# 5
# Johann Heinrich Lambert (1728–1777)

Lambert's principal contribution to the philosophy of mathematics is his discussion of the status of the Axiom of Parallels—a discussion which raises issues that will be encountered in the selections from Gauss, Riemann, von Helmholtz, Klein, Poincaré, and Hilbert. But before we consider Lambert's work on this problem, some words of historical background on the Axiom of Parallels are in order.

Even before Euclid, Aristotle had spoken of the sophistries that occurred in the theory of parallel lines—in particular, the logical fallacy of *petitio principii*:

To beg and assume the point at issue is a species of failure to demonstrate the problem proposed; but this happens in many ways. A man may not deduce at all, or he may argue from premisses which are more unknown or equally unknown, or he may establish what is prior by means of what is posterior; for demonstration proceeds from what is more convincing and prior. Now begging the point at issue is none of these; but since some things are naturally known through themselves, and other things by means of something else (the first principles through themselves, what is subordinate to them through something else), whenever a man tries to prove by means of itself what is not known by means of itself, then he begs the point at issue. This may be done by claiming what is at issue at once; it is also possible to make a transition to other things which would naturally be proved through the point at issue, and demonstrate it through them, for example if $A$ should be proved through $B$, and $B$ through $C$, though it was natural that $C$ should be proved through $A$; for it turns out that those who reason thus are proving $A$ by means of itself. This is what those persons do who suppose that they are constructing parallel lines; for they fail to see that they are assuming facts which it is impossible to demonstrate unless the parallels exist. So it turns out that those who reason thus merely say a particular thing is, if it is: in this way everything will be known by means of itself. But that is impossible.[a]

Euclid's *Elements* removed the *petitio* by explicitly stating the Parallel Postulate as an axiom. Euclid's Parallel Postulate says:

If two straight lines are cut by a third straight line so that the interior angles on the same side are less than two right angles, then the two straight lines, if produced indefinitely, meet on that side on which the angles are less than the two right angles.

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[a] *Prior analytics*, II 16, 65a 4 (translated by A.J. Jenkinson in *Aristotle 1984*). See also *Prior analytics*, II 17, 66a 11–15, and *Posterior analytics*, I 5, 74a 13–16. For a commentary on this passage see the discussion by Sir Thomas Heath in *Euclid 1925* (Vol. i, pp. 308–9) or in *Heath 1921* (Vol. i, pp. 335–48).

Already in antiquity this Postulate was criticized for not being self-evident; in particular, it was objected that the existence of asymptotic approximations shows that two curves can converge indefinitely without intersecting. (This objection is criticized by Lambert in the following selection.) Proclus, for example, in his *Commentary* on the Postulate, said:

> This ought even to be struck out of the Postulates altogether; for it is a theorem involving many difficulties, which Ptolemy, in a certain book, set himself to solve, and it requires for the demonstration of it a number of definitions as well as theorems. And the converse of it is actually proved by Euclid himself as a theorem. It may be that some would be deceived and would think it proper to place even the assumption in question among the postulates as affording, in the lessening of the two right angles, ground for an instantaneous belief that the straight lines converge and meet. To such as these Geminus correctly replied that we have learned from the very pioneers of this science not to have any regard to mere plausible imaginings when it is a question of the reasonings to be included in our geometrical doctrine. For Aristotle says that it is as justifiable to ask scientific proofs of a rhetorician as to accept mere plausibilities from a geometer; and Simmias is made by Plato to say that he recognizes as quacks those who fashion for themselves proofs from probabilities. So in this case the fact that, when the right angles are lessened, the straight lines converge is true and neccssary; but the statement that, since they converge more and more as they are produced, they will sometime meet is plausible but not necessary, in the absence of some argument showing that this is true in the case of straight lines. For the fact that some lines exist which approach indefinitely, but yet remain non-secant ‖ἀσύμπτωτοι‖, although it seems improbable and paradoxical, is nevertheless true and fully ascertained with regard to other species of lines. May not then the same thing be possible in the case of straight lines which happens in the case of the lines referred to? Indeed, until the statement in the Postulate is clinched by proof, the facts shown in the case of other lines may direct our imagination the opposite way. And, though the controversial arguments against the meeting of the straight lines should contain much that is surprising, is there not all the more reason why we should expel from our body of doctrine this merely plausible and unreasoned ‖hypothesis‖?
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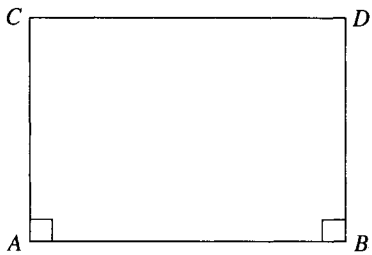
> *It is then clear from this that we must seek a proof of the present theorem, and that* it is alien to the special character of postulates. But how it should be proved, and by what sort of arguments the objections taken to it should be removed, we must explain at the point where the writer of the Elements is actually about to recall it and use it as obvious. It will be necessary at that stage to show that its obvious character does not appear independently of proof, but is turned by proof into matter of knowledge. (Quoted in *Euclid 1925*, Vol. i, pp. 202–3. The passage is also translated in *Proclus 1970*, pp. 150–1.)

