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Les Enjeux de la Controverse Frege-Hilbert sur les Fondements de la Géométrie

Une étude philosophique sur la logique et les mathématiques

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Résumé

L'auteur entreprend dans ce mémoire de faire une présentation des débats axiologiques de philosophie de la logique sous-jacents à la controverse opposant Frege et Hilbert sur les fondements de la géométrie. Contre le parti pris philosophique selon lequel la logique est une discipline achevée, l'auteur entreprend une mise en contexte des positions de Frege et Hilbert afin de montrer que dans leur conception de la logique se trouvent des paradigmes incommensurables, résultant de l'influence de traditions philosophiques et scientifiques diverses. Dans cette perspective, Frege est le défenseur de la vision traditionnelle de la logique comme medium universel de la science, tel qu'incarnée dans la géométrie euclidienne. La logique symbolique de Frege est ainsi vue comme la mise en œuvre de moyens raffinés pour lutter contre la « perversion des sciences » ayant lieu au 19$^{\text{ième}}$ siècle et pour la défense de la vision traditionnelle de la science. À l'opposé, l'approche métathéorique de Hilbert représente la conception moderne dite algébrique de la logique telle que développée au 19$^{\text{ième}}$ sous l'influence des métamathématiques, et certains rapprochements avec les conceptions « model-theoretic » et catégorielles de la logique viennent appuyer cette thèse.

Abstract

This memoir presents some axiological debates of philosophy of logic underlying the Frege-Hilbert controversy on the foundations of geometry. Against the philosophical bias according to which logic is an achieved discipline, a contextualized presentation of the respective positions of Frege and Hilbert is done in order to show that incommensurable paradigms are found in their view of logic, that is due to the influence of various philosophical and scientific traditions. From this standpoint, Frege is the defender of the traditionalist view of logic as the universal medium of science, as embodied in Euclidean geometry. In this perspective, Frege's symbolic logic is seen as the achievement of a refined means to counter the 19^{th}-century perversion of science with the purpose of defending the traditional conception of the role of science. On the other hand, Hilbert's metatheoretical approach represents the so-called algebraic modern conception of logic as developed in the 19^{th} century under the influence of metamathematics. Following this, parallels between Hilbert's approach and the model-theoretical and categorical conceptions of logic are drawn to show their proximity.

Foreword

Writing this memoir has been to some extent very difficult. I had to make do with a terrible time pressure, and the many constraints it imposes are always unpleasant. For this reason some parts had to be set aside, and some others may seem somewhat unfinished. Moreover, my choice of writing my thesis in English – for obvious institutional reasons – added much difficulty to the composition. And, most of all, the result has certainly something of a feather in the wind, not strictly following a line of advance. I may only apologize for it and ask for the leniency that a first lengthy writing may deserve.

These obvious defects notwithstanding, I consider that the goal I had has been attained. I have always been interested by the justification of opinions, of ideas, of theories, etc. Any conceivable debate raises the question of the justification of one's argument, but also the more abstract issue of what a relevant justification may consist in. Some of those interested with these problems would like to rely on a solid ground, an unshakable basis, that would allow them to give a clear and final answer: "What offers a good justification is what follows the rules of logic." I have always been attracted by such an answer, for I scarcely had any idea how one could push the issue further. Even if I am still in lack of such an insight, the work I have done while preparing this paper allowed me to understand the complexity and non-apodictic character of such a claim. I wished to understand a little more the evolution of logic through its history, in parallel with the history of philosophy and mathematics; it had obviously a connection with this issue. From this, I expected to have some insights concerning what happens in many axiological[1] debates underlying researches in logic. It became clear to me that it was simplistic to affirm without further ado that logic was the immutable subject-matter fulfilling the role of ultimate ground for justification.

Though my initial plan was to analyze technically the positions of Frege and Hilbert

[1] I repeatedly use that term in my thesis in a figurative way, i.e. to speak of what is related to the goals and values promoted by logical and/or scientific inquiries that tangibly determines what is considered relevant for a given inquiry.

in their debate on the foundations of geometry and to confront them point by point, I had been diverted. This is certainly due to a lack of definiteness in my methodology, but also due to the strong attraction that the aforementioned historical perspective exerted on me. The point is that I was more of less conscious of what was happening; I started this research with an almost non-existing knowledge of the issues of the debate. I was analyzing the ins and outs of the problem at the same time that I was learning what it was about. I have therefore been strongly influenced by some philosophers, e.g. Hintikka, Shapiro, Nagel, van Heijenoort, Weyl and Bell, who have written penetrating papers on the history of mathematics and logic. Certainly, the temptation to situate historically the problem I was working on, was great. And it led me to distant lands...

Here's an excerpt of what my thesis supervisor, Pr. François Tournier, wrote concerning my thesis in his assessing report:

> [...] la recherche du candidat est un argument à l'intérieur d'un raisonnement beaucoup plus général et que, pour cette raison, il aurait été plus qu'intéressant que le candidat indique l'ampleur de sa perspective dans son introduction ainsi que dans sa conclusion.

Of course, he is right. Throughout the time I spent on this thesis, he constantly recalled me to define clearly the aim of my work and to stick with it. For sure, there remains many inaccuracies concerning this aspect, but I can hardly imagine the Capernaum that would have resulted without his judicious advices. He always helped me as well as one could expect, and I am extremely indebted to him for this. For the fantastic discussions we had, for the matchlessly stimulating lecture he gave, and for the constant support he offered to me, I show him a gratitude of magnitude x, when $x \to \infty$.

I am indebted to many magnificent persons for what they did for me. I can't name them all – for the list would be too long – but I thank them sincerely. However, I have some special thanks to address to Renée Bilodeau for her constant support and for her interesting courses; it has helped me more that she could imagine. I must also thank Bernard Hodgson for his course of history of mathematics and for the precious time he generously spent on me while I was harassing him with questions. My friends also deserve their sheer amount of acknowledgment. I have a special thank for Sébastien Malette, a friend that one may only be proud to have; without our discussions, philosophy would have been to me a dead letter. Also, I thank François Chassé for having patiently listen to my elucubrations.

Finally, I thank François Tournier, Reneé Bilodeau and Mathieu Marion for having carefully read, assessed and carefully commented my thesis.

À mes parents, pour tout, et même plus.

The sole aim of all science is the honor of human spirit.
Hilbert, 1930, p. 1165

In fact [geometry and philosophy] belong to one another. A philosopher who has nothing to do with geometry is only a half philosopher, and a mathematician with no element of philosophy in him is only half a mathematician.
Frege, 1924-5a, p. 273

Nowhere do mathematics, natural sciences, and philosophy permeate one another so intimately as in the problem of space.
Weyl, 1949, p. 67

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Introduction

In the last years, much has been written on the Frege-Hilbert controversy. A quick survey of the literature undoubtedly shows that it is a "slippery ground". This situation requires a very careful handling of the subject matter and that is why my analysis of the problematic relies on a twofold methodology.

The first step of my inquiry will consider the controversy in its historical context. The first chapter shows that the idea of logic as an achieved discipline, admitting only of minor changes throughout its history, should be superseded by another one assuming a succession of paradigms which would trace out a more reliable picture of its historical development. That is why the axiological aspect of the controversy will be stressed and will occupy a large part of this research. These remarks, however, will only be briefly specified and not thoroughly developed, since they aim at specifying our approach to the Frege-Hilbert controversy. Chapter 2, in a similar fashion, is a historical overview of the debate, intended to set our perspective rather than to give a complete historical account and to avoid major anachronisms or obvious misinterpretations. I will simply present a brief summary of what was the general mathematical context, that is, what were the breakthroughs immediately preceding the debate and the fashionable methods.

The following chapters initiate our rational reconstruction of Hilbert's and Frege's foundational projects. The theoretical framework underlying their respective approach will clearly be delimited as to show the philosophical aspect of the problem generated by their conflicting ideas. In this way, the articulation of their goals, their tools, their methods, their means, their claims, etc. will become the means by which can be clarified the theoretical status of each of their arguments. In other words, the paradigms of logical analysis implied by Frege's and Hilbert's respective foundation of geometry, that is their view on axiomatic systems, will become the central question in their debate.

In the chapter 3, I will present *The Foundations of Geometry* of Hilbert and the related issues. This will include the presentation of the components of his axiomatic system, and an overview of one of its metatheoretical aspect, that is, the proofs of consistency. We will also examine the continuing project inaugurated by his *Foundations* in the '20s, i.e. to the so-called Hilbert's program. Once again a detailed exposition of the vast literature is not our primary concern which is rather to briefly indicate the direction taken by Hilbert's research. The following section will discuss some key points of Hilbert's original project, i.e. axiomatics, metatheory, formalism and completeness. The last chapter will make more explicit what we means in this way: if Hilbert's pretensions are to undertake a logical analysis of the foundations of geometry, the properly logical part of his analysis is left implicit. However, I will argue that we have sufficient elements to understand what he could have consistently done within his framework. Here a tolerant approach will be required, that is, one which avoids all unnecessary restrictions as to show how Hilbert's approach is a great deal similar to the increasingly popular category theory. This way a good overview of Hilbert's logical paradigm around 1899 will have been traced.

After the presentation of Hilbert's approach, I will introduce his controversy with Frege by relating the most important passages of their correspondences on these matters (cf. 4). This will enable us to outline the striking points of opposition between Hilbert and Frege. However, the nature of the debate will ask for more precision than the mere indications that we find in these letters (though some are really penetrating).

Chapter 5's goal is to explicit the most important philosophical assumptions underlying Frege's development of his philosophy of logic and geometry. First of all, we will specify what are the goal, the sources and the means of science according to Frege. This will provide us with the assumptions that are underlying some famous themes that Frege developed in the 1890s, namely sense and reference, function and concept, concepts and their relations, truth-values, etc. Once the theoretical framework will have been outlined, we will afterward consider the main critics addressed to Hilbert, i.e. his conception of definitions and axioms, the meaning of the primitive terms, the hierarchy of concepts, and the uselessness and misleading character of metatheoretical inquiries. Since Frege's approach is not very fashionable today, I will also present a short sketch of what a Fregean foundation of geometry would have looked like.

This done, I hope, a good overview of the nature and the stakes of the controversy will appear a little clearer.

Chapter 1

Preliminary Remarks

Among philosophers, there is a widespread opinion about logic, according to which it cannot change, whether for regressing or progressing. A classical example of this attitude can be find in *Critique of Pure Reason* of Kant:

> [...] since the time of Aristotle [logic] has not had to go a single step backwards [...]. What is further remarkable about logic is that until now it has also been unable to take a single step forward, and therefore seems to all appearance to be finished and completed. (Kant, 1787, p. 106)

However, this opinion seems to be more a bias related to philosophical positions than on a reconstruction of the actual course of the history of logic[1]. By such a reconstruction, it would be seen that logic actually underwent important periods of transformation, both in its subject matter and in its methods. Hence, though it is largely thought that logic is "out of space and time" and therefore immutable, it should nevertheless be recognized that its history contains some of these paradigm switches. As any science includes in its history some radical periods of transformation that can be called revolutions (Kuhn, 1970), we will thus consider that the history of logic follows the same pattern: it consists in many important periods to which corresponds a form or a dominant orientation, punctuated by episodic revolutions. The present research is concerned with the stakes of one of these revolutions.

[1] This is not to say, of course, that the reconstruction we propose here is unbiased.

1.1 A Revolution in Logic

In the beginning of modernity, Leibniz put forward a revolutionary conception on logic. As he actually never developed it in a systematic way, "insight" would probably be a more precise word than "conception". He nonetheless established the main ideas that were to guide developments for the following centuries. At this time, it was already felt that the mere syllogistic was too restrictive, and that some logical tools had to be developed in order to express all forms of valid inferences (and not only syllogistic ones). This logic was thus to present itself as a general theory of inferences, more general than classical syllogistic.

From the very beginning, it was clear to Leibniz that the linguistic aspect of the new logic had to be taken into account. Comparing the methods of reasoning in mathematics and logic, he considered that it would be a great advance for logic to have a language as powerful, precise and perspicuous as algebra to express inferences. This is his famous project of *lingua characterica*. It was not to use the ordinary language because it contains too many logical inaccuracies and limitations. In 1854, George Boole talked about the importance of this "perfected" language in the same way:

> That language is an instrument of human reason, and not merely a medium for the expression of thought, is a truth generally admitted. It is proposed [here] to inquire what it is that renders Language thus subservient to the most important intellectual faculties. (Boole, 1854, p. 24)

On a par with this foreseen rigorous way of expression, Leibniz conceived a *calculus ratiocinator*. It was a device intended to make logical deductions in an "algebraic" way, by doing mere operations on symbols (like in a mathematical calculus). These operations were to be done according to certain precise rules corresponding to the laws of logic. The strict observance of the prescribed procedure was to guarantee the correctness of inferences. Hence, with the contribution of the *lingua characterica* and the *calculus ratiocinator*, the new logic aimed at replacing staggering mental reflexion – as much as possible – by a calculus. The attitude embodied is very clearly expressed by Leibniz's words:

> De la il est manifeste, que si l'on pouvoit trouver des caracteres ou signes propres à exprimer toutes nos pensées, aussi nettement et exactement que l'arithmetique exprime les nombres, ou que l'analyse geometrique exprime les lignes, on pourroit faire en toutes les matieres *autant qu'elles sont sujettes au raisonnement* tout ce qu'on peut faire en Arithmetique et en Geometrie.
>
> Car toutes les recherches qui dependent du raisonnement se feroient par la transposition de ces caracteres, et par une espece de calcul; ce qui rendroit l'invention des belles choses tout a fait aisée. Car il ne faudroit pas se rompre la teste autant qu'on est obligé de faire aujourd'huy, et neantmoins on seroit asseuré de pouvoir faire tout ce qui seroit faisable.
>
> [...] *Et si quelqu'un doutoit de ce que j'aurois avancé, je luy dirois: contons, Monsieur, et ainsi prenant la plume et de l'encre, nous sortirions bientost d'affaire.* [Emphasis of mine] (Leibniz, 1903, pp. 155-6)

With all these projected innovations, plus some fundamental components of 1$^{\text{st}}$-order logic partly actualized by the end of the 19$^{\text{th}}$ century, logicians departed from traditional syllogistic, thus effectuating an authentic revolution. According to Quine (1965, p. 1), "mathematical logic differs from the traditional formal logic so markedly in method, and so far surpasses it in power and subtlety, as to be generally and not unjustifiably regarded as a new science." Without having to talk of "a new science" as Quine does, it seems that the switch is radical enough to talk of a new paradigm.

Even though Leibniz had put forward the general idea, he nevertheless only developed it in a schematic way. How did this new logic develop? It was not before the middle of the 19$^{\text{th}}$ century that the first conclusive attempt to build a formal language on the model of algebra designed for logical analysis was realized by Boole (1847, 1854), whose work is generally considered to be the first embodiment of the new ideal. However, it is almost a consensus amongst *contemporary* logicians that the authentic founder of modern logic, i.e. the logician that actually accomplished the first prototype of our modern mathematical logic, was Gottlob Frege. This is so because he developed with great rigor and precision an entire formal system of 1$^{\text{st}}$-order logic in a modern fashion (except for his symbolism) (e.g. Frege, 1879, 1893). His system included:

- A description of the formal language to be used (the vocabulary and the syntax);
- The explicit rules of inference;
- The axioms of the system;
- The universal and existential quantifiers.

Moreover, Frege applied his system to mathematics, by doing a contribution to the foundations of arithmetic. Up to this day, his work is considered as a cornerstone in logical analysis of arithmetic, since it greatly contributed to the interpretation of many fundamental mathematical concepts.

But even if almost all modern logicians were to rally this new paradigm, the story was not to continue so quietly, as a Leibniz has expected.

1.2 Theoretical Importance of the Controversy

Theoretical works in mathematical logic in the second half of the 19^{th} century and of the beginning of the 20^{th} showed that if the Leibnizian project aimed at avoiding controversies once a method of analysis was accepted, as had nicely been said Leibniz, the controversies concerning the acceptation of this method were not excluded. One of these controversies, a famous and fundamental one, opposed Frege and Hilbert. This debate on the foundations of geometry concerned the way a logical analysis should be undertaken.

In 1899, Hilbert published his *Foundations of Geometry*, where he exposed an axiomatic foundation for geometry. He did not explicitly exposed what he understood as an axiomatic system, but used it implicitly in his presentation. In this way, he adopted a view where most of the developments of the above mentioned "new logic" were implicitly applied. He adopted innovative methods and rejected some of the traditional ones; or, as Frege saw it, what he rejected was crucial and made him miss the point of what is really the nature and purpose of logical analysis.

Frege had also an interest for the foundations of geometry, and he went on to exchange letters with Hilbert in view of debating on these matters. But for Frege, it was more a question of convincing Hilbert that his own conception was "authentically scientific" while Hilbert's was pure verbiage. After Hilbert decided to cease the correspondence, Frege published a first series of papers entitled *On the Foundations of Geometry* (1903). In these papers, he presented his critic of Hilbert's method and exposed briefly his own. Three years later, Frege (1906a) published a second series under the same title in which a more precise and extended version of his critic was found.

Instead of explicitly building a foundation for geometry and leaving the question of the nature of his method in the background – as did Hilbert –, he meticulously argued about his method without actually building a foundation for geometry. On the basis of a distinction between first- and second-level concepts that he considered fundamental for any science, he accused Hilbert of giving theological-style arguments. However, Hilbert explicitly rejected this method, and answered to Frege that his recommendations are actually not relevant for his enterprise of foundations of geometry in agreement with the strictest rigor asked by logic. The nature of the debate thus seems to be this: *Both Frege and Hilbert seem to share the ideal of Leibniz's new logic, without agreeing on the method that ought to be used to reach that goal.*

The following two quotes expresses it boldly: whereas Frege (1900a, p. 48) claims that "I cannot accept such a method of inference from lack of contradiction to truth. [...] It also seems to me that there is a logical danger in your speaking [. . .]" , Hilbert (1899, p. 51) answers him that "I found myself forced into it by the requirements of strictness in logical inference and in the logical construction of a theory." Actually, one finds litigation of this kind on several basic concepts (e.g. formal system, axiomatic system, deductibility, coherence, existence, truth, etc) while both of them claim logical rigor! What one considers to be fundamental for a rigorous logical analysis is considered irrelevant by the other!

From a 20$^{\text{th}}$-century standpoint, knowing the immense influence that the works of Frege and Hilbert had on philosophy (philosophy of language, philosophy of science, epistemology, logic, metalogic) and on science, it seems that this debate was – and is still – fertile and worthy of theoretical interest. Is it simply a misunderstanding between Frege and Hilbert, or is the debate deeper? As we shall see, the treatment of these problems will lead us to some fundamental questions of logic. If one thinks it presents no real difficulty because it answers to the demands of logic, there remains a lot of work to do before making it clear. This leads us to believe, at least from a preliminary standpoint, that *what animates the debate is not an internal problem of logic or metalogic* (i.e. a question of "purely deductive" argument in an established calculus), *but a problem of philosophy of logic.* It is precisely this philosophical problem that our study proposes to examine.

1.3 Divergent Commentaries

There is still a lot of discussions around the debate between Frege and Hilbert. However, after more than a century, there is not even a shadow of consensus in the specialized literature. Some thinks that the arguments proposed by Frege were of higher quality standards than those of Hilbert. As an example, we find these comments of Resnik (1980, p. 14):

> [...] Frege made an important methodological study concerning the distinction between axioms and definitions which is contained in his masterful critique of Hilbert's views on implicit definitions.

Later in his book, he even goes on to say that in this debate, only Frege had a good understanding of the nature of the philosophical problem:

> These papers show that Frege apparently had a clearer grasp of Hilbert's method than Hilbert himself did; indeed, he actually anticipated some of Hilbert's ideas. Much of the confusion concerning "implicit definitions" can be traced to Hilbert, while Frege's essays contain excellent accounts of the roles of axioms and definitions in mathematics. (Resnik, 1980, p. 107)

And, finally, he claims that "formalists confused use and mention and had no idea of how to construct a formal system" (Resnik, 1980, p. 18). To say the least, these comments do not give a lot of credit to Hilbert's works.

Some others think that Frege has been blinded by his traditionalist view of axiomatic systems:

> Hilbert repeated the role of what is now called "implicit definitions" (or, in philosophical circles, functional definition) noting that it is impossible to give a definition of "point" in a few lines since "only the whole structure of axioms yields a complete definition".
>
> Frege did not get it, or did not want to. (Shapiro, 2005, p. 67)

Accordingly, it is Hilbert that understood the genuine stakes of the debate:

> More than anybody else has Hilbert, through ingenious construction of suitable arithmetical models, contributed to the clarification of the logical relations that connect the various parts of the geometrical system of axioms. (Weyl, 1949, p. 22)

However, commentators are in general more mitigated, trying to find both the good and the bad aspects of Frege's and Hilbert's respective arguments, as the following passage shows:

> Although Frege's logical objections were well taken, and although a correct understanding of the axiomatic method must begin with Frege, the dominant view, especially among mathematicians, is still the view expressed recently by H. Scholz: "... no one doubts nowadays that while Frege himself created much that was radically new on the basis of the classical conception of science, he was no longer able to grasp Hilbert's radical transformation of this conception of science, with the result that his critical remarks, though very acute in themselves and still worth reading today, must nevertheless be regarded as essentially beside the point." While Freudenthal has tried to give a more balanced historical account, his study points nevertheless in the same direction. H. Steiner does more justice to Frege's arguments, but his verdict, too, is largely determined by the traditional interpretation of Hilbert's school. A contrary interpretation has been proposed by Kambartel, who argues that Frege's objections can be construed almost in their entirety as a well-founded critique of Hilbert's ideas. (McGuinness, 1980, pp. 31-2)

The *fact* that there is still no consensus after more than a century of controversy is quite revealing. It indicates that we are here dealing with a *typical philosophical problem*, i.e. that *there is no consensus in the community of logicians on the subject, either on the way of setting the problem, or on the solutions.* It will not be possible to evaluate Frege's and Hilbert's position by confronting them with a generally accepted standard. For this reason, it will be essential to defend as well as possible the respective conception of both Frege and Hilbert in our reconstruction of them and even, if necessary, to enhance them (within the limit of their conception), in order to have a more thoroughgoing look on this debate, which have a theoretical significance even today.

Chapter 2

The Historical Context of the Controversy

The manifold developments in the 19^{th} century have seriously changed the mathematical landscape: the birth of non-Euclidean geometry, developments in projective geometry, the mathematical enhancement of set theory, the new arithmetic for infinite numbers, many breakthroughs in algebra, major improvements in mathematical physics, the rigorization and refinement of analysis, etc. All these overthrows are enough, according to Stein, to think that during the 19^{th} century, mathematics underwent "a transformation so profound that it is not too much to call it a second birth of the subject" (1988, p. 238).

But there was also a "dark side": doubts on the foundations of arithmetic, the non-representationalist approach preached by some mathematicians and philosophers, paradoxes in set theory, provocation of the commonsense, etc. Moreover, the birth of non-Euclidean geometry has not only brought enthusiasm, but also insecurity: what will actually become of our scientific knowledge if, after two millennium of certainty, even Euclidean geometry – the science *par excellence* – can be put back into question?

This has certainly stimulated the manifold foundational and ordering works of both the new and the traditional disciplines. In what follows, I will present a sketch of the "intellectual context" of this period by briefly looking at some important events. There is no pretension of exhaustive historical rendering; my point is only to give milestones of this intellectual climate.

The first subsection will show how arithmetic and analysis have gone through a wave of rigorization, with the replacement of "intuitive evidence" by "analytic proof" (2.1.1). The first steps of set theory and the integration of infinite and transfinite numbers in mathematics brought the same kind of transformations (2.1.2). In both cases, this led to an improvement on the methods of proof having ultimately led to set theory and mathematical logic. This will bring me to speak about some difficulties it engendered, namely the discovery of antinomies by the turn of the century (2.1.3).

In the second section, an overview of the developments of 19$^{\text{th}}$-century geometry will be drawn. First, it will be indicate what these radical changes represented for philosophers (2.2.1), from a general point of view. Thereafter, I will present a short account of the changes most relevant to the problem treated here, namely the apparition of non-Euclidean geometries (2.2.3), the development of projective geometry (2.2.2), the Erlanger program (2.2.4) and the works done in view of perfecting the axiomatic method (2.2.5).

To close this chapter on contextualization, I will present a short account of Frege's and Hilbert's life and first works (2.3.1 and 2.3.2). With those informations, we will be able to see, in the following chapters, that *Hilbert actually followed and enhanced the fashionable conception of his time* (the trend to abstract proofs, regardless of the possibility of a representation), while *Frege reacted against the increasing popularity of this view.*

2.1 The Rise of Foundational Inquiries

2.1.1 The Foundations of Arithmetic and Analysis

According to Coffa, 17$^{\text{th}}$- and 18$^{\text{th}}$-century calculus was a kuhnian paradise: there was a lot of wonderful exemplars fueling a staggeringly successful puzzle-solving tradition. The growing ranks of practitioners shared several basic symbolic generalizations, which were fruitfully applied to all sorts of problems in mathematics or in other expanding sciences. To make things match with Kuhn's conception of normal science even better (Kuhn, 1970), no one really knew what exactly was going on. Most people had esoteric interpretations of what these formulas said, and most or all of these interpretations made only modest sense. However, some voices, as Berkeley's, were complaining about the incoherence of the whole enterprise, and specifically about the fact that no meanings were attached to the more crucial expressions of calculus (Coffa, 1982, p. 685). Nonetheless, as long as mathematicians has not felt this to be an impediment to their work, they did not allowed much attention to such critics.

At the turn of the century, mathematicians started to try to figure out what was happening, to try to figure out what the fundamental expressions of calculus meant, like continuity, differentiability, infinitesimal, function, etc. (Coffa, 1982, p. 685) In this way, one of the first major step was taken by Bolzano. This engendered a movement that is now known as the *rigorization of analysis*.

The Rigorization of Analysis

At the beginning of the nineteenth century, some foundational insecurity began to be felt by mathematicians. For sure, analysis was praised as an extremely powerful method; but there was no clear understanding of *why* and *how* analytical methods led to fruitful result that fruitful. The procedures of proof in analysis often seemed to be a mere juggling with symbols very remote from "clear and distinct" reasoning. In France and England, some mathematicians were trying to establish analysis on a solid footing with the help of empiricist frames like those of Locke or Condillac. But all this remained shaky, as it could be expected (Richards, 1986, pp. 301-2).

Geometry was considered by some mathematicians to be foundationally secure, for many different reasons. As we shall see in subsection 2.2.2, it gave birth to attempts of foundation of analysis by the means of projective geometry. However, for many people like Lagrange, d'Alembert, Cauchy, Bolzano, Weierstrass, etc, rigorization of mathematics was almost synonymous with *arithmetization of mathematics* because, according to their conception of a mathematical science, the focus on the consistency and the rigor of proofs was more important than on the representability and the interpretability. If "infinitesimals" and other geometrical "fictions" could act as a good heuristic device, it was nonetheless banished from the field of mathematics by these mathematicians who strove to build up a precise arithmetical definition of limit (Robinson, 1966). This is obvious from this comment by Bolzano (1810, p. 177):

> None the less, it seems to me that arithmetic is still by far the most complete of the mathematical disciplines; geometry has much important defects which are more difficult to remove. At present a precise definition is still lacking for the important concepts of *line, surface, and solid*.

The type of defect present in geometry in his time seemed to him to be a non-neglectable impediment to authentic mathematical research. As he expresses clearly, the strictness in the proofs of arithmetic were more in agreement with the demands of science: "I stipulate the rule that the *obviousness of a proposition* does not absolve me from the obligation still to look for a proof of it [...]." (Bolzano, 1804, p. 172) And he adds:

> It therefore happens that one discounts the obligation to prove propositions which in themselves already have complete certainty. This is a procedure which, where we are concerned with the practical purpose of certainty, is quite correct and praiseworthy; but it cannot possibly be valid in a scientific exposition, because it contradicts its essential purpose. (Bolzano, 1810, p. 192)

The works of Bolzano were popularized among mathematicians by his famous "purely analytic proof" that for any continuous function for which there is a positive and a negative value, then there is also a zero value somewhere between (Bolzano, 1817). If one relies on intuition, this will appear obvious. As Coffa (1982, p. 686) remarks, Bolzano's problem is one only to someone who thinks that intuition is not an indispensable aid to mathematical knowledge. In this perspective, even the most obvious things are in need of a proof, if such a proof is possible. Many mathematicians followed this trend of arithmetization as a way to rigorize mathematics, the most eminent names being Cauchy, Bolzano, Weierstrass, Dedekind, Cantor, etc.

To fulfill the mathematical demands of the arithmetization of mathematics, it became necessary to provide a rigorous understanding of what a number is, independently of its geometrical interpretation. To this task can be mainly attached names like Frege, Dedekind, Cantor, Peano, etc. They all tried to grasp what was the essential properties of numbers. First of all, Frege attempted a reduction of them to logical properties (Frege, 1879, 1884, 1893). As a condition, the actualization of his project supposed the availability of a highly rigorous and precise system of logic, which he himself developed. However, his work has not been very popular among fellow mathematicians, except maybe for a note that we find in Dedekind's preface to the second edition of (Dedekind, 1888). On the ground of what is now known as naive set theory, Dedekind based his construction on the insight that each real number is in some sense determined by all the rational numbers to the left and right of it; it is what is now known as "Dedekind cuts". Thus, the real numbers could be defined as a pair of sets of rational numbers. A year later, Peano provided a mathematical logical account of natural numbers (without, however, pretending that he reduced arithmetic to logic as Frege did); the axioms he used are now known as Peano's axioms for arithmetic. Frege's, Dedekind's and Peano's axioms for the natural numbers have all been discovered independently (at least according to what they officially claim). For Cantor's account of the real numbers, see 2.1.2. The fact that so many mathematicians independently achieved important results about this problem is sufficient, in my view, to conclude that it was an central preoccupation of the time to replace intuitively vague notions by precise concepts. However, neither Dedekind nor Cantor succeeded in proving that their "real numbers" were in fact just like the geometric number line, in the sense that each point on the number line corresponded to a "number" in their system and vice versa. It was considered to be an *axiom*, i.e. a basic truth accepted without proof. This "hypothesis" will be discussed in the next subsection.

