, he states that Gauss did check the truth of Euclidean geometry on measurements of the triangle formed by the mountains Brocken, Hohenhagen, and Inselsberg (BHI), and found it to be approximately true. This claim was most recently advanced by Scholz [22], on the basis of a number, quoted by von Waltershausen elsewhere in his reminiscence, relating to the very close agreement between the measurements of this triangle and the predictions of Euclidean geometry (once the mountain tops are treated as three points on a sphere). Scholz concludes that "there is no longer any reason to doubt that Gauss *himself* conducted such a test of the angle sum theorem." (Scholz [22, p. 644]).

Those who dispute that Gauss made such a test argue that the problem that occupied Gauss, and figures so prominently at the end of the *Disquisitiones generales circa superficies curvas*, is the question of the spheroidal or spherical shape of the Earth, and that von Waltershausen was simply confused about the hypothesis that Gauss found to be approximately confirmed. This is the opinion of Miller [17].

The most thorough analysis of the question is Breitenberger [5]. He confronts the question: 'if von Waltershausen was not simply confused in some way, what was he saying?' and he gives it an elegant answer. Surveying Hanover threw up many triangles and many numbers (a figure of a million is sometimes mentioned). Conclusions were drawn (and maps made) on numbers which are the result of many calculations, and at every stage discrepancies between real and expected results lay within expected error bounds (Gauss analysed the errors quire carefully). Not only was Euclidean geometry never called into question, because the errors were only what was to be expected, each calculation amounted to a tacit defence of Euclidean geometry. But the measurements of the BHI triangle were not fed into such a mill. They show that, within experimental error, Space is described by Euclidean geometry. To be sure "as a single instance it proves very little, but it has been designed so as to be transparent, and hence it will drive a point home" (Breitenberger [5, p. 288]). Newton dropped an apple in conversation to similar purpose and effect. The myth, Breitenberger concludes, is that the BHI triangle was surveyed as part of a *deliberate* test of Euclidean geometry. But it did incidentally show that Euclidean geometry is true to within the limits of the best observational error of the time. Put that way, the gap between Scholz and Breitenberger may be quite small.

3. Interpretations

What then is it to discover – or, if you prefer, invent – non-Euclidean geometry? One way of thinking about this question must be ruled out straight away. Had it just been a question of exhibiting an axiom system for something fairly geometrical, then spherical geometry would have done. One needs, of course, to strike out two of Euclid's axioms: the parallel postulate and the indefinite extendibility of the straight line. That this was not done suggests that the question in 1800 was not one about 'axioms for geometry'. It makes it clear that what was to be investigated was the geometry of physical space. The ongoing question was not 'Is the parallel postulate independent of the other axioms of geometry'?, but 'Is the parallel postulate independent of the other axioms of geometry when giving an account of space?'. This is a different enterprise from the much more overtly logical one in fashion around 1900. But in 1900 axioms were very fashionable, Hilbert's *Grundlagen der Geometrie* was not the only book to exemplify the merits of thinking axiomatically, and from first to last, Bonola's account of the origin and development of non-Euclidean geometry is rooted in an analysis of axioms; their equivalence and their independence. It was published in Italian in 1906 and has become the standard account of the subject in English (into which it was translated in 1912), and it has many merits. Indeed, an Italian geometer, and a pupil of Enriques, writing between 1900 and 1911, would naturally see geometry as a matter of axioms, and so see history as a history of axioms.

It will bring the question into sharper focus if we ask a seemingly absurd other question: Why do we not simply say that non-Euclidean geometry was discovered by Ferdinand Karl Schweikart, the professor of Jurisprudence at Marburg, and communicated to Gauss in 1818? If the answer is simply that Gauss already knew what Schweikart told him, then what Gauss knew in 1818 counts as a discovery, and Bolyai and Lobachevskii are condemned to come second. They have only the honour of being the first to publish – and much good did it do them. A better reply would be that what Schweikart did is too flimsy, that it had no chance of convincing anyone, and does not amount to the sort of activity that counts as a discovery. It is validated only after the event, by the subsequent discovery of a system of geometry that is, in all its glory and detail, what Schweikart had merely glimpsed. Nothing in his account should have swayed the prudent sceptic, unwilling to abandon Euclid for a wish list of new figures.