Moreover, in his commentary on Euclid's Proposition I: 29, Proclus considered the possibility (to be realized in Lobatchevsky's geometry) that the straight lines of Euclid's postulate might converge asymptotically for all interior angles *slightly* less than two right angles; that is to say, only after the interior angles had been sufficiently decreased would the lines actually intersect.

For more than two thousand years, mathematicians attempted to prove the Parallel Postulate; the literature is enormous, and one bibliography (*Riccardi 1887–93*) lists twenty pages of titles of works on the Parallel Postulate between

1607 and 1887. For a compendious account of the principal efforts to prove the Postulate, the reader is referred to T.L. Heath's discussion in *Euclid 1925* (Vol. i, pp. 202–20). Although these attempts to prove the Parallel Postulate did not succeed, they did clarify the logical implications of the Postulate within Euclidean geometry, and they led to such fruitful innovations as Desargues's 1639 explanation of parallel lines as lines which intersect at the same infinitely distant point. Most of the proofs involved assuming the Parallel Postulate in an equivalent but disguised form; for instance, John Wallis, in the *De postulato quinto*, proved the Parallel Postulate from the postulate of the existence of similar figures of arbitrary size (*Wallis 1695–9*, Vol. ii, pp. 665–78); similarly, Legendre, in the appendices to the twelve editions of his *Elements of geometry* (*1794*), gave many proofs of the Postulate from such starting-points as the assumption that the sum of the angles of a triangle equals two right angles (*Legendre 1833*). Elegant accounts of these proofs are given by Heath, *loc. cit.*

Another line of approach to the Axiom of Parallels commences with the work of Gerolamo Saccheri (1667–1733); Lambert's researches stand in this tradition. In his *Euclid freed from every flaw* (*1733*) Saccheri considered the quadrilateral ABCD in which AC = BD and in which CAB and DBA are both right angles.




He easily showed that the angles at C and D are equal; if the parallel postulate were true, they would be right angles. Saccheri explored the consequences of assuming that the angles at C and D are acute and of assuming that they are obtuse—the 'acute hypothesis' and the 'obtuse hypothesis', as he called them. He hoped to derive a contradiction from each of these hypotheses, and thereby to prove the Parallel Postulate. Instead, he unwittingly derived many of the basic principles of non-Euclidean geometry.

The most important research on non-Euclidean geometry was eventually to be carried out in Germany during the nineteenth century, by mathematicians such as Gauss, Riemann, von Helmholtz, and Klein. But the Germans were late-comers to this field. Although Christoph Schlüssel's discussion of the Axiom of Parallels (in his 1574 commentary on Euclid) was mentioned by Wallis in the *De postulato quinto*, German mathematicians ignored the problem of parallel lines until 1758, when Abraham Gotthelf Kästner (1719–1800) called attention to its importance in the preface to his influential *Anfangsgründe der Arithmetik und Geometrie* (*Kästner 1758*). Kästner (who was later to be the teacher of Gauss) wrote:

The difficulty which arises in the theory of parallel lines has occupied me for many years. I used to believe that it had been entirely removed by Hausen's *Elementa matheseos* ⟦1734⟧. The former preacher to the French congregation in Leipzig, Mr. Coste, shook my complacency when, during one of the walks that he often granted me, he mentioned that, in the above-mentioned work by Hausen, an inference is made that does not follow. I soon discovered this mistake myself, and from that moment exerted myself either to remove the difficulty or to find an author who had removed it; but both efforts were in vain, although I soon assembled virtually a small library of individual writings or works on the first principles of geometry where this topic was considered especially closely. After the present work caused me to reflect on the subject anew, I have been able to find no expedient that comes closer to satisfying me than the one I have adopted in the corollary to theorem eleven and in theorem twelve.

(The 'expedient' mentioned by Kästner was essentially that of Wallis—i.e. assuming the existence of similar figures of arbitrary size.)

Kästner's 'small library' of writings on the Axiom of Parallels was used by his student Georg Simon Klügel to produce a Göttingen dissertation (*Klügel 1763*) containing the first extensive discussion of the history of the Parallel Axiom. Klügel examined some thirty attempts to prove the Axiom, and showed that each was a failure. He concluded 'To be sure, it might be possible that non-intersecting lines diverge from each other. We know that such a thing is absurd, not in virtue of rigorous inferences or clear concepts of straight and crooked lines, but rather through experience and the judgement of our eyes' (*Klügel 1763*, p. 16). Kästner himself is alleged to have despaired of ever finding a proof of the Axiom, and to have recommended that it simply be accepted as a given (see *Schweickart 1807*, p. 6). These inconclusive remarks appear to be the first tentative expressions of the view that the Axiom of Parallels might not be provable, and that its plausibility might rest on experience.