2.1.2 The Integration of the Infinite in Mathematics

The Infinite up to Bolzano

According to the tradition, it is said that Aristotle vehemently condemned the infinite. The starting point of his argument was the now classical distinction between actual infinite and the potential infinite (Aristote, *Phys.*, III 4-8). His conclusion is that infinity has merely potential existence but are never actualized[1]. From this standpoint, it was therefore necessary to exclude them from the realm of science.

> Beginning with Aristotle (384-322 B.C.E.), two thousand years of Western doctrine had decreed that actually existing collections of infinitely many

[1] However, it can be asked whether it is an abuse of the tradition to impute this position to Aristotle. Hintikka, for instance, argues that Aristotle does not endorse such a position. He points that there are two different senses in which Aristotle uses the term 'infinite': one in which it exists neither actually nor potentially, and an other in which it exists both potentially and actually (Hintikka, 1966).

> objects of any kind were not to be part of our reasoning in philosophy and mathematics, since they would lead directly into a quagmire of logical contradictions and absurd conclusions. (Laubenbacher and Pengelley, 1998, p. 54)

There are many examples of these paradoxes on the infinite. Everybody heard about the famous four Zeno's paradoxes. Even if they are tied to commonsense in their presentation, they are nevertheless mathematical in nature (as long as the problem of the infinite is mathematical). These debates about the infinite are not mere Greek quibbles; actually, even a pioneer like Galileo considered that the infinite is not proper to science, since he discovered a paradox of his own. He proved that there is as much perfect squares as there are natural numbers, by pairing off squares and naturals:

$$\begin{array}{ccccc} 1 & 2 & 3 & 4 & \dots \\ \updownarrow & \updownarrow & \updownarrow & \updownarrow & \updownarrow \\ 1 & 4 & 9 & 16 & \dots \end{array}$$

As some natural numbers are not perfect squares, it would be perfectly normal to think that there are more natural numbers than square numbers; this is obviously paradoxical. From this, Galileo concluded that the attributes "smaller", "larger", and "equal" cannot be used to compare infinite quantities with each other or to compare finite with infinite quantities (Laubenbacher and Pengelley, 1998, p. 55). Even another great pioneer as Gauss rejected it:

> I protest . . . against using infinite magnitude as something consummated; such a use is never admissible in mathematics. The infinite is only a *façon de parler*: one has in mind limits which certain ratios approach as closely as is desirable, while other ratios may increase indefinitely. (In (Fraenkel, 1953, p. 1))

For such reasons, only the so-called potential infinite were to be admitted. However, the development of some branches of mathematics (e.g. calculus) asked for more. In calculus, this is central to know what is the behavior of $f(x)$ when x becomes arbitrarily large. It would be very useful to be able to replace x by the actual value of which it approaches infinitely. For this, the mathematical infinite had to be examined more scrupulously, and it was necessary to provide an algebra for these "infinite magnitudes".

From Bolzano to Cantor

It is Bolzano who began to examine systematically the paradoxes of the infinite (Bolzano, 1851). According to him, it was imperative to solve these paradoxes:

> [...] a satisfactory refutation of apparent contradictions [of the paradoxes of the infinite] is requisite for the solution of very important problems in such other sciences as physics and metaphysics. (Bolzano, 1851, p. 250)

His position was completely different from the traditional one, since he concluded that the infinite is a genuine part of mathematics. Instead of using one-to-one correspondence to reject investigation of the infinite (as Galileo did), he claimed that it was the fundamental tool to explore the nature of infinite sets. Unfortunately, his works did not receive much attention. For this reason, it was Cantor who made the decisive step. As for Bolzano, many of his contemporaries rejected his works, among whom were many mathematicians and most of the philosophical authorities (Fraenkel, 1953, p. 2). However, within twenty years, it became generally accepted and talked about with great enthusiasm.

Cantor's first works concerned number theory and analysis. While he was trying to generalize some results obtained in his studies of infinite trigonometric series, it became clear to him that the sorts of questions he needed to answer about infinite sets of points on the real number line required a much deeper understanding of the nature of real numbers than what was possible with the essentially geometric representation. What was needed was a "functional definition" based on the idea that each real number could be defined by a sequence of rational numbers converging to it.

Cantor recognized different sizes among infinite sets. He defines infinite "cardinal numbers" as abstractions of infinite sets, by ignoring the order of their elements, that is, by retaining only an unordered collection. In order to compare different kinds of infinite sets, Cantor used the notion of one-to-one correspondence, as Bolzano did before. Two set X and Y are considered to have the same cardinality if there is a one-to-one correspondence between them. The first important result Cantor obtained was a rather counter-intuitive fact about the power of the continuum (i.e. the set of real numbers $\mathbb{R}$). Cantor had found a proof that the set of rational numbers ($\mathbb{Q}$) had the same power as the natural numbers ($\mathbb{N}$), i.e that it was a *denumerably infinite* set:

$$card(\mathbb{Q}) = card(\mathbb{N}) = \aleph_0$$

Cantor also gave a proof that each interval of real numbers had a strictly *larger* cardinality than denumerable sets, i.e. that they are *undenumerable*:

$$card(\mathbb{R}) = card(\wp(\mathbb{N})) = 2^{\aleph_0} \quad \text{so that} \quad card(\mathbb{R}) > card(\mathbb{N})$$

Hence, every interval had to contain infinitely many numbers that were not rational. He then hit upon an essential characteristic of the *continuous* set of real numbers versus a set of smaller cardinality (e.g. the *discrete* set $\mathbb{N}$).

He continued his investigations on the infinite with even more troubling results. He thought that he succeeded in showing that it was possible to put the points of the plane in a one-to-one correspondence with the points of the line, and more generally the points of a space of arbitrarily high dimension. But it was thought to be a unique characteristic of a space. This result was directly opposed to all common sense!

The works of Cantor suggested a "very plausible theorem" (as Hilbert said), establishing that the cardinality of every set of infinitely many real numbers is either equal to $card(\mathbb{N})$ or equal to $card(\mathbb{R})$. In other words, *all infinite sets of real numbers are either denumerable or the continuum* (Hilbert, 1902a). However, it turned out that this claim of Cantor was not proved, and accordingly it became known as the continuum *hypothesis* (and not *theorem*). The problem of giving a proof of Cantor's hypothesis has been one of the most discussed. In 1900, in his celebrated lecture which was to draw the 20$^{\text{th}}$-century mathematics by a series of major unsolved problems, Hilbert ranked this problem first. Since this time, the questions on transfinite numbers and on the continuum hypothesis occupied a very wide place in mathematical discussions (Fraenkel, 1953, p. 3).

All these works for the rigorization of arithmetic and analysis dealt with new questions, with new demands. To treat them, tools like mathematical logic and set theory had been developed with great precision. However precision is a two-edged sword: if there are ambiguities or contradictions, they will emerge. And that is what happened by the turn of the century.

2.1.3 Antinomies

If the studies mentioned previously solved some of the traditional problems and paradoxes, they also engendered new ones, maybe more profound. What is sure is that these paradoxes deeply influenced the world of mathematics and the development of mathematical logic:

> Although logic is basic to all other studies, its fundamental and apparently self-evident character discouraged any deep logical investigations until the late 19^{th} century. [...] This new interest, however, was still rather unenthusiastic until, around the turn of the century, the mathematical world was shocked by the discovery of paradoxes [...]. (Mendelson, 1987, p. 1)

The importance of these paradoxes for the development of abstract proof methods should not be overemphasized, since the increasing abstractness of the new mathematical disciplines mainly stimulated this transformation. However, even if they were not the main catalysts, they nonetheless had an importance. I will here give a sketch of two famous examples of these paradoxes, namely Burali-Forti's and Zermelo-Russell's.

Burali-Forti. By these years, Cantor's account of set theory was becoming more and more known and accepted. It has been a shock when, in 1897, Cesare Burali-Forti published a note exposing a paradox lying in set theory (Burali-Forti, 1897). Many important mathematicians discussed it (e.g. Cantor discussed it with Dedekind (Cantor, 1899) and Hilbert (Cantor, 1897) in his correspondence), in order to identify what was wrong with their approach: "Dozens of papers dealt with it, and it gave a strong impulse to a reexamination of the foundations of set theory." (van Heijenoort, 1967a, p. 104) In his paper, Burali-Forti presents a proof of a paradoxical sentence formulated in Peano's formula language.

> The principal object of this note is to prove that there actually exist *transfinite numbers* (or *order types*) a and b such that a is not equal to b, not smaller than b, and not larger than b. (Burali-Forti, 1897, p. 105)

In more contemporary terms, it can be formulated in this way: as the set of ordinals is well-ordered, it has an ordinal; this ordinal is at once an element of the set of ordinals and greater than any ordinal in the set (van Heijenoort, 1967a, p. 104). But the ordinal number determined by the set of all ordinal numbers is the largest ordinal number, which brings us to a paradox. Some years later, Burali-Forti himself recognized that when he formulated his paradox, he made a confusion in the use of the notion of well-ordered set (van Heijenoort, 1967a, p. 104). Nevertheless, it was another example of the necessity of clearly delimitating the mathematical concepts, by strict definitions and proof methods.

Zermelo-Russell paradox. According to Russell, he discovered this paradox in 1901 (van Heijenoort, 1967a, p. 124). However, the first note about the existence of his discovery is in the famous letter to Frege that he sent on 16 June 1902 (Russell, 1902). The first published discussion of this paradox is to be found in 1903 (Russell, 1903). This well-known paradox goes as follows:

> [...] this view now seems to me dubious because of the following contradiction: Let w be the predicate of being a predicate which cannot be predicated of itself. Can w be predicated of itself? From either answer follows its contradictory. We must therefore conclude that w is not a predicate. Likewise, there is no class (as a whole) of those classes which, as wholes, are not members of themselves. (Russell, 1902, pp. 130-1)

However, it is now known that Zermelo independently discovered the same paradox. In 1908, Zermelo himself claimed in a note that he discovered the paradox before Russell published it: “I had, however, discovered this antinomy myself, independently of Russell, and had communicated it prior to 1903 to professor Hilbert among others.” (Zermelo, 1908, p. 191) Some discussions on the question can be find in (Reid, 1970, p. 98) and (Ewald, 1996b, p. 923). The decisive proof that Zermelo actually discovered the paradox independently can be find in (Rang and Thomas, 1981) where it is shown that he communicated it to Husserl, thanks to a note dated 16 April 1902 (2 months before Russell's letter to Frege). The formulation that we can find in this note is similar to Russell's:

> A set M, which contains each of its subsets $m, m', \ldots$ as elements, is an

> inconsistent set, i.e., such a set, if at all treated as a set, leads to contradictions. (cited in (Rang and Thomas, 1981, p. 17))

The point of talking about this dispute here is not so much to establish who discovered the paradox first, but only to show that the mathematicians of the group of Göttingen were aware of such paradoxes in set theory, and surely worked actively to solve the problem[2]. This brought many people to study the foundations of set theory more carefully and, for sure, this stimulated investigations in mathematical logic.

2.2 The 19$^{\text{th}}$-Century Revolution in Geometry

It is often said that the radical turn of nineteenth-century geometry is due to the apparition of non-Euclidean geometry. However, it should be taken into account that this is only partially the case, since both projective geometry, non-Euclidean geometry, and n-dimensional geometry played an important role (Shapiro, 1996, p. 149). In the following subsections, I will present briefly some of these changes.

2.2.1 Geometry for Philosophers

Starting with Hellenistic Greece, Euclidean geometry always represented the prototype of an "achieved science", a science without flaws, where everything is evident in itself and founded in reason. For philosophers, it was *the* instance of an actualized system of concepts that is giving us unquestionable truths on nature, even though it was the product of our "human, all too human" understanding. Through the centuries, it continued to represent the classical example of a paradigm of epistemic certainty (e.g. for Descartes, Hobbes, Spinoza, Locke, Hume, Kant, etc.) (Schlimm, 2003). This reveals the importance of this science for philosophers.

However, a certain problem remained throughout this time: geometry seems to provide necessary truths in an abstract sense (*a priori*), and at the same time it formulates relations about "natural bodies" (*a posteriori*). Traditionally, the first are necessary by essence, while the seconds are contingent. It has always been a difficult problem for

[2] As we have established, Hilbert also discussed of Burali-Forti's paradox with Cantor.

philosophers to give an account of both aspects. In the 18th century, Kant has presented a solution that occupied a central place in philosophical discussions about geometry:

> A philosopher may find it difficult to reconcile these two features. Kant's account of geometry as synthetic *a priori*, relating to the forms of perceptual intuition, was a heroic attempt to accommodate both the necessity and the empirical applicability of geometry. (Shapiro, 1996, p. 149)

For the particular case of Germany, some interprets go as far as saying that for the "better or worse, almost every philosophical development since 1800 has been a response to Kant" (Coffa, 1991, p. 7). However, 19th-century developments in geometry imposed new questions, themselves imposing doubts on the Kantian-style philosophy of geometry. Actually, there was a cataclysmic growth both in its content, in its internal diversity and in its methods. With the apparition of projective methods and non-Euclidean geometries, new questions aroused: which axioms are true? Is it still a necessity that Euclidean space is true? Is there really a sense in asking *a priori* if one of them is true? And if, from a geometrical standpoint, it were possible to keep them all, without having to select one of them as "the true theory of space"?

During the second half of the nineteenth century, through a process still awaiting explanation, the community of geometers reached the conclusion that all geometries were there to stay (Coffa, 1986). The paradigm of epistemic certainty that Euclidean geometry represented then became a particular case of a more general mathematical theory, thus shattering without mercy many "philosophical illusions". It asked for a revision of all our conception of knowledge, whether scientific or philosophical. From then, Euclidean geometry could not serve anymore as the paradigmatic example of those advocating absolute knowledge. But, if even Euclidean geometry would not fulfill this role, which science could do so?

> It was now up to philosophers [...] to make epistemological sense of the mathematicians' attitude toward geometry [...] For decades professional philosophers had remained largely unmoved by the new developments, watching them from afar or not at all [...] As the trend toward formalism became stronger and more definite, however, some philosophers concluded that the noble science of geometry was taking to harsh a beating from its practitioners. Perhaps it was time to take a stand on their behalf. In 1899,

> philosophy and geometry finally stood in eyeball-to-eyeball confrontation. The issue was to determine what, exactly, was going on in the new geometry. (Coffa, 1986, p. 17)

What happened in 1899 was Hilbert's publication of the *Grundlagen der Geometrie*. The *rendezvous* in question is nothing else than the debate between him and Frege. But before going to the debate straightforwardly, let's have a look at the developments that preceded Hilbert's publication.

2.2.2 Projective Geometry

In the nineteenth century, projective geometry was practically synonymous to modern geometry. As its name says, it studies projective properties, i.e. those preserved by projections. This study does not depend on relations of mutual equality and inequality of magnitudes; in a sense, it is a "non-quantitative" geometry, not involving the actual size of objects, but only their relative positions. The systematic investigation of geometry without the relation of congruence of given magnitudes came firstly in the 17$^{\text{th}}$ century with Desargues and Pascal.

Following Desargues, parallel lines are intersecting at infinity. He also regarded circles, ellipses, parabolas, and hyperbolas as a single family of curves, on the ground that all could be viewed as projections of a common figure, with the center of projection at the so-called "improper point" at infinity. This was obviously not something to be visualized. Furthermore, whether two curves intersect at a "real" or "imaginary" point was also of no consequence, because the occurrence of "real" or "imaginary" intersections depends entirely on the accidental position of the bodies to be projected with respect to one another and on the plane upon which they are projected. Hence, the difference between "imaginary" and "real" points was no more to be considered as absolute (in this framework), but as relative. The consequences for geometrical techniques were important, startling, and to some geometers rather disquieting (Nagel, 1939, p. 140). For this reason, it is fair to say that projective geometry signified a much deeper and far-reaching revolution than did the "mere" denial of Euclid's parallel postulate (Torretti, 2003).

However, this method has thereafter been partially eclipsed by Descartes' analytic method, until Monge and his school revived it one century later. As for Desargues, but with a more far-reaching treatment, projective method for them involved establishing connections among propositions previously considered separated. This level of generality brought to the recognition of some new geometrical entities which had no place in Euclidean space, e.g. the "points at infinity" (who were the points of intersection of parallel lines) (Richards, 1986, p. 302). By the use of these "imaginary" elements of space, projective geometers wanted to make geometry as powerful as analysis and Cartesian coordinate method. Later, Poncelet was brought to think that it was at least equally powerful than Descartes's method and certainly more "geometrical" than Cartesian procedure (Nagel, 1939).

But as it could be expected, these "geometrical fictions" had not been accepted easily. Poncelet was one of the first to make a great effort to try to "justify" their introduction, by postulating them and thereafter showing powerful results. Many contemporaries were nevertheless not ready to go as far as postulating such things in the (real!) space. In the 1820's, Cauchy has severely criticized him, and this led to a controversy between projective geometers and analysts (see 2.1.1). To convince their contemporaries, mathematicians like Chasles and von Staudt constructed these elements from already accepted one (in the same sense that Dedekind built $\mathbb{R}$ from $\mathbb{Q}$). In 1826, Gergonne adopted another way of introducing these imaginary elements, by implicitly defining them. According to Nagel, he was the first to make a sharp distinction between explicit and implicit definitions (Nagel, 1939, p. 168)[3]. This attitude towards imaginary elements was certainly to encourage the unifying work later done by Klein (see 2.2.4) and the axiomatization work of Pasch (see 2.2.5), who both started with projective geometry.

[3]It is a very important point for our development. In 1818, Gergonne published an *Essai sur la théorie des définitions* where he gave a characterization of the notion of implicit definition: "Si une phrase contient un seul mot dont la signification nous est inconnue, l'énoncé de cette phrase pourra suffire à nous en révéler la valeur. Si, par exemple, on dit à quelqu'un qui connaît bien les mots *triangle* et *quadrilatère*, mais qui n'a jamais entendu prononcer le mot *diagonale*, que chacune des deux diagonales d'un quadrilatère le divise en deux triangles, il concevra sur-le-champ ce que c'est qu'une diagonale et le concevra d'autant mieux que c'est ici la seule ligne qui puisse diviser le quadrilatère en triangles. Ces sortes de phrases qui donnent ainsi l'intelligence de l'un des mots dont elles se composent, au moyen de la signification connue des autres, pourraient être appelées *définitions implicites*, par opposition aux définitions ordinaires, qu'on appellerait *définitions explicites*." (cited in Blanché (1967, p. 30))

2.2.3 Non-Euclidean Geometries

When Euclid compiled and presented the elements of the geometry of his time, he made use of axioms, definitions and common notions in order to render his proofs rigorous. However, the information they provided was insufficient. Four axioms appeared independent facts about space, while the fifth, the parallel postulate, seemed to be a consequence of the other four. Throughout history, many tried to prove that the parallel postulate derives from the others. A well-known attempt was Saccheri's *reductio ad absurdum*. He however only derived a proposition "which is repugnant to the nature of the straight line". This understanding of "the nature of the straight line" was, to be sure, nourished by Euclidean geometry. The question remained: was the postulate of parallel independent of not?

In the 1820's, N.I. Lobachevskii and J. Bolyai independently turned the question over. Lobachevskii built a system of geometry in which a negation of the parallel postulate was true. On the other hand, Bolyai simply expelled the postulate from geometry, the remaining four being the core forming "absolute geometry". We should note that Gauss worked on these things also from 1790, but refrained from publishing his results fearing a scandal. From these non-Euclidean geometry, it was possible to derive many surprising theorems. For example, in Lobachevskian geometry, it is possible to prove that the three interior angles of a triangle add up to less than two right angles, that the difference with 180 is proportional to the triangle's area, that in a quadrilateral formed with three right angles the fourth is necessarily acute (i.e. there are no Euclidean rectangles), etc. They thus created the first non-Euclidean geometries.

It has been harder for philosopher to accept non-Euclidean geometry than for mathematicians, the latter having been prepared very well by two domains of the 19th-century geometry, namely projective and differential geometry. In this way, Beltrami proved mathematically in 1868 that non-Euclidean geometry was as consistent as Euclidean geometry. His proof showed that hyperbolic geometry, as all other kinds of "absolute geometries" in Bolyai's sense, can be seen as a model of non-Euclidean geometry (Pambuccian, 2002, p. 331). Since there is a formal correspondence between the equations in Lobachevskian trigonometry and those of standard spherical geometry, it gave a proof that if there were to be a contradiction in this geometry, there would also be one in Euclidean geometry. This seems to have been the first argument of relative consistency. As Poincaré pointed out, their mathematical footing were then equal:

> We know rectilinear triangles the sum of whose angles is equal to two right angles; but equally we know curvilinear triangles the sum of whose angles is less than two right angles. The existence of the one is no more doubtful than that of the other. (cited in (Shapiro, 1996, p. 152)

This gave a new meaning to the question “which of these geometry is true?” Were scientist supposed to make experiment to prove something as obvious as the parallel postulate? Some geometers thought so. Riemann gave a disquieting account of the problem in a lecture of 1867, in Göttingen. He presented radically innovative views, that the measurable properties of a discrete manifold can be readily determined by counting, but that continuous manifold do not admit such an approach (in particular the metric properties of physical space): “These matters of fact are – like all matters of fact – not necessary, but only of empirical certainty; they are hypotheses.” (Riemann, 1868, p. 653) Hence, he thought that the different models of geometry were not susceptible of being empirically tested. But if their mathematical footing were equal and if it was not to be decided on empirical grounds, how were scientists to make claims as to the nature of space? Pursuant to the Erlanger Program, each of these geometries of constant curvature has been characterized by its own group of homomorphisms, and were ultimately equivalent. This “no-criteria” situation was really troublesome for many mathematicians and philosophers. An instance of this tension will be considered in the debate between Hilbert and Frege.

2.2.4 Klein’s Erlanger program

After the apparition of hyperbolic geometry (of which Lobachevskian is a type) and elliptic geometry (of which Riemannian is a type), geometry was separated in many fields. Klein’s feat of skills was to propose a standpoint from which its many branches could be organized into a unified field, their differences being ultimately only the value attributed to the value of a certain variable k (measure of curvature). As it was usually in projective geometry, he rejected the parallel postulate and introduced numerical coordinates by von Staudt’s method (as in analytic geometry, but with projective methods). In this framework, where it was possible to consider projective geometry as a theory of linear transformations, he formulated the general connection existing between a group of transformations and the properties left by them (under these transformations).

The differences between the various geometries are in fact the differences between the relations they explore. For example, school geometry discusses metrical relations, such as the conditions under which line segments, angles, areas and volumes are equal or unequal; projective geometry studies conditions under which a set of points remains co-linear, etc. Within each geometry we discover that certain transformations may be performed which leave unchanged or invariant the relations which are characteristic of that geometry (e.g. translation or rotation without altering metrical relations (for Euclidean geometry), projection without destroying collinearity of points (for projective geometry), etc.). Klein's point is that *the relations or properties which a geometry explores are those which are invariant under a set of transformation*; the invariant properties and permitted transformations mutually determine each other, so that either the invariant properties or the set of transformations may be taken to characterize that geometry. Hence, instead of saying that Euclidean geometry studies relations of magnitude between segments, angles, etc., we may say that it studies properties which are invariant under translations, rotations and reflexions (Nagel, 1939, p. 204-5).

Now, the question becomes "can metric properties be fixed in this way?". Traditionally, the distance between two points $(x_1, \ldots, x_n)$ and $(y_1, \ldots, y_n)$ was defined by mean of this formula:

$$\sqrt{(x_1 - y_1)^2 + \ldots + (x_n - y_n)^2}.$$

The group of Euclidean relations consists of the transformations that preserve this function. In other words, characterizing distance between points by this formula is no more than a convention adopted to ensure that this geometry is Euclidean. With projective geometry, Klein thought of something more general: he employed the cross-ratio for defining projectively invariant distance functions on specific regions of projective space. He defined three types of ratios, the first coinciding with Euclidean geometry, the second with Lobachevskian, and the third with Riemannian. The following table compares some of their main properties (Carnap, 1966, p. 133):

type of geometry	number of parallels	sum of angles in triangle	ratio of circumference to diameter of circle	measure of curvature
Lobachevskii	∞	$< 180°$	$> \pi$	< 0
Euclid	1	$180°$	π	0
Riemann	0	$> 180°$	$< \pi$	> 0

Another of Klein's important result was the extension of the principle of duality in order to show that the properties holding for a geometry $\langle A, B\rangle$ (elements A invariant under the group of transformations B) are dual to the properties of another geometry $\langle A', B'\rangle$ for which $A \cong A'$. That is, for two geometries $\langle A_i, B_j\rangle$ with A_i as elements and B_j as invariant transformations, we have

$$A_1 \cong A_2 \Rightarrow B_1 \cong B_2{}^{4}.$$

Hence, there is no fact about space expressible in Euclidean terms that cannot be stated in a non-Euclidean system. The only difference is that things under one name in one system (e.g. 'straight line') will receive a different name in the other. This important result brought many geometers, like Klein and Poincaré, to argue that it is impossible to figure out by an experiment with a series of measurement if physical space is Euclidean or not (Shapiro, 1996).

2.2.5 Perfectibility of Axiomatics

At the end of the 19^{th} century, axiomatics has been enhanced by the integration of many tools developed in relation with the refinement of abstract proof methods mentioned before. It is often said that the first account of a "modernized" axiomatics, in the spirit of what Hilbert will do some years later, is to be found in Pasch's *Vorlesungen über neuere Geometrie* (1882). He denied that geometry as a natural science requires or could have an *a priori* foundation, and maintained that the evidence for the truth of its axioms rested on the facts of sensory intuition, the application of the concepts and axioms to physical bodies being always associated with a certain amount of uncertainty (Nagel, 1939, p. 193). So, it is important for him that geometry be *formal* in a strict sense, because the validity of the deductions of theorems should be completely independent of the intuitive meanings of the terms they contain. From a set of formalized nuclear propositions (the axioms), all other geometric sentences should be derived by the strict deductive method:

> In fact, provided the geometry is to be truly deductive, the process of inference must be entirely independent of the *meaning* of the geometrical

[4] I.e. if the elements of two geometries are homomorphic, their invariant transformations will also be homomorphic.

> terms, just as it must be independent of the figures. (cited in (Kennedy, 1972, p. 133))

Another important step along these lines was done in 1889 when Peano published *The Principles of Arithmetic*. In this paper, he adapted Pasch's work and "translated" it in his notation of mathematical logic which is intuitively opaque, but mathematically precise. By this transformation, Peano increased Pasch's standard of formalization by actually giving a complete description of his language and by specifying the axioms that the logical calculus presupposed with the deductions inside the axiomatic system built for arithmetic.

> With these notations, every proposition assumes the form and the precision that equations have in algebra; from the propositions thus written other propositions are deduced, and in fact by procedures that are similar to those used in solving equations. This is the main point of the whole paper. (Peano, 1889, p. 85)

The pattern of formal relations which is alone relevant to the validity of demonstrations thus receives a systematic expression. The systematic aspect of his presentation represents an explicit account of the "algebraic" attitude toward the primitive terms and relations. Another important aspect of his systematic view is that he had a concern – before Hilbert – for the mutual independence of the axioms of the system. In his *Principles of Geometry*, he approaches the problem without however providing an effective proof of their independence. The only thing he says is that this "ordering of the propositions clearly shows the value of the axioms, and we are morally certain of their independence" (cited in (Kennedy, 1972, p. 134)). However, mathematicians were not satisfied with such moral proofs. Ten years later, in his axiomatization of Euclidean geometry, Hilbert wanted to prove these claims concerning the independence of the axioms. As we will see, he produced such a proof by means of models of the structure determined by all axioms but one. This brought Frege to complain that axioms thus understood can only be use in a meaning violating the natural one. This, once again, will lead us to the controversy (4).

2.3 Frege and Hilbert: Biographical Notice

2.3.1 Frege

Frege was born on November 8th 1848 in Wismar. He attended at gymnasium, and then went to the University of Jena. In 1871, he left for the University of Göttingen where he studied mathematics, physics, chemistry and philosophy. Two years later, he received his doctorate with a thesis entitled *On a Geometrical Representation of Imaginary Forms in the Plane* (Frege, 1873), and it was immediately published. A year later, in 1874, he presented his habilitation at Jena with a work essentially on Abelian groups and Invariant theory entitled *Methods of Calculation based on an Extension of the Concept of Quantity* (Frege, 1874), and was appointed Privatdozent the same year. He taught there all his life, with minimal contacts with his colleagues and students.

After his habilitation, his first important published work was to be the *Begriffsschrift* (Frege, 1879) in 1879. This booklet is now considered as a turning point in the history of logic. As underlined before (1.1), he was one of the founders of modern symbolic logic. In this booklet, he introduced much of the tools still in fashion today with the exception of his notation. Frege did not only proposed a way of calculating inferences, but also a language precise and perspicuous enough for the most rigorous demands of science. At least, it was so in his view. The booklet has been reviewed by six persons, including Schröder, and they did not show great enthusiasm. During this period, he also lectured on all branches of mathematics, in particular analytic geometry, calculus, differential equations, and mechanics, even though he made few publications in these fields.

On the other hand, he published in the 80s and 90s several important writings on the philosophy of logic, philosophy of mathematics and philosophy of language. It is sometimes considered that in these fields, he made the greatest contribution since Aristotle. Some also say that it is the most significant development in our understanding of axiomatic systems since Euclid. Without going that far, we should admit that his work had a real revolutionary aspect. Firstly, he wanted to cut short with ordinary language – he was not the first to want this! –, but he actually developed a way of doing so (with (Frege, 1879) being the first sketch). Secondly, he wanted to give mathematics a foundations "by means of pure logic" – again, he was not the first to want this! –, but he was the first to give an actual presentation of this logicism. In the trend of the definition of numbers mentioned previously (2.1.1), he was the first to develop this thesis verbally in the *Foundations of Arithmetics* (Frege, 1884) and then adapted his concept-script in order to actually reduce arithmetic to logic in *The Basic Laws of Arithmetic* (Frege, 1893). Both of these books received almost no attention, except for a blasting review by Cantor of his *Foundations*. However, in the first years of the 20$^{\text{th}}$-century, Russell showed much interest in his works and by this intermediary, Frege had a major influence on the development of philosophical logic.