Any one seriously interested in the question of non-Euclidean geometry by the 1810s could have found out quite a lot just by asking around and reading in a decent library. Certainly, people were interested. 'Not a year goes by', Gauss himself wrote in a book review, 'without a book being published on the parallel postulate' [8, VIII, p. 170]. The problem had a striking degree of visibility. Even if one assumes, as is likely, that German authors only read Germans, and the French only French, that leaves the many attempts of Legendre and the open-ended speculations of Lambert. Both of these were major authors, and one supposes that Legendre's repeated attempts hinted clearly enough that he was not convinced of any but his most recent version. Lambert, even more clearly, knew that his attempts had not led to the defence of the parallel postulate that he sought, and that the matter remained unresolved. If one concluded that all this failure was in the nature of the problem, that indeed a specific non-Euclidean geometry was possible, one could copy out quite a number of oddities and simply proclaim them, not as steps on the way to the final refutation, but as fine new theorems. One could be courageous in so doing, or simply giving up too soon from a failure to see deeply enough into the problem.

The fairest answer is surely that to discover non-Euclidean geometry is to describe a system of geometry that is convincing and persuasive (even if it did not, in its day, persuade, and even if by the much higher standards of a later time it was in fact imperfect). Applied to Schweikart and his memorandum, this leads to what is indeed the charitable consensus, which is to grant Schweikart the courage, and the freshness of mind to see what too many had not been willing to see (and what, indeed, his nephew Taurinus was to avert his gaze from some six years later). But one cannot find in his work that persuasive character required in a true discoverer.

But if a mere memorandum does not clinch it, did Gauss know so much more that he could claim the prize? Does his ability to draw together Schweikart's ideas better than Schweikart had done rest on sufficiently more than the lawyer already possessed? If we recall that Gauss's reply to Schweikart is only too close to the famous reply he was to send the Bolyais in its assertion that he knew the ideas already, then we might conclude there was a chance that sending good ideas to Gauss was, at the very least, likely to stimulate him to add to what he already knew, and quite innocently assume that he was simply restating what he had known for some time. The point is worth noting, because as we have seen, there is no other evidence to corroborate his assertions that at these dates he had a coherent theory drawing together the consequences of assuming some standard definition of an alternative geometry to Euclid's.

We have to ask: What kind of knowledge, at what time, is Gauss to have before he can be regarded as a true discoverer of non-Euclidean geometry? Is it a worked-out theory that looks consistent and has some chance of being true of Space? Is it a deep empirical dissatisfaction with Euclidean geometry as an account of Space? What degree of conviction should it impart to all but the most logically, even legalistically, minded? In what ways should it surpass Schweikart's level of knowledge or belief? How much should it resemble what Bolyai and Lobachevskii were to do? Or was it, perhaps, significantly different?

When, in 1808, Gauss was aware that in the hypothetical non-Euclidean geometry similar triangles are congruent, and therefore there is an absolute measure of length, he found this conclusion absurd, and therefore held that the matter was still unclear. This is a long way from believing that there is indeed a meaningful non-Euclidean geometry.

The historical sources then go quiet until 1816, and there is no evidence at all. In 1816 Gauss wrote to Gerling that the idea of an absolute measure of length is somewhat paradoxical but not self-contradictory, and that it would be remarkable if Euclid's geometry was not true, because then we would have an a priori measure of length. As Dunnington correctly observed, being remarkable is consistent with being attractive.

Also in 1816 there is a glimpse of what Gauss knew as seen through Wachter's eyes. But it is less convincing than is often thought. It is hard to know precisely what Gauss's letter to Olbers (quoted above) actually means. 'Our geometry' is surely Euclidean geometry. Gauss was a devout man, not given to presuming to understand the mind of the Divinity. Human understanding would inevitably fall short of God's. But what would a proof – of a kind that surpasses human understanding – be that establishes the necessity of our (Euclidean) geometry? How would it differ from a proof that does not surpass human understanding? Would it be some argument, compelling even to God, that made Euclidean geometry the right geometry for Space? Would Gauss understand it in the afterlife, or is merely a further century of this mortal life going to be enough for somebody to resolve the matter? Arithmetic, it seems, has an apodictic status. It is pure a priori. Whatever that might mean, whether Kantian or Friesian terminology should be imputed here or some equation of arithmetic with logic, the truth of arithmetic is being said to be of a higher kind than the truth of geometry, which is down with mechanics. The problem is the twist of thought conveyed in the two words 'for now'. The sense of the passage is that the status of the truth claims of Euclidean geometry is unclear, and might remain so either forever, or only for a while. The passage does not say that there *are* two geometries at some logical level and some experiment must choose between them. That would be the meaning of a remark like this: "... the necessity of our geometry cannot be proved. Geometry stands, not with arithmetic which is pure a priori, but with mechanics." The passage is agnostic, not heretical. Knowledge is lacking, says Gauss; he does not claim to possess new knowledge, of a new geometry. In the context, that surely means that even the ideas he was discussing with Wachter he considered to be hypothetical, and capable of proving to be false.