Klügel's dissertation—and in particular his discussion of Saccheri's attempted proof of the Axiom—seems to have inspired the work of Johann Heinrich Lambert, who cites it in §3 of the selection that follows. Lambert was a self-taught Swiss mathematician, astronomer, physicist, economist, logician, cartographer, meteorologist, and philosopher. At the suggestion of his fellow-countryman Leonhard Euler he emigrated to Berlin in 1764, and was elected to the Academy of Sciences in 1765. Lambert wrote voluminously on scientific topics, and published some 190 essays on philosophy, astronomy, mathematics, and the physics of light and heat. He also published 21 books, and his *Nachlass* contains a further 837 manuscripts. He developed a highly personal system of dress (multicoloured) and of etiquette (which required him to stand at right angles to his interlocutors). Frederick the Great, on meeting this strange creature for the first time, is said to have exclaimed that the biggest blockhead in the kingdom had been proposed to him for membership in the Prussian Academy. (Frederick later became Lambert's vigorous supporter.) In mathematics, Lambert is now best remembered for being the first to prove the irrationality of $\pi$ and $e$; the historian of mathematics Moritz Cantor ranked him after Euler, Lagrange, and Laplace as one of the great mathematicians of his generation. Further details of his life and scientific publications can be found in *Scriba 1973*.

Lambert's scientific work was closely tied to his writings on philosophy. He published two important works of metaphysics, the *New organon, or Thoughts on the investigation and designation of truth and of the distinction between error and appearance* (*1764*) and the *Foundations of architectonic, or Theory of the simple and primary elements in philosophical and mathematical knowledge* (*1771*), in which he attempted to reform the systems of Locke, Leibniz, and Wolff. He began by investigating the origins and scope and interrelationships of the basic *a priori* concepts of metaphysics—to present them as the foundation for metaphysics and the empirical sciences. He then attempted to construct the propositions of metaphysics by deducing them from the basic concepts in analogy with the mathematical method. Once metaphysics had been constructed in this mathematically exact way, the deduced propositions were to be applied to the empirical subject-matter of the individual sciences, for which they would form the foundation. Lambert thus was led to discuss epistemological questions at the foundations of science; the nature of axioms; the possibility of a logical calculus; the relationship between mathematical knowledge and metaphysical knowledge; the nature of logic and its relationship to language and the theory of signs.

Lambert, like Leibniz, tried to develop an *ars characteristica combinatoria*, a logical calculus in which syllogistic reasoning would be represented by algebraic operations. Lambert represented the combination of two concepts $a$ and $b$ into a common concept by $a + b$; and the common part of the two concepts by $ab$. He then explored the laws governing these operations of 'addition' and 'multiplication,' and introduced inverse operations of 'subtraction' and 'division'. However, he had no effective way of expressing logical negation, and his system did not embrace the whole even of syllogistic logic; that task was not to be accomplished until Boole's *Mathematical analysis of logic* in 1847.

Lambert was greatly admired by Kant, especially for his celebrated *Cosmological letters* (*1761*) and for his *New organon*. Indeed, many of the insights of the *Critique of pure reason* can be found in embryo in the writings of Lambert. The two thinkers carried out a regular correspondence, and Kant, in a letter to Johann Bernoulli of 16 November 1781, acknowledged Lambert's influence on the discussion of space and time in the *Critique of pure reason*. (Indeed, at one point Kant intended to dedicate the *Critique* to Lambert; Lambert, however, died before the work was completed. This Johann Bernoulli, incidentally, was the nephew of the mathematician Johann Bernoulli (1667–1748).)

Lambert's most extensive treatment of the foundations of Euclidean geometry is his *Theory of parallel lines* (*1786*). This work was not published by Lambert himself, presumably because he was dissatisfied with its failure to solve the problem of the Axiom of Parallels. The manuscript was first published in 1786, after Lambert's death, by Johann Bernoulli, who gives the date of composition as 1766.

In this work, Lambert followed Saccheri's strategy for proving the Axiom of Parallels. (He does not, however, appear to have known Saccheri's work at first hand, but only the brief account of it given in Klügel's dissertation.) Lambert went much further than Saccheri in deducing the geometrical consequences of the obtuse and acute hypotheses and their implications for the sums of the angles of a triangle and for the measurement of area; in effect, his work amounted to an unintentional deduction of the basic properties of Riemannian and Lobatchevskian geometry. In particular, he observed (§82) that the obtuse hypothesis holds for triangles on the surface of a sphere, and he conjectured that the acute hypothesis holds on a sphere with imaginary radius. This was a remarkably prescient conjecture, and was only to be fully confirmed in 1829 with the first publication of Lobatchevsky's work on non-Euclidean geometry. But Lambert's attempt to prove the Axiom of Parallels in effect presupposed Bolyai's axiom (that it is always possible to draw a circle through any three points of the plane), an axiom which is equivalent to the Axiom of Parallels. The inadequacy of Lambert's proposed proof at the end of his treatise (§88) was quickly noted when the work was published, and was very probably obvious to Lambert as well—a fact which may explain his failure to publish the manuscript himself.