Frege died on July 26$^{\text{th}}$ 1925 in Bad Kleinen, at the age of 77.

2.3.2 Hilbert

David Hilbert was born on January 23$^{\text{rd}}$ 1862 in Königsberg, the famous city of Kant. He attended the gymnasium *Friedrichskolleg*, where the instruction was centered on the study of humanities. He was considered to be not very quick to understand new ideas, except in mathematics. In 1879, he changed for another gymnasium (*Wilhelm*) where he felt much more happy. After this, he went at the university of Königsberg, where he met his all-life-long friends Hermann Minkowski and Adolf Hurwitz. He received his doctorate in 1884 under Lindemann for a work on algebraic invariants. After a study trip in Germany and France, he was appointed in 1886 member of the staff in his home town and became Privatdozent in 1892. He was named afterward Extraordinary Professor and, in 1893, Full Professor. In 1895, Klein made Hilbert chairman of mathematics in Göttingen, who was then considered the world's mathematical center, where he taught for the rest of his life.

In 1888, Hilbert proved his famous *Basis Theorem* on invariants, then solving Gordan's problem. But when he submitted his paper containing a revolutionary approach, Gordan (then the "king of invariants") thought it was insufficient and suggested to refuse it. However, Klein permitted him to publish it in the *Annalen*. From 1893 to 1897, Hilbert worked on a report on algebraic number theory for the German Mathematical Society. The resulting work entitled *Zahlbericht* largely surpassed the expectations; he not only did a good report, but also unified algebraic number theory and, by the way, included many important innovations.

From 1897, he turned on to geometry and published in 1899 the *Grundlagen der Geometrie*, that immediately became a classic. In this book, he used a rigorous though not traditional method of axiomatization for the foundation of Euclidean Geometry. According to many historians, it turns out that it has been one of the most influential book of the 20$^{\text{th}}$ century in mathematics.

The lecture that contributed the most to Hilbert's fame is certainly the *Mathematical Problems* (Hilbert, 1902a) delivered in 1900, at the Second International Congress of Mathematicians. In this lecture, he wanted to "lift the veil behind which the future lies hidden" by giving a list of twenty-three problems that were to shape the next century mathematics. This lecture turned out to be "visionary"; the list's problems turned out to be of real importance since any mathematician who would have succeeded in resolving anyone of them would become famous from this achievement.

Not satisfied with the resolution of a fundamental problem in the theory of invariants, a unification and contribution in number theory and a revolution in geometry, he thereafter gave a foundation for functional analysis by studying integral equations in what is now called the "Hilbert spaces". He also contributed to mathematical physics, where he contributed to the foundation of general relativity with his works on field equations and to the foundation for quantum mechanics. To this already outstanding work, we must add many important contributions to mathematical logic and philosophy of mathematics.

He was not only a prolific scientific, but also a great teacher. Through his life, he had many doctoral students of high ranks, including some important names like W. Ackermann, H. Behmann, O. Blumenthal, R. Courant, H. Curry, G. Gentzen, H. Weyl, etc. For two or three decades, he has been considered the most prominent mathematician of the world.

He died in 14 February 1943 in Göttingen, at the age of 81.

Chapter 3

Hilbert: Foundations of Geometry

I believe: anything at all that can be the object of scientific thought becomes dependent on the axiomatic method, and thereby indirectly on mathematics, as soon as it is ripe for the formation of a theory.
Hilbert, 1918, p. 1115

Hilbert certainly marked an important moment in the history of mathematics, logic and philosophy. However, his profound and refined approaches and solutions did not come out of the blue. An indication of some of the developments that prepared the way to Hilbert's work were mentioned in the previous chapter. Our "historical thesis" is that Hilbert's work is the *natural continuation* of 19^{th}-century mathematics[1]. This can be seen by the impressive number of important contributions to almost all effervescent branches of mathematics (and physics) inherited from the 19^{th} century. Weyl (1944, p. 245) outlines these contributions as follows:

> i. Theory of invariants (1885-1893). ii. Theory of algebraic number fields (1893-1898). iii. Foundations, (a) of geometry (1898-1902), (b) of mathematics in general (1922-1930). iv. Integral equations (1902-1912). v. Physics (1910-1922). (Weyl, 1944, pp. 245-6)

In view of giving a systematical and mathematical formulation to his numerous insights, Hilbert redefined what is commonly known as the formal and axiomatic method.

[1] Whereas Frege's contribution is in *reaction* to it.

Though it is a very abstract method, it was conceived by Hilbert to be directly applicable to the sciences, i.e. that it would bring us a knowledge of their object. On the other hand, as everybody knows for having read it somewhere, Hilbert proceeds formalistically – a procedure supposed to be a meaningless game played with meaningless symbols providing a meaningless knowledge. At first sight, these two aspects seem hardly reconcilable. In this chapter, a particular effort will be made to clarify this apparent incompatibility: how can we maintain in one hand, that Hilbert's foundational work is formalist by nature – if formalism is only a meaningless game of symbol shifting – and, on the other hand, that his results have an epistemological value?

The discussion in the present chapter begins with a description of the *Grundalen* (3.1). It will start with general introductory remarks in view of understanding what Hilbert intends to do in this book (3.1.1). I will consider afterward a technical (though schematic) description of three fundamental aspects of Hilbert's project: 1- the components of the system (3.1.2), 2- the five groups of axioms (3.1.3) and 3- the consistency proof (3.1.4). The development of this metasystematic work imposed new question, e.g. the consistency of arithmetic. In the next section, I will present the strategy later adopted by Hilbert to solve this problem, by the well-known "Hilbert's program" (3.2).

After having drawn landscape, I will initiate a more precise discussion of some important aspects of Hilbert's approach (3.3). As everything in Hilbert's foundational work revolves around reflexions on the axiomatic method, I will first present the intention behind the axiomatic method (3.3.1). On this basis, we will examine some features of the axiomatic method as it was transformed by the second half of the 19$^{\text{th}}$ century, and some parallels with the model theoretic approach will be drawn (3.3.2). Since it is almost impossible to discuss Hilbert's method without saying something about formalism, the subsection 3.3.3 will emphasize the way we can apply this label to Hilbert. This section will close with a technical discussion of the axiom of completeness (3.3.4), that will allow us to see more precisely the way in which Hilbert thought of the relation between consistency and existence, and may have conceived mathematical structures.

Though Hilbert announced that the *Grundlagen* were to present a logical analysis of our intuition of space, it should be observed that no calculus is explicited in the book. The section 3.4 aims at exploring what kind of calculus could have been used according to a Hilbert's style. The subsection 3.3.4 will give us a hint of the way this could have been done, since an important requirement for a model of geometry is its categoricity. In the subsection 3.4.2, basic features of set theory and category will be outlined. The chapter will close with a short overview of the categorical approach to foundations, and the striking similarities with Hilbert's approach will be stressed 3.4.3. Hence, it will be shown that Hilbert's approach to foundations is essentially the same as our contemporary approach.

3.1 A Description of the *Grundlagen*

The literature on Hilbert's work is mainly concerned with the so-called Hilbert's program of the '20s. This can lead us to think that all of Hilbert's paper are programmatic. However, the *Grundlagen* is in no way a programmatic book. All the methodological considerations are embedded in the actual foundations of geometry that Hilbert presents. For this reason, it is important to begin a discussion of this book by a precise description of what Hilbert actually realized.

His main task is to understand and organize in an axiomatic theory the most important results of the investigations of geometry in the 19^{th} century, i.e. the independence of the parallel axiom, the consistency of non-Euclidean geometries, etc. He takes it for granted that the truth of geometry's axioms, as a science of space, should be established empirically[2]. However, to make an empirical geometry possible we must have a theoretical framework capable of giving a rigorous conceptual (mathematical) analysis of these axioms. This is why Hilbert replaced the axioms considered as true of the real space by axioms considered as logico-deductive hypothesis in a formally axiomatized system, a replacement that had already been done before by Pasch, Peano, etc. However, as we shall see, Hilbert would like to connect geometry with our intuition of space. He still used the common vocabulary of Euclidean geometry, he drew diagrams, etc., because he intended to discuss the traditional Euclidean geometry (our intuitive theory of space), though in a metageometrical perspective[3]

[2] "We can say: in recent times the conception of the empirical nature of geometry, as represented by Gauss and Helmholtz, has become a secure result of science." (Hilbert, 1930, p. 1163)

[3] "According to the emerging demands of rigor, and the banishment of intuition, diagrams may be dangerous. A reader who relies on a diagram in following a demonstration cannot be sure that the conclusion is a logical consequence of the premises. Intuition may have crept back in. Apparently, Hilbert did not want to go so far as to echo Lagrange's boast that his work did not contain a single diagram." (Shapiro, 1996, p. 157)

3.1.1 Introductory Remarks

The one-page introduction of Hilbert's book give us many important details. It opens with a revealing quotation of Kant: "All human knowledge begins with intuitions, thence passes to concepts and ends with ideas." Of course, we may not conclude from this that Hilbert's enterprise is Kantian from side to side[4]. Nonetheless, they seem to agree on the starting point for a study of geometry: intuition. Hilbert is not one of these logicians/mathematicians for whom intuition is deprived of any value, as "historians" of logic and geometry so often prejudge about him at the sight of the label "formalist".

What is the role of intuition in his foundation of geometry? In the very first sentence of the book, he says that "geometry, like arithmetic[5], requires for its logical development only a small number of simple, fundamental principles" (1902b, p. 1). In the case of geometry, these fundamental principles are called the axioms of geometry. For Hilbert, the relation between the axioms and our intuition of space is direct: "Each of these groups [of axioms] expresses, by itself, certain related facts of our intuition." (1902b, p. 3) However, the nature of this relation is not clearly stated; but such a thing is normal for a non-programmatic book on geometry (and not on philosophy of geometry).

Hilbert did not pretend to break with tradition, but only to provide a better account of what is customarily done. The first task, for a mathematician providing an axiomatic foundation for geometry, is to select some axioms (i.e. facts of our intuition) and to analyze them logically.

> The choice of the axioms and the investigation of their relations to one another is a problem which, since the time of Euclid, has been discussed in numerous excellent memoirs to be found in the mathematical literature. This task is tantamount to the logical analysis of our intuition of space. (1902b, p. 1)

A study of geometry must necessarily begin with an intuition of what space is. The

[4] However, we note that Hilbert had a serious interest with Kant's philosophy of mathematics. As a secondary question for his doctorate, Hilbert defended this proposition: That the objections to Kant's theory of the *a priori* nature of arithmetical judgments are unfounded. However, there is no record of his defense of this proposition (Reid, 1970, p. 17). Moreover, we note that Hilbert claimed to be a follower of Kant in (Hilbert, 1925) and maintained Kantian views in almost all his foundational papers.

[5] Hilbert presented an axiomatization of arithmetic in the same period (Hilbert, 1900c).

axioms are selected among facts of our intuition. However, as Kant's quote shows, it is only the beginning, not the final word. Our intuition of space is not sufficient in itself for scientific purpose, and has to be analyzed logically. What characterizes a system of geometry is not the reality that our intuition is supposed to render, but the conceptual structure following from a set of axioms and some rules of logical analysis. This means that we have to formulate our intuitions of space in an axiomatic system to make clear what is assumed, what is derived, and from what it is derived, in order that the consequences of what we know intuitively becomes explicit. It is necessary for Hilbert to study a system of geometry independently of the intuition, by a logical analysis[6]. These axioms could as well have been picked up randomly from a hat that it would not have change the *mathematical task* about geometry. Therefore, if there is a traditional aspect in the problem to which Hilbert applied himself, the emphasis he added to the obligation of giving a logical justification for the claims concerning a geometrical system has something that is absent from the tradition.

> The following investigation is a new attempt to choose for geometry a *simple* and *complete* set of *independent* axioms and deduce from these the most important geometrical theorems in such a manner as to bring out as clearly as possible the significance of the different groups of axioms and the scope of the conclusions to be derived from the individual axioms. (1902b, p. 1)

The emphasis on some words denotes a particular preoccupation that we don't find in traditional works, except, maybe, in a schematic way.

3.1.2 Components of the System

The presentation of the five groups of axioms begins with the explicit statement of the primitives, namely three distinct systems of things (*Systeme von Dingen*)[7]. These systems are distinct. The things composing the first system are to be called *points* and

[6] We remark, though Hilbert does not discuss this point in the *Grundlagen*, that the possibility that the axioms of this very logical analysis be validated by intuition is not excluded, though not affirmed.

[7] The things (*Dingen*) in question are what Hilbert will introduce in (Hilbert, 1904) under the name "thought-objects"; that it is what he talk about in the *Grundlagen* can be seen by the formulation with which he introduces the three systems: "let us think of things called . . . " (1902b, p. 3) On this question, see (Webb, 1997) and (Peckhaus, 2003, p. 149), who call these *Dingen* "thought things".

be designated by the letters $A, B, C, \ldots$ Those composing the second system will be called *straight lines* and be designated by the letters $a, b, c, \ldots$ And those of the third system will be called *planes* and be designated by the Greek letters $\alpha, \beta, \gamma, \ldots$ The points are called the *elements of linear geometry*; the points and lines are the *elements of plane geometry*; and the points, lines and planes together are the *elements of the geometry of space* (1902b, p. 3).

We note that Hilbert *do not say* that things contained in the first system are points, or elements of linear geometry, or that things of the second system are lines, etc., but only that they are thus *called*. From the names 'point', 'straight line' and 'plane', we are not supposed to know which objects are designated (outside the system); we say that the language in which these terms occur is not interpreted. Instead of 'point', 'straight line' and 'plane', it would be possible to use any other labels: e.g. 'tables', 'chairs' and 'beermugs' would do as well (Reid, 1970, p. 57). The idea of Hilbert is that properties of the axiom system should not be dependent on what are the things represented by the labels, with the sole exception that they are of three distinct kinds, and that they can be put in the relations fixed by the axioms. There is absolutely no need of an "actually existing" counterpart of these things.

3.1.3 The Five Groups of Axiom

These things of which the three systems consist (points, straight lines, planes) come in mutual relation with one another. They are indicated by means of words such as 'are situated', 'between', 'parallel', 'congruent', 'continuous', etc. Of these relations, we also have an intuitive knowledge. For this reason, the studied relations are not picked up at random. Hilbert chose those that are of main interest for researches in mathematics

Another technical remark. It is important here to be careful with the word 'system' present in the English translation. Nowadays, this word generally connotes structured elements, i.e. that the elements of the set are mutually connected (or even constituted) by a certain (non-zero) number of relations. Thus, we are not talking about a mere collection of things (a discrete set). But here, with the word 'system', Hilbert simply talks about a collection of things, the relations connecting the elements of the collection being only laid down later by the axioms. This word has been used in the mostly influent (Dedekind, 1888), where a general theory of systems was presented: "It very frequently happens that different things, $a, b, c, \ldots$ for some reason can be considered from a common point of view, can be associated in the mind, and we say that they form a *system* $\mathcal{S}$; we call the things $a, b, c, \ldots$ *elements* of the system $\mathcal{S}$, they are *contained* in $\mathcal{S}$; conversely, $\mathcal{S}$ *consists* of these elements. Such a system $\mathcal{S}$ (an aggregate,a manifold, a totality) as an object of our thought is likewise a thing [...]." (Dedekind, 1888, p. 797) In the *Mathematical Problems*, Hilbert also uses that word: "two systems, i.e. two assemblages of ordinary real numbers or points". It is thus clear that he means something like an aggregate.

and mathematical physics of his time. He organized most of the "classical axioms" of geometry in a different fashion, by distributing them into five groups, according to some important problems.

However, in conformity with the demands of logical analysis, these relations should not only be taken as "intuitively given", but incorporated in the rigorous frame of an axiomatic system: "The complete and exact description of these relations follows as a consequence of the *axioms of geometry*." (1902b, p. 3) In this way, the meaning of these relations is not taken as something evident, sufficient for logical analysis. By providing such conditions, axioms *do define*[8] the relations; they give an exact account of what can be done with them and what cannot. The axioms together with their consequences thus give a complete and exact description of the relations.

Axioms are separated in five groups, in order to allow one to know precisely which axioms are involved in any theorem. Hence, for a group of axioms characterizing a relation in a precise way, we have such and such consequences. From another group, other consequences. If we take two groups together, then we have the two lots of consequences, and their combination. And so on. I will reproduce here only the axioms of connection, since it is sufficient to illustrate how Hilbert formulated them; reproducing all axioms would be useless.

In the first group, the axioms of connection, Hilbert introduces a first relation, namely "determine" (which is represented by the sign "="). These axioms establish a connection between the concepts indicated above; namely, points, straight lines and planes.

Axiom I,1. Two distinct points A and B always completely determine a straight line a. We write $AB = a$ or $BA = a$.

This should be seen as a construction postulate, i.e. that for any two points respecting the condition of distinctness, it is possible to construct a line, and we have a notation for this construction (von Plato, 1997, p. 128).

[8] "When we are engaged in investigating the foundations of a science, we must set up a system of axioms which contains an exact and complete description of the relations subsisting between the elementary ideas of that science. The axioms so set up are at the same time the definitions of those elementary ideas ..." (Hilbert, 1902a)

Hilbert gives some expressions that are part of the common usage among mathematicians, and claim that they express the fact described by this axiom. In this way, instead of "determine", he says that we may also employ other expressions, e.g. we may say A "lies upon" a, A "is a point of" a, a "goes through" A "and through" B, a "joins" A "and" or "with" B, "the straight lines a and b have the point A in common", etc. However, these expressions does not add anything to the system; they are explicit definitions. For example, we can reformulate them from primitive terms like:

Definition. lie on $=_{df.}$ determine, in such a way that saying "A and B determine a" is equivalent to "A and B lie on a".

Definition. join with $=_{df.}$ determine, in such a way that saying "A and B determine a" is equivalent to "a joins A with B".

It is tantamount to say, in sentencial logic, that we can also use the sign "&" for the sign "$\wedge$". These alternative expressions have the same role than what is traditionally called a definition in the books on geometry, i.e. that they introduce new symbols (a shortcut) that are explicitly reducible to primitive terms.

Axiom I,2. Any two distinct points of a straight line completely determine that line; that is, if $AB = a$ and $AC = a$, where $B \neq C$, then is also $BC = a$.

Axiom I,3. Three points A, B, C not situated in the same straight line always completely determine a plane α. We write $ABC = \alpha$.

We may also use the expressions: A, B, C, "lie in" α; A, B, C "are points of" α, etc.

Axiom I,4. Any three points A, B, C of a plane α, which do not lie in the same straight line, completely determine that plane.

Axiom I,5. If two points A, B of a straight line a lie in a plane α, then every point of a lies in α.

In this case we say: "The straight line α lies in the plane α," etc.

Axiom I,6. If two planes α, β have a point A in common, then they have at least a second point B in common.

Axiom I,7. Upon every straight line there exist at least two points, in every plane at least three points not lying in the same straight line, and in space there exist at least four points not lying in a plane.

From this first group of axioms, there is already many theorems that follow, one which Hilbert formulates in this way (without giving the proofs, which is trivial): "Through a straight line and a point not lying in it, or through two distinct straight lines having a common point, one and only one plane may be made to pass." (1902b, p. 5)

The second group contains axioms of order, for which Hilbert claims to be indebt towards Pasch. This group define the idea expressed by the word "between" and makes possible *an order of sequence* of the points upon a line, in a plane, and in a space. Once again, these axioms – alone and combined with the first group – have many consequence, e.g. that on any segment there in an unlimited number of points (theorem 3). The third group of axioms contains only one axiom, namely the axiom of parallels (Euclid's axiom)[9]. The introduction of this axiom simplifies greatly the fundamental principles of geometry and facilitates in no small degree its development. The axioms of groups IV are those of congruence. The axioms of this group define the idea of congruence or displacement. By an explicit definition, Hilbert then introduces the notion of angle:

Angle Let α be any arbitrary plane and h, k any two distinct half-rays lying in α and emanating from O so as to form a part of two different straight lines. We call the system formed by these two half-rays h, k an *angle* and represent it by the symbol $\angle(h, k)$ or $\angle(k, h)$. The half rays h and k are called the *sides of the angle*, and the point O is called the *vertex of the angle*. (1902b, pp. 13-4)

In this way, he can establish relations of congruence between angles and, for instance, he proves the laws of congruence for triangles. Moreover, his precise axiomatic formulation allow him to prove these laws *without* the axiom of parallels, then showing their

[9] "In a plane α there can be drawn through any point A, lying outside of a straight line a, one and only one straight line which does not intersect the line a. This straight line is called the parallel to a through the given point A." (1902b, p. 11)

independence toward this axiom. The group V also contains a single axiom, namely the axiom of continuity (Archimedes's axiom). This axiom makes possible the introduction of the idea of continuity in geometry[10].

3.1.4 Consistency Proof

The axiom system having been laid down, Hilbert considers a meta-systematic analysis of independence and consistency. The formulation of the consistency problem for the five groups of axiom is as follows:

> The axioms [...] are not contradictory to one another; that is to say, it is not possible to deduce from these axioms, by any logical process of reasoning, a proposition which is contradictory to any of them. To demonstrate this, it is sufficient to construct a geometry where all of the five groups are fulfilled. (1902b, p. 27)

To produce such a geometry, Hilbert considers a domain Ω consisting in all algebraic numbers obtained by beginning with the number 1 and applying to it a finite number of times the arithmetical operations (addition, substraction, multiplication and division) and the operation $\sqrt{1+\omega^2}$, where ω holds for a number arising from the five operations mentioned. We regard a pair of numbers (x, y) of Ω as defining a point and, likewise, a ratio of three points of Ω $(u : v : w)$ where u and v are not both equal to zero, as defining a straight line. In such a geometry, the fact that a point (x, y) lies on a straight line $(u : v : w)$ is expressed by the equation $ux + vx + w = 0$. We thus see that in such a geometry, the axioms I, 1-2 and III are fulfilled. Now, the numbers of the domain Ω are all real numbers; considering that these numbers can be arranged by magnitude, we can easily lay down conventions as to make the axioms of group II also holds.

The laying off of segments and angles follows from the methods of analytic geometry. A transformation of the form $x' = x + a$ and $y' = y + b$ produces a translation of segment. Likewise, we can produce a transformation for the rotation of angles. By

[10] From the 1902 French edition, a second axiom of continuity (as Hilbert calls it) has been introduced under the name of "axiom of completeness". However, no use of this axiom is made in the proofs of the *Grundlagen*. Nonetheless, this axioms turns out to have a great methodological significance and we will discuss it in the subsection 3.3.4.

such a procedure, we lay down conventions allowing the axioms of Group IV to be fulfilled in this geometry. We thus achieve our goal:

> From these considerations, it follows that every contradiction resulting from our system of axioms must also appear in the arithmetic related to the domain Ω. (1902b, p. 29)

The domain Ω is a denumerable one; in modern parlance, we would say that Hilbert produced a denumerable model of his axiomatic system. He remarks that it would also be possible to produce an undenumerable model with the domain $\mathbb{R}$ of real numbers, though it is not necessary for the demonstration he aimed at.

This kind of consistency proof is said to be *relative*; the axiom system is consistent *if* arithmetic is[11]. Now, the question of the consistency of arithmetic arises. Hilbert (1900c) approaches this problem without much specifying how it could be resolved[12]. He there shows enthusiasm, seemingly thinking that it will be proved without much difficulty. However, it was to generate problems to the mathematical community for the thirty following years.

3.2 Hilbert's Program

With the relative proof of consistency of the *Grundlagen*, Hilbert reduced the problem of the consistency of geometry to the consistency of arithmetic. Now the task is to prove the consistency of arithmetic. However, a proof of relative consistency would not be sufficient, for it amounts to report the problem in another discipline: there is a discipline for which an absolute proof of consistency is needed.

[11] "The chief requirement of the theory of axioms must go farther, namely, to show that within every field of knowledge contradictions based on the underlying axiom-system are *absolutely impossible*.

In accordance with this requirement I have proved the consistency of the axioms laid down in the *Grundlagen der Geometrie* by showing that any contradiction in the consequences of the geometrical axioms must necessarily appear in the arithmetic of the system of the real numbers as well." (1918, p. 1112)

[12] "Hilbert spoke about a consistency proof of arithmetic, or analysis, already in his famous 1900 talk on the problems in mathematics. This may give the wrong impression that Hilbert's program was already there. However, in 1900 Hilbert thought that this consistency proof would be carried by exhibiting a realization, that is, a model. Only in 1904 did Hilbert consider the syntactical notion of consistency." (Raatikainen, 2003, p. 158)

> In only two cases is this method of reduction to another special domain of knowledge clearly not available, namely, when it is a matter of the axioms for the *integers* themselves, and when it is a matter of the foundation of *set theory*; for here there is no other discipline besides logic which it would be possible to invoke. (1918, p. 1113)

To achieve this, a more detailed proof theory was needed than what has been used in the first period of foundational research. In (Hilbert, 1922), a sharp distinction between the logico-mathematical formalism and the contentual metamathematics is drawn, whereas it was not very clearly done (if done at all) in (Hilbert, 1902b) or (Hilbert, 1904)[13]. This distinction has been refined in (Hilbert, 1923), where he distinguishes clearly between the modes of inference permissible in each: in the metalanguage, one operates with a finite language, i.e. that one deals with finite totalities, while modes of inference are more powerful in the language. In this way, he laid down his metamathematical project explicitly:

> In addition to this formalized mathematics proper, we have a mathematics that is to some extent new: a metamathematics that is necessary for securing mathematics, and in which – in contrast to the purely formal modes of inference in mathematics proper – one applies contentual inference, but only to prove the consistency of the axioms. (1923, p. 1138)

At the level of metamathematics, Hilbert adopts the criticisms of infinitary mathematics and the doubtfulness of the principle of excluded third middle. He sought to use only reasoning that was intuitionistically acceptable and completely secure[14]. According to his program, finitistic reasoning will be the secure element by which the proof of consistency of infinitary mathematics will be given:

> And has the contentual [finitistic] logical inference ever deceived us anywhere when we applied it to real objects or events? No, contentual logical inference is indispensable. It has deceived us only when we accepted arbitrary abstract notions, in particular those under which infinitely many objects are subsumed. (1925, p. 376)

[13] Poincaré (1905, 1906) severely criticized Hilbert's *Grundlagen* for the uselessness and the circularity of its proofs. By this clear distinction, Hilbert answers convincingly to this critic.

[14] At this level, Hilbert's finitism went further than that of Brouwer himself (Ewald, 1996b, p. 1116).

In this way, the "free use and the full mastery of the transfinite is to be achieved on the territoty of the finite!" (1923, p. 1140) He compares his project to the introduction of ideal elements in various branches of mathematics. In logic as in mathematics, the transfinite propositions are to be considered ideal elements. As imaginary numbers allowed mathematicians to unrestrictedly calculate square roots, transfinitary propositions allow logicians and mathematicians to unrestrictedly calculate truth-functions: "[...] we must here *adjoin the ideal propositions to the finitary ones* in order to maintain the formally simple rules of ordinary Aristotelian logic." (1925, p. 379) The only restriction imposed is that the system including transfinite operations be consistent. In this way, following (Simpson, 1988), Hilbert's program seems to develop in three points:

1. The first step is to isolate the unproblematic, "finitistic" portion of mathematics. This finitistic mathematics is adequate for elementary number theory and the manipulation of finite strings of symbols[15].
2. The second step is to reconstitute mathematics as a big elaborate formal system (a stock of formulas).
3. The third step consists in giving a finitistically correct consistency proof of the big system.

While the method for relative consistency proof was fundamentally semantical, Hilbert proposes to proceed syntactically for the absolute proof of consistency. Already in the second problem of his 1900 Paris address, he called for a proof of consistency of the arithmetical axioms. But at this moment, it was rather vague which axioms he thought of. With the development and refinement of his program, it became clearer; moreover, he asked that the proof method for consistency be completely finitary in character. This holy mathematical quest ended abruptly in 1931, when Gödel's incompleteness theorem was to bring a negative answer: such a proposition is undecidable. It is generally accepted in the literature that Gödel's result put an end to Hilbert's program.

[15] The question of the nature of finitistic mathematics is somewhat puzzling. According to (Feferman, 1988, p. 365), "no one has come up with a generally accepted formal characterization of the informal concept of finitary proof". However, again this opinion, (Hintikka, 1997) supports that finitistic mathematics is combinatorial analysis, and (Tait, 1981) claims that it is nothing but primitive recursive reasoning. This latter hypothesis seems to find some support in Hilbert's text: "The elementary theory of numbers can also be obtained from these beginnings by means of 'finite' logic and purely intuitive thought (which includes recursion and intuitive induction for finite existing totalities); here it is not necessary to apply any dubious or problematical mode of inference." (1923, p. 1139)

3.3 Discussion on the Method of the *Grundlagen*

3.3.1 Motivations for Axiomatics

In the studies on the foundations of various branches of mathematics, two main methods have occupied the field: the genetic and the axiomatic. The genetic method proceeds to a piecemeal construction, by a successive extension of a theory. The classical example of this procedure is the construction of real numbers step by step; we first start from what is intuitively obvious, the natural numbers, and then build the integers, the rationals and, by something like Dedekind's cut, we construct real numbers. But if this method allows one to construct any real number, it does not characterize what the concept of 'real number' is, and does not allow to study the real-numbers system as a whole. Of course, this method follows the way we discovered the mathematical objects, and the way we learn them at school, but it proves to be insufficient.