The 'transcendent trigonometry' is usually taken to be the hyperbolic trigonometry appropriate to non-Euclidean geometry, but there is very little evidence to support any interpretation. The geometry apparently depends on an undetermined constant, which indeed non-Euclidean geometry does, but the implication that this constant was unproblematic is unwarranted. Why should it not yield to two incompatible determinations, and thus the contradiction most previous writers on the subject had hoped to find?

On 25 January 1819 Gerling passed Schweikart's note on to Gauss, who replied in March that he was "uncommonly pleased with the note, and can do all of astral geometry once the constant is given." The angle sum of a triangle is proportional to its angular defect, and there is a maximum area for triangles which is attained by trebly asymptotic triangles. This does not make clear if Gauss now possessed formulae from which all the elementary mensuration of a non-Euclidean geometry can be derived, or if he knew only that part which resembles Euclidean geometry. What, precisely, was covered by the phrase 'all of astral geometry'? And whatever Gauss was claiming, it leaves the time of his discoveries vague. Should it be part of the transcendent trigonometry that he discussed in 1816 with Wachter? Could it be something that only came to him in the year between receiving Schweikart's note and replying to it? The first alternative is the more likely, but even if it is true the trigonometry per se does not seem to have clarified everything in Gauss's mind.

In November 1824, writing to Taurinus, Gauss said that "...the assumption that the angle sum is less than 180° leads to a geometry quite different from Euclid's, logically coherent, and one that I am entirely satisfied with." Taurinus, of course, was looking for reasons to deny the possibility of non-Euclidean geometry, and Gauss did not want to lend his name in any way to that enterprise. But even if we allow that by 1824 Gauss was comfortable with a novel geometry for space, we learn nothing from this letter about what theorems the new geometry might contain, and what its implications might be for science, geodesy, or astronomy.

There is an unfortunate disparity throughout this part of Gauss's work between the material that can be precisely dated, and his claims in letters to his friends that he has known this or that fact for 30, even 40 years. These claims may well be true, in the sense that the teen-age Gauss could well have found some arguments in elementary geometry inconclusive and begun to harbour suspicions about them. They are harder to square with interpretations that impute specific knowledge to the young man, especially when they contradict later, datable, evidence. For example, there is a proof which Gauss himself noted he found on 18 November 1828 that the angle sum of a triangle cannot exceed 180°. This may mark the occasion on which Gauss discovered what, for him, was a new and particularly perspicacious proof of a result he already knew. Equally well, it may mark the discovery of a proof of something he had hitherto only suspected.

The same is true of the passages on the theory of the line and the plane, which Stäckel very plausibly dated to the years 1828-1832. Here Gauss noted that many of the statements mathematicians make about the plane conceal theorems which, if not difficult, are not entirely trivial either. The assumption that Gauss had thought his way through this tangle in the 1790s is unwarranted; what then was he doing writing it down in the late 1820s? The same is true of the notes on the theory of parallel lines, which Stäckel dated to 1831. Stäckel connected them to letters that Gauss wrote to Schumacher in May of that year, where Gauss explained that he had had these ideas for 40 years but never written them down. It is hard to believe that Gauss could write down, after such a long period of time, exactly what he had had in mind when he was 14. If one takes 40 as a round number, even if one replaces it with 32 (because in 1799 Gauss was involved with the elder Bolyai on the question of the foundations of geometry), it is still implausible that Gauss was just writing down ideas from memory. It is not what good mathematicians do. They polish the ideas even as they recall them, and writing for themselves alone, as they are, they are under no obligation to prevent subsequent historians from becoming a little confused. We can grant that Gauss, as he said, was already concerned in the 1790s about the foundations of geometry, without supposing that the notes of 1831 are a true and faithful record of what he had believed so many years before.

On the other hand, it is worth noting that these pages are firmly in a style which may be called classical, with the implication that adherence to a classical formulation denies trigonometric methods a fundamental role. A 'classical style' or 'classical formulation' is an approach to geometry that regards terms such as point, line, plane, distance, and angle as undefinable, primitive terms. The relationship between them, the properties of these objects and of figures composed of them, may be obscure and in need of elucidation, an author in this style may feel that basic questions may have been begged by all previous writers, but the fundamental terms are not to be reduced to others (for example, numbers). The disentangling of the ideas will be done by patient combing, not by radically new definitions. Gauss's surviving early notes are very much in this classical style.

Of course, the problem of parallels is prominent among these difficulties. But the famous letter to Bessel of January 1829, where Gauss claims to have harboured these thoughts for almost 40 years, does not give parallels pride of place. There Gauss wrote that he hadn't written up his extensive researches, and perhaps he never would, because he feared the howl of the Boetians, however "my opinion that we cannot ground geometry completely a priori has become, if anything, even stronger."