The passage translated below is the methodological introduction to the *Theory of parallel lines*, where Lambert discusses the logical and philosophical status of the Axiom of Parallels and of various attempts to prove it. After an analysis of Euclid's methodology and of the controversy surrounding the Axiom,[b] Lambert (§§4–8) criticizes Wolff's attempt to *define* parallel lines in such a way as to remove the difficulty. For (§5) the Wolffian definition is not obtained by abstraction from things of which we have experience—both because actual 'straight lines' are never precisely straight, and because we have no experience of lines that are infinitely extended. So Wolff's definition must be an 'arbitrarily conjoined concept'. But then Wolff must supply a justification for introducing such a concept; and this he conspicuously fails to do. Lambert concludes that Wolff has merely moved the difficulty from the axioms to the definitions.

Lambert's own strategy is set out in §10 and 11. He first argues that the *truth* of the Axiom of Parallels is not what is at issue: for its plausibility is established inductively by the plausibility of the consequences one is able to derive from it. Rather, the question is *whether the Parallel Postulate can be derived by rigorous, logical inferences from the other Euclidean axioms*. And here (§11) Lambert makes it clear that by 'derivability' he means a purely syntactic proof which abstracts altogether from the intended reference of terms like 'parallel line'. Lambert gives a remarkably clear early statement of what was later to be the kernel of Hilbert's approach to the axioms of geometry: 'It must be possible', Hilbert said, 'to replace in all geometic statements the words, *point*, *line*, *plane* by *table*, *chair*, *mug*.'[c]

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[b] In a roughly contemporaneous letter to Baron Georg Johann von Holland (1742–84), Lambert discusses these matters further, and makes clear that the difficulty with parallel lines is that one must imagine them as being extended 'into the *infinite*' [ins *Unendliche*] in both directions. The letter, which is dated 11 April 1765, is reproduced in *Stäckel and Engel 1895* (pp. 141–2).

[c] (*Weyl 1944b*; reprinted in *Weyl 1968*, Vol. iv, p. 153.) Lambert's discussion of axioms and definitions should also be compared with Gergonne's important *Essay on the theory of definitions* (*Gergonne 1818*), where the notion of implicit definitions occurs for the first time.

Lambert thus held in his hands most of the tools necessary for justifying and developing non-Euclidean geometry: the idea that Saccheri's obtuse and acute hypotheses could be modelled on the surfaces of spheres (possibly of imaginary radius); a rigorous deduction of many of the basic properties of Riemannian and Lobatchevskian geometry; a clear understanding of the methodological issues; and the concept of an uninterpreted, formal system of axioms. The issues Lambert touches on in this short piece—the status of axioms and definitions, the problem of the infinite in geometry, the logical character of deductions within a formal axiom system, the nature of mathematical proof, the truth of Euclidean geometry, the reference of algebraic symbols—were to play a large role in the mathematics of the nineteenth century, and will recur in most of the selections that follow. But Lambert—unlike Gauss—never made the leap to conceiving of the possibility of alternative geometries, and his subtle methodological distinctions were intended instead to pave the way for a proof of the Axiom of Parallels, a proof whose possibility he appears never to have doubted. And in contrast to Hilbert, Lambert did not actually *use* the idea of a purely symbolic calculus to obtain mathematical theorems; and so his idea, despite the clarity of his formulation, lay neglected until Pasch and Hilbert independently rediscovered it at the end of the nineteenth century. (Even Stäckel and Engel, who edited and reprinted Lambert's treatise in their *1895*, attached no particular importance to his §11.)

The translation is by William Ewald from the version in *Stäckel and Engel 1895*; references to *Lambert 1786* should be to the section numbers, which appeared in the original edition.

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## A. *FROM THE* THEORY OF PARALLEL LINES (*LAMBERT 1786*)

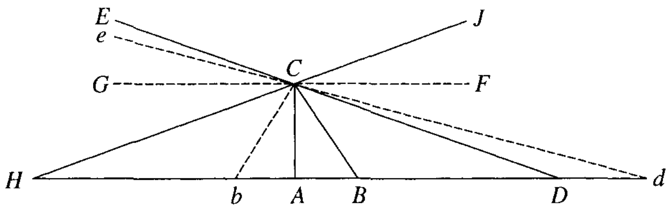
### 1. Preliminary considerations

§1.

The present treatise deals with a difficulty that arises in the very beginnings of geometry, and that since the days of Euclid has been a stumbling-block to those who do not wish to believe the doctrines of this science on the testimony of others, but wish rather to be convinced by reasons, and never to forgo the rigour that they find in most proofs.

This difficulty leaps to the eye of anybody who reads Euclid's *Elements*—and does so at the very outset, for it does not arise in the theorems but in the axioms with which Euclid prefaces the First Book. The eleventh[a] axiom assumes

[a] [As the text makes clear, Lambert is here referring to the Axiom of Parallels—the Fifth Axiom in Heath's edition of Euclid (*Euclid 1925*).]




as something clear and in no need of proof, *that, when two lines CD, BD* (Fig. 1) *are intersected by a third, and the two inner angles DCB, DBC taken together, are less than two right angles, then the two lines CD, BD meet on the side of D, or the side where these angles are found.*

§2.