The other method is the axiomatic one. It consists in an explicitation of the entirety of a theory as something achieved. Euclid offered the paradigmatic example of this method. He laid down axioms, definitions, common notions, etc., in order to make everything constituting the axiomatic system explicit. Moreover, the axioms were supposed to be truths about the world, and the justification for this truth was intuitive evidence. In more modern parlance, we would say that Euclid's presentation had nothing to do with the context of discovery – to which the genetic method is attached – but centers on the context of justification (Reichenbach, 1963, p. 248):

> From Euclid you get no idea how mathematics is actually discovered, how one arrives at the constructions, in many cases ingenious, that lead from the data to the conclusion; one can only go through his proofs step by step to see that they are indeed correct. (Feferman, 1998, p. 12)

According to Hilbert, there is not a single doubt concerning which method is proper to foundations: the axiomatic one. Let us consider three excerpts, where he firmly affirms that axiomatic is *the* method, and no doubt the unique one, to study the foundations of mathematics and logic:

> My opinion is this: *Despite the high pedagogic and heuristic value of the genetic method, for the final presentation and the complete logical grounding of our knowledge the axiomatic method deserves the first rank.* (1900c, p. 1093)
>
> The axiomatic method is and remains the indispensable tool, appropriate to our minds, for all exact research in any field whatsoever: it is logically incontestable and at the same time fruitful; it thereby guarantees the maximum flexibility in research. (1922, p. 1120)
>
> The method that I follow is none other than the axiomatic. Its essence is as follows.
>
> In order to investigate a subfield of a science, one bases it on the smallest possible number of logical principles, which are to be as simple, intuitive, and comprehensive as possible, and which one collects together and sets up as axioms. (1922, p. 1119)

However, if axioms are to be intuitive and comprehensible, this is only as an *intention*. In no way does Hilbert justify the axioms by their evidence or intuitive character. Rather, he replaces these modes of justification by three metaaxiomatic conditions: the axiomatic system has to be consistent, the axioms have to be independent from one another and the axiomatic system has to be complete (Peckhaus, 2003, p. 143). Hilbert is only too conscious of the major defects in Euclid's axiomatics; many notions are not explicited, the rules of deduction are not stated, there are many interventions of non-explicited assumptions due to the intended intuitive subject matter, etc[16]. This is why even if he did not fight to exclude intuition from science, Hilbert held that "the role of intuition [be] carefully and rigorously limited to motivation and heuristic. Once the axioms have been formulated, intuition is banished." (Shapiro, 1996, p. 156)

Another question that brought Hilbert to re-think axiomatics is the metageometrical researches generated by the discovery of non-Euclidean geometries. The independence of the parallel axiom, the construction of Euclidean models of non-Euclidean geometries, the relative consistency of theses geometries, etc., were to Hilbert "the most important results of geometrical inquiries" (1899, p. 38). From this point of view, axiomatics had another important capacity: it allows both for positive and *negative* (e.g. *in*dependence, *non*-contradiction, etc.) metageometrical results.

[16] Hilbert's attitude is the same that Hartshorne describes as being typical for modern mathematicians: "The modern mathematician goes one step further, by trying to make *all* assumptions explicit and create a consistent mathematical structure that no longer derives its validity from the real world. The "truth" of a particular result in the real world is then no longer relevant." (Hartshorne, 2000, p. 10)

Geometry was thereby changed: as a branch of mathematics, space (the real space in the actual world) was no longer an object for its study which became the mathematical "formal structures"[17]. Even for these early metasystematic inquiries, I consider this formulation fits his views, as we shall see in subsection 3.3.4.

3.3.2 Modern Axiomatics and Model Theory

This subsection aims at showing in what way the modern axiomatic metatheory oriented study is related to the model-theoretical viewpoint[18]. This idea has been supported by many 20$^{\text{th}}$-century philosophers of mathematics, particularly Hintikka and Shapiro – who both wrote penetrating papers on the subject.

Hilbert's book intends to present another study on the foundations of geometry. The axioms are to be facts of our intuition. Moreover, he uses the traditional vocabulary of Euclidean geometry and adds schemas to the text in view of simplifying the grasp of his abstract proofs. To be sure his presentation of the *Grundlagen* is not as abstract as what we find in most contemporary axiomatic foundation of geometry. This is so because Hilbert *intended* to study various systems for the traditional axioms of geometry and the traditional basic facts of geometry, but in a different perspective. If our intuition of what space is motivates the choice of axioms and the interpretation of the axiomatic system, it however never appears in the proofs.

> The representation may be created by looking at something and representing selected aspects of it, but, once created, it does stand of its own accord, and can be used to represent something else. [. . .] The connection acts as an umbilical cord that can be cut once the theory has been formulated. (Sterrett, 1994, p. 5)

[17] This word is not in the early writings of Hilbert, but we find it in (Hilbert, 1922, p. 1127).

[18] A typical apology of Hilbert's contribution follows these lines: "These metageometrical investigations were to have a considerable influence not only on geometry, but also on logic: for proof theory and model theory grew out of Hilbert's adept exploitation of the insight contained in his remark about *table, chair* and *mug*." (Ewald, 1996b, p. 1090) I consider this exact; however, from our point of view, it can be misleading to locate on the same level Hilbert's contributions to model-theory and proof-theory. The first are issued from his works on geometry and mathematics in general. The others are tied to finitism and his endorsement of a variety of logicism in the '20s (see next subsection). These are somewhat intertwined, even if we must not forget their relations to *different* foundational projects.

We say that Euclidean geometry is the *intended model* of Hilbert's axiomatic system. However, when the system is laid down, its properties are studied independently of this underlying motivation[19].

> A more accurate way to describe such a formalized calculus is to say that its symbols may *appear* to have meanings (and its rules may well push its symbols around in such a way that they *act* exactly as symbols with the desired meanings would act), but their behavior is not a *consequence* of their meanings; indeed, quite the reverse is the case. To the extent that formal symbols appear meaningful, this appearance devolves entirely from their behavior, which in turn is wholly determined by the system's rules and initial formulas (axioms). (Nagel and Newman, 2001, pp. 26-7)

Officially, as long as proofs in and about the system are concerned, the intended meaning of the primitive terms has no relevance. From a traditional logical point of view, this procedure raises doubts: what gibberish is this kind of talk about meaningless logic? As if a deduction could be made in a meaningless language!

Of course, Hilbert was conscious of this objection and tried to bypass it by using the implicit definition method for his terms borrowed from mathematic. This is nothing but the famous notion thematized by Gergonne (see 2.2.2). The terms in a mathematical theory need not a unique interpretation. As long as the logico-mathematical analysis of a theory is concerned, Hilbert thought it impossible to uniquely determine the objects of a theory. An axiomatic system is first of all characterized by its axioms, whose expressions contain names of relations and names of objects. Though their inclusion in the axiomatic system is intuitively motivated, their role in the system is limited to what is explicitly expressed. In this way, the relation names in the axioms act as rules for the use of objects' names. Though the word "implicit" does not occur in the *Grunlagen*, *the axioms implicitly define the objects the system is about.* In this way, objects are nothing but a complex of properties such that they satisfy the axioms: "*Anything at all* can play the role of the undefined 'primitives' so long as the system satisfies the axioms." (Shapiro, 1996, p. 156) These implicit definitions properly characterize a

[19] "Using techniques from analytic geometry, [Hilbert] constructed a model of all of the axioms using real numbers, thus showing that the axioms are 'compatible', or satisfiable. If spatial intuition were playing a role beyond heuristics, this proof would not be necessary. Intuition alone would assure us that all of the axioms are true (of real space), and thus that they are all 'compatible' with one another. Geometers in Kant's day would wonder what the point of this exercise is. As we shall see, Frege also balked at it." (Shapiro, 1996, p. 158)

formal structure, i.e. a structure of relations between objects implicitly defined. For a given axiomatic system, the objects are characterized up to satisfaction of the axioms[20]. The metageometrical orientation of axiomatics adopted by Hilbert centers, in modern parlance, on the notion of isomorphism (and not on equality). An axiomatic system determines a structure – a morphism, a form –, a thing that Hilbert called a 'scaffolding of concepts' in a letter to Frege:

> There remains one objection to be touched on: you say that my concepts, e.g. 'point', 'between', are not unequivocally fixed. [...] But it is surely obvious that every theory is only a scaffolding (schema) of concepts together with their necessary connections, and that the basic elements can be thought of in any way one likes. E.g., instead of points, think of a system of love, law, chimney-sweep... which satisfies all axioms; then Pythagoras' theorem also applies to these things. Any theory can always be applied to infinitely many systems of basic elements. One only needs to apply a reversible one-one transformation and lay it down that the axioms shall be correspondingly the same for the transformed things [...]. Thus the circumstance I mentioned is never a defect (but rather a tremendous advantage) of a theory. (1899, p. 42)

As a good follower of Hilbert's axiomatic view, Weyl expressed it that way:

> A science can determine its domain of investigation up to an isomorphic mapping. (Weyl, 1949, p. 27)

Considering that mathematical structures are the subject matter of Hilbert's metamathematical approach to axiomatics, it becomes easy to understand this excerpt about existence that has been so problematic for Frege[21]:

> If we succeed in proving that the properties given to our objects never can lead to a contradiction in a finite number of logical inferences, I will say that the mathematical existence of an object, say a number of function fulfilling certain properties, has been demonstrated. (Hilbert, 1902a, p. 7)

[20] A direct consequence of this method is that no clear difference between concepts and objects can be drawn. As we will see, this is in direct conflict with Frege's approach.

[21] The discussion of this point is in Frege (1899) and Hilbert (1899).

If an axiomatic system is consistent, then it determines a mathematical (or any abstract) structure, and this straightforwardly entails that such a structure exists. In this way, Hilbert claims that from consistency we can infer existence. Now, we have seen how Hilbert proves consistency in the *Grundlagen*. First, he laid down a series of axioms implicitly defining a structure or relation and objects. For heuristic considerations, he formulated it in a way that a model of Eulidean geometry would naturally follow. After he has shown the adequacy of his system for this geometry, he proved the consistency of the system of axioms by producing an arithmetical model (specifically, the model furnished by analytic geometry). This gave him a consistency proof *relative* to the arithmetic of real numbers. He therefore prove that geometry and arithmetic had the same structure, i.e. that they were isomorphic, and that if one turned out to be inconsistent, the other necessarily would also be inconsistent. This way of proceeding is completely in the spirit of model theory: "The crucial concept employed in this perspective is that of 'truth in a model', which is the central notion of contemporary model theory" (Shapiro, 1996, p. 158) and this "way of treating geometrical statements is generally regarded as a major step forward in the development of logic towards model-theory, the study of relationships between formal languages and their interpretations." (Sterrett, 1994, p. 2)

3.3.3 Hilbert's Formalism

The status and peculiarities of Hilbert's formalism is a delicate question, so delicate that many commentators prefer to reject this label[22]. Certainly, this is due to the ambiguity of the word. For this reason, we will have to specify to the foundation of what is this formalism supposed to apply, and in what way can we say that it is present in Hilbert's foundational work.

In the *Grunlagen* and *On the Concept of Number*, Hilbert does not endorse the philosophy of logic and mathematics called 'logicism' (e.g. as conceived by Frege); it

[22] For example: "As for the term 'formalist', it is so misleading that it should be abandoned altogether as a label for Hilbert's philosophy of mathematics." (Ewald, 1996b, p. 1106)

"We note at once that there is no evidence in Hilbert's writings of the kind of formalist view suggested by Brouwer when he called Hilbert's approach 'formalism'." (Kreisel, 1958, p. 346)

"Hilbert's philosophy of mathematics is almost universally labeled *formalism*. [...] such a qualification is highly misleading. It does not do justice to the leading ideas of Hilbert's thinking about the foundations of mathematics. His so-called formalism was the result of several independent ideas most of which he could have maintained even if he had given up his formalism. (Hintikka, 1997, p. 15) means that he does not start from the reducibility (equality) of mathematics to logic. For this reason, he would adopt such a perspective:

$$\text{philosophy of mathematics} \neq \text{philosophy of logic}$$

It would be perfectly possible for him to be formalist in one case and not in the other. To make things a little clearer, let us enumerate some ways in which the word 'formalism' is understood in the literature[23].

Firstly, formalization is said to consist in the "use of symbols". It is in this sense that we talk of "symbolic logic". One use for symbols is to identify the clearly formal parts. This sense of 'formalism' is closely linked to the proof-theory that Hilbert developed in the '20s, i.e. to finitism: a formal system is a set of finitary rules for the manipulation of concrete symbols. It is a kind of word processing. But this concerns his philosophy of logic (i.e. the logical grounding of metamathematics). It should be noticed that according to this sense, the *Grundlagen* are not much formalized: it has symbols for points, lines, planes; a symbol for congruence (and another one introduced by definition for angles). But that's all. The rest is a text in natural language. It is clear from the presentation that Hilbert does absolutely not understand his foundational work as word processing: he talks not of the symbols as such and their manipulation, but of the underlying structure they represent[24].

A second meaning of 'formalization' concerns machine executability. This sense calls for an explicit syntax that is equated with formalization. It puts emphasis on the purely mechanical application of rules[25]. There is something apparented to this in Hilbert's book; however, the presentation from the early editions can hardly allow one to proceed mechanically. As Weyl noted, Hilbert formalized the genuinely geometrical principles and left the logical principles implicit (even though the meaning of logical terms – in opposition to geometrical terms – should be understood) (Weyl, 1944, p. 269). Of course, without the explicitation of the logical principles – i.e. logical axioms and rules of inference –, one can hardly proceed mechanically.

[23] These distinctions are from von Plato (1997), that I slightly modified. I do not pretend that this enumeration is exhaustive or completely precise, but it can give useful hints.

[24] In this sense, we have to conclude, with Hintikka: "In reality, the right diagnosis of Hilbert's axiomatic approach shows that his philosophy of mathematical theories was in 1899 as far from formalism as one can get." (Hintikka, 1988, p. 6)

[25] The first sense is mainly concerned about the "expression of a content" by the use of symbols (e.g. Frege); the nuance here is the primary concern with the mechanical calculus of inferences.

A third way is to understand formalization as a process in which structure is made explicit. This sense of 'formalization' is linguistic in that it consists in the addition of structural information in the expressions, by making semantics formal (von Plato, 1997, pp. 133-4). From this standpoint, 'structure' consists of semantical type information and functional syntactic structure. Expressions of a language are, from the grammatical point of view, built up by the application of rules. There is something very close to this in the *Grundlagen*; as I have shown is the previous subsection, there is a clearly distinguishable emphasis on structure. However, Hilbert's approach can hardly fit in this category, since there is no precise distinction between semantics and syntax in his reasoning[26]. To say that Hilbert is a formalist in this sense would be an anachronism, though the similarities are undeniable.

Why, then, is Hilbert called a formalist? Taking into account that he rejects the equality of logic and mathematics, we say that he was a formalist when speaking of philosophy of logic, i.e. in his program of the '20s. He there draws a precise distinction between syntax and semantics, he completely formalizes logic (in the first sense indicated above), he emphasizes the finitist aspect of proof-theory, etc. The most important, however, is the force with which he requires that logical deductions be equated with manipulations of signs:

> The solid philosophical attitude that I think is required for the grounding of pure mathematics – as well as for all scientific thought, understanding, and communication – is this: *In the beginning was the sign.* (1922, p. 1122)

For this reason, we conclude with Hintikka that Hilbert was "not a formalist in his philosophy of mathematics, but perhaps in a sense in his philosophy of logic." (Hintikka, 1988, p. 10) Since we are here interested in his early philosophy of mathematics, mainly of geometry, we will not further stress this formalistic aspect.

3.3.4 The Axiom of Completeness

In the first edition, there was only one axiom in the group V, namely Archimedes' axiom. From the 1902 French edition on, Hilbert added a second axiom in this group

[26] At least, not as it is understood today.

– as he did in (Hilbert, 1900c)[27] –, namely the axiom of completeness. As in the case of his alleged formalism, dangers of misunderstanding are here important. As a matter of fact, the status of the axiom of completeness as exposed in the *Grundlagen* is not quite clear (Majer, 1997, p. 39). Also, beginning with Gödel's work, this notion has been examined thoroughly with methods absent from the *Grundlagen*: "Interpretation of Hilbert's *Grundlagen* by means of the notion of semantic completeness is erroneous, for Hilbert had not yet formulated a single logical axiom!" (Moriconi, 2003, p. 131).

Here is the original formulation of the 1902 French Edition:

Axiom of Completeness To a system of points, straight lines, and planes, it is impossible to add other elements in such a manner that the system thus generalized shall form a new geometry obeying all of the five groups of axioms. In other words, the elements of geometry form a system which is not susceptible of extension, if we regard the five groups of axioms as valid.

According to Hilbert, this axiom makes possible to establish a one-to-one correspondence between the points of a segment and the system of real numbers. However, he specifies that no use of this axiom is made in the development of the book. Why, then, was he brought to introduce this axiom?

The first "strange" thing about this axiom is that the completeness is *postulated*, not proved. As such, it is an axiom *in* the system and not a theorem *about* the system, as completeness is commonly understood today. Zach (1999, p. 354) relates that it is only from Mollerup's discussion (1906) of Hilbert's book that the focus shifts from completeness as something to be stipulated to something to be proved. The argument is that completeness can be no more a postulate than consistency can; it is something that has to be proved. It is possible that the problematic character of this aspect has been a determining factor in Hilbert's change of attitude towards the notion of proof, since it is only possible to lay down the question of semantic completeness (i.e. that all valid formulas are provable) from a metasystematic perspective once the distinction between syntax and semantic has been made.

[27] The formulation of the axiom of completeness in the *Zahlbegriff* is as follows: "It is not possible to add to the system of numbers another system of things so that the axioms I, II, III, and IV-1 are also satisfied in the combined system; in short, the numbers form a system of things which is incapable of being extended while continuing to satisfy all axioms." (1900c, p. 1094)

But in the *Grundlagen*, this is not precisely the way completeness is seen. In the first edition, where the only axiom of continuity available was Archimedes's axiom, Hilbert constructed a denumerable model for Euclidean geometry. However, in such a model, many facts of geometry that we would accept as intuitively evident are indeed not provable (e.g. that for any line having points both inside and outside a circle, it has a point on the circle[28]). Therefore, in this sense, Hilbert's 1899 axiom system is not complete.

Completeness in this sense was closely related to issues about consistency. Hilbert was interested with metageometrical questions about geometry, Euclidean or not. From the works of Beltrami and Poincaré, a Euclidean model of non-Euclidean geometry had already been given, thus showing the relative consistency of non-Euclidean geometry over Euclidean geometry. The next step was thus to study the consistency of Euclidean geometry, with the aid of the axiomatic method. However, to achieve this, Hilbert had to be in possession of an axiomatic system allowing him to include the most important facts of Euclidean geometry, i.e. "that the system of axioms is adequate to prove all geometrical propositions" (Hilbert, 1900c, p. 1093)[29]. Nowadays, instead of the word 'completeness', we use for this notion the expressions 'adequacy' or 'descriptive completeness'[30]:

> This is a semantic property, which could tentatively be explained by saying that the intuitive theory (i.e. the genetically developed theory) must be the intented model of the axiomatic theory. (Moriconi, 2003, p. 134)

The fact that the 1899 edition did not allow Hilbert to prove all facts of traditional Euclidean geometry seriously handicapped his metageometrical plan. Hilbert knew that the problem concerned continuity; in this way, he introduced the completeness axiom in the 1902 edition as a continuity axiom, and he placed it in the group V with

[28] This asks for an uncountable model. In the same way, it cannot be proved in a Cartesian plan over the field extension $\mathbb{Q}$ that the circles 01 and 10 (with a half-unit ray) have an intersection point, since the intersection point $(\frac{1}{2}, \frac{1}{2}\sqrt{3})$ is not included in $\mathbb{Q}$. Proving such a fact is of particular importance, since it is the analytic representation of the basic construction on which all Euclidean geometry lies.

[29] A good discussion of this point is founds in Zach (1999, p. 340): "Before this [1904], Hilbert had formulated completeness as the question of whether the axioms suffice to prove all "facts" of the theory in question."

[30] "That is to say that the axiomatic theory must be adequate with respect to the known, and *not* the knowable, true statements of the given field (not to mention the logically valid ones)." (Moriconi, 2003, p. 131)

Archimedes' axiom[31]. Doing this, Hilbert *postulated* that his axiomatic system was adequate or descriptively complete, though it was by no means an obviously acceptable claim. In the subsequent editions, this axiom has been modified many times.

However, the axiom of completeness does not only postulate descriptive completeness. In order to prove that Euclidean geometry was consistent, Hilbert had to be in position to affirm that his model is not capable of further extension without becoming inconsistent, i.e. that any model of his axiomatic system would be unique up-to-isomorphism; this is needed because Hilbert talks about *the* Euclidean geometry[32]:

> [...] what is to be shown is that it is impossible that two *distinct* structure both satisfy the axioms; one the other hand, to show that at least one structure *must* exist for which the axioms are true.
>
> [...] Once this aim is achieved we have at the same time obtained a rational foundation for the notion of "Euclidean geometry", which can be defined as the unique, up to isomorphism, structure satisfying the axiom. (Moriconi, 2003, p. 130)

The axiom of completeness thus postulates two things: 1- the axiomatic system is adequate for Euclidean geometry and 2- that the model (structure) satisfying the axiomatic system is unique. On the other hand, the proof of consistency proves that there is at least one structure satisfying the axiomatic system. If everything had work as Hilbert had planned, he would thus have an axiomatic system allowing one and exactly one structure, i.e. *the* structure of Euclidean geometry[33].

The second condition postulated by Hilbert's axiom of completeness would not be called 'completeness' nowadays. Since this condition is about the things satisfying the axiom system – that they are capable of no further extension, as long as all the other

[31] Webb (1997, p. 8) gives a strong argument according to which it did not succeed to descriptively complete the system, since the model remains denumerable.

[32] Such a way of speaking would make no sense in a Fregean approach to Euclidean geometry. However, considering Hilbert's approach, it is certainly correct to lay down the problem in this way.

[33] However, among other problems, it may be mentioned that since Hilbert's axiomatic system allows a denumerably infinite model, this model cannot be unique up-to-isomorphism: "[...] no theory Σ with an infinite model can be categorical, for by the Löwenheim-Skolem theorem Σ must then have models of many cardinalities, and no pair of models of different cardinalities can be isomorphic." (Bell and Machover, 1977, p. 186)

axioms hold –, we say that the condition imposed is that of *categoricity*[34]. Compare what have been said with these contemporary definitions of catogoricity:

> A natural strengthening of the condition for completeness [...] is to insist that each pair of models of Σ be *isomorphic*. Under these conditions Σ is said to be *categorical*. (Bell and Machover, 1977, p. 186)
>
> "A theory K with equality is said to be $\mathfrak{m}$-*categorical*, where $\mathfrak{m}$ is a cardinal number, if and only if (1) any two normal models of K of cardinality $\mathfrak{m}$ are isomorphic, and (2) K has at least one normal model of cardinality $\mathfrak{m}$." (Mendelson, 1987, p. 87)

Due to the emphasis put on structure, to the close proximity with the model-theoretic approach, and the fact that the axiom of completeness indeed imposes the condition of categoricity, we will consider in the following section the relation to be established between Hilbert's approach and the categorical one, developed in the second half of the twentieth century.

3.4 Hilbert's Implicit Calculus

This subsection will be a somewhat deeper analysis of Hilbert's approach. It will consist in an examination of key points that Hilbert left implicit in the first editions of the *Grundlagen*, namely the calculus *in* his geometry and *about* his geometry (i.e. *in* his metageometry).

3.4.1 A Tolerant Calculus

In the very first page of the *Grundlagen*, it is announced that the book will present a logical analysis of our intuition of space. In the previous subsections, I tried to make

[34] "In modern terms, Hilbert's axiom of completeness asserts the *categoricity* of an axiom system – not its deductive completeness!" (Majer, 1997, p. 52)

"A set of axioms is *categorical* if it has a unique model up to isomorphism. Having investigated his axioms for geometry with models, Hilbert with his Completeness Axiom simply posited categoricity with the maximal geometry." (Dreden and Kanamori, 1997, p. 83) clear the relation of intuition to logical analysis proper. However, nothing is explicitly said about the specific calculus used for the logical analysis. Of course, as I have shown, some aspects of what he actually did show the character of his enterprise. Nonetheless, there remains a blank to be filled. In the later editions, the gap has partly been filled with predicate calculus and set theory[35]. I consider that these approaches are not adequate to Hilbert's original project, though their introduction in the *Grundlagen* can be explained by the fact that they were the only refined and theoretically well-developed theories of the time[36]. However, an alternate approach has been developed in the second half of the 20$^{\text{th}}$ century, namely category theory. Considering what we previously said of Hilbert's first foundational project, comparing set theory and category theory as possible gap-filler for the *Grundlagen* will now retain our attention.

To use a term now established in the literature, Hilbert's metaaxiomatic approach is markedly *tolerant*. Many theoretical strategies are said to be intolerant: choosing a system of axioms as the unique one appealing to its truth; requiring the use of refined type theory intended to be ontologically sound; imposing a calculus with non-economical rules of inference; etc. These strategies impose prohibitions, i.e. negative requirements by which certain common forms of language (methods of expression and/or inference) are excluded. Hilbert's attitude towards requirements of this kind corresponds exactly to Carnap's general formulation of the *Principle of Tolerance*:

> It is not our business to set up prohibitions, but to arrive at conventions. (Carnap, 1936, p. 51)

In this way, instead of imposing prohibitions, the strategy is to proceed to definitional differentiation. In many cases, this is brought about by the simultaneous investigations of axiomatic systems of different kinds (analogously to Euclidean and non-Euclidean geometries). It is thus possible to make a theoretical analysis of many systems without being blocked by expansive prohibitions even before having the possibility to consider what it is about.

[35] The modifications, however, have all been made by Bernays.

[36] The influence of Zermelo certainly contributed a lot to turn Hilbert to set theory, as Behmann's to predicate calculus. (Mancosu, 2003)

3.4.2 Category Theory vs. Set Theory

In this subsection, we will consider some characteristics of set theory and category theory in order to see which one would "naturally" fill the calculus gap in Hilbert's *Grunlagen*, i.e. which one can be genuinely said to be a "Hilbert-style extension". Of course, for historical reason, Hilbert could not have known category theory. We also underline the fact that he did not directly contributed to the development of set theory (Dreden and Kanamori, 1997, p. 77).

Set theory is a semantical framework presenting objects (sets) in an iterative hierarchy. What makes the specificity of an object is its *internal constitution*. It centers on the principle of *extentionality*. The only way in which one can simulate intensionality is by forming a set from a property; in doing so we must choose to apply the choice axiom. This procedure, though aiming at simulating the laying down of a concept, is nonetheless limited by the fact that everything in set theory is semantically characterized in an extensional way. In such a framework, the central notions are those of membership of an individual to a set and equality of an individual to itself or of an extention to another one.

Contrariwise, category theory stresses that concepts cannot genuinely be characterized by reference to the "internal" membership notion, but rather by "external" reference to connections with other concepts, these connections being established by functions. Thus, the emphasis shifts from internal constitution to external relationships or, as Bell (1981, p. 352-3) specifies it, from the

> *extensional semantical aspect* to the *intensional combinatorial aspect*[37].

In the categorical framework, since the only aspect characterizing objects is their external relations to other objects, the notion of equality cannot be central; rather the central notion is that of isomorphism. Isomorphic objects "look the same", and one can pass from one to the other by isomorphic mappings (arrows, transformations). Moreover these mappings preserve any relevant structure. Thus, the notion of membership is replaced by that of structure-preserving functions:

[37]Concerning the combinatorial character of Hilbert's foundations of geometry, see (Hintikka, 1997).

> Instead of defining properties of a collection by reference to its members, i.e. *internal* structure, [the categorial approach] proceed by reference to its *external* relationships with other collections. The links between collections are provided by functions, and the axioms for a category derive from the properties of functions under composition. (Goldblatt, 1979, p. 1)

This means that we can replace some or all of the members of one object by their counterparts in the other object without making any difference to the structure of the object, to its "behavior"[38]. The direct consequence of such an approach is that within any theory, isomorphic objects are indistinguishable in terms of that theory. An object will be said to be "unique up to isomorphism" if the only other objects possessing that attribute are isomorphic to it. A concept will be "defined up to isomorphism" if its description specifies a particular entity, not uniquely, but only uniquely up to isomorphism (Goldblatt, 1979, p. 42). From a philosophical point of view, a category may be viewed as the explicit presentation of a *mathematical form* or *concept*. This presentation is both *intensional* in the genuine philosophical sense, and explicitly stated in an axiomatically formulated theory.

Also, category theory provides a logical calculus (more precisely, *many* logical calculus), not necessarily asking for a linguistic view of logic, in a way analogous to model-theory. With the development of the model-theoretic approach in logic and of abstract algebra in mathematics, it became felt that the features stressed by category theory (isomorphism, homomorphism, substructure, etc.) were seen as having a kind of universality and even inevitably that was apparently *independent* of their set-theoretical origin (Bell, 1981, p. 349). This has been confirmed by the actual construction of various models of set theory. Certainly, an important result in set theory was Cohen's building of models of set theory in which the continuum hypothesis and the axiom of choice fail. This brought a "relativistic" attitude toward the set-theoretical foundations for mathematics:

> This attitude involves abandoning the idea that mathematical constructions should be viewed as taking place within an 'absolute' universe of sets with fixed and predetermined properties. (Bell, 1981, p. 358)

[38] The similarity with the method used in Hilbert's proof of consistency is striking.

In the '70s and the '80s, it was discussed whether or not it was possible to understand category theory in a pre-set-theoretical sense, i.e. as prior to it (e.g. Feferman, 1977; Bell, 1981). An answer to that question was given by Joyal and Moerdijk's invention of the concept of *Zermelo-Fraenkel algebra*. This is essentially a formulation of set theory based on *operations*, rather than on properties of the membership relation. Zermelo-Fraenkel algebras are the algebras for the two basic operations *union* and *singleton* (Bell, 2001, p. 4). By such an algebra, it is proved that

$$\text{set theory} \subseteq \text{category theory}$$

Both Zermelo-Frankel set theory and its intuitionist counterpart are particular cases of category theory; it is possible to obtain elementary toposes for each version of set theory. This fact certainly provides an evidence of the logical *pluralism* within mathematics, as already exemplified by the existence of classical, constructive, many-valued, paraconsistent, etc., logics. The fact that a truth-value object is found in a topos implies that logic is possible in it. However, it is generally not done with a boolean algebra, but with a Heyting algebra, making the 'internal' logic of a topos in general intuitionnistic. In this way, toposes carry with them their own language, their own calculus (Bell, 2005, p. 4). From this point of view, set theory can hardly be said to be as tolerant as category theory; set theory can be seen as category theory with a set of mathematically unnecessary prohibitions.