Would it be fair to point out that, to someone of Gauss's high standards, a strong opinion becoming even stronger is still an opinion that falls short of certainty? And while the well-known gap is that staple of the literature, the problem of parallels, but it is firmly situated here, as it is throughout the years 1828 to 1832, in the context of a number of other problems in the foundations of geometry. Bessel's reply encouraged Gauss to state that geometry has a reality outside our minds whose laws we cannot completely prescribe a priori. This is entirely consistent with the classical formulation. The concepts of point, line, plane and so forth are formed in whatever way concepts are generated (by abstraction, through experience, introspection, it doesn't matter) and whatever problems there might be in saying how this is done belong to philosophy, not mathematics. The tacit implication is that forming these concepts is among the simpler pieces of concept formation people do, and can be treated, for mathematical purposes, as entirely unproblematic. The task of the mathematician is to get them truly clear in the mind, which, Gauss suggested, is liable to involve one in two kinds of activity. One is teasing out tacit assumptions and fitting them up with proofs (Gauss did not suggest there were likely to be any erroneous beliefs). The other, which concerns parallels, is the elaboration of new ideas about a hitherto unsuspected species of geometry, which might nonetheless turn out to be (for some value of an unknown constant) the true geometry of Space.

4. Evidence in geodesy

Can one then connect transcendent trigonometry with Gauss's work on differential geometry and geodesy? The famous Hanover survey kept Gauss occupied for most of the three years from 1822 to 1825 (too much of it, he complained to Olbers in October 1825) and in the last three months of that year he wrote the *Disquisitiones*. The book is famous for the discovery that Gaussian curvature is intrinsic, which, in the *Disquisitiones*, has a very computational proof, giving the (Gaussian) curvature in terms of the coefficients of the first fundamental form and their derivatives with respect to the coordinates. He then went on to deduce the elegant theorem already quoted:

"The excess over 180° of the sum of the angles of a triangle formed by shortest lines on a concavo-concave surface, or the deficit from 180° of the sum of the angles of a triangle formed by shortest lines on a concavo-convex surface, is measured by the area of the part of the sphere which corresponds, through the direction of the normals, to that triangle, if the whole surface of the sphere is set equal to 720 degrees."

Now Gauss knew very well that the area of a non-Euclidean triangle is proportional to the deficit from 180° of the sum of the angles of the triangle. It would therefore be tempting to suppose that Gauss would connect this elegant theorem with the study of non-Euclidean geometry, by considering a concavo-convex surface of constant negative Gaussian curvature. But such a conclusion is highly speculative; Minding in 1839, produced just such a surface without making that connection, and Codazzi in 1859 even showed that on such a surface the appropriate trigonometric formulae are the hyperbolic analogues of the formulae in spherical trigonometry. Neither man observed the connection with non-Euclidean geometry.

If we grant that Gauss might have suspected that non-Euclidean geometry was the geometry on a surface of constant negative curvature, we must none the less note that Gauss did not develop the trigonometry of triangles on surfaces of constant (positive or negative) curvature until after 1840, when he had read Lobachevskii's *Geometrische Untersuchungen.* Moreover, he did not have such a surface to hand; there is every reason to suppose that Minding was the first to discover it. Minding's example, moreover, is a surface of revolution, and therefore has a number of topological properties that rule it out as a model of space, notably self-intersecting geodesics and pairs of geodesics that meet in more than one point. Minding's example also has singular points, at which the surface comes to a halt, which marks a significant difference from the sphere.

It is not at all obvious that these properties of Minding's surface do not suggest that there can, after all, be no surface obeying the rules of non-Euclidean geometry. Why should there not be local models of parts of a 'non-Euclidean' space, but no global model? But even if a surface had been known to Gauss, and even if (in contradiction to Hilbert's later theorem about surfaces of constant negative curvature) it had not had any unfortunate properties, what would it establish? Only that there is a surface in Space whose intrinsic geometry is non-Euclidean. It would not establish that Space could be non-Euclidean, because Space is three-dimensional. There is no sign that Gauss had any of the concepts needed to formulate a theory of differential geometry in three-dimensions.

While it is always dangerous to speculate on what Gauss could not do, it is worth noticing that adherence to the classical formulation makes thinking of novel three-dimensional geometries very difficult indeed, if not impossible. According to the classical formulation, Space is our source of knowledge of all primitive geometric terms. It is difficult enough to follow out the implications of this for surfaces in Space, to find formulae for geodesics on surfaces and so forth. At the end of such an analysis, Gauss discovered that although all geometric concepts are impressed on the surface from the ambient Space, some, such as curvature, are after all intrinsic. To imagine that there is another type of Space altogether with any chance of proving theorems about it, it would be best to take a big step backwards and consider how to do geometry at all. One cannot (easily, at least) think one's way from Euclidean three-dimensional Space to another three-dimensional Space. What, in the end, Riemann did, was to step back and create new ways of thinking about geometry that made it possible to think of many, many kinds of Space.