This axiom is incontestably neither as clear nor as evident as the others; and one's natural reaction is not merely to desire a proof of it—instead, one somehow feels that it is capable of proof, that there must exist a proof of it.

As I see the matter, this is the *first* reaction. But if one reads further in Euclid, one must not only admire the care and acuteness of his proofs, and a certain noble simplicity in his manner of proceeding; but one becomes even more perplexed about his eleventh axiom when one sees that he proves theorems that one would far more readily have conceded without proof.

It is sometimes said that Euclid did this in order to make his doctrines secure against even the most hair-splitting objections of the Sophists. But if this is so, then I confess that I am utterly unable to comprehend these Sophists, if Euclid could imagine that they would not contest his eleventh axiom, just because without it the majority of the theorems of geometry would collapse. One should rather think that Euclid and the Sophists (if these latter did not raise any objections in Euclid's day) must have had standards for judging the axioms and the manner of executing a geometrical proof which were quite different from the standards of those who thought about this matter in later times, or who made difficulties about the proofs that had been attempted by others.

Of these difficulties or objections, one that occurs to me is the following: that in order to prove the Euclidean axiom rigorously, or to establish ‖festzusetzen‖ geometry at all, one may neither *visualize* nor make *a representation of the thing itself*.[b] It is clear that with such a requirement one can also contest the twelfth Euclidean axiom, *that two straight lines do not enclose a space*.[c]

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[b] ['weder *sehen* noch sich *von der Sache selbst* eine *Vorstellung* machen dürfe'. *Sache* in Lambert's usage sometimes means the subject-matter of Euclidean geometry, and sometimes a particular geometrical object.]

[c] [This axiom was an interpolation into Euclid, and was criticized by Proclus as superfluous; in some editions of the *Elements* it appears as 'Common Notion 9'. See the discussion by Heath in *Euclid 1925*, p. 232.]

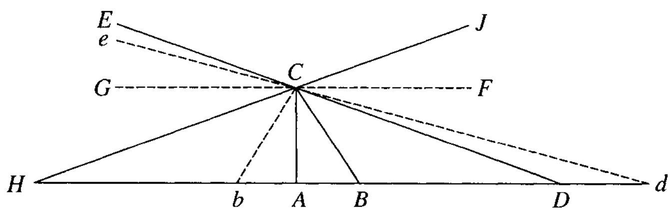
§3.

But it seems to me equally clear that the Sophists of Euclid's day were less strict, and that they must have conceded the *representation* ⟦*Vorstellung*⟧ *of the thing.* But with this presupposition Euclid's manner of proceeding can be quite adequately justified (at least in the absence of an alternative, and because it is subject to fewer difficulties).

Specifically, one can put off the eleventh axiom until one comes to *Prop.* XXIX of the first book. Meanwhile, one learns *to know the thing itself* (of which the axiom speaks) and also to add in thought that which seems to be missing in the axiom and in its representation, even if one cannot express this in words. In the two immediately preceding Propositions XXVII and XXVIII one learns that, if the angles

$$FCB + CBD = 180°$$

or if the angles $FCB = CBA$, then the lines $AB$, $CF$ do not intersect in the direction either of $F$ or of $G$. One thereby learns that the 34th definition[d] is not an




absurdity ⟦Unding⟧ or a figment of the imagination; but that non-intersecting straight lines actually occur in the realm of reality. For until now this definition remained open; and until now one could also allow the axiom to remain open, because it stands in close connection with the parallel lines, and as it were marks the boundary between parallel lines and lines that meet.

What one now further *imagines* ⟦sich *vorstellt*⟧ in order to convince oneself of the correctness and *thinkability* of the axiom comes in my opinion to this: one imagines $CF$, $AB$ in Propositions XXVII or XXVIII as not converging, and imagines an arbitrary *straight* line $CD$ drawn through the angle $BCF$; and then one knows that, no matter how small the angle $DCF$ may be, necessarily

$$DBC + BCD < 180°$$

and consequently satisfies the condition of the axiom. If one should now in the same way imagine that $CD$, $BD$ converge, then one will need to *imagine* the lines $CF$, $CD$, $AD$ as *straight* lines. And this thought-experiment shows that as $CD$ is extended it not only increases its distance from $CF$ but also approaches $AD$ in such a way that it must intersect it at some distance $BD$.

[d] ⟦In *Euclid 1925* it is the 23rd.⟧

Whoever at this point objects that *CD* could perhaps approach *AD* asymptotically (like, for instance, the hyperbola and other asymptotic bent lines) in my opinion changes what the logicians call the *statum quaestionis*, or he deviates from Euclid, where the talk is not about *proofs* but about *representation* and the *thinkability*[e] *of the thing*—because one can certainly assume of Euclid that he would not otherwise have counted or placed his proposition among the *axioms*. But if it is a matter of the *representation*[f] *of the thing*: then I do not see how in the representation of *straight* lines objections about hyperbolas can be made. One could equally well doubt that two straight lines cannot be placed together so as to enclose a space—as happens when two circular arcs of equal size are placed together.