Category theory's *raison d'être* is not only the reorganization of mathematical material furnished by set theory. By its tolerance and its structure-oriented approach, it opens to many fruitful fields. It is illustrated by synthetic differential geometry (e.g. Bell, 1998). It has the form of a smooth continuum incorporating actual infinitesimals, which is *inconsistent with classical set theory* though providing elegant and accurate ways of making calculations. This smooth continuum is a form of continuum which cannot be reduced to discreteness (Bell, 2001, p. 5), as it is done in the set-theoretic treatment of analytic geometry. Thus, category theory allows to analyze and use rigorously notions like continuum and infinitesimals, that had previously been rejected due to unnecessary prohibitions.

3.4.3 Hilbert and the Categorical Approach

With the developments of the 20th century in mathematics and logic, the opinion that "the crucial characteristic of mathematical structures is not their internal constitution as set-theoretical entities but rather the relationships among them as embodied in the network of morphisms" (Bell, 1981, p. 350) became more and more accepted. Likely, it came to be seen that the notion of *identity* appropriate for structures is not set-theoretic equality but *isomorphism*. Thus the idea that the fundamental notions of abstract algebra are rooted in the single notion of *structure-preserving function* (morphisms) began to impose itself:

> So category theory itself came to be viewed as a theory of (mathematical) Structure. (Bell, 1981, p. 352)
>
> The success of category theory, and its significance for foundations is due to the *ubiquity of structure* in mathematics. (Bell, 1981, p. 356)

Category theory frees mathematics from the particular form imposed on it by having to regard these entities as pluralities of elements. For any kind of mathematical investigation, it is possible to construct an adequate category, without losing the nature of what it is about, thus providing a foundation to the subject matter:

> It would be somewhat misleading to [think] that foundational systems act primarily as a basis out of which mathematics is actually created. The artificiality of that view is evident when one reflects that the essential content of mathematics is already there before the basis is made explicit, and does not depend on it for its existence.
>
> [...] The axioms codify ways we regard mathematical objects as actually behaving. The theory of these objects is then developed in the form of statements derived from the axioms by techniques of deduction that are themselves rendered explicit. (Goldblatt, 1979, pp. 13-4)

> The practice of topos theory quickly spawned an associated philosophy [...] whose chief tenet is the idea that, like a model of set theory, any topos may be taken as an autonomous universe of discourse or "world" in which mathematical concepts can be interpreted and constructions performed. (Bell, 2005, p. 6)

I would like to call this "constructive tolerance". For each single mathematical (or, more generally, theoretical) subject, we can build a universe of discourse, i.e. a category, that will allow us to study its systematic properties. Thus, the first task is to construct a category *adequate* to our subject matter. As it has been expressed, category theory allows a shift from *absolute* to *local* mathematics:

> This possibility of varying the category of interpretation leads to what I have called in (Bell, 1986) *local mathematics*, in which mathematical concepts are held to possess references, not within a fixed absolute universe of sets, but only *relative* to categories of interpretation of a certain kind – the so-called *elementary toposes*. Absolute truth of mathematical assertions comes then to be replaced by the concept of *invariance*, that is, "local" truth in every category of interpretation, which turns out to be equivalent to constructive provability. (Bell, 2001)

The similarities between the categorical approach and Hilbert's approach to foundations of geometry are striking. Striking enough to discard set theory without further ado. In both case, the distinction between the discovery of a science and its foundation in an axiomatic system is done in a way that the intuitive character of the achieved system is not lost. In both cases, the first task is to find a structure that is adequate to the science having to be founded. In both case, when this adequate structure has been constructed, the "umbilical cord is cut", in order that the subject matter becomes a structure, a form, a manifold of invariant transformations, specified only up-to-isomorphism. And, in both cases, this is done in an axiomatic and algebraic way. I hardly see how it would be possible to have a more harmonious union.

Chapter 4

Frege-Hilbert Correspondence

As long as I have been thinking, writing and lecturing on these things, I have been saying the exact reverse.
Hilbert, 1899, p. 39

At the very origin of the controversy was the correspondence between Frege and Hilbert. The exchange resulted from the reaction of Frege to Hilbert's *Foundations of Geometry*, published in 1899. In what follows, I will try to clarify the conflictual notions and the underlying motives.

In the last chapter (3), I explained Hilbert's foundations, having in mind the objections Frege was to raise hereafter. So, in a sense, I already presented Hilbert's axiomatization in a way that most of Frege's objections are prevented. For this reason, I will only expose the aspects of their discussion still relevant to the debate. A survey of their correspondence will introduce both historically and theoretically Frege's reaction as the source of the series *On the Foundations of Geometry* (1903; 1906a). However we will not extend the study of the subject since we already presented Hilbert's standpoint and Frege's own views will carefully be considered in the next chapter (5).

In this chapter, I will proceed chronologically by first presenting the origin of their discussion, i.e. the context in which they met and the presuppositions on the basis of which they engaged in a dialogue (4.1). After this prelude, I will go directly to the confrontation as such, in which Frege severely reprimands Hilbert for his lack of rigor and for the uselessness of his enterprise, and we will sketch Hilbert's answer (4.2).

As my review will develop, it will become clear that they do not succeed in having a genuine discussion, since their positions are incommensurable. This is why Hilbert finally put an end at this correspondence. In the section 4.3, I will cursorily present the last important fragments from the letters they exchanged.

4.1 Prelude to the Debate

Generally speaking, we can suppose that people engage in a discussion mainly when they have a common background on the basis of which they can agree. This section aims at presenting their shared background knowledge. As we shall see, Frege and Hilbert generally agree on the nature and purpose of symbolism in mathematics; needless to say that the scope of their agreement will become narrower as the discussion continues.

They first met in Lübeck at the 67$^{\text{th}}$ Convention of German Scientists and Doctors held from 16 to 20 September 1895. On the 17, Frege gave a lecture in the section on mathematics and astronomy, where he discussed the advantages of a conceptual notation (that is to say, in this case, a symbolic language) over 'word languages'[1]. Hilbert presumably attended the lecture, since he discussed of this aspect with Frege later in the congress. Frege started the correspondence to go further with this discussion.

In the first letter, Frege reiterates the general views on conceptual notations that he had presented in Lübeck. In his view, theorems and methods should be use with a wide scope, not a narrow one. In this way, "where a line of thought can be perfectly expressed in symbols, it will appear briefer and more perspicuous in this form than in words." (1895, p. 33) These advantages (perspicuity and precision) of symbolism are so great that, in the case of mathematics, many investigations could not even be conceived without a language of symbols. Thus, the use of symbols "must not be equated with a thoughtless, mechanical procedure, although the danger of lapsing into a mere mechanism of formulas is more immediate here than with the use of words. One can think also in symbols." (1895, p. 33) To fall in such a thoughtless procedure is dangerous in two ways: 1- for the truth of the results and 2- for the fruitfulness of the

[1] This lecture became the basis of a paper (Frege, 1897) in which he presents a detailed account of his views concerning conceptual notations and the contrast with Peano's on many general and technical points. However, considering Hilbert's answer (1895), it can be thought that Frege had only presented his general views without the technical part.

science. However, he thinks that the first one can be avoided almost entirely by the use of a logically perfect system of signs. As for the second, it would stop science if it were to happen, but this situation would only be temporary.

He then considers the use of such symbolism in science to which we are drawn in the following natural way:

> [...] in conducting an investigation in words, one feels the broad, imperspicuous and imprecise character of word language to be an obstacle, and to remedy this, one creates a sign language in which the investigation can be conducted in a more perspicuous way and with more precision. Thus the need comes first and then the satisfaction. (Frege, 1895, p. 33)

In his reply Hilbert did not say much but expressed much enthusiasm. He said of this letter that it was of an "extraordinary interest" to him and that he planned to bring it to the mathematical society for a discussion:

> I believe that your view of the nature and purpose of symbolism in mathematics is exactly right. I agree especially that the symbolism must come later and in response to a need, from which it follows, of course, that whoever wants to create or develop a symbolism must first study those needs. (Hilbert, 1895, p. 34)

His too short ten lines letter can not help us to identify exactly on what subject matter he shared Frege's view. In chapter 5, we will see that the extent of their agreement was purely nominal.

4.2 The Core of the Confrontation

Four years later, Hilbert published the *Grundlagen der Geometrie*. The following letters represent Frege's first reaction and Hilbert's tentative reply; they are the very origin of their debate and briefly exhibit their respective arguments.

Frege's first letter on the Foundations begins with the announcement that he himself was working on the foundations of geometry though he never published anything on the subject and specifies afterward that while their were many points of contact between their respective works, there were also profound divergences[2]. In view of Frege's saying, he discussed with his colleagues Gutzmer and Thomae about Hilbert's paper. Apparently, they both defended formalist views and, for this reason, Hilbert's position[3]. However, many points remained obscure to Frege and he wrote to Hilbert in view of obtaining clarifications.

Frege wanted to know what kind of distinctions were made in Hilbert's book between definitions, explanations and axioms. In presenting the group II of axioms, Hilbert says that the "axioms of this group define the idea expressed by word 'between' " (1902b, p. 5). Frege thought that this group of propositions had nothing resembling a definition and Thomae agreed by saying that it "is not a definition; for it does not give a characteristic mark by which one could recognize whether the relation Between obtains" (Frege, 1899, p. 35).

Frege also complains that it is not clear what should be called a point. One would think that when Hilbert talk about points, he talks of points in the sense allowed by Euclidean geometry; this hypothesis would be seemingly corroborated by the fact that he says that axioms express fact of our intuition. However, when Hilbert tries to prove the compatibility of his system of axioms, he considers a pair of coordinate numbers as a point (Hilbert, 1902b, p. 27).

> I have my doubts about the proposition that a precise and complete description of relations is given by the axioms of geometry (sect.1) and that the concept 'between' is defined by axioms (sect.3). Here the axioms are made to carry a burden that belongs to definitions. (Frege, 1899, p. 35)

In Frege's view, Hilbert does overthrow the fundamental distinction between definitions and axioms thus confusing their respective function. Beside the old traditional meaning

[2]The first claim which he does not insist on is that he had himself build a system (likely formal and axiomatic) with fewer primitive terms – though it still remains unpublished and unheard of nowadays. In chapter 5, I will partially reconstruct a Fregean-style foundation for geometry in view of showing what it could have look like

[3]This fact is related by a note from McGuinness (in Frege, 1899, p. 34.). I know nothing precise about Gutzmer. However, I know that Thomae upholds a kind of formalism (Thomae, 1906a,b) which includes many difference with Hilbert's project.

of 'axiom' according to which they express fundamental facts of our intuition, there is also another meaning to the term that Frege does not quite grasp.

Frege explains at great length to Hilbert his own conceptions which aims to eliminate the "complete anarchy and subjective caprice now prevail[ing]" (1899, p. 36). I will here only briefly sketch his conception since it will receive a more extended analysis in the next chapter 5 where Frege view will be more thoroughly examined. He starts by laying down a fundamental dichotomy between definitions and other propositions (among them are axioms, fundamental laws, theorems, etc.)[4].

In Frege's view, definitions contain a sign (an expression, a word) which had no meaning before having been defined, i.e. that its meaning is first given by its definition. The definition then becomes a self-evident proposition. Hence, a definition does not assert anything, but lays down something. In this way, something in need of proof or some other confirmation for its truth should not be presented as a definition. The distinction should be strictly preserved if the rigor of mathematical investigations is to be maintained. Other types of proposition must not contain a sign whose meaning was not completely laid down, so that there is no doubt about the sense of the proposition and the thought it expresses. We can only ask whether this thought is true and on what ground its truth rests. Other kinds of propositions should never lay down the meaning of a sign, since they presuppose that it has already been laid down. Moreover, definitions must not contradict themselves if they are not to be considered faulty. In Frege's view, Hilbert's fashion of sign defining is different and must be seen as mistaken.

Among other types of propositions, Frege considers axioms. Axioms are propositions that are true without being proved. They are true because our knowledge of them "flows from a source very different from the logical source, a source which may be called spatial intuition" (1899, p. 37). From their truth, it follows that they can not contradict one another. There is no need for further proof on this subject and that is why Frege thinks that when Hilbert engages himself in the task of proving the consistency of his system of axioms he commits himself to a fallacy. He considers that his sense of 'axiom' is the traditional acceptation of the word. Thus Hilbert's procedure cannot possibly be justified since axioms cannot define anything and if a word has no fix meaning, it cannot

[4]According to Frege, one may recognize a third kind of propositions, namely elucidations. However, they cannot be count as part of mathematics, but as its propædeutical stage. They are also involved in laying down the meaning of a sign (Frege, 1899, p. 35).

express a fundamental fact of our intuition[5]. Thus, axioms cannot define anything.

In his sole careful reply, that he sent back to Frege two days later, Hilbert tried to explain the sources of their misunderstanding. This letter is particularly important to us, since it is the only one in which he takes the time to answer to Frege's objections. Hilbert first stresses that it should be kept in mind that their objectives are different:

> It was of necessity that I had to set up my axiomatic system: I wanted to make it possible to understand those geometrical propositions that I regard as the most important results of geometrical inquiries: that the parallel axiom is not a consequence of the other axioms, and similarly Archimedes' axiom, etc. (1899, p. 38)

He thinks his system of axioms allows one to answer these questions in a very definite way, even though the answer is somewhat surprising or unexpected.

Hilbert then goes on to explain why he considers that his axiomatic system "satisfies the strictest demands of logic" (1899, p. 39). If it could make Frege happy, he would not object that one uses the traditional sense of 'definition' instead of what he called 'axioms' in the *Grundlagen*. One will then have to say: " 'Between' is a relation which holds for the points on a line and which has the following characteristic marks: II/1 ... II/5." (1899, p. 39) However, Hilbert points that he does "not want to assume anything as known in advance" (1899, p. 39):

> If one is looking for other definitions of 'point', e.g. through paraphrase in terms of extensionless, etc., then I must indeed oppose such attempts in the most decisive way; one is looking for something one *can never find because there's nothing there*; and *everything gets lost and becomes vague and tangled and degenerates into a game of hide-and-seek*. (1899, p. 39)[6]

If Frege prefers to call his axioms 'characteristic marks of the concept', Hilbert would have no objection at all, "except perhaps that it conflict with the customary practice

[5] It would introduce "highly suspect ambiguity" (1899, p. 37)

[6] Emphasis of mine.

of mathematicians and physicists" (1899, p. 39).[7] Briefly, he just thinks that "the renaming of 'axioms' as 'characteristic marks' is surely an extraneous matter as well as a matter of taste" (1899, p. 40). After these remarks on the use of the word 'axiom', Hilbert continues with considerations on truth, existence and contradiction.

> You write: 'I call axioms propositions... From the truth of the axioms it follows that they do not contradict one another.' I found it very interesting to read this very sentence in your letter, for as long as I have been thinking, writing and lecturing on these things, I have been saying the exact reverse: *if the arbitrarily given axioms do not contradict one another with all their consequences, then they are true and the things defined by the axioms exist. This is for me the criterion of truth and existence.*[8] (1899, p. 39-40)
>
> This conception is indeed the key to an understanding not just of my *Festschrift* but also for example of the lecture I recently delivered in Munich on the axioms of arithmetic[9] [...] (1899, p. 40)

To try to give a definition of a point in three lines is to Hilbert's mind an impossibility, for the whole structure of axioms yields a complete definition. Every axiom contributes something to the definition, and hence every new axiom changes the concept. Adding axioms brings confusion, since after "a concept has been fixed completely and unequivocally, it is on my view completely illicit to add an axiom – a mistake made very frequently, especially by physicists" (1899, p. 40). And he adds: "One of the main source of misunderstanding is precisely the procedure of setting up an axiom, appealing to its truth (?), and inferring from this that it is compatible with the defined concepts." (1899, p. 40) To understand what points are, we should consider the place they occupy in the axiomatic system and, if we change the system, we change the meaning of 'point'. Hilbert precises in a clearly model-theoretical way:

> It is surely obvious that every theory is only a scaffolding or schema of concepts together with their necessary relations to one another, and that the basic elements can be thought of in any way one likes. ... Any theory can always be applied to infinitely many systems of basic elements. One

[7] It is interesting to see that Frege and Hilbert do use the word 'axiom' in a very different way, and that they both claim that their use is in agreement with the 'customary use'. It is clear that their point of reference is different: for Frege it is traditional philosophy of mathematics, and for Hilbert it is 19$^{\text{th}}$-century mathematics.

[8] Emphasis of mine.

[9] He is referring to (1900c).

> only need to apply a reversible one-one transformation and lay it down that the axioms shall be correspondingly the same for the transformed things. ... But the circumstance I mentioned can never be a defect in a theory (it is a tremendous advantage), and it is in any case unavoidable. (Hilbert, 1899, p. 40)

In the following letter, Frege (1900a) wanted to understand more deeply Hilbert's view; however, incommensurability became clear. To do so, he tried to understand the difference in Hilbert's use of some keyword and to 'reduce' it to his own 'traditional' use. He says: "It seems to me that you want to detach geometry entirely from spatial intuition and to turn it into a purely logical science like arithmetic." (1900a, p. 43) The axioms of Hilbert are to be carried along in every theorem as its conditions, included in the word 'point', 'line', etc. From a logical point of view, it seems to be always the same: Hilbert wants to show the lack of contradiction of certain determinations. 'D is not a consequence of A,B,C' says the same thing as 'the satisfaction of A,B,C, does not contradict the non-satisfaction of D'. 'A,B are independent means that A is not a consequence of B and B is not a consequence of A'. Frege thinks that the only means to show non-contradiction of some properties is to point to an object with all these properties. To show the independence of axioms of Euclidean geometry would then demand to point an example where an axiom is not satisfied, but it is impossible because these axioms are all true. Hence, Hilbert tries to do something impossible. Nevertheless, he thinks that there is some value in what Hilbert has done, i.e. by the adoption of a higher standpoint (larger than Euclidean geometry), because it includes examples that make the independence evident. However, this undertaking on independence extended to a system of propositions which are arbitrarily set up (i.e. not limited to the axioms in the old traditional sense) would be of no scientific importance.

Frege admits that he "would be reluctant to confess that a 'point' cannot properly be defined at all" (1900a, p. 44). He observes that Hilbert's system of axioms is like a system of equations with several unknowns, where there remains a doubt whether the equations are soluble and, especially whether the unknown quantities are uniquely determined. This unknown quantity here acts like the meaning of the fundamental expressions of geometry! Frege would like to know expressly what is the meaning of these expressions (the solutions of the system of equations):

> It they were uniquely determined, it would be better to give the solutions,

> i.e. to explain each of the expressions ‘point’, ‘line’, ‘between’ individually through something that was already known. Given your definitions, I do not know how to decide the question whether my pocket watch is a point. (1900a, p. 45)

He then goes on to compare Hilbert’s procedure with theology, and he explains his view in details, what we will see in the chapter 5.

4.3 The End of the Discussion

Through their correspondence, Frege’s attitude toward Hilbert has always been highly unpleasant. Hence, Hilbert’s interest sheeply dropped. It is as if he had understood that their standpoints were much too apart from one another to ensure a fruitful discussion. I consider that after the long and “moralist” letter from Frege (1900a), the discussion can be considered finished, at least from Hilbert’s point of view. This can be seen by this “polite” reply: “As I am at the moment overburdened with all kinds of work, it is unfortunately impossible for me to reply in detail to your letter.” (1900a, p. 48) However, they exchanged some short letters, and I will here just present some important excerpts.

Having received two papers from Hilbert (1900c, 1902a), Frege sent some comments where, once again, he shows that he did not understand Hilbert’s position (Frege, 1900b). Hilbert says there that the axioms contain a precise and complete description of those relations that obtain between the elementary concepts of a science. Frege cannot reconcile this with Hilbert’s saying that they are component parts of the definitions of these concepts. According to him, it is admissible to talk about relations between concepts only after these concepts have been given sharp limits, but not while they are being defined. Thus, axioms cannot both define the meaning and allow one to know if the relations obtain. To this, Hilbert answers clearly:

> In my opinion, a concept can be fixed logically only by its relations to other concepts. These relations, formulated in certain statements, I call axioms, thus arriving at the view that axioms (perhaps together with propositions assigning names to concepts) are the definitions of the concepts. [. . .]

> I found myself forced into [this view] by the requirements of strictness in logical inference and in the logical construction of a theory. (Hilbert, 1900b)

Finally, there is a passage worth quoting[10] in the letter that Hilbert sent to Frege in 1903 to thanks him for the second volume of the *Basic Laws of Arithmetic*:

> [the contradictions][11] led me to conviction that traditional logic is inadequate and that the theory of concept formation needs to be sharpened and refined. As I see it, *the most important gap in the traditional structure of logic is the assumption made by all logicians and mathematicians up to now that a concept is already there if one can state of any object whether or not it falls under it. This does not seem adequate to me. What is decisive is the recognition that the axioms that define the concept are free from contradictions.*[12] (Hilbert, 1903)

[10] McGuinness (1980, p. 31.) talks of this letter that way: "Letter 9, which follows the others after an interval of three years, adds little to the controversy." It seems to me much more important that what he says, for it is the best evaluation made by Hilbert of the state of their controversy.

[11] As we have seen in the subsection 2.1.3, the type of contradiction that emerged from Frege's system has been known some years before Russell's "discovery" by the mathematicians of Göttingen. To be sure, this contradiction had been thought for at least 2-3 years at the moment Hilbert wrote this letter.

[12] Emphasis of mine.

Chapter 5

Frege: On the Foundations of Geometry

A good deal [of Frege's philosophy of mathematics], indeed, is patently wrong; but of which philosopher of mathematics is that not true? Despite his blindness to things his contemporaries perceived, despite his unawareness of much that concerns us but wholly failed to strike him, [...] he is, in my judgment, the best philosopher of mathematics.
Dummett, 1991a, p. xii-xiii

This chapter aims at reconstructing Frege's system more than at exposing in details his critique towards Hilbert. It will certainly be more appropriate, since the question concerns the general grounds motivating their respective enterprises[1]. Without a general overview of his system, no real understanding of the axiological nature of the debate is to be expected. Now, it would be unreasonable to expect an exhaustive analysis of Frege's lifework. According to Dummett (1991a), three periods can be distinguished in his work. The early period starts with the *Dissertation* (1873) and goes up to 1887 – where he wrote mainly about logic and mathematics. Between 1887 and 1891, Frege stopped publishing papers completely; he was working at refinements of his scientifico-philosophical project, the *Begriffsschrift*. Frege then came back with *Function and Concept* in 1891, *On Sense and Reference* in 1892 and with *The Basic Laws of Arithmetic* in 1893, three landmarks in philosophy of mathematics, logic, and

[1] "In sum, Frege and Hilbert did manage to understand each other for the most part. Nonetheless, they were at cross-purposes in that neither of them saw much value in the other's point of view." (Shapiro, 1996, p. 165)

language. The middle-period works developed on this background – "and it scarcely altered throughout the whole period" (Dummett, 1991a, p. 2) –, it includes works up to 1906. During this period was the controversy with Hilbert, but also the discovery of a contradiction in the *Basic Laws* by Russell. The later period opens in 1906 when Frege understood that the system – as a whole – had to be revised. This was due to his becoming aware that his former solution to the paradox did not work, and that deeper changes were asked for.

Such radical changes in Frege's writing may set problems to a reader[2]. In what follows, we will limit our analysis to the middle period[3]. Besides this "temporal" limitation, we set aside questions relative to psychologism, to ordinary language, etc, in view of keeping our attention specifically on his works about logic and mathematics from the middle period – only with cursory glances elsewhere. For this reason, the present chapter will begin with an outline of what science should be according to Frege (5.1), i.e. its goal, its sources, its means, etc. From there we will review the fundamental principles of logic that Frege acknowledges, and on which he founded the entirety of his system (5.2). In this section, we will have a deeper insight of what Frege means by 'function', 'concept', 'object', 'sense', 'reference', etc, since this is unavoidable in a presentation of Frege's philosophy of geometry.

No doubt, Frege's creative masterpiece is the *Begriffsschrift* (concept-script)[4]. It will be important for our inquiry to have a closer look at it, since he conceives of geometry as its extension:

> It seems to me to be easier still to extend the domain of this formula language to include geometry. We would only have to add a few signs for the intuitive relations that occur there. In this way we would obtain a kind of *analysis situs*. (1879, p. 7)

I will present the ins and outs of the themes related to the controversy by a systematic examination of the fundamental concepts he had developed. This will be done in

[2]From an exegetical point of view: "The greatest difficulty is to decide how much carried over from the early to the middle period." (Dummett, 1991a, p. 3)

[3]Thus, the *Begriffsschrift* (1879) and the *Foundations of Arithmetic* (1884) will scarcely be examined. Therefore, the use of '*Begriffsschrift*' without further specification will refer not to (Frege, 1879) but to (Frege, 1893). In this way, we will avoid the problem mentioned in the last footnote.

[4]In *What way I regard as the Result of my Work?* (1906b, p. 184), while he was engaged in his conflict with Hilbert, he claims: "it is almost all tied up with the *Begriffsschrift*."

agreement with this motivation: to render Frege's claim intelligible and plausible, that is, that geometry is but an extension of the *Begriffsschrift*. Following this thread seems relevant for at least this simple reason: the *Begriffsschrift is* the realisation of Frege's ideal conception of science and philosophy.

5.1 The Aim and Means of Science

5.1.1 The Aim of Science

Frege's view of science, in many respects, is closer to the traditional conception of science that we can find, for instance, in ancient Greece. He is completely opposed to some contemporary philosophies of science according to which science produces its object[5]. For Frege, science is objective and it succeeds in knowing its object:

> If we want to emerge from the subjective at all, we must conceive of knowledge as an activity that does not create what is known but grasps what is already there. The picture of grasping is very well suited to elucidate the matter. (1893, p. 23)

Hence science has an epistemic role to fulfill: to produce objective knowledge. Considering this epistemic function, it is necessary for scientific discourses to have a content – to talk about the realm to which they refer –, and not be mere word-playing:

> In the majority of cases what concerns us about a thought[6] is whether it is true. The most appropriate name for a true thought is a truth. A science is a system of truths. A thought, once grasped, keeps us pressing us for an answer to the question whether it is true. We declare our recognition of the truth of a thought, or as we may also say, our recognition of a truth, by uttering a sentence with assertoric force. (1899-1906, p. 168)

[5] "The work of science does not consist in creation, but in the discovery of true thoughts." (Frege, 1918-9, p. 368)

[6] For now, it is enough to know that a thought is the objective content of a proposition. We will come back on this in 5.2.1.

From this excerpt, we get precisions: the task of science is to grasp objective contents (thoughts), to discover truths (i.e. to judge of the truth of these thoughts), to systematize them, and finally to express these true thoughts in assertions.

> The goal of scientific endeavour is *truth*. Inwardly to *recognize something as true* is to *make a judgement*, and give expression to this judgement is to make an assertion. [...] The *grounds on which we make a judgement may justify our recognizing it as true*[7]. (1879-91, p. 2)

And this is the question whether a thought is true or false that is usually the reason why, in scientific work, we are concerned with thoughts (1899-1906, p. 167).

Now two questions command attention: 1- what are these grounds that can justify us to acknowledge the truth of thoughts (5.1.2) and 2- how can these true thoughts be expressed and systematized? (5.1.3 and 5.1.4)

5.1.2 The Sources of Knowledge of Science

The grounds that can justify us to acknowledge the truth of thoughts are the sources of knowledge. Frege recognizes three sources of knowledge: 1- sense perception, 2- the logical source of knowledge and 3- the geometrical and temporal sources of knowledge (1924-5a, p. 267). However, they are not all relevant for mathematics: "For mathematics on its own, we don't need sense perception as a source of knowledge: for it the logical and geometrical sources suffice." (1924-5a, p. 268) For this reason, we will no more consider sense perceptions.

The logical source of knowledge consists in grasping and applying the laws of logic – which are objective. This source of knowledge provides science with demonstrative knowledge – as opposed to intuitive one (Frege, 1893, p. 3) –, i.e. knowledge produced by the grasping of the basic laws of logic and their application to inferences. These primitive laws of logic cannot be defined – they are *primitive* –, but at best indicated by metaphorical talking in ordinary language. Moreover, the imperfections of ordinary language renders it impossible to think logically in words, since many non-logical dispositions contributed to its construction. However, language is a creation of man; in

[7] This emphasis is mine.

this way, if we only consider the logical dispositions (and not, for instance, poetical ones), it would be possible to build a language adequate for logical thinking. Hence, logical thinking is closely related to the fact that we can think in symbols, at least if our language is sufficiently well constructed. Therefore, the task of conducting valid proofs and to express them perspicuously rests on logic. However, it is not all the field of mathematics that flows from the logical source of knowledge. There is also the geometrical one[8]:

[8] It is well known that Frege's logicist project aimed at proving that mathematics – at the exception of geometry – can be reduced to logic, i.e. that they flow from the logical source of knowledge alone. However, some later papers show clearly that his view has changed (Frege, 1924-5a, 1924-5b, 1924-5c). He there considers that numbers of different kinds must be distinguished. Of course, it starts with the natural numbers (kindergarten-numbers), from which we can logically build the rationals. But they form a discontinuous (discrete) series, essentially different from the series of points on a straight line (a continuum). Hence, anything resembling a continuum remains impossible from such a logical construction, i.e. that there is no "logical bridge" between them. In this way, he replaced his logicism by what we can call a "geometricism".

However, he maintains that there is no empirical route to the basic laws of mathematics. For that, *an* a priori *mode of cognition must be involved* (Frege, 1924-5b, p. 277). However, this *a priori* cognition does not have to flow from purely logical principles, as he originally assumed. It can have a geometrical source: "The more I have thought the matter over, the more convinced I have become that arithmetic and geometry have developed on the same basis – a geometrical one in fact – so that mathematics in its entirety is really geometry. Only on this view does mathematics present itself as completely homogeneous in nature." (Frege, 1924-5b, p. 277) Also, in Frege (1924-5c), he claims that the geometrical source provides intuitive knowledge. From this it follows that mathematical knowledge flows from a *synthetic* a priori *intuition of space*.

The textual facts, together with the fact that Frege undoubtedly adopts a Kantian terminology in (Frege, 1884), would make of Frege a somewhat neo-Kantian philosopher of mathematics. If there were a label to attach to his work, it is this one that I would choose. However, the point is not generally accepted in the literature: it is under discussion.

In Dummett (1991a, p. 3), it is remarked that Frege ceased to use the Kantian terminology of the *Grundlagen* during the middle period (this is not quite true: in Frege (1892), the Kantian terminology is omnipresent), but that he never explicitly distanced himself from this tradition. Moreover, he explicitly used its terminology again in the later writings, and with strong emphasis. This seems to point in the direction I adopt.