Riemann also produced the first pieces of essential mathematical machinery for doing differential geometry in three or more dimensions. There is no sign that Gauss did that, but without it almost nothing useful can be said.

To conclude: Gauss's work on differential geometry in the 1820s, remarkable as it is, does not connect with any kind of transcendent trigonometry. Nowhere in the work on differential geometry did Gauss even hypothesise, much less study, a surface of constant negative curvature. He did not do that until he had read Lobachevskii's *Geometrische Untersuchungen*, and even then the connection between the differential geometry and the trigonometry rests on the choice of the same symbol for a constant of integration as for the (Gaussian) curvature. This symbol, k, is real for spherical trigonometry, and purely imaginary for non-Euclidean trigonometry.

We are returned, empty-handed, to where we began. In 1816 Gauss possessed a form of trigonometry applicable to more than just spherical geometry. He had proposed a surface which was a sphere of infinite radius and was not a plane. By 1831 he knew that the circumference of a semi-circle of radius r is $\frac{1}{2}\pi k \left(e^{r/k} - e^{-r/k}\right)$. This formula, dropped so casually in a letter to Schumacher, together with the one in his reply to Schweikart in 1818 for the maximum area of a triangle in terms of Schweikart's Constant (the maximum altitude of an isosceles right-angled triangle), are the only evidence we have that Gauss knew anything about non-Euclidean geometry in detail.

As has already been remarked, these formulae are not difficult to obtain if one simply assumes that non-Euclidean geometry is described by the formulae of hyperbolic trigonometry. Could Gauss have defended that assumption? There is no evidence either way. Given Gauss's very high standards, he might have felt confident of the validity of such an assumption but not been able to defend it to his own satisfaction. To introduce hyperbolic trigonometry into the study of non-Euclidean geometry properly is, as Bolyai and Lobachevskii found, a considerable labour of which no trace remains in Gauss's work. It would seem reasonable to assume that he made the assumption, but not the derivation from basic principles.

Another factor that must be considered is Gauss's aversion to the new geometry, expressed in the references to howling Boetians, wasp's nests, and the like. The absence of evidence is consistent with Gauss not giving this truly difficult topic sufficient attention. There are many occasions when Gauss worked, over a period of years, to bring topics to his satisfaction, and others, the theory of elliptic functions, for example, when he pushed hard to achieve the right levels of insight and of generality, to obtain the most appropriate proofs. In these cases the result is often books, book length memoirs, and many pages of nearly publishable notes. When we turn to the problem of parallels, there is nothing like so much material, and one is driven to wonder if it ever existed.

5. The need to study three-dimensional space

Let us grant that, perhaps by 1816, Gauss was convinced that there could be a non-Euclidean geometry, a geometry in which the angle sum of triangles is less than π, and the area of triangles is proportional to their angular defect and is bounded by a finite amount. The trigonometric formulae for this geometry are those of hyperbolic trigonometry, and the analogy with spherical geometry and trigonometry extends to formulae for the circumference and area of circles. Such a position is unsatisfactory, to Gauss and to us, because it is purely and simply an analogy. What is lacking is any argument that starts from an idea of geometry and derives the formulae according to convincing rules. In the case of spherical geometry, there is a basis: the Euclidean geometry of three-dimensional Space. Ordinary arguments about how geodesics and angles may be defined on a sphere (a surface sitting in three-dimensional Space) then lead to the familiar formulae. In the non-Euclidean case, the basis of Euclidean three-dimensional geometry is in question, and there is no surface.

Why, after all, does talk of two-dimensional geometry matter when the nature of three-dimensional Space is at stake? Because one believes that there is no essential obstacle to going up one dimension. In the Euclidean case, there seems to be no reason to suspect a problem, and there is an abundance of results about three dimensions even in Euclid's *Elements*. The methods of Cartesian coordinate geometry seem adaptable. Plus, if you believe that Space is actually Euclidean, you *know* there is no obstacle. But in the non-Euclidean case, there may be no reason to suspect a problem but equally there is no reason to believe success is guaranteed. There are no pre-existing results about three-dimensional non-Euclidean geometry, no coordinate methods to adapt. Even if one found a surface in three-dimensional Euclidean space with non-Euclidean geometry as its induced geometry, that would not licence the inference that three-dimensional Space was non-Euclidean, any more than the existence of spheres in three-dimensional Space forces the conclusion that Space is a three-dimensional sphere.

A more plausible way forward would be to start with non-Euclidean three-dimensional Space, and to derive a rich theory of non-Euclidean two-dimensional space from it. This would be better evidence that the assumption that there could be a non-Euclidean three-dimensional space made sense (although not convincing, to be sure). This is what Bolyai and Lobachevskii did, but not Gauss. Gauss did believe that such an assumption made sense, but he equivocated about it. Its consequences were paradoxical, but not self-contradictory; as a joke he might even wish it were true; its existence implies that the laws of geometry cannot be prescribed a priori.