I mention this only to show that, if you start by presupposing the actual representation of the thing, and if you do not simply demand only words, then Euclid's procedure can be justified; all the more so because his way of proceeding, so far as I am aware, even today encounters fewer difficulties than all the attempts that have been made since his time to proceed differently. Here one can read a short and concisely written dissertation by Herr Klügel, who illustrates, with ingenuity and moderation, the defects that occur in such attempts—often with hidden logical circles, gaps, leaps, paralogisms, and incorrectly used and gratuitously assumed definitions and axioms.

§4.

Although (as this dissertation relates) many such daring attempts have appeared in print in this century, there is no doubt that there would not have been so many, particularly in Germany, if Wolff (who, for a period of forty or more years, was *dux gregis* with respect to these geometrical writings—and who, to be sure, deserved to be, for many good reasons)—if Wolff, I say, had had a better sense of the above-mentioned difficulty, or, more importantly, if he had been more critical in his first principles. The latter course would for obvious reasons have elicited a multitude of writings on the subject. And the former, as I see the matter, would even have had a marked influence on Wolff's world-view.

The problem is not that Wolff did not know full well *that arbitrarily conjoined concepts must be established* ‖*erwiesen*‖. He emphasized the point in both of his treatises on reason, and even in his preliminary reports on mathematical method, and he illustrated it with examples from geometry. But I conclude from this that Wolff must not have regarded his definition of parallel lines as an *arbitrarily conjoined concept*, because I am confident that otherwise he would have thought about giving a proof of their possibility, or at least remembered that something remained to be done; or else he would have adhered to Euclid's method, and then the difficulties would have become apparent, as they did in Euclid.

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[e] ‖*Gedenkbarkeit.*‖
[f] ‖*Vorstellung.*‖

But if we ask why Wolff, not thinking of anything arbitrary, contented himself with calling the parallel lines *equidistant*, we must then assume that he found this concept by applying his other method of finding concepts, namely, by *abstraction from individual examples*. He says of such concepts and definitions that they need no further proof. This I concede. But in the execution of the argument one must nevertheless play fair with the reader and show him how one has abstracted the concept. Otherwise the reader could legitimately suppose that a *vitium subreptionis* has slipped in. For concepts that one abstracts from examples are also to that extent always *a posteriori*; and one can regard them *a priori* only if, after one has found them, they are thinkable for themselves[g]—that is to say, are *simple*. Otherwise, if one wishes to fend off the suspicion of a *vitium subreptionis*, one must produce the examples for the reader and give an account of all the precautions one has taken in the abstraction.

Bülfinger[h] was well aware of the necessity of this procedure, and precisely for that reason he was in a better position than Wolff himself to lessen the difficulties that had arisen for the Wolffian world-view. But it would have been desirable if Wolff himself, in the central chapters of his two treatises on reason (where he deals in part with definition, and in part with the written presentation of dogmatic propositions), had shown, in detail and with full emphasis, both the manner whereby one can fend off the suspicion of a *vitium subreptionis* in working with definitions that have been found by abstraction, and the necessity for doing so.

§5.

Now, for the definition of parallel lines this would have been quite out of the question. For however many of these one draws, there remain two marked deficiencies. First, the drawing lacks geometrical precision. Second, it is absolutely impossible to continue both lines into the infinite. And so one does not get anywhere *a posteriori* and with abstraction; and the definition (or, to put it better, the possibility of the thing) must be established from other and simpler grounds that are thinkable for themselves.

Wolff incontestably did not make these reflections. And one finds in him indications from which one can clearly infer that he conceded too much to the definitions; and because he wanted to arrange them suitably for the subject-matter ‖Sache‖, he imported the difficulties that are in the subject-matter into the definitions.[i] That in the case of parallel lines they were often more hidden there than in the subject-matter itself, one could at any rate infer from the fact that in those times, when a widespread mania for demonstrations was the

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[g] ‖für sich gedenkbar.‖

[h] ‖Georg Bernhard Bilfinger or Bülfinger (1693–1740), author of *Dilucidationes philosophicae*; for a discussion of his work, see *Zeller 1875* p. 231.‖

[i] ‖In Lambert's letter of 3 February 1766 to Kant, he remarks: 'Wolff assumed nominal definitions as it were *gratis*, and, without noticing it, he shoved or hid all the difficulties there.'‖

prevailing fashion, more of a fuss would have been made had Wolff retained the Euclidean procedure in his *Anfangsgründe der Messkunst*.

§6.

I just said that Wolff conceded too much to the definitions. Now, this happened in actual fact rather than expressly in words; and for many it became the fashion, without noticing it, *to believe that they had no concept whatsoever of a thing* ‖*Sache*‖ *if its name had not been defined*. Even all axioms had to be preceded by definitions, without which they supposedly could not be understood. So it is hardly surprising that the proposition: *every definition, until it has been proved, is an empty hypothesis*—that this proposition, which Euclid knew so well, and which he continually observed, was forgotten, even if it was not entirely lost.