According to Coffa (1982, p. 687), "[...] Frege accepts the principle of synthetic judgment." Shapiro (1996, p. 163) goes the same way, claiming that "Frege's logicism did not extend to geometry, which he regarded as synthetic *a priori*." Dummett (1991b, p. 128) agrees, though he keeps some reserve: " [...] it is going very far beyond the demonstrable facts to assert that, throughout his life, he held them to be synthetic a priori; we have no positive reason to affirm, though no specific reason to doubt, that he continued to view the Kantian trichotomy analytic/synthetic a priori/a posteriori with favour." And he continues: "It is, nevertheless, quite likely to be true that Frege always regarded the truths of geometry as synthetic a priori."

However, Dummett (1991b, p. 128) thinks that it does not follow from this that he accepted intuition in a Kantian sense: "That he always connected our knowledge of them with intuition in anything like a Kantian sense is very much more dubious." And Demopoulos (1998, p. 496) seems to agree on this: "[...] in particular, they do not rest on any Kantian notion of intuition of the sort Frege sought to refute." We note, however, that Demopoulos is mainly concerned in this paper with Frege's philosophy of arithmetic.

On this question, it is hard – if possible – to give a definite answer, for Frege often speaks of intuition, but nonetheless tells little on its nature. Maybe, we can just indicate that in any way something like

> From the geometrical source of knowledge flow the axioms of geometry. It is least of all liable to contamination. Yet here one has to understand the word 'axiom' in precisely the Euclidean sense. (1924-5a, p. 273)

For Frege, the axioms of geometry are understood in the Euclidean way, i.e. that they are true thoughts concerning the constitution (form, structure) of real space. It will become clearer as we will advance through his philosophy of geometry that it cannot be otherwise because they provide us with objective knowledge of reality. If there is to be a science named geometry, then there should be a source of knowledge on the grounds of which we can acknowledge the truth of thoughts about space. For this reason, the propositions that we find in mathematics are *the expressions of truths about their respective realm*.

If a science is expressed in a sufficiently rigorous language, Frege pretends that we will be able to state explicitly which source(s) of knowledge do contribute to it (1893, p. 3): "Accordingly, we divide all truths that require justification into two kinds, those for which the proof can be carried out purely by means of logic and those for which it must be supported by facts of experience." (1879, p. 5) As it should now be clear, Frege considers that geometry is not a purely logical system, that it is a true system of thoughts, and that the acknowledgment of these truths flows from an *a priori* source of geometrical knowledge. Therefrom, an important task of a philosophy of geometry is to identify what is properly logical and what is properly geometrical in an axiomatized geometry.

5.1.3 The Means of Science

Sciences are generally abstract. Whereas commonsense can mitigate the consequences of its impreciseness by sense perceptions, it is generally not possible to do so in science. This is so because sciences – particularly mathematics – are mainly constituted of judgments, i.e. of assertions on concepts. But concepts are not accessible by sense perceptions. However, our language has the capability to express these assertions, then "giving presence to what is absent, invisible and inaccessible to senses" (1882, p. 63).[9]

a transcendental deduction of the possibility of synthetic a priori judgments cannot be the thing on which Frege's system is based (at least after the early period), since there should be basic truths and these cannot be proved, whether by a transcendental argument or whatever else.

[9]For all passages of this paper, the translations from the French edition are mine.

For this reason, Frege claims that science is in need of a mode of expression that can prevent mistakes of interpretation and mistakes in reasoning, since both mistakes find their origins in the imperfection of language. However, though the origin of these mistakes lays in language, Frege considers that language itself can be use to bypass these defects: "Signs have, for thought, the same importance that the idea of using wind to go against wind had for navigation." (1882, p. 64) Thus, the sensible signs of language allow us to access conceptual thinking with all the rigor needed, at least if the choice and the structure of these symbols is adequate. And, as one can expect, ordinary language is not such an adequate language whereas, of course, the *Begriffsschrift* is:

> I believe that I can best make the relation of my ideography to ordinary language clear if I compare it to that which the microscope has to the eye. Because of the range of its possible uses and the versatility with which it can adapt to the most diverse circumstances, the eye is far superior to the microscope. Considered as an optical instrument, to be sure, it exhibits many imperfections, which ordinarily pass unnoticed only on account of its intimate connection with our mental life. But, as soon as scientific goals demand great sharpness of resolution, the eye proves to be insufficient. The microscope, on the other hand, is perfectly suited to precisely such goals, but that is just why it is useless for all others. (1879, p. 6)

Frege's concept-script is a device designed according to the aim of science presented in 5.1.1. This language has the task of making explicit what is the logical structure – the logical interconnections among truths composing the systems – and the content – that derives from the axioms which are expressions of true knowledge. This ideal logical language should be purified from all ambiguities in order that the strictly logical form perspicuously renders the content (1882, p. 65). In other words, for this language to be able to fulfill the aim of science, its logical structure should mirror the structure of the object the science it serves is about, in order that we obtain "a system of notation directly appropriate to objects themselves" (1879, p. 6). In this way, science will make an important step because of its perspicuity. Hence, for Frege, the means that science is to use in order to reach its goal is a logically perfect language, and his *Begriffsschrift* is an attempt at such a thing.

5.1.4 *Calculus Ratiocinator* and *Lingua Characterica*

We have foreseen that a language designed for science, according to Frege, should be able to perspicuously render the logical relations and the content it has to express. These are the two fundamental part of "highly developed languages". However, people has often been misled concerning Frege's intention, and this is why comparisons with other formulas languages of his time, like those of Boole and Peano, are useful.

Traditionally, formal logic has been conceptual, and this from Aristotle to Boole[10]. But this is not Frege's approach to logic: "As opposed to this, I start out from judgements and their contents, and not from concepts." (1881, p. 16) "This is one of the most significant difference between my conception and Boole's, and I would add Aristotle's, that I start not from concepts but from judgements." (1882-3, p 74) Accordingly, the kind of enterprise to which he opposes considers only the relation of subordination of a concept to another one. This is why he thinks of Boole's formula language that it only consists in the dressing up of abstract logic with algebraic signs. Moreover, he thinks that this is not proper to the expression of a content (1882-3, p. 73). In Boole's formula language, there is no sign for individuals, and Frege considers that it is an important defect. Even when there is only one individual subsumed under a concept, there remains a fundamental difference between concepts and individuals (1882-3, p. 71). Therefore, since "right from the start [Frege] had in mind the *expression of a content*" (1881, p. 12), it is fundamental to be able to express perspicuously the difference between concepts and individuals, and the differences of the relation of subordination of a concept to another one and the relation of subsumption of an individual under a concept – a thing that Boole does not do. For this reason, we say that Frege's logic is not only conceptual, but also extensional[11]:

[10] At least, this is what Frege claims (1881).

[11] According to Cocchiarella (1988), it should be characterized as a (2^{nd}-order) extensional predicative logic.

However, I think it is not possible to establish so sharply the status of Frege's logic, due to important ambiguities present in his texts. Certainly, Frege makes the distinction between a concept and its extension. Thus, a concept is not for him the same as its extension. However, his logic is not only based on predication, as for example Boole's algebra, but also on membership. Nonetheless, it is not a purely extentional theory as, for example, set theory is. However, when we examine the relation of a term to its extension, i.e. the sense, we observe a similarity with a set description by intension. We may thus suspect an implicit intervention of something analogous to the axiom of choice, which requires the priority of the extensional perspective.

> Boole's logic [...] deals solely with logical form, and not at all with the injecting of a content into this form – while this is exactly the intention of Mr. Peano. In this regard his enterprise more closely resembles my conceptual notation than it does Boole's logic. From another point of view, however, we can recognise a closer affinity between Boolean logic and my conceptual notation, in as much as the main emphasis is on inference, which is not stressed so much in the Peano logical calculus. In Leibnizian terminology[12] we can say: Boole's logic is a *calculus ratiocinator* but not a *lingua characterica*; Peano's mathematical logic is in the main a *lingua characterica*, and at the same time also a *calculus ratiocinator*; whereas my conceptual notation is both, with equal emphasis.[13] (1897, p. 242)

This is why he has always been vehemently opposed to what he called "formal theories of arithmetic" (e.g. Frege, 1885); arithmetic is not a mere symbol-shifting game – like we shift chess pieces according to some rules –, but a formula language aiming from the very beginning at the expression of a content. Therefore, Frege proposes this Euclidean ideal of science (1893, p. 2):

- It cannot be demanded that everything be proved, because it is impossible;
- But we can require that every proposition used without proof be declared as such (hence, we can see what the "whole structure" rests on);
- We must try to diminish these primitive laws as far as possible (i.e. proving everything that can be);
- We demand that all methods of inference employed be specified in advance.

Without any reserve, I consider that this serves to Frege as a guiding star throughout all his works.

[12]This distinction of Frege resulted of a debate with Schröder, where both pretended to be closer to the Leibnizian ideal than the other.

[13]Here's a relevant comment from (van Heijenoort, 1967b, p. 324): "If we came to understand what Frege means by [the opposition between *lingua characterica* and *calculus ratiocinator*, we shall gain a useful insight into the history of logic."

As Peckhaus (2004) said, this distinction is certainly outstanding. He remarks, however, that van Heijenoort's characterization of a logic as language (has a quantification theory, universality, internal semantics, and a fixed universe) does not justice to those (e.g. Schröder, Peirce, etc.) having developed logic as calculus (who, supposedly, have no quantification theory, no universality, external semantics, and no fixed universe). By a comparison with Shcröder's algebra of logic, he shows that van Heijenoort is mistaken on two points: about quantification and about universality. However, van Heijenoort's distinction remains insightful.

Cocchiarella (1988), on his part, considers that the distinction is really about the fact that a logic is based on the notion of membership or on the notion of predication, since it is possible to have predicative and set-theoretical account of both intensional and extensional logic.

5.2 Logical and Linguistic Considerations

In what follows, I will not analyze the *Begriffsschrift* as such, but introduce the "logically primitive phenomenon" on which Frege's system rests.

5.2.1 Sense and Reference

Frege's paper *On Sense and Reference* (1892) is probably the best known paper of his works. So, I will be brief.

When one examines the meaning of the sign "=" – which is somewhat a formal counterpart of "is" – Frege is brought to ask: "Is it a relation? A relation between objects, or between names or signs of objects?" (1892, p. 56) He assumes that it is a relation and, on this basis, tries to understand between what this relation stands. Let us consider the expressions '$a = a$' and '$a = b$'. Frege remarks that they are clearly of differing cognitive value: " '$a = a$' holds *a priori* and, according to Kant, is to be labeled analytic, while statements of the form '$a = b$' often contain very valuable extensions of our knowledge and cannot always be established *a priori*." (1892, p. 56) It is on this *epistemic* argument emanating from his conception of science that Frege's analysis is founded. Now, between what the relation holds? Frege considers three cases (1892, pp. 56-7):

1. *A relation between what the names 'a' and 'b' designate.* In this case '$a = b$' would not differ from '$a = a$', for in both cases it would be a relation of a thing to itself. This would not account for their differing epistemic values.

2. *A relation between the names (signs) 'a' and 'b'.* Indeed, it seems that by the expression '$a = b$' we intend to say that the names 'a' and 'b' designate the same thing. Thus, it would be an assertion on signs, insofar as they name or designate something. The relation would then be mediated by the connexion of each sign with the same designated thing. However, this connection is arbitrary since we can use any producible event or object as a sign for something. Hence, we would *express no proper knowledge by its means*[14] and, of course, it does not account for their differing epistemic values.

[14] We will find this argument again in the subsection 5.4.3. This obligation of objective mediation between signs and references – by senses – has important implications; as we will see, it precludes Frege of any model-theoretic metatheory of the kind Hilbert developed.

3. *A relation between signs having a sense and a reference.* An understanding of their differing epistemic value can arise only if the difference between the signs corresponds to a difference in the mode of presentation of that which is designated. We can have different names for the same objects, and these names indicate the mode of presentation; and hence the statement contains actual knowledge.

From this epistemic argument, it follows that it is "natural to think of two elements connected with a sign" (1892, p. 61): 1- there is that to which the sign refers, which may be called the *reference* of the sign, and 2- the mode of presentation, which can be called its *sense*. These signs having *definite objects* as reference are called *proper names*. A proper name *expresses* its senses, and *stands for* or *designates* its reference (1892, p. 61). Hence, in '$a = b$', the references of 'a' and 'b' are the same, but not their senses.

Typically, to a sign there corresponds a definite sense and to that in turn a definite reference, while to a given reference there does not belong only a single sign. Also, to a given sense can be associated different expressions, whether in different languages or in the same[15]. Moreover, if "words are used in the ordinary way, what one intends to speak of is their reference" (1892, p. 58). But this is not to say that there is always a reference for which an expression with a sense stands for.

Now, Frege applies to proper names the same analysis than to propositions[16]. From the supposition that propositions have epistemic value, it follows that they contain a thought (an objective content)[17]. The question is now whether this thought is the sense or the reference of the proposition. Considering that a proposition has a reference, we

[15]However, as we will see in the subsection 5.5.1, one may require of a logical language that names correspond one-to-one to senses.

[16]Whether Frege starts from an analysis of proper names to extend it to propositions or the reverse is an important question. It seems plausible that this analysis applied to propositions is philosophically prior to the analysis of proper names (though not heuristically), since the leitmotiv of his philosophy of logic is that the starting point of logic is judgement (i.e. the establishment of the reference of a proposition). Now, since propositions are proper names of truth-values, the analysis cannot differ from that of singular terms.

[17]In Frege's philosophy, thoughts are neither things of the external world nor ideas: "An idea is the content of *my* consciousness. But a thought is not an idea for it is not a content of *my* consciousness. The thought in the Pythagorean theorem when I express it can be assented by other people (which is not the case for *my* idea). We don't say *my* Pythagorean theorem, but *the* Pythagorean theorem. An idea necessitates an owner, but not a thought. If thoughts were ideas, no common knowledge would be possible. No contradiction between the claims of different people would be possible." (1918-9, p. 363)

Moreover, it should be recognized that thoughts are actual, for they have a reciprocal action (1918-9, p. 371). This brings Frege to this uncommon conclusion: "A third realm must be recognized. Anything belonging to this realm has it in common with ideas that it cannot be perceived by the senses, but has it in common with things that it does not need an owner so as to belong to the contents of his can substitute a part of the sentence by a co-referential non-synonym expression without changing the reference of the sentence. However, its sense would have change, since one could believe one of these proposition to be true and the other to be false. Therefore, the thought cannot be the reference of the proposition, but must be rather considered as its sense (1892, p. 62).

In this case, what is the reference of the proposition? In the same "epistemically minded way", Frege remarks that our concern for the reference of a part of the sentence is an indication that we generally recognize and expect the sentence itself to have a reference. Once we recognize that one of its part has no reference, *the thought loses its epistemic value*. This is why we are justified in looking not only at the sense of a proposition, but also at its reference. Hence, if we ask that all proper names in a proposition should have not only a sense, but also a reference, this is because it "is the striving for truth that drives us always to advance from the sense to the reference" (1892, p. 63). This is why Frege has been led to accept the truth-value of a proposition as its reference[18]. *The mere thought yields no knowledge; only a thought together with its reference – a truth value – has an epistemic value.* This is why the basis of Frege's system are not concepts, but judgements: "Judgements can be regarded as advances from a thought to a truth value." (1892, p. 65) Since science should be designed according to its epistemic role, only judgements can form its basis.

5.2.2 Function and Object

In the subsection 5.2.1, we have seen that it is fundamental to Frege's logico-mathematical enterprise to distinguish an expression, its sense and its reference. This distinction is also fundamental when we examine what is a function; one cannot simply say that a function of x is an expression in which 'x' occurs, for the difference "of sign cannot by itself be a sufficient ground for difference of the thing signified" (1891, p. 22). Therefore, we should distinguish a function and the expression of a function, as we should distinguish a number and its expression (a numeral). Thus, if we consider the expressions

consciousness." (1918-9, p. 363)

It has been suggested that entities similar to these thoughts, from the logical point of view, could be considered as nothing more than equipollence classes (Couture and Lambek, 1991; Tarski, 1946; Russell, 1914). Or course, in this case, Frege's point has nothing to do with a strong acceptation of Platonism.

[18] This has the consequence that truth-values will be considered to be objects. Moreover, propositions will be considered as proper names of truth-values, in the same way that '4' is the proper name of 4.

'2', '1 + 1', '3 − 1', '6 : 3', we observe that they have the same reference, i.e that they stand for the same thing. The fact that the first expression is a numeral, the second an addition, the third a substraction and the fourth a quotient only points to the fact that their sense (mode of presentation) is different.

However, the expression 'function' is usually not meant to indicate a definite reference (as '3' or 'π' would), but rather to indicate indeterminately a number by the means of a letter (variable), e.g. 'x^2+x-6', as opposed to '$2-6$' that designates determinately the number -4. In such a functional expression, 'x' is said to be the *argument-place*. 'x' only indicates "the the kind of supplementation that is needed; it enables one to recognize the places where the sign for the argument must go in" (1891, p. 25). Thus, it would be possible to replace the occurrence of 'x' by blanks, e.g. '$(\quad)^2 + (\quad) - 6$'.

As we have seen in 5.2.1, a proposition expresses a thought. In the linguistic counterpart of a thought, we distinguish two parts of different nature: the first constituent is a proper name (it designates an object, a whole not requiring completion), and the second is the predicative component. The latter is always in need of completion and does not refer to an object. The first is said to be a *saturated* expression while the second is *unsaturated*. Frege considers that this fundamental distinction is not merely linguistic[19], that to the linguistic aspect there corresponds one in the realm of references. In this way, *to the proper name there corresponds an object* and *to the predicative part there corresponds a function* (1903, p. 33). In other words, the function is only a part of a thought, that need to be completed by a proper argument (object). To see this, we can consider the relation of identity that we already studied in 5.2.1: "The functional sign cannot occur on one side of an equation by itself, but only when completed by a sign that designates or indicates a number." (1904, p. 114)

Frege considers the distinction saturated/unsaturated to be logically primitive and properly undefinable; all that can be done is "showing" what it is about:

> 'Complete' and 'unsaturated' are of course only figures of speech; but all that I wish or am able to do here is to give hints. (1893, p. 55)
>
> This is not supposed to be a definition; for the decomposition into a saturated and an unsaturated part must be considered a logically primi-

[19] "If a distinction is grounded only in the nature of our language, this is not a properly logical one." (1903, p. 34)

> tive phenomenon which must simply be accepted and cannot be reduced to something simpler. (1903, p. 33)

Another way of indicating what is meant is by noting that an object cannot adhere to another object and, for this reason, *can never occur predicatively* or *unsaturatedly*. On the other hand, *a concept can never substitute for an object* (1903, p. 34). Thus, we see that the argument is an object, a whole complete in itself. Contrariwise, the function is not a complete whole; it is incomplete, in need of supplementation, or 'unsaturated'. In this respect functions differ fundamentally from numbers (1891, p. 24). However, when we complete the function, we obtain a complete whole, something saturated. Linguistically, for each proper name (argument or object name) replacing 'x' in the functional expression, we obtain a new proper name. Correspondingly, for each object filling a one-argument function we obtain a new reference. Now, we can have functions still more unsaturated, that are doubly in need for completion (e.g. "$(\xi + \zeta)^2 + \xi$") (1893, p. 73). When we saturate this function once – i.e. we complete one argument-place –, we obtain a function of one argument. In the same way, if we complete such a function twice, we then have an object (reference), which is the value of the two-place function. Of course, following the same pattern, it would be possible to obtain different many-place functions. However, references obtained from the saturation of a function are not the function. Rather the function is the connection between the arguments that substitute for 'x' and the references of the function for these arguments.

Let us make Frege's terminology clear: 1- 'x' *holds the place of a proper name* (e.g. a numeral) that is to *complete the expression*; 2- an argument is said to be *fitting* or *not fitting* an argument-place according to its saturatedness or unsaturatedness 3- these occurrences of 'x' are *argument-places* that stand for arguments of the function; 4- *what the expression becomes on completion is the proper name of the value of the function for this argument*; 5- for a given argument that completes a function, we obtain the value of the function for that argument. For example, "$(2 + 3 \cdot 1^2) \cdot 1$" is a name of the number 5, composed – for instance – of the function-name "$(2 + 3\xi^2)\xi$" and the proper name "1". The function $(2 + 3 \cdot \xi^2) \cdot \xi$ for the argument 1 has the value 5.

The value of the function is always different from the function – even when the function is constant – because the function expresses a general connection between the arguments and the values of the function. This connection can be represented: "The method of analytic geometry supplies us with a means of intuitively representing the values of a function for different arguments." (1891, p. 25) In such a representation, any point on the curve corresponds to a pair formed by an argument together with the associated value of the function. Frege calls this a *course-of-values*. Now, two functions can have the same course-of-values. However, the expression "$x^2 - 4x = x(x-4)$" would be ambiguous without further clarifications: are we expressing an equality of function or an equality of course-of-values? If we understand this equation as an equality of course-of-values, it means that whatever argument may be substituted for x, the reference of the complete expression on each side of '$=$' is to be the same. We can also say: " 'the value-range of the function $x(x-4)$ is equal to that of the function $x^2 - 4x$', and here we have an equality between ranges of values" (1891, p. 26)[20]. To avoid all ambiguity, Frege introduces a new notation for courses-of-values: if a sign is to stand for the course-of-value of a function, then we replace the variable by a Greek vowel and we prefix it with a smooth breathing; thus we obtain the expression '$\acute{\epsilon}(\epsilon^2 - 4\epsilon)$' for the course-of-value of the function '$x^2 - 4x$'. The fact that such equality between courses-of-value can hold is, for Frege, another primitive logical phenomenon:

> The possibility of regarding the equality holding generally between values of functions as a [particular] equality, viz. an equality between ranges of values, is, I think, indemonstrable; it must be taken to be a fundamental law of logic. (1891, p.26)

Finally, we should add that some functions can take functions as arguments. These are called second-level functions. In the same way, we can have third-level functions that takes second-level functions as argument. With these notions explained, I will quote extensively Frege where he summarizes his theory of types:

> We see [...] the great multiplicity of functions. We see, too, that there are basically different types of functions, since the various argument-places are basically different. Those argument-places, in fact, that are appropriate for admission of proper names, cannot admit names of functions, and vice versa. Further, those argument-places that may admit names of first-level functions of one argument, are unsuited to admit names of first-level functions of two arguments. Accordingly, we distinguish:
>
> - *arguments of type 1:* objects;

[20]'Value-range' and 'course-of-value' are here considered to be synonyms.

- *arguments of type 2:* first-level functions of one argument;
- *arguments of type 3:* first-level functions of two arguments.

In the same way we distinguish:

- *argument-places of type 1*, which are appropriate to admit proper names;
- *argument-places of type 2*, which are appropriate to admit names of first-level functions of one argument;
- *argument-places of type 3*, which are appropriate to admit names of first-level functions of two arguments.

(1893, pp. 77-8)

5.2.3 Extended Use of 'Function' and 'Object'. Concepts, Relations, Truth-values

Through the development of science, the reference of the expression 'function' has been extended. This extension took place in two ways: 1- new functions have been introduced and 2- the field of possible arguments have been extended (1891, p. 28). When an extension of the second kind occurred, the sense of previously used functions had to be changed in order that they can be defined for new domains of arguments. Frege extended still further in extending the reference of 'function':

> In both directions I go still further. I begin by adding to the signs $+, -$, etc., which serve for constructing a functional expression, also signs such as $=, >, <$, so that I can speak, e.g., of the function $x^2 = 1$, where x takes the place of the argument as before. (1891, p. 28)

But if we talk of these expressions as being functions, we should find what is their value. Let us consider the expressions '$(-1)^2 = 1$', '$0^2 = 1$', '$1^2 = 1$' and '$2^2 = 1$'. The first and the third are true whereas the second and the fourth are false. According to what has been said about propositions in 5.2.1, Frege says: " 'the value of our function is a truth-value' and distinguish between truth-values of what is true and what is false." (1891, p. 28) As long as they are propositions, they make a judgement on a thought (i.e. they assert). They make different assertions, but nevertheless stand for the same value. For this reason, Frege says that these *propositions are names of truth-values.*

Now, one may ask what is the point of admitting the signs '=', '>' and '<' as functional signs. Frege's answer is clear: "I too am of [the opinion that arithmetic is a further development of logic], and I base upon it the requirement that the symbolic language of arithmetic must be expanded into a logical symbolism." (1891, p. 30) If we consider an expression like '$x^2 = 1$', we admit that it is either true or false for any numerical argument (e.g. -1). This means that we can also use the logical expressions 'the number -1 has the property that its square is 1' or '-1 falls under the concept *square root of 1*'. This is on the basis of such considerations that Frege is brought to claim that concepts are nothing but particular cases of the extended acceptation of 'function':

> We thus see how closely that which is called a concept in logic is connected with what we call a function. Indeed, we may say at once: a concept is a function whose value is always a truth-value. (1891, p. 30)

More precisely, we say that *a concept is a one-place function whose value is always a truth-value*. Hence, $\Phi(\xi) = \Psi(\xi)$ is a concept. If we consider this equality as holding between the courses-of-values of the functions, we can paraphrase with this logical expression: the concept $\Phi(\xi)$ has the same extension as the concept $\Psi(\xi)$. For functions whose value is always a truth-value (concepts), one can say instead of "the course-of-values of the function" the expression "extension of the concept".

Now, Frege not only extends 'function' by admitting '=', '>' and '<', but also by admitting (assertive) propositions in general. A proposition contains a thought as its sense; and this thought is either true or false. This truth-value is regarded as the reference of the proposition (cf. 5.2.1). From this, Frege draws two conclusions:

1. "[Propositions] in general, just like equations or inequalities or expressions in Analysis, can be imagined to be split up into two parts: one complete in itself, and the other in need of supplementation, or 'unsaturated'." (1891, p. 31)
2. "we must go further and admit objects without restriction as values of functions." (1891, p. 31)

We have seen that Frege considers that the distinction function/object is undefinable. But with all these extensions of 'function' and 'object', it becomes still harder to see what is an object and what is a function. Following Frege, an "object is anything that is not a function, so that an expression for it does not contain any empty place" (1891, p. 32). The distinction is thus exhaustive: for every conceivable thing, this should be either an object or a function. Is it complete in itself? It is an object. Is it incomplete? It is a function. From this, it follows that both truth-values and courses-of-values are to be admitted amongst objects. For example, $\acute{\epsilon}(\epsilon^2 = 1)$ may be argument of $(\quad) = \acute{\alpha}((\alpha + 1)^2 = 2(\alpha + 1))$. Also, we can have function that takes truth-value as argument, and we would call them 'truth-functions' (1891, p. 33).

Frege's conception of science imposes that all signs should actually refer. For this reason, Frege judges important to introduce a new kind of function:

> It is thus necessary to lay down rules from which it follows, e.g., what '$\odot + 1$' stands for, if '$\odot$' is to stand for the Sun. What rules we may lay down is a matter of comparative indifference; but it is essential that we should do so – that '$a + b$' should always have a reference, whatever signs for definite objects may be inserted in place of 'a' and 'b'. (1891, p. 33)

The function Frege introduces for this is called 'horizontal', and stipulate the following rule that returns a truth-value whatever argument it takes, thus providing us with a concept having definite reference for any argument:

$$——\,\xi = \begin{cases} \text{True} & \text{for } \xi = \text{True} \\ \text{False} & \text{for } \xi \neq \text{True} \end{cases} \tag{5.1}$$

When we consider one-place functions whose values are truth-values, i.e. concepts, this requirement is tantamount to say that it shall be determinate, for any object, whether it falls under the concept or not.[21] In other words: "[...] as regards concepts we have a requirement of sharp delimitation; if this were not satisfied it would be impossible to

[21] This long passage of van Heijenoort (1967b, p. 325) is worth being quoted extensively: "However, the opposition between *calculus ratiocinator* and *lingua characterica* goes much beyond the distinction between the propositional calculus and quantification theory. [...] In [Frege's] system the quantifiers binding individual variables range over all objects. As is well known, according to Frege, the ontological furniture of his universe divides into objects and functions. Boole has his universe class, and De Morgan his universe of discourse, denoted by '1'. But they have hardly any ontological import. They can be changed at will. [...] For Frege, it cannot be a question of changing universes. [...] His universe is *the* universe. Frege's universe consists of all that there is, and is fixed.

This conception has several important consequences for logic. One, for instance, is that functions (hence, as a special case, concepts) must be defined for all objects."

set forth logical laws about them" (1891, p. 33)[22].

Some two-argument functions have truth-values as values for any argument (e.g $\xi = \zeta$ and $\xi > \zeta$). Such functions are called 'relations'. Here is Frege's vocabulary for relations as it follows: the object Γ *stands to* the object Δ *in the relation* $\Psi(\xi, \zeta)$ if $\Psi(\Gamma, \Delta)$ is the True (1893, p. 72). As we have seen for concepts, relations have courses-of-values, and they can take as argument truth-values, courses-of-values, or any object at all.

5.2.4 Subordination and Subsumption. First- and Second-Level Concepts

In the subsections 5.2.2 and 5.2.3, we became acquainted with what Frege takes as the primitive and irreducible distinction between functions (unsaturated) and objects (saturated). This allowed us to see on what grounds he claims that concepts are particular cases of functions. We have also seen that a concept is always different from its extension ($a \neq \{a\}$). This allows one to talk about empty concept and to render logically the mathematical notion of set emptiness. But for concepts that are not empty, how are we to talk about their relation to their content?