The only hint that Gauss explored the non-Euclidean three-dimensional case in order to obtain new, suggestive, results about non-Euclidean two-dimensional geometry is the remark by Wachter about the sphere of infinite radius. This is a remark by Wachter, a mathematician of whom Gauss had a good opinion, and one must therefore wonder what Wachter himself brought to the discussion. There is no other reference to it at all in the surviving Gaussian *Nachlass*. What Wachter says is not encouraging: "Now the inconvenience arises that the parts of this surface are merely symmetrical, not, as in the plane, congruent; or, that the radius on one side is infinite and on the other imaginary[.]" and more of the same. This is a long way from saying, what enthusiasts for Gauss's grasp of non-Euclidean geometry suggest, that this is the Lobachevskian horosphere, a surface in non-Euclidean three-dimensional Space on which the induced geometry is Euclidean.

If the conclusion is that Gauss possessed tantalising hints of a new, non-Euclidean, geometry, but never worked his ideas up into a systematic theory, then his conviction is no less, and no greater, than that of Schweikart or Bessel. The grounds for his conviction are greater, but still insubstantial (we should not be too swayed by the fact that he turned out to be right). He did not possess almost all the substantial body of argument that gives Bolyai and Lobachevskii their genuine claim to be the discoverers of non-Euclidean geometry.

6. Another question about geometry

There is, however, a way of looking at what Gauss did that makes more sense of the available evidence. Our access to it starts with the letter to Bessel of January 1829, the problem of the definition of a plane as a surface that contains the line joining any two of its points, a definition, Gauss said, that contains more than is necessary for the determination of the surface, and tacitly involves a theorem which must first be proved. That this was a concern of Gauss's as early as 1797 is documented in his mathematical diary, indeed it is the only entry on a geometrical topic in the entire diary. Gauss's insight is that at one or another time in elementary geometry different properties of the plane are being used, and if one is taken as basic the others must be deduced as theorems. Moreover, it might be that unsuspected properties are being used, and these novelties should also be made explicit.

The plane has a problematic relationship to three-dimensional Space because it need not be a primitive given term but can be defined (Gauss proposed to obtain it by rotating a line about a perpendicular axis, other definitions are possible). In the same way, 'parallel' might be a primitive term, or a reducible one, and if primitive, capable of generating only one geometry, or more than one. Good housekeeping requires that one sort through these possibilities, and perhaps there will be no surprises, perhaps there will be. This locates the problem of parallels in the family of problems about the classical formulation of geometry, which is overwhelmingly how authors treated it. Only Legendre, among those who sought to defend the parallel postulate, used methods lying outside the classical formulation.

When Gauss wrote up his ideas about parallels in 1831, which Stäckel implied might be the ideas Gauss regarded as almost 40 years old, they were of this classical kind. One might argue that they had, for Gauss, a biographical aspect, but he could have gone on to say that these were the ideas of his youth and now he thinks something else. He did not, most likely because he thought these were the still good ideas. Even when he writes to Wolfgang Bolyai to praise Johann for knowing what he has known for some time, what he said by way of mathematical detail is about the area of triangles, a matter belonging to the classical formulation. He did not engage with the trigonometric aspect of Bolyai's work.

Gauss's investigations into the classical formulation of geometry were inconclusive. They remind us, as every attempt before and after Hilbert's does, of how slippery some elementary geometry is, and how hard it is to get it into a rigorous order. They do not put matters right, and they do not, in themselves, merit an account here (Stäckel's suffices). They are, however, the context for almost all of Gauss's investigations of the parallel postulate and non-Euclidean geometry. In this context, he concluded that there is more to the concept of the plane than meets the eye, but it could, with some effort, be spelled out properly. On the other hand, there was a lot more to parallelism than meets the eye. The fundamental intuition of lines that never meet no matter how far they are extended is literally ambiguous; it can be made to yield two theories. How much confidence Gauss placed in the new theory has been discussed already, but the bulk of the evidence is of the psychological kind: repeated failure to defend the parallel postulate gave way steadily to a feeling that the parallel postulate was indefensible and an alternative geometry therefore possible.

7. Gauss's letter to János Bolyai

If indeed János Bolyai's analysis of non-Euclidean geometry and the nature of space was more profound and wide-ranging than Gauss's, then the question of why Gauss answered as he did is raised with a new force. There is no evidence to suggest that Gauss was lying. On the contrary, everything we know about Gauss suggests that he was scrupulously honest, honest, indeed, to a fault. His own stated reason for writing as he did was that he 'could do no other'. He was not one to dissemble, and people who knew found him straight-forward and plain-speaking. But there is a wide gulf between saying something that is false, and telling the truth. One can be sincere, but mistaken, or wrong but innocent of any attempt to deceive.