I note this here all the more, because it had very detrimental consequences for the procedure of the philosophical sciences; also because it is precisely the point where Wolff lagged behind when he abstracted his method from Euclid; and finally because parallel lines give the most obvious example, that *a definition given in advance, so long as it has not itself been established, proves nothing*.

§7.

It is false that, before he had established the possibility of the thing, Euclid used any of his definitions other than as a *mere hypothesis*, or that he regarded it as a *categorical principium demonstrandi*. For him, the expression *per definitionem* means no more than *per hypothesin*. And if one looks more closely: he does not obtain the *categorical* features in his theorems from the *definitions* but actually and primarily from the *postulates*. It is to these that Cicero's words actually apply: *si dederis, danda sunt omnia*.[j]

Among the axioms, I find that only the eleventh contains a positive category which immediately concerns the figures. But this is precisely the axiom that one does not wish to count as valid. What is *categorical* in it is supposed to be extracted from the postulates by inferences. The others concern for the most part only the concept of *equality* and *inequality*, and for that reason, because they concern *relation-concepts*, they do not belong to the *matter*, but to the *form of the inferences* that Euclid makes in his proofs, where they always appear only as auxiliary propositions. The twelfth axiom, *that two straight lines enclose no space*, is negative, and, like the ninth, *that the whole is greater than its part*, is used by Euclid where the proof is *apagogic*, or where the truth of the proposition is established from the impossibility of the opposite.

[j] ‖The quotation occurs in Cicero's *De finibus bonorum et malorum*, Book 5, XXVIII, 83. Cicero is praising the cogency of an argument in moral philosophy, and remarks, *Ut in geometria, prima si dederis, danda sunt omnia*'—'As in geometry, if you have conceded the first ‖principles‖, all must be conceded'.‖

§8.

This is a brief sketch of the spirit of the Euclidean method; I find little or nothing of it in Wolff's theories of reason, and often the opposite in his procedure and arguments.

For example, Wolff, with several others, believes that one could remove the difficulty caused by Euclid's eleventh axiom if one were to alter his definition of parallel line. But this would neither remove it, nor avoid it, nor yet get around it in a clever way and, as it were, remove it indirectly. Rather (if all goes well) the difficulty is only *taken away from the axiom* and *brought into the definition*; so far as I can see, it is not in the process made any easier to remove. Indeed, Euclid's definition can be proved without regard for his eleventh axiom. Wolff's definition, on the other hand, either cannot be proved without this axiom, or, if it can, then the axiom is as good as established at the same time.

But actually it is not a question of the definition at all. One can leave it wholly out of consideration in Euclid; and in Props. XXVII and XXVIII one will replace the expression *parallelae lineae* with *lineae sibi non coincidentes.*[k] And, when one notices that this is a peculiar property, one will oneself hit on a short and convenient *term* for it, or give a *name* to such lines which never intersect no matter how far one extends them on either side. And one will be even more encouraged to do this when one subsequently sees that precisely these lines also remain the same distance from each other.

This is the genuine synthetic way of proceeding, in which one thinks of the *term* only after the thing ⟦Sache⟧ has been brought forward and only if it is significant enough to deserve a special name. There are countless examples in mathematics, and there should be others in any science where one can (or thinks one can) proceed *a priori.*

§9.

Proclus, who also found Euclid's eleventh axiom problematical, demanded a proof of it on the grounds that *its converse can be established.*

In fact, the converse proposition is proved in Bk. I, Prop. XVII. It seems to me quite right that for an axiom it must be clear for itself ⟦für sich⟧ what sort of a reason there is for the axiom or its converse. For precisely speaking an axiom should consist purely of simple concepts that are thinkable for themselves; and it must be evident immediately from the representation ⟦Vorstellung⟧ of the concepts whether and to what extent they can be conjoined.

So, for example, the eighth Euclidean axiom, *that extended quantities which are congruent with one another are equal (Quae sibi mutuo congruunt, sunt aequalia)*—this proposition is thinkable for itself. But the following is also in

[k] ⟦'lines which do not intersect themselves'.⟧

the same way thinkable for itself: that the converse holds only for straight lines and angles, while for figures one needs yet another condition, namely that of similarity, if the converse is to apply.

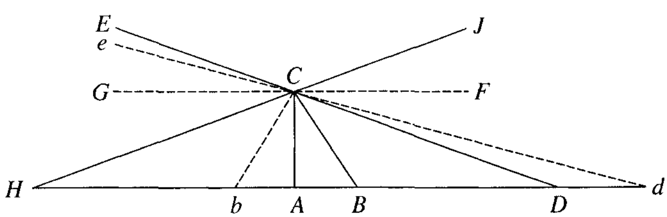
§10.

In order after these general reflections to come closer to the theory of parallel lines (where I intend both to make the difficulties apparent and to remove them) I shall first state the actual *statum quaestionis*.