To talk of the relations between concepts and objects, we need a definite terminology. We say that a concept *sursumes* an object or that an object *is subsumed by* or *falls under* a concept. Now, a concept is generally composed of component-concepts, which are themselves concepts. For example, the concept 'black silken cloth' is formed from the components 'black', 'silken' and 'cloth'. These components are said to be the *characteristic marks* of the concept. The characteristic marks of a concept should be distinguished from the *properties* of this concept; the concept 'black silken cloth' is neither black nor a cloth. We say that *the characteristic marks of a concept specify what properties a object must have to fall under this concept* (1903, p. 35). Of course, whether there exists an object with these properties is a question that remains. When

[22] "Frege compares a concept to an area and says that an area with vague boundaries cannot be called an area at all." (Wittgenstein, 1953, p. 29) This is a metaphorical way of expressing Frege's claim: "The requirement of the sharp delimitation of concepts thus carries along with it this requirement for functions in general that they must have a value for every argument." (Frege, 1891, p. 33)

This requirement is noteworthy because it is a way of saying that only a science accepting the principle of excluded-middle is acceptable.

we define a concept (i.e. when we give its characteristic marks), we are not giving the properties of this concept; we should never confuse the characteristic marks of a concept and the properties of an object. Now, if a concept is always composed of components which are themselves concepts, doesn't it result in a regress *ad infinitum*? Not in the case of Frege's theory, for he accepts that there are unanalyzable, simple and primitive concepts (1903, p. 59).

The relation between a concept and another concept is however different from the relation between a concept and an object. We can distinguish two cases. Firstly, we have a relation between concepts as in 'black silken cloth': 'black', 'silken' and 'cloth' are components of the concept 'black silken cloth' and are said to be *subordinated* to it. We therefore have a relation saying that *the characteristics of the subordinate concept* are also *characteristics of the superordinate concept*.

Secondly, we have a relation analogous to the relation of subsumption of an object under a concept. Concepts generally come with an indefinite article (i.e. 'all', 'some', 'many', ...) in the context of a proposition. Indefinite articles are logically rendered by quantifiers. Let us consider Frege's example: 'there is a square root of 4'. Instead of this, one can also say 'there is something which is a square root of 4' or also 'it is false that whatever a may be, a is not a square root of 4'. This proposition is not about a particular object (e.g. about 2 or -2, the particular square roots of 4); it is about the concept 'being square root of 4'. Contrary to the relation holding between a concept and an object, it is here impossible to distinguish a part which is the unsaturated concept and another which is the object, complete in itself. And thus we arrive at the expression 'there is something that', which contains the assertion proper. In this case, we are not talking of an object nor of the components of a concept; rather, something is asserted of a concept (1903, p. 35). When we observe the expression "there is something that ...", we remark that an object name can never be substituted for the the dots: "It is linguistically inappropriate and *nonsensical* to say "there is Africa" or "there is Charlemagne" (1903, p. 35). We thus have this result:

> The *there is something which*, therefore, is also unsaturated, but in a manner quite different from that of *is a prime number*. In the former case, the completion can occur only through a concept; in the latter, only through an object. (1903, p. 35)

In such a relation, something is predicated but it is not a first-level concept. As the relation of concepts like ‘being a square root of 4’ to ‘there is something which’ is analogous to the relation of an object to a concept of the first level, we say in this case that the first-level concept is *subsumed* or *falls within* the second-level concept. We can also have the same kind of relation between second- and third-level concepts[23]. However, Frege reminds us that the “distinction between first- and second-level concept is just as sharp as the distinction between objects and first-level concepts” (1900c, p. 5) and that, for this reason, the relations ‘falls under’ and ‘falls within’ are completely different.

As one could expect, first-level concepts have only first-level characteristics and second-level concepts have only second-level characteristics. *A concept may never be formed by a mixture of characteristics of different levels*: “This follows from the fact that the logical places for concepts are unsuitable for objects, and that the logical places for objects are unsuitable for concepts.” (1903, p. 36) When talking of the properties of a first-level concept, we thus talk about the properties allowing him to fall under a second-level concept having the required characteristic marks.

5.3 Three Kinds of Mathematical Propositions

We have already had an overview of the way Frege distinguishes mathematical propositions in the chapter 4. To understand more precisely his classification of mathematical propositions, two dichotomies that we already met will be useful: 1- epistemically valuable/not valuable and 2- derivable/underivable. From these, we obtain the following table:

	Epistemically valuable	Not Epistemically Valuable
Underivable	Axioms, Principles, Basic Laws, Rules of Inference	Definitions
Derivable	Theorems	None in the *Begriffsschrift*

In what follows, we will examine two of them carefully, namely definitions (5.3.1) and axioms (5.3.3). Frege also recognizes a third kind of proposition not fitting in this scheme, called elucidations, and we will also examine them (5.3.2).

[23]As Resnik (1974, p. 393) remarks, Frege conceives of quantifiers as higher-order predicates.

5.3.1 Definitions

Frege talks about what a definition should be in almost all of his paper. For him, it was the touchstone of his theoretical confrontation with the supporters of formal theories of logic and arithmetic: "It is absolutely essential for the rigor of mathematical investigations, not to blur the distinction between definitions and all other propositions." (1903, p. 25) In agreement with the conception we have hitherto seen, Frege considers that a definition is the stipulation of the meaning of a word or sign, i.e. that it contains a signs which hitherto has had no sense nor reference. Definitions are the only propositions which are allowed to contain unknown signs, i.e. signs whose reference is unknown. This is what makes the difference between definitions and all other propositions (1903, p. 23). In the *Basic Laws*, Frege specifies seven principles concerning what a definition should be in the *Begriffsschrift* (1893, p. 90-2)[24]:

1. Every name correctly formed from the defined names must have a denotation. Thus, it must always be possible to produce a name, compounded out of our primitive names, that is the same as it in meaning, and the latter must be *unambiguously determined* by the definitions.

2. It follows from this that the same thing may never be defined twice, because it would remain in doubt whether these definitions are consistent with one another.

3. The name defined must be simple; that is, it may not be composed of any familiar names or names that are yet to be defined; for otherwise it would remain in doubt whether the definitions of the names were consistent with one another.

4. A proper name formed from our primitive names or defined names always has a denotation, and we can use a simple sign not previously employed to form a definition; this will assert that the two proper names have the same same meaning. In this way, it becomes possible to replace this sign wherever it occurs by the name defined in the demonstrations.

For Frege, all propositions in science must have a reference (that is always mediated by a sense, since it gives the epistemic value to a proposition), and that reference should be *exactly determinate* because "a sign without determinate reference is a sign without

[24] The four first are interesting for us here; three are ignored since they are only technical.

reference" (1906a, p. 62). Now, this is precisely the role of definitions to associate a name (sign) with a reference. Definitions therefore set the reference of a name by stipulating that it should be the same as that of another name or expression, somewhat like a "shortcut". However, one can then ask again about the reference of the terms used to define, and we will have to give a definition of them, and so on. Obviously, such a process cannot go on indefinitely; there must be an end. Frege admits this willingly, as its Euclidean ideal of science prescribes it: "The danger of having to define *ad infinitum* arises if and only if one demands that everything be defined. But who forces us to do this?" (1906a, p. 58) Accordingly, his opinion is that we must admit logically primitive elements that are undefinable[25] (1906a, p. 59). These are expresses by the axioms (cf. 5.3.3).

From these considerations, we see that the "real importance of a definition lies in its logical construction out of primitive elements" (1906a, p. 61). When the stipulation made by the definition is accepted – i.e. when it follows the rules –, the defined sign becomes known; the definition then becomes an assertion whose truth is self-evident and it can be use in the system as a premise for inferences. Therefrom a definition formally play the same role as principles or axioms. However, if *definitions formally play the role of principles*, they are not at all principles: to be a principle, a greater epistemic value is required. Since no definition extends our knowledge, they cannot be principles. A definition creates what it talks about, i.e. a linguistic link. We thus have two opposite theoretical roles, i.e. those of asserting and stipulating; the first extends our knowledge and the second does not:

> Just as the geographer does not create a sea when he draws boundary lines and says: the part of the ocean's surface bounded by these lines I am going to call the Yellow Sea, so too the mathematician cannot really create anything by his defining. (1893, p. 11)

[25] We can also add this relevant comment: "Of course, definitions do presuppose knowledge of certain primitive elements and their signs. A definition correctly combines a group of these signs in such a way that the reference of this group is determined by the references of the signs used." (1906a, p. 60)

5.3.2 Elucidations

For Frege, there are only two ways of introducing new terms: by definition or by elucidation. We have seen what definitions are about. However, when definitions are well understood, there remains primitive terms which are not defined. These should be *explained or elucidated*: achieving this is the role of elucidations (or explanations). One cannot ask for elucidations the same standard of rigor as for definitions or inferences since they specify what is presupposed by the scientific language and can only be presented in a figurative fashion[26]. In other words, they are not as such parts of the system, but rather a preamble, a propædeutics. What enters here is not very clear, for nowhere Frege explains systematically what he means by 'elucidation'[27]. All we know is that what is impossible to do with definitions, i.e. to stop the regress *ad infinitum* in defining, is the burden of elucidations. Frege says that they serve "the purpose of mutual understanding among investigators, as well as of the communication of the science to others" (1906a, p. 59). To give a meaning to unprovable or undefinable terms, "we cannot do without a figurative mode of expression" (1906a, p. 59). However, the leniency accorded to these elucidations is harmless, since no conclusions are based on them.

The requirements for elucidations are not really detailed by Frege, at the exception of two remarks. Firstly, since Frege stated that their role is only pragmatic – mutual understanding –, we must be satisfied if they reach that goal (1906a, p. 59). Secondly, one can ask them to settle the question objectively – as much as this can be done in a preamble –, so that "it is merely on account of our incomplete knowledge of the object that we cannot answer the question". If it is the state of our knowledge that makes the elucidation miss the point, then the elucidation as such is not to blame. However, if the elucidation is such that the question would "remain unanswered no matter how complete our knowledge, then the explanation is faulty" (1906a, p. 63).

[26]Maybe this comment from Wittgenstein will shed some light on what Frege meant: "Imagine a picture representing a boxer in a particular stance. Now, this picture may be used to tell someone how he should stand, should hold himself; or how he should not hold himself; or how a particular man did stand in such-and-such a place; and so on. One might (using the language of chemistry) call this picture a proposition-radical. This will be how Frege thought the 'assumption'." (Wittgenstein, 1953, p. 9)

[27]"Perhaps ostension enters; we just don't know – Frege never talks about this. But it is unimportant here, for the distinction between definitions and explications stands even without this." (Kluge, 1971, p. xxviii) Also, Resnik (1974, p. 390) remarks: "Frege is not employing a very precise notion here, since in the appropriate content almost anything can count as an elucidation."

5.3.3 Axioms

According to Frege, it is undoubtedly the case that axioms contain real knowledge (1903, p. 27). Moreover, they are primitive and underivable propositions. When he gives this meaning to the word 'axiom', Frege considers that he is simply in continuation of the tradition:

> Traditionally, what is called an axiom is a thought whose truth is certain without, however, being provable by a chain of logical inferences. Logical laws, too, are of this nature. [...] Here we shall not go into the question of what might justify our taking these axioms to be true. In the case of geometrical ones, intuition is generally given as a source. (1903, p. 23)

Axioms are supposed to express the basics facts of our intuition, i.e. that they must express true thoughts that are primitive and undefinable. In this way, axioms are assertive while definitions are stipulative. This is why axioms do extend our knowledge – they have an epistemic value – while definitions do not. Now, we have seen that an assertion is the judgement of the truth of a thought. In order that such a thing be possible, a proposition must of course be complete, i.e. that it should refer and not be a mere pseudo-proposition. Thus, the fact that axioms extend our knowledge precludes the appearance of unknown terms, i.e. terms whose sense and reference are not determined. Therefore, only known terms – typically by elucidations – may occur in an axiom; an axiom can never settle the reference of a sign.

From this point of view, Frege cannot understand what is the point of formal theories of arithmetic and geometry, according to whom axioms do define primitives terms. According to him, they face this dilemma: 1- claiming that axioms define terms supposes that unknown terms occur in axioms, and in this case axioms cannot be assertions (judgements on the truth of thoughts) and they cannot extend our knowledge[28] or 2- axioms extend our knowledge, in which case they contain no unknown signs and cannot define anything (1903, p. 26). If one accepts the second alternative – which is suppose to be the traditional one –, axioms are the expression of basic facts of our intuition

[28] Moreover, if axioms are true by definition, then they cannot express facts of our intuition, since their validity would be precisely based on that intuition. The truth of a definition is founded on the fact that it is a definition, and not on intuition (1903, p. 27).

and therefore true[29], and from their truth if follows that they cannot contradict one another. It makes no sense to talk of contradiction amongst truths, and for this reason their consistence requires no proof at all (1903, p. 25).

5.4 More on Frege's Critique of Hilbert

In the chapter 4 we have seen that, according to Frege, Hilbert's use of the words "axiom", "definition" and "elucidation" among others is confusing. Now, the last subsection gave us a precise account of Frege's position on the subject matter in the light of his more general conception of science. In what follows, we will look with more scrutiny at three aspects of this critique, since we are now acquainted with Frege's conception.

5.4.1 The "Knowability" of Primitive Terms

Hilbertian axioms are said to define the primitive terms. This is so because it is impossible to define all terms in the traditional way (explicitly), for it would lead to a regress *ad infinitum*. Hilbert proceeds as if primitive terms were the unknowns in a system of equations. There is a certain number of unknowns and a certain number of equations, and the system is solvable for each unknown only if the number of equation is greater than or equal to the number of unknowns. This brings Frege to ask this question:

> [...] an axiom contains several unknown expressions such as "point,"

[29] Dummett (1991a, p. 25) has an interesting point: "Now Frege unwaveringly believed that any deductive proof must have a starting-point in the form of initial premisses. [...] If we can claim to know anything more than particular facts, therefore, if we know any general truths, we must know, without the need or possibility of proof, some fundamental general laws. [...] Frege believed all this because he consistently rejects the legitimacy of deriving a consequence from a mere supposition: all inferences must be from true premisses. This excludes the use of reasoning under a hypothesis to be discharged by a rule of inference such as *reductio ad absurdum*."

It is not before the works of Tarski (1956) and Gentzen (1969), thus it seems to Dummett, that this natural way of reasoning had been rehabilitated. Indeed, it seems to be Gentzen's intention: "My starting point is this: The formalization of logical deduction, especially as it has been developed by Frege, Russell, and Hilbert, is rather far removed from the forms of deduction used in practice in mathematical proofs. [...] In contrast, I intended first to set up a formal system which comes as close as possible to actual reasoning. The result was a '*calculus of natural deduction*'." (Gentzen, 1969, p. 68)

> "straight line," "plane," "lie," "between," etc.; so that only the totality of axioms, not single axioms or even groups of axioms, suffices for the determination of the unknowns. But does even the total suffice? Who says that this system is solvable for the unknowns, and that these are uniquely determined? If a solution were possible, what would it look like? Each of the expressions "point," "straight line," etc. would have to be explained separately in a proposition in which all other words are known. If such a solution of Hilbert's system of definitions and axioms were possible, it ought to be given [...]. (1903, p. 31)

Thus, if the system of definition that Hilbert proposes is designed in a way to avoid regress *ad infinitum*, there remains that *some terms are not uniquely determined*. Hilbert, Frege and Korselt (who defended Hilbert's foundations) would agree on that: "we cannot [...] demand that every system of principles be solvable for the unknowns (basic concepts) that occur in them [...]" (Korselt, 1903, p. 47) by means of explicit definitions only. For Hilbert, admitting this is tantamount to admit that it is a vain enterprise to try to determinate completely the meaning (either sense or reference) of a sign by the means of logic. However, for Frege, the implications is not the same since "no science is completely formal" (1906a, p. 109). A system that is to fulfill the requirement of logic cannot allow undefined terms to be introduced in the system.

However, it does not mean that all terms should be defined explicitly – with the unpleasant regress *ad infinitum* –, for Frege recognizes another way of introducing unknown terms: the elucidations. Now, the question Frege asks is: are Hilbert's definitions (or axioms) elucidations? If they were considered so, they would be blameworthy for two reasons. First, we have seen that elucidations should only be a preamble to a system, i.e. that they should not occupy a theoretical function in the system and that no conclusion should be based on them. According to Frege, this is not the way Hilbert uses them: "It is not intended that they belong to the propædeutic but rather that they serve as cornerstones of the science: as premises of inferences." (1906a, p. 60) Secondly, even if they were considered elucidations, Hilbert's definitions (axioms) would not be satisfying since they do not fulfill their pragmatic role. With elucidations such as those presented by Hilbert, no one know sufficiently well what is a point to "answer the question whether an object, for example my pocket watch, is a point [...]." (1903, p. 31)[30]

[30] "For Frege, the quantifiers of mathematics range over *everything*, and a concept is a function that takes *all* objects as arguments. Thus, 'my pocket watch is a point' must have a truth value, and our theory must determine this truth value." (Shapiro, 1996, p. 163)

Thus, when Hilbert affirms that there should remains unknown terms in a system, this "arises from unclear thinking and insufficient logical insight" (1906a, p. 66), from the ignorance of the principles of definition, elucidation and assertion[31]. To Frege, such a claim is unacceptable: "In no way is it necessary to have ambiguous signs, and consequently such ambiguity is quite unacceptable. What can be proved only by means of ambiguous signs cannot be proved at all." (1906a, p. 69) The confusion finds its source in another one between first- and second-level concepts.

5.4.2 Confusion of Concept Levels

According to Frege, it seems that Hilbert defines the concept of point twice: once as a first-level concept and a second time as a second-level one. When he talks about facts of our intuition, one thinks that he talks about points in the traditional Euclidean way. Now, if 'point' designate a first-level concepts, every single point is an object, as it is the case with Euclidean geometry. But the problem is that Hilbert's axioms do not state characteristics of the first level (e.g. they state existence, which is a second-level concept). So, they are not properties an object must have to be a point. The characteristics he provides for the concept 'point' are second-level characteristics and, for this reason, they can only be second-level concepts.

> No doubt the relationship of the Euclidean point-concept, which is of the first level, to Hilbert's concept, which is of the second-level, will then have to be expressed by saying that according to the convention we adopted above, the former falls within the latter. It is then conceivable – in fact probable – that this does not apply to the Euclidean point-concept alone. (1903, p. 36)

For Frege, it is irritating to use the name 'point' for concepts of different levels as Hilbert does, for obviously the name has different references in both cases. He compares this confusion between first-level characteristics and second-level ones to the proof of the existence of god. If it were allowed to define primitive terms with axioms as Hilbert does, a thing like that would be logically acceptable:

[31] "According to Frege, axioms should express *truths* and definitions should give the *meaning* and *fix the denotations* of certain terms. With an implicit definition *neither* job is accomplished." (Shapiro, 1996, p. 161)

Explanation Conceive of objects which we call gods.

Axiom 1 Every god is omnipotent.

Axiom 2 There is at least one god.

Any proposition that could be proof on the basis of such a model would be as valueless, from a logical point of view, as the ontological proof of the existence of god. This is why we should distinguish objects, first-level concepts and second-level concepts. Any mixture brings confusion and mislead science from its objectives. Therefore, even if it were allowed to Hilbert's axioms to define something, they would be reprehensible for the confusion between levels of concepts.

5.4.3 Impossibility of a Proof of Independence of Axioms

If logic is well understood, according to Frege's conception, every thought can be judged. A proposition expresses a thought, to which corresponds a truth-value. Moreover, no false propositions are acceptable in science: "Only true thoughts can be premises of inferences." (Frege, 1906a, p. 105) Keeping this is mind, in his series on the foundations of geometry (1903; 1906a), Frege gives a good account – in his relatively refined system – of how 'independent' and 'consistent' are to be understood. It can be summarized this way: a proposition is independent of a collection Ω of propositions if and only if it is not a consequence of the collection Ω, and a collection of propositions Υ is consistent if and only if no contradictory proposition is a consequence of it. However, "despite developing an extremely sophisticated apparatus for demonstrating that one claim is a consequence of others, Frege offers not a single demonstration that one claim is *not* a consequence of others" (Blanchette, 1996, p. 317). Moreover, these considerations seems to be only about derivable propositions, and not about axioms. Now, the question commanding attention is: is it by accident, or is it due to a constitutive feature of its system?

As it should now be clear to us, the controversy turns around this unassailable claim from Frege: Axioms are true and could not be otherwise. More precisely:

> Frege's endorsement of [this proposition] is meant to include the strong modality; what he meant was not just the truism that no false proposition

> could be an axiom, something that no one would dispute, least of all Hilbert, but rather that if a proposition is *genuinely* an axiom, then it is not even sensible to consider it to be other than true. (Antonelli and May, 2000, p. 244)

We will see in this subsection that it is a consequence of his conception of a scientific language, a conception resting on his doctrine of sense and reference[32]. The proposition in question – that axioms are true and could not be considered otherwise – may seem odd from our perspective, since metalogic with which we are acquainted has developed on the basis that they can. We thus ask, following Antonelli and May (2000), from what source of underlying assumptions does his stubborn attachment to this proposition follow, particularly the second clause?

It turns out that the answer is: *in a fundamental difference between Frege and Hilbert in what they understood a language to be.* Frege has a fixed conception of what science should be, and his conception of a perfect language is fundamentally permeated with epistemic considerations. His basic notion is that of sign, which is a symbol expressing a sense (and not a mere concatenation). A language can be considered a system of signs, i.e. a system of symbols to which senses are attached (for a language, this pairing is not arbitrary[33]). Changing the signs (i.e. changing the pairing symbol-sense) is tantamount to a change of language.[34]

As we know from the subsection 5.2.1, Frege applies exactly the same analysis to propositions and to signs. A proposition is a pair constituted of a symbol (it is a concatenation, a sentence) and a sense (in this case, a thought). In a well constituted language like the *Begriffsschrift*, all signs and propositions are pairwise disjoints (there

[32] The following argument is in part due to the excellent paper of Antonelli and May (2000).

[33] Cf. 5.2.1, the second argument.

[34] It must be kept in mind that such a change of language, in the case concerning us here, means that we will no longer work in the *Begriffsshcrift*. Certainly, Frege would accept that there is possible synonymy or "almost-synonymy", but this would be only for elucidations (the preamble). And, when Frege argues with Hilbert, it is clear that the arguments he proposes concern what takes place *in* the system. van Heijenoort (1967b, p. 326) remarks: "Another important consequence of the universality of logic is that nothing can be, or has to be, said outside the system. And, in fact, Frege never raises any metasystematic question (consistency, independence of axioms, completeness)."

For the reasons developed in this subsection, it seems to us that Resnik (1980, p. 80) makes a claim completely untenable: "Thus, Frege, who in 1904 had the object language and metalanguage distinction more firmly in mind than Hilbert did, narrowly missed formulating the consistency problem." Second-level concept and concept in a metalanguage should not be confused! Explicitly defining a second-order concept to precisely fix its characteristics and implicitly defining a formal system in order to proceed to a metasystematic analysis are two things completely different by their methods and their presuppositions.

is no ambiguity). Also, the language is designed in a way that any sign or proposition will also refer[35]; if it were not the case, epistemic value would be lost.

To characterize the contrast with the model-theoretic way of proceeding, we say that Frege's language is *interpreted*[36]. In model-theory, we have an uninterpreted language to which we *assign* an interpretation. Such a procedure is unacceptable for Frege: since a language is a system of signs meant to have an epistemic value, it is always *interpreted*, inherently interpreted.

> To Frege, it would be a non sequitur to speak of an interpreted language in the sense of a interpretation being *assigned* to a language, for this presupposes something Frege rejects, that a language itself is a system of meaningless marks or symbols. (Antonelli and May, 2000, p. 246)

This is a direct consequence of his philosophy of language according to which sense determines reference. Moreover, since sense uniquely determine reference, the system is uniquely interpreted; in this way, changing the system of signs would not be just a different interpretation for the language but a different language altogether: "With respect to a particular language, it is just not possible for a sign to have a reference different than what it has." (Antonelli and May, 2000, p. 246)

With this in mind, we ask again: can it be possible to do metatheory concerning axioms in Frege's system? Axioms are propositions and, moreover, they are underivable. As any other proposition, axioms comes with an indissociable truth-value. A proof, according to Frege, is a sequence of propositions showing how true thoughts follow from true thoughts. Now, considering the possibility that an axiom be otherwise than true would be considering that its reference is other than the true. In this case, this axiom would necessarily expresses a different thought, and be a different proposition. But this new proposition would not be an axiom, since its reference would be the false. Therefore, it is not even possible to consider, according to Frege, that an axiom may be other than true. His approach to logic is completely alien to metatheory[37]:

[35] Frege (1893, p. 87-9) explains that all primitives terms of the *Begriffsschrift* do refer. Other terms, if they are properly defined or derived, will therefore refer too.

[36] About this, we note this comment from Frege (1906a, p. 16): "The word interpretation is objectionable, for when properly expressed, a thought leaves no room for different interpretation."

[37] At least, in the model-theoretic way we generally understand this word today.

> Rather, the lack of metatheory in Frege is to be traced back to the assumption, at the heart of his view of logic, that logic is the *universal* system of reasoning; it is something that is reasoned *in*, not *about*. (Antonelli and May, 2000, p. 243)

5.5 A Sketch of the *Begriffsschrift*

We have seen in subsection 5.1.4 that Frege claims of his *Begriffsschrift* that it is both a *lingua characterica* and a *calculus ratiocinator*, the former being in view of the expression of a content, and the latter in view of the calculus of inferences. In this section, I will sketchily present the way Frege proceeds for the actual construction of his system. To achieve this, I will emphasize the "*lingua characterica*" aspect, i.e. the way he pretends to express perspicuously logical and mathematical contents. I will not develop the "*calculus ratiocinator*" aspect, because it does not help that much to see what opposes Frege to Hilbert.

5.5.1 Expressing a Content: Name Forming

When one reads the *Basic Laws*, he cannot help wondering why Frege spends so much space describing the way he constructs the symbols in his system. This is so because he wants to construct a perfect language, i.e. a language that is perspicuous, conotationless and unambiguous. To reach this goal, he has to establish a procedure that will produces one and only one sign for each thing that has to be expressed and, conversely, one and only one thing expressed for any sign. In this way, we can find how to express perspicuously and unambiguously whatever we want and, moreover, find what was to be expressed for each sign. The two ways are possible and readily accomplished.

A typical example of this procedure is the method of numbering introduced by Gödel (Gödel, 1931, p. 45). His way of proceeding is to associate to each primitive sign, formula or proof expressible in a language a definite whole number. This association is made in order that the association will be one-to-one. On the basis of the unique factorization theorem[38], he designed a rule allowing him to produce a unique number

[38] Every natural number greater than 1 either is a prime number or can be written as a product of prime numbers. For example, $1981 = 7 \cdot 283$, $1982 = 2 \cdot 991$, $1983 = 3 \cdot 661$, $1984 = 2^6 \cdot 31$.

for each expression of the language. We will say that the association is "perspicuous", since it allows one to go the way back; for any whole number, the rule allows one to say whether it is a number that represents an expression of our language and, if that is the case, whether it is the number of a primitive sign, a formula or a proof, i.e. at which type of expression it is associated. Moreover, thanks to the property of unique factorization, it even allow to find back exactly which expression is associated with the given number. Thus, this is a genuine one-to-one association that is unambiguous and perspicuous.

However, the analogy is somewhat limited by the fact that Frege, contrary to Gödel, does not map a language into another one. Rather, he has in mind the association of linguistic elements (expressions) with non-linguistic ones, that characterizes logically the things composing the realm of reference under study. Thus, what is to be expressed is the content of the theory of types that we reviewed informally in the subsection 5.2.2. In what follows, I will introduce what is relevant in view of the subsection 5.5.4 and 5.5.5, where I will present a fragment of a Fregean-style geometry.

5.5.2 Primitive Signs

The elucidations using a figurative way of expression provided in the section 5.2 are such that the primitive logical phenomenon on which the *Begriffsschrift* is based are supposed to be known to us. Now, we will see how to express them.

The functions are unsaturated, i.e. that their expression should contain argument places that only indicate indeterminately an object. Frege marks these argument places by the lowercase Greek letters 'ξ' and 'ζ'. In this way, from the expression '$(2+3\cdot 1^2)\cdot 1$', we can form the one-place function-name '$(2+3\cdot\xi^2)\cdot\xi$' and the two-place function '$(\zeta+3\cdot\xi^2)\cdot\xi$'. We can also use the uppercase Greek letters 'Φ' and 'Ψ' to indeterminately indicate functions; we may thus form the one-place function-name '$\Phi(\xi)$' and the two-place one '$\Psi(\xi,\zeta)$'. The objects that will complete these functions will be denoted by the uppercase Greek letters 'Γ' and 'Δ'.

The formation of a proposition along the *Begriffsschrift*'s lines implies writing the name of a truth-value to which we add an assertorical force (it is to be remembered that Frege's logic is constructed on judgments). To mark the assertive force, Frege uses the symbol '$\vdash$'; this symbol will be at the beginning of all propositions of the *Begriffsschrift*. The formation of a name of a truth-value is realized by the use of the function-sign '$\text{——}\,\xi$' that we already introduced in 5.2.3. Everything on the right of the horizontal line is the argument of the function. If 'ξ' stands for the True, the value of this function is the True; otherwise it is the False. We can amalgamate the signs in order that the proposition-name '$\vdash(\text{——}\,\xi)$' becomes '$\vdash\!\text{——}\,\xi$'.

We now introduce our truth-functions: negation, implication. On the horizontal, we can add a negation-stroke in this way: $\text{—}\!\!\top\!\!\text{—}\,\xi$. The negation should be understood as follows: the proposition $\text{—}\!\!\top\!\!\text{—}\,\xi$ will be the True for each argument at which $\text{——}\,\xi$ stands for the False, and the False when it stands for the True[39]. We also have the condition-stroke that allow us to form conditional propositions. Where ξ is the antecedent and ζ the consequent, we write the sign '$\begin{array}{l}\text{—}\!\!\!\!\!\!\!\!\;\text{——}\,\zeta\\ \llcorner\text{——}\,\xi\end{array}$'. We stipulate that this sign stands for the False if $\text{——}\,\xi$ is the True and $\text{——}\,\zeta$ is the False; its value is the True in the other cases. Conjunction (both ξ and ζ) can thus be expressed this way: $\begin{array}{l}\text{—}\!\!\top\!\!\text{——}\!\!\top\!\!\text{—}\,\zeta\\ \quad\llcorner\text{———}\,\xi\end{array}$.