In Gauss's case, several possibilities suggest themselves. To someone like Gauss, to return to a topic was surely to see it afresh. He need not have noticed what ideas were occurring to him for the first time, and which were recalled from earlier investigations (still less did he have to leave an accurate paper trail for subsequent historians). The facts of non-Euclidean geometry, as Bolyai presented them, could have been absorbed quickly by Gauss, much as if he had thought of them himself. The best mathematicians often have the habit, on hearing of a new result, of thinking how they could prove it themselves (Poincaré was said to have read a paper only if a proof of its results did not quickly occur to him). If Gauss was like that, he may not have read Bolyai's *Appendix* at all carefully.

It is not certain, for that matter, that Gauss read the *Appendix* properly at all. To this day there are stories (always apocryphal, to protect the people involved) of referees not reading papers carefully. Since János

Bolyai was a particularly concise writer, even Gauss may have taken the easy route of working his own way through the material by dipping in and out of the text. It is impossible to interpret the disparity between Gauss's stout claim that he knew all this already, and the two points he then discussed at length in his letter. These were János's unappealing choice of names for what are nowadays called, using Lobachevskii's terms, the horocycle and the horosphere; and an elementary proof of the relationship between area and angle sum of a triangle. But this response provides no evidence that Gauss engaged with the genuine novelties of the work: the systematic introduction of hyperbolic trigonometry, and the fact that the new geometry was introduced in three dimensions.

If Gauss read the *Appendix* his way, assimilating some of it to what he already knew and discarding the rest, then his reply is sincere, but not, without further corroboration, evidence that Gauss already new what Bolyai had published. And, as this paper has noted, such confirmatory evidence is lacking. What it is evidence for, of course, is what no-one who knows about Gauss can doubt, that he was not a charmer or a flatterer. Throughout his life he formed few friendships. His closest relationships were with astronomers such as Bessel, Olbers, Schumacher and others, with whom he enjoyed a long correspondence, but with whom he was not in daily contact. Such people could treat Gauss more nearly as an equal, something which most mathematicians, even János Bolyai, could not. Outside this limited circle, Gauss lacked the ability to respond warmly, to come over in the right way, to win people to his side. What he undoubtedly saw as the only honest way to proceed, if he was not to lapse into vanity, inevitably came over as arrogance, and permanently damaged János's enthusiasm for publishing.

8. Conclusion

It becomes clear that a mathematician persuaded of the truth of non-Euclidean geometry and seeking to convince others is almost driven to start by looking for, or creating, non-Euclidean three-dimensional Space, and to derive a rich theory of non-Euclidean two-dimensional Space from it – as Bolyai and Lobachevskii did, but not Gauss. The only hint that he explored the non-Euclidean three-dimensional case is the remark by Wachter, but what Wachter said was not encouraging: "Now the inconvenience arises that the parts of this surface are merely symmetrical, not, as in the plane, congruent; or, that the radius on one side is infinite and on the other imaginary" and more of the same. This is a long way from saying, what enthusiasts for Gauss's grasp of non-Euclidean geometry suggest, that this is the Lobachevskian horosphere, a surface in non-Euclidean three-dimensional Space on which the induced geometry is Euclidean. In particular, there is no three-dimensional differential geometry leading to an account of non-Euclidean space.

Gauss, by contrast, possessed a scientist's conviction in the possibility of a non-Euclidean geometry which was no less, and no greater, than that of Schweikart or Bessel. The grounds for his conviction are greater, but still insubstantial, because he lacks almost entirely the substantial body of argument that gives Bolyai and Lobachevskii their genuine claim to be the discoverers of non-Euclidean geometry.

Bibliography

[1] Bolyai, F. 1832, 1833 *Tentamen juventutem studiosam in Elementa Matheosis purae*, etc. Marosvásárhely

[2] Bolyai, J. 1832, *Appendix scientiam spatii absolute veram exhibens*, in Bolyai, F. [1832], tr. J. Houël, La Science Absolue de l'Espace, *Mémoires de la Société des Sciences physiques et naturelles de Bordeaux* 5, 1867, 189-248, tr. G. Battaglini, *Sulla scienza della spazio assolutamente vera*, Giornale di matematiche 6 1868, 97-115, tr. G.B. Halsted, *Science Absolute of Space*, Appendix in Bonola [1912], *János Bolyai Appendix: The theory of space, with introduction, comments, and addenda.* Edited by Prof. Ferenc Kárteszi, Doctor of the Mathematical Sciences. Supplement by Prof. Barna Szénássy. Akadémia Kiadó, Budapest, 1987, and North-Holland, Amsterdam, North-Holland Mathematics Studies. Number 138