First, the *question itself* concerns *neither the truth nor the thinkability of the Euclidean axiom*: things would have looked bad hitherto for the greater part of geometry if this were the question. As for *thinkability*, I have already shown (§3) the order in which it arises in the reading of Euclid. That the axiom is also thought of as *true* is entirely clear. But its truth is also established from all the consequences that one draws from it in every respect—established in such an illuminating and necessary way that one can regard these consequences, taken together, as an *induction* that is in many ways complete.

Then one also finds, in the many attempts that one can make to prove this axiom, that it almost always presupposes itself in the proof, and is a consequence of itself in many different ways, but that there is no way of refuting it.

This may also be a reason why Euclid, in the absence of a proof, included it among the axioms; especially since he chose the definition that could be established without recourse to this axiom, and which could be most immediately linked to it. For one sees quite clearly that his Prop. XXIX, where this axiom is used, serves principally only to prove that that there are no parallel lines other than the ones established in the two Props. XXVII and XXVIII. And in this respect a very small gap is thereby filled, because one can imagine without difficulty that only those among the non-intersecting lines remain to be excluded that make a *smaller* angle with *CF* (Fig. 1) than do all those lines *CD, Cd* whose intersection *D, d* can be given—that is, whose intersection has a finite distance from *A*. For, if one rotates *CF* around the point *C* down towards *D*, then Prof. Kästner remarks correctly that the first intersection point cannot be given because, wherever one locates it on *AD*, one can always find another one that is more distant. But in my opinion this has the consequence that, where the angles *DCF*. *dCF* are very small, the distances *AD, Ad* must increase in



inverse proportion to the angles *DCF, dCF* (or a function of them that is not very different). For they cannot increase in direct proportion to the angles *ACD, ACd* (or a function of them) because otherwise, if

$$DAC + ACF = 180°$$

or even if it is greater, then *CF* would have to intersect the line *AD* at a finite distance from *A*; and this would contradict Euclid's Bk. I, Prop. XXVII.

However, I do not believe that the topic ‖Sache‖ can be discussed in this manner; although it can easily be shown, if the problem ‖Sache‖ has been correctly stated, that in order to bisect the angle *DCF* one need only set $Dd = DC$. But there are also other ways of conceiving the topic.[1]

For example, whoever regards the two non-intersecting lines *CF, AD* as making an angle that equals zero will easily be able to prove that every line *Cd* makes an angle with *Ad* that is greater than zero, and that these two lines accordingly intersect somewhere. The proof is precisely the one where one shows that $CDA > CdA$ (Euclid, Bk. I, Prop. XVI). For if one rotates *CD* upwards around point *C*, then the angle *CDA* becomes ever smaller, and finally completely negative, as soon as *CD* comes over *CF*. It must therefore equal zero somewhere, and that this occurs in the position *CF* follows, in my opinion, from the *idea* ‖*Vorstellung*‖ that *AD, CF* are *straight* lines—an idea which cannot coexist with the idea of an *asymptotic approach.*

But whether this consideration of negative angles, and of such as are equal to zero, is proper in the first book of Euclid—that is a completely different question, which one can easily deny, maintaining that such a procedure is more algebraic than geometric.

§11.

I furthermore remark that the difficulties concerning Euclid's eleventh axiom essentially come down to the question: *whether it can be derived ‖hergeleitet‖ in proper order from the Euclidean postulates together with his other axioms.* Or, *if these are not sufficient, whether there cannot then be produced other postulates or axioms (or both) which have the same obviousness ‖Evidenz‖ as the Euclidean do, and from which his eleventh axiom could be proved?*

In the first part of this question, one can abstract from everything that I earlier called *representation of the thing*. And since Euclid's *postulata* and other axioms have been expressed in words, it can and should be demanded that the proof never appeal to the thing itself, but that the proof should be carried out purely symbolically—when this is possible. In this respect, Euclid's *postulata* are as it were like so many algebraic equations which one already has in front of oneself and from which one is to compute *x, y, z,* etc. without looking back to the thing itself. But since they are not exactly such formulas, one can concede the drawing of a figure as a guideline for the execution of the proof.

[1] ‖sich die Sache vorzustellen.‖

On the other hand, it would be absurd if in the other part of the question one were to forbid the contemplation and representation of the thing, and were to demand that the new postulates and axioms should be found without any thought about the thing—out of thin air, as it were. But I also do not see how one is fairer to Euclid if one rejects his axioms without asking the question I have asked at the beginning of this section. For since Euclid counts his proposition among the axioms, he thereby incontestably presupposes the representation of the thing; and one can presume that, in the absence of a proof (which is yet to be found), he chose his own method of proceeding consciously.

Moreover, I have no doubt that Euclid did not himself think of a way of bringing his eleventh axiom among the theorems. At any rate, in the first book of his *Elements* there are several dim hints of this. How easily, for example, his Proposition XVII follows from Proposition XXXII once this latter has been proved! But Euclid specially proves the former proposition—probably to show how much one can determine about the angles of a triangle without invoking the eleventh axiom.