In the same way, inclusive disjunction (either ξ or ζ) is thus expressed: $\begin{array}{l}\text{———}\,\zeta\\ \llcorner\text{——}\!\!\top\!\!\text{—}\,\xi\end{array}$. Introducing the sign '$\top$' for the True and '$\neg\top$' for any object but the True (including the False), we thus obtain the following truth-table[40]:

ξ	ζ	$\text{——}\,\xi$	$\text{—}\!\!\top\!\!\text{—}\,\xi$	$\begin{array}{l}\text{——}\,\zeta\\ \llcorner\text{—}\,\xi\end{array}$	$\begin{array}{l}\text{—}\!\!\top\!\!\text{——}\!\!\top\!\!\text{—}\,\zeta\\ \llcorner\text{—}\,\xi\end{array}$	$\begin{array}{l}\text{——}\,\zeta\\ \llcorner\text{—}\!\!\top\!\!\text{—}\,\xi\end{array}$
$\top$	$\top$	$\top$	$\neg\top$	$\top$	$\top$	$\top$
$\top$	$\neg\top$	$\top$	$\neg\top$	$\neg\top$	$\neg\top$	$\top$
$\neg\top$	$\top$	$\neg\top$	$\top$	$\top$	$\neg\top$	$\top$
$\neg\top$	$\neg\top$	$\neg\top$	$\top$	$\top$	$\neg\top$	$\neg\top$

A rigorous notation is also needed for quantifiers. To express that a function $\Phi(\xi)$

[39]We remember the reader that $\text{——}\,\xi$ is the True when ξ is the True, and False in the other cases (e.g. $\xi = 2$).

[40]Frege did not define these functions by the truth-table method. However, for heuristic considerations, it is certainly useful to introduce it.

holds generally (universally), we replace the argument-letters by lowercase German letters and prefix that letter in a concavity in a way that we obtain the sign '$\overset{\mathfrak{a}}{\smile}\!-\,\Phi(\mathfrak{a})$'. The concavity is the sign for universality, and the German letter in this concavity is the range of the quantifier, i.e. that for any function $\overset{\mathfrak{a}}{\smile}\!-\,\xi$, the quantifier ranges over all occurrences of $\mathfrak{a}$ in ξ. In the same way, we write that it is not the case that $\Phi(\xi)$ holds generally by the use of the sign '$\top\overset{\mathfrak{a}}{\smile}\!-\,\Phi(\mathfrak{a})$'. Also, we express that it is generally the case that $\Phi(\xi)$ does not hold by the sign '$\overset{\mathfrak{a}}{\smile}\!\top\,\Phi(\mathfrak{a})$'. Finally, we express that it is not the case that $\Phi(\xi)$ never holds by the expression '$\top\overset{\mathfrak{a}}{\smile}\!\top\,\Phi(\mathfrak{a})$'; this is tantamount to say that there are some cases where $\Phi(\xi)$ holds.

In the subsection 5.2.3, we have introduced the notation $\acute{\epsilon}\Phi(\epsilon)$ for the course-of-value of the function $\Phi(\xi)$. The expressions '$\overset{\mathfrak{a}}{\smile}\!-\,\Phi(\mathfrak{a}) = \Psi(\mathfrak{a})$' and '$\acute{\epsilon}\Phi(\epsilon) = \acute{\alpha}\Psi(\alpha)$' always have the same value.

According to Frege, a formal substitute for the definite article is needed and expressed by the function-sign '$\backslash\xi$'. If $\acute{\epsilon}(\Delta = \epsilon)$ would be generally equal to Δ, then $\acute{\epsilon}\Phi(\epsilon)$ would be an adequate substitute for the definite article. If we assume that $\Phi(\xi)$ is a concept under which falls the object Δ and only Δ, then the value of $\overset{\mathfrak{a}}{\smile}\!-\,\Phi(\mathfrak{a}) = (\Delta = \mathfrak{a})$ would be the True and, consequently, the value of $\acute{\epsilon}\Phi(\epsilon) = \acute{\epsilon}(\Delta = \epsilon)$ would also be the True; since we assumed the equality of $\acute{\epsilon}(\Delta = \epsilon)$ with Δ, then $\acute{\epsilon}\Phi(\epsilon)$ would equal Δ. In other words, if we assume a concept $\Phi(\xi)$ under which falls one and only one object, then $\acute{\epsilon}\Phi(\epsilon)$ would refer to that object. Now, this assumption does not generally holds, and a function that is always defined is needed (cf. 5.2.3). We thus stipulate this function:

$$\backslash\xi = \begin{cases} \Delta & \text{if to } \xi \text{ there corresponds an object such that } \acute{\epsilon}(\Delta = \epsilon) \\ \xi & \text{in other cases} \end{cases} \tag{5.2}$$

In this way, $\backslash\acute{\epsilon}(\Delta = \epsilon) = \Delta$ is True and $\backslash\acute{\epsilon}\Phi(\epsilon)$ denotes the object falling under the concept $\Phi(\xi)$ if there is only one such object; in the other cases, where there are many objects falling under $\Phi(\xi)$, $\backslash\acute{\epsilon}\Phi(\epsilon)$ takes the value $\acute{\epsilon}\Phi(\epsilon)$. With the use of this function, we are thus able to speak of '*the* positive square root of 4' or of '*the* point through which only one parallel line passes'.

5.5.3 Expression of the Basic Laws

Now that the logically primitive phenomenon have been explained and that we are acquainted with the expression that Frege gives them, we can lay down the Basic Laws that form the core of his logic. The laws I, IV and VI needs no further explanations; they are True by the definitions of the truth-functions, i.e. truth-functionally true:

$$\vdash\!\!\begin{array}{l} \text{——}\ \Gamma \\ \quad \llcorner\ \Delta \\ \llcorner\ \Gamma \end{array} \tag{5.3}$$

$$\vdash\!\!\begin{array}{l} \text{——}\ (\text{——}\ \Delta) = (\text{——}\ \Gamma) \\ \llcorner_{\top}\ (\text{——}\ \Delta) = (\text{—}_{\top}\text{—}\ \Gamma) \end{array} \tag{5.4}$$

$$\vdash\text{——}\ \Delta = \backslash\acute{\epsilon}(\Delta = \epsilon) \tag{5.5}$$

The following law (IIa) says that if a function holds for any argument, then we can substitute at each argument-place any object (of course, respecting the argument-places)[41]:

$$\vdash\!\!\begin{array}{l} \text{——}\ \Phi(\Delta) \\ \llcorner\!\smile^{\mathfrak{a}}\!\text{—}\ \Phi(\mathfrak{a}) \end{array} \tag{5.6}$$

We have hitherto seen expressions of generality only for the argument. However, it is also possible to hold generally for functions. The law III is such[42]:

$$\vdash\!\!\begin{array}{l} \text{——}\ \Phi\left(\smile^{\mathfrak{f}}\!\!\begin{array}{l} \text{—}\ \mathfrak{f}(\Delta) \\ \llcorner\ \mathfrak{f}(\Gamma) \end{array} \right)^{43} \\ \llcorner\ \Phi(\Delta = \Gamma) \end{array} \tag{5.7}$$

The law V expresses that an identity of courses-of-value may be transformed into a generality of identity:

$$\vdash\text{——}\ (\acute{\epsilon}\Phi(\epsilon) = \acute{\alpha}\Psi(\alpha)) = (\smile^{\mathfrak{a}}\!\text{—}\ \Phi(\mathfrak{a}) = \Psi(\mathfrak{a})) \tag{5.8}$$

[41] It seems to be a non-constructive version of the axiom of choice.

[42] I will use lowercase German letters for quantified functions, for typesetting reasons.

[43] This holds because if the argument of the antecedent is True, the argument of the consequent is also True.

5.5.4 Definition of a Function out of Primitive Terms

On the basis of the primitive expressions hitherto introduced, we can define new names. Of course, the principles of definition established in the subsection 5.3.1 should be respected. If a definition is properly constructed out of primitive terms and announced by the sign ‘$=_{df}$’[44], then it is a proposition of the *Begriffsschrift*. We will present here the two-place function ‘$\xi \frown \zeta$’ – that is the counterpart of the set-theoretic relation of membership[45] – because it will be important in the sequel.

We have seen that it is possible to represent a function by its course-of-value. A function $\Phi(\xi)$ for the argument Δ has the value $\Phi(\Delta)$. We can thus also represent this value by means of Δ and $\acute{\epsilon}\Phi(\epsilon)$. It is stipulated that $\Phi(\Delta)$ will mean the same as

$$\Delta \frown \acute{\epsilon}\Phi(\epsilon) \tag{5.9}$$

$\Phi(\Delta)$ is thus the value of the function $\xi \frown \zeta$ for Δ as ξ-argument and $\acute{\epsilon}\Phi(\epsilon)$ as ζ-argument. Now, we should define the function for any possible argument:

$$\backslash\acute{\alpha}\left(\neg\forall\mathfrak{g}\,\neg\left(\Delta = \acute{\epsilon}\mathfrak{g}(\epsilon) \rightarrow \mathfrak{g}(\Gamma) = \alpha\right) \right) =_{df} \Gamma \frown \Delta \tag{5.10}$$

Two cases must be distinguished in order that $\xi \frown \zeta$ be completely determined: if the ζ-argument is a course-of-values, then the value of $\xi \frown \zeta$ is the value of the function whose course-of-values is the ζ-argument for the given ξ-argument. In other cases it is the False. Said otherwise, if the ζ-argument is a course-of-values, the value of the function will be the True or the False, according to the fact that the ξ-argument falls or not in the extension in question; and if the ζ-argument is not a course-of-value, then the function has the value False.

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[44] Frege uses ‘⊩’ instead.

[45] Russell (1903, p. 512) confirms the point: “By means of variable propositional functions, Frege obtains a definition of the relation which Peano calls $\in$, namely the relation of a term to a class of which it is a member.” By ‘variable propositional function’, Russell means that the proposition is second-orderly quantified by $\smile^{\mathfrak{g}}$.

According to Dummett (1991a, p. 217), this is precisely this kind of use of quantifiers that is at the origin of the contradiction Russell have found in Frege’s system: “The second-order quantifier presents an altogether different problem; and it is to its presence in Frege’s formal language that the contradiction is due. It was indispensable for Frege’s purposes, since it was only by means of it that he could define his application operator $\frown$, $a \frown g$ being the value for the argument a of the function whose value-range is g; when g is a class, $a \frown g$ is the truth-value of ‘a is a member of g’.”

Moreover, Dummett (1991a, p. 218) remarks that “His amazing insouciance concerning the second-order quantifiers was the primary reason for his falling into inconsistency.”

This is in such a way that Frege defines many functions by a construction out of primitives terms.

5.5.5 A Fragment of a Fregean-style Geometry

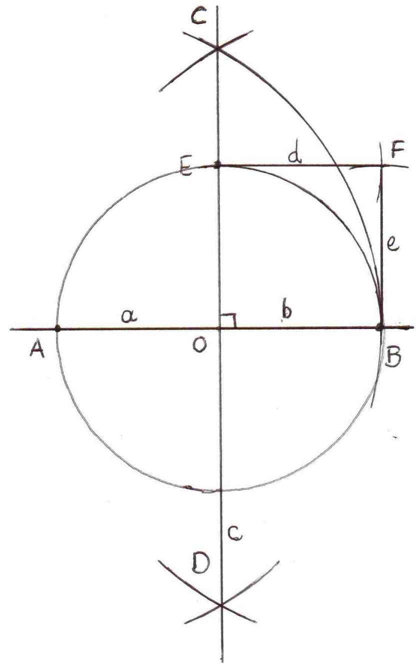
Frege wrote a lot through his life *on* the foundations of geometry, but he never published an actual realization of what should have been done. From what have hitherto been said, the kind of work having to be done in order to produce a Fregean-style foundation for geometry is however now supposed to be relatively clear. In what follows, I will do a brief Fregean-style presentation of a fragment of Euclidean geometry, but only in order to illustrate what he meant. To achieve this, I will not use the traditional five axioms of geometry; Frege himself would certainly not deny that Euclid himself had some lack of rigor in his axiomatization of geometry. Rather a modified version of the axioms from analytic geometry would be better adapted[46].

To understand the axioms of incidence of plane geometry, two basic geometrical concepts are needed: *Point* and the *Line*. The objects falling under the concept *Point* are called *points* and the objects falling under the concept *Line* are called *lines*. These concepts are primitive and can therefore not be defined in the *Begriffsschrift*, the language in which our axiomatic system is to be expressed. In the *Begriffsschrift*, points are denoted by uppercase latin letters $A, B, C, \ldots$; lines are denoted by lowercase latin letters $a, b, c, \ldots$ The concept *x is a point* will be denoted by $\Pi(\xi)$, whereas the concept *x is a line* will be denoted by $\Lambda(\xi)$. We will grasp the sense of these primitives by considering the following elucidations.

Let us consider the figure 5.1 on page 111. This is a construction that is used in geometry to represent graphically the geometrical concepts used in proofs. Such a construction is made with the help of two instruments: the ruler and the compass. The

[46]Moreover, from the *Dissertation* (Frege, 1873) until the end of his life, he always privileged the methods of analytic geometry. However, two approaches to analytic geometry can be find today. Firstly, there is the more abstract one, in which geometry has nothing to do with space, and can be thought as a subsystem of set theory (e.g. Borsuk and Szmielew, 1960). This is certainly of a great value from a purely proof-theoretical point of view, but not very "geometrical" in nature, as long as geometry is conceived in the Euclidean way. However, other approaches are "more geometrical" (e.g. Hartshorne, 2000) in that they keep the proof-theory connected with our intuition of space. This can be in no way avoided if we want to respect Frege's ideal of science. This is why, in a Fregean perspective, we must not ignore the proof-theoretical aspect, even though we must keep focusing on the ruler-and-compass basis of geometry.

Figure 5.1: A Ruler-and-Compass Construction



existence of any figure constructible by means of a ruler and a compass is intuitively evident.

A ruler allows us to draw lines. However, strictly speaking, lines are endless, as is the space available in a schema, i.e. that they could be continued indefinitely. This is so because geometry is concerned with ideal elements of geometry, i.e. by the conceptually abstract understanding of what space is. Now, anywhere in the schema, it is possible to choose an arbitrary point and mark it by a letter. In the figure 5.1, seven points have been chosen and marked by a letter, namely A, B, C, D, E, F, O. Choosing an arbitrary point is the only thing that can be done without ruler or compass, though the choice of a (non-arbitrary) point with specific properties has to be justified by a ruler-and-compass construction.

If a line crosses a point, we say that this point *lies on* or *is included in*[47] the line. Of course, all the points crossed by a line are included in this line. We can always select an arbitrary point lying on line. In this way, the points A, O, B lie on the line a, the points

[47]For this presentation, I choose the membership relation developed in the subsection 5.5.4. The motivation for this choice – though I admit it is not very "geometrical" – is that Frege (1899) pretends he succeeded in axiomatizing Euclidean geometry with less primitive terms than Hilbert. This way of proceeding, i.e. the "reduction" of a geometrical primitive term (or a group of them) to a logical one (or a group of them) seems to me to be the only one plausible.

D, O, E, C lie on the line c, etc. Any point on a line separates this line in two halves. Likewise, any two points on a line separates this line in three parts. For example, the points A and O separates the line a in a section to the left of the point A going at infinity, a section comprised between A and O, and a section to the right of O going at infinity. The section delimited by two points is called a segment, and is denoted by the letters of the points delimitating it (e.g. AO or OA in the above example). In this way, with the point D, O, E, C, we can distinguish six segments on the line c, namely DO, DE, DC, OE, OC, EC. From this, we see that two points completely determine a line; for example, the points O and E are sufficient to determine a unique line c, the points O and B are sufficient to determine a unique line b, etc. Since the segment AO is the continuation of the segment OB, they lie on the same line. We thus say that the lines a are b are the same line, i.e. $a = b$.

These remarks are not meant to be definitions, but only a figurative explanation without which one can in no case enter in an axiomatic system. This elucidation allowed us to grasp the meaning of the primitive concepts *Point* and *Line*. From this intuitive understanding, the truth of these three propositions derives:

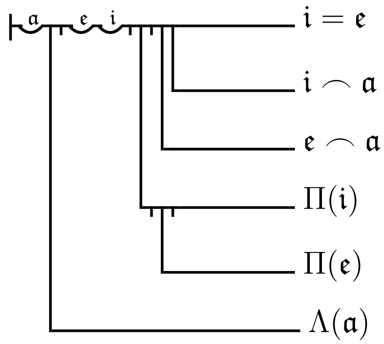
1. For any line there exists two distinct points through which the line runs: this is obvious from the fact that we can arbitrarily choose two distinct points on a line in any construction.

2. For any two points (not necessarily distinct), there exists at least one line running through these two points; this is obvious from the fact that in any construction, we can arbitrarily choose two points and use the ruler to construct a line joining them.

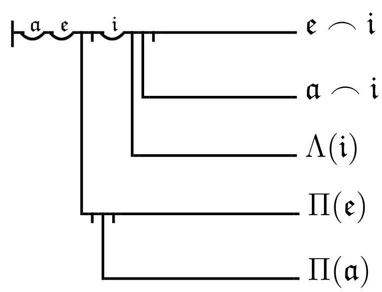
3. There is no more that one line running through two distinct points; this is also obvious from the ruler-and-compass constructions, since two distinct points completely determine a line.

These three propositions can be used as axioms for the relations of incidence holding between points and lines. We will thus find an expression for them in the rigorous and perspicuous notation of the *Begriffsschrift*. In this way, we will become possible to prove logically geometrical facts without any chance of being deceived by inaccurate drawings.

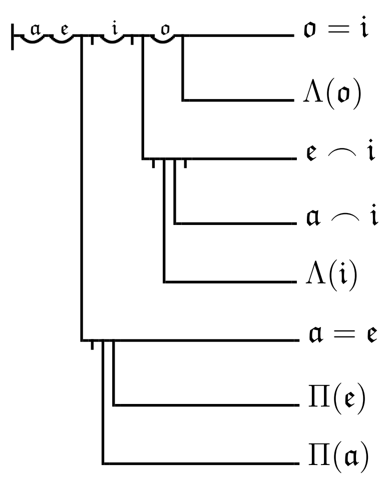
Axiom 5.1 *For any line there exists two distinct points through which the line runs. Expressed rigorously and perspicuously in the* Begriffsschrift, *we obtain:*



Axiom 5.2 *For any two points (not necessarily distinct), there exists at least one line running through these two points.*



Axiom 5.3 *There is no more that one line running through two distinct points.*



I think it is sufficient to understand what a Fregean-style geometry would look like. Now, one would certainly like to ask if the primitive terms I used are dependent on each other and if the axioms are. Likely, one would ask if they contradict each other. And one would like to have a proof of this. This kind of question was central to Hilbert's metamathematical project and is still central in today's research. For Frege, it is useless and makes no sense to go on with such metamathematical "proofs". These axioms are true. And axioms "do not contradict one another, since they are true; this does not stand in need of proof" (Frege, 1903, p. 25). It follows from their begin axioms. Likewise, their independence needs no proof, for it goes with their being axioms. This is the last word to the story.[48]

[48] Even a one-line proof consisting in reiterating the premise cannot be allowed; i.e. even writing down on a paper "are the axioms consistent?", then writing the axioms with the help of the *Begriffsschrift*, and to write after this "Q.E.D" without further ado would be unacceptable. It is in the nature of the axioms to be unprovable. And Frege makes no compromises on this point: unprovable means that there validity does not follow from a proof. Hence, any attempt to prove something in a Hilbertian way is to use pseudo-propositions and cannot be accepted.

Chapter 6

Conclusion

At the beginning of our analysis, I established that the Frege-Hilbert controversy was a slippery ground. As a matter of fact, much have been said by supporters of the arguments of Frege and Hilbert in a way that no consensus has been established. My preliminary assumption was that this may be considered as a sign that the controversy has become a battlefield where many levels of argumentation are involved. Among these levels, I then guessed, was the axiological one. Now that I have more carefully examined the problematic, it seems to me that this hypothesis was right. Far from being a problem to be solved in an established logical calculus by a mere reformulation of Frege's and Hilbert's arguments in the language of this calculus, the fundamental question is *about* the justification of what a calculus should be and should do. Should all logic be conceived as a calculus, or should there be another aspect? This question is undoubtedly connected with another one: what place logic should take in philosophy and science and how its aim should be achieved.

Frege and Hilbert certainly agreed that logic had to occupy a central place both in philosophy and in science. Nonetheless, they failed to agree on the way logic would better fulfill its function. We can summarize the critics they addressed one another in this two-line statement:

> *For Frege, Hilbert is a thoughtless thinker;*
> *For Hilbert, Frege plays a hide-and-seek game.*

Through our reconstruction of Frege's conception, the ubiquity of epistemic arguments have been observed: his argument-type *par excellence* is epistemic. When he discusses the purpose and the methods that are properly scientific – e.g. in his debate against formal theories of arithmetic –, the emphasis is always on the fact that the role of science is to provide us with *objective* knowledge. In science, there is certainly an intra-theoretical aspect, i.e. a purely analytical aspect. However, if this intra-theoretical aspect is to be isolated, all epistemic value would be lost. For Frege, the analytical step is necessary but insufficient. There is another step preceding it in importance in the foundation of a science: before all, we have to have an intuitive knowledge of reality, and this knowledge can be true. Moreover, is it necessary to specify in Frege's case, truth does not admit of degrees. These truths are a complete adequation of our knowledge to reality. For example, we have an intuitive knowledge of the form of space that is true. This aspect is a necessary condition of scientificity, since it gives it its epistemic value. Nothing is to be expected of a study which pretends to cut off science or a part of it from this basis, e.g. to do a formal analysis of a theory, because it makes no sense to deprive a theory of its intuitive grounds. For this reason, it makes no sense to think the language of science otherwise than as being genuinely interpreted. This requirement alone, as understood by Frege, is sufficient to exclude all metatheory: truths are true, and therefore mutually consistent and independent. It is by appealing to this requirement that he sets down the basic concepts of his epistemology, his philosophy of science, his philosophy of logic, his philosophy of language, etc:

In the beginning was the intuitive knowledge.

Frege thought that from this requirement alone, the Euclidean ideal of science that he defended was supposed to follow. A foundation for any science begins with intuition: this has a fundamental epistemological importance, since from it the truth of the axioms of a science derives. Hilbert seems not to attach much theoretical importance to this beginning: therefore he is a thoughtless thinker.

Is it to say that Hilbert attaches no epistemic value to science? Of course not! However, Frege would say that Hilbert is not aware that in view of his method, science becomes a thoughtless game. Contrariwise, Hilbert would say that Frege is not aware of the difficulties that must be resolved in order to justifiedly claim that the epistemic role of science has been fulfilled. Hilbert would say that Frege's approach is simplistic, that it does not take into account the real technical problems facing a foundational enterprise and that for this reason his approach degenerates into a game of hide-and-seek.

For Hilbert, the basic facts of geometry, as a science of space, have to be established *a posteriori*, by an experimental method. And as such, they are not truths in Frege's sense: they are at best plausible hypothesis, and have nothing to do with *a priori* truths. Likewise, any knowledge of the world, i.e. having an empirical content, is nothing more than a plausible hypothesis. For him, though he never states it explicitly, intuition is only a way to justify empirical knowledge and, moreover, this is a mode of justification that admits of degrees. As such, the analytical aspect of scientific theories, as examined in the frame of an axiomatic system, cannot require as preliminary condition the truth of the axioms:

> One of the main sources of mistakes and misunderstandings in modern physical investigations is precisely the procedure of setting up an axiom, appealing to its truth (?), and inferring from this that it is compatible with the defined concepts. (Hilbert, 1899, p. 40)

The point is the following: a proposition can have a meaning only in the frame of a theory implicitly defining a scaffolding of concepts; since only meaningful propositions can be adequationally true, it means that the construction and examination of a theory comes before the establishment of the adequational truth of a theory. Moreover, as long as the concepts occurring in a proposition are considered from the metatheoretical point of view, they do not completely determine a subject matter; they are implicitly defined and, as such, only define a field of application up-to-isomorphism. From this standpoint, the metatheoretical analysis of a theory has nothing to do with adequational truth, but only with functional truth. The stress that Hilbert puts on metatheory is therefore due to this: as long as we do not know the properties of a theory and the field it implicitly defines up-to-isomorphism (and this is established with the notion of truth-functionality), it makes absolutely no sense to talk about adequational truth. Skipping this step makes us derive into a game of hide-and-seek.

However, I have tried to show clearly that Hilbert also gives an important role to intuition in the formation of a theory. Since Frege also does, what is the quarrel then? Frege starts from judgements. It means that no sense can be made of a conceptual analysis if it does not start from judgements. Such a thing would have no epistemic value, for it would have no grounds in our source of knowledge. The basic epistemic act is judging, i.e. establishing truths. For Hilbert, if I am allowed to prompt the argument, the basic epistemic act would not be judgement, but rather reflection[1]. In this way, reflection is not meant as a truth-producing epistemic act, but as a theoretical-grid (scaffolding of concepts) analysis. Thus, before judging of the truth of a theory (in its entirety), we have to "reflect" *about* that theory. Since reflecting *about* a theory – determining its subject matter up-to-isomorphism – is something done by a truth-functional analysis, metatheory occupies the first place in the justification of a science, as long as it has an epistemic role to fulfill. Moreover, the properties of a theory (at the exception of its adequational truth and the related issues) have no dependency over the adequational truth of this theory. Therefore:

In the beginning was the metatheoretical knowledge.

Now, if Hilbert had a non-geometrical conception of geometry from a metatheoretical standpoint, he nonetheless had a geometrical conception of geometry from the standpoint of geometry. It is made possible by the fact that Hilbert's approach is multi-leveled. For Hilbert, it is not because science has an epistemic role to fulfill that we should adopt a perspective like Frege's. In particular, nothing forces us to accept its philosophy of the language of science. Indeed, the Fregean epistemic dichotomy meaningful/meaningless would be refused by Hilbert. According to him, there are two layers of meaning: 1- a properly logical one and 2- an non-logical one. The first layer, the properly logical one, is given by the axioms conceived as implicit definitions. It provides a sense to the basic proposition, though what they are about is only defined up-to-isomorphism. They define structures that can admit various models. Likewise, as long as models are only defined formally (i.e. that their semantics is purely formal), they are also defined only up-to-isomorphism. In the second layer of meaning enters many non-logical considerations, e.g. interpretation[2], intuition, sense of reality, etc. It is in this way that Hilbert may claim to work with uninterpreted languages; though they are uninterpreted, they have a layer of meaning sufficient for logical analysis. And as long as foundational works are concerned, only once this structural layer has been analyzed are we justified to superimpose the second layer of meaning. Therefore, as opposed to Frege, the intuition that contributes to the sense of a science only plays a role *after* (theoretically the logical analysis has been done[3].

[1] This expression is mine.

[2] Understood in a non-model-theoretical sense.

[3] "After" is not understood here in the chronological way, but in the justificational one. Of course, chronologically, intuition has always the first role.

***

Hilbert's approach opens the way to metatheory. On the other hand, Frege's approach closes the way to metatheory; in his view, language being genuinely interpreted, the only metatheory possible would be one like Wittgenstein's philosophical atomism which concluded that at that level of generality only nonsense could be said and, therefore, that metatheory is nonsense.

This, certainly, is an important stake issuing from the position one adopts concerning the Frege-Hilbert controversy. And there is many corrolaries. If we consider scientific theories like general relativity or quantum mechanics, that appear to be consistent though hardly intuitively graspable – if graspable at all –, will there be a possibility for logic to examine them? For me, at least, it is not intuitively obvious that their postulates are true. We find the same situation in the more abstract theories of various sciences, first of all mathematics. Are we to accept an approach that *a priori* closes us the way to these theories?

The answer one gives is certainly dependent on the epistemic attitude one adopts, which itself depends on the kind of problems to which the first importance is given. Frege's epistemology is profoundly realist. He believes that empirical truths – in the strong sense – can be obtained, and that they form the basis of science. Therefore, he sees no point in finding ways to justify science with something else than its truth, except for the pleasure of perverting science from its goal. For him, the science of the 19$^{\text{th}}$ century was being corrupted, and to bring back science on the right track asked for rigorous and powerful means. The development of his *Begriffsschrift* was this powerful means; he did not worked it out to revolutionized logic and science, but to react against what he considered to be a decadence. On the other hand, Hilbert was the prototype *par excellence* of what Frege would have called a perverted scientist. Hilbert was one of those who thought that empirical science was not true – at least not in the strong sense. Therefore, he judged that the rational justification of science was not to be found on the *a priori* claims of its truth. He therefore pushed and refined as far as he could the methods having been foreseen in the 19$^{\text{th}}$ century, thinking that this change in the epistemological attitude toward foundations required innovative methods. If a revolution in logic is to be found by the turn of the century, it is Hilbert who personified it[4].

[4] I note, as Kuhn did, that a revolution is however not synonym of progress.

Let us have a look at this mostly interesting passage from Weyl, concerning the historical period of the Frege-Hilbert controversy:

> The stages through which research in the foundations of mathematics has passed in recent times correspond to the three basic possibilities of epistemological attitude. The set-theoretical approach is the stage of *naive realism* which is unaware of the transition from the given to the transcendent[5]. Brouwer represents *idealism*, by demanding the reduction of all truth to the intuitively given. In axiomatic formalism[6], finally, consciousness makes the attempt to 'jump over its own shadow,' to leave behind the stuff of the given, to represent the *transcendent* – but how could it be otherwise?, only through the *symbols*. [...] It cannot be denied that a theoretical desire, incomprehensible from the merely phenomenal point of view, is alive in us which urges toward totality. Mathematics shows that with particular clarity; but it also teaches us that that desire can be fulfilled on one condition only, namely, that we are satisfied with the symbol and renounce the mystical error of expecting the transcendent ever to fall within the lighted circle of our intuition. So far, only in mathematics and physics has symbolical-theoretical construction gained that solidity which makes it compelling for everyone whose mind is open to these sciences. Their philosophical interest is primarily based on this fact. (Weyl, 1949, pp. 65-6)

I do not know to what extent Weyl's trichotomy is philosophically sound. However, from my point of view, it leads to this choice: 1- reject most of 20^{th}-century science for which no naive realist nor idealist account seems possible or 2- accept the challenge of "jumping over its own shadow". Of course, the second alternative is nowadays more popular. But its popularity can not justify anything. I would like to leave the question open, though the reader certainly understood what my position is.

[5] He thinks here of Dedekind, Frege, Russell, etc.
[6] Hilbert is a representative of this attitude.

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