[3] Bonola, R. 1906, *La geometria non-Euclidea*, Zanichelli, Bologna

[4] Bonola, R. 1912, *History of non-Euclidean geometry*, tr, H.S. Carslaw, preface by F. Enriques, Open Court, Chicago, Dover reprint, New York, 1955

[5] Breitenberger, E. 1984 *Gauss's Geodesy and the Axiom of Parallels*, Archive for History of Exact Sciences 29 273-289

[6] Coxeter, H.S.M. 1977, *Gauss as a geometer*, Historia Mathematica 4.4, 379-396.

[7] Dombrowski, P. 1979, *150 Years after Gauss' Disquisitiones generales circa superficies curvas, astérisque*, 62 (with the original text of Gauss and an English translation by A. Hiltebeitel and J. Morehead).

[8] Gauss, C.F. 1880, *Werke*, Vierter band, herausgegebenen von der Königlichen Gesellschaft der Wissenschaften zu Göttingen

[9] Goe, G. and van der Waerden, B. L. 1972, *Comments on Miller's 'The myth of Gauss's experiment on the Euclidean nature of physical space'* Isis 63, 345-348. With a reply by Arthur I. Miller Isis 65 (1974), 83-87.

[10] Gray, J.J. 1984, *A commentary on Gauss's mathematical diary*, 1796-1814, with an English translation, Expositiones Mathematicae, 2, 97-130 (reproduced with minor alterations in the new edition of G.W. Dunnington *Gauss – Titan of Science* 1956 published by the Mathematical Association of America in 2002)

[11] Gray, J.J. 1989, Ideas of space: *Euclidean, non-Euclidean, and relativistic* Oxford University Press, 2nd edition, Romanian edition, Idei Spatiu, Editura All Educational, 1998.

[12] Hilbert, D. 1899, *Grundlagen der Geometrie*, many subsequent editions.

[13] Hilbert, D. 1971, *Foundations of geometry*, 10^{th} English edition, translation of the second German edition by L. Unger.

[14] Lambert, J.H. 1786, *Theorie der Parallellinien*, in F. Engel and P. Stäckel (eds.) *Theorie der Parallellinien von Euklid bis auf Gauss*, Teubner, Leipzig, 1899

[15] Legendre, A.M. 1794, *Éléments de Géométrie*, Paris, with numerous subsequent editions, e.g. 12th ed. 1823.

[16] Lobachevskii N. I. 1840, *Geometrische Untersuchungen etc.* Berlin, tr. G. B. Halsted as Geometrical researches on the theory of parallels, appendix in Bonola [1912].

[17] Miller, A. I. 1972, *The myth of Gauss' experiment on the Euclidean nature of physical space.* Isis 63, 345-348.

[18] Minding, F. 1839, *Wie sich entscheiden lässt, ob zwei gegebener Krummen Flächen, etc*, Journal für die reine und angewandte Mathematik 19, 370-387.

[19] Neumann, O. 1981, ed. *Mathematisches Tagebuch*, 1796-1814. With a historical introduction by Kurt-R. Biermann, translated from the Latin by Elisabeth Schuhmann, with notes by Hans Wussing. Third edition. Ostwalds Klassiker der Exakten Wissenschaften, 256. Leipzig, Akademische Verlagsgesellschaft Geest and Portig K.-G.

[20] Pasch, M. 1882, *Vorlesungen über neuere Geometrie*, Teubner, Leipzig

[21] Reichardt, H. 1976, *Gauss und die nicht-euklidische Geometrie*, Teubner, Leipzig

[22] Scholz, E. 1992, *Gauss und die Begründung der "hoheren" Geodäsie*, S.S. Demidov, M. Folkerts, D.E. Rowe, C.J. Scriba, (eds.) Amphora, Birkhäuser Verlag, Basel, Boston and Berlin, 631-648

[23] Stäckel, P. 1917, *Gauss als Geometer*, in Gauss Werke, X.2, Abh. 4, separately paginated

[24] Szénássy, B. 1980, *Remarks on Gauss's work on non-Euclidean geometry* (Hungarian), Mat. Lapok 28 1-3, 133-140.

[25] Waltershausen, W.S. von 1856, *Gauss zum Gedächtnis* Hirzel, Stuttgart, reprinted Dr. Martin Sändig, Wiesbaden, 1965, and translated from the German by Helen W. Gauss, Gauss, a Memorial (Colorado Springs, Colorado) 1966

[26] Zormbala, K. 1996, *Gauss and the definition of the plane concept in Euclidean elementary geometry*, Historia Mathematica, 23.4, 418-436

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