Chapter 3
Hilbert: Making It Formal

In the standard textbooks Hilbert's philosophy of mathematics is commonly labelled *formalism* and under this title distinguished from Brouwer's *intuitionism*, on the one hand, and Russell's *logicism*, on the other hand. However, as Hintikka (1997a) rightly remarks, this popular name is very misleading. There are difficulties of two sorts. First of all, Hilbert's work in foundations of mathematics was a long-term project that began in 1890s and continued more than 40 years. Although Hilbert unlike Russell never abruptly changed his mind about foundational matters the development of Hilbert's project involved significant shifts in its philosophical underpinning. When one takes this into consideration it becomes impossible to identify Hilbert's views with any particular "ism". Second of all, the meaning of being *formal* is also changing: Hilbert and his contemporaries often use this term not in the same sense in which we use it today, and even today this term is often used in different ways in the mathematical and the philosophical communities. The two difficulties are mutually related because Hilbert's work in foundations strongly affected the changing meaning of being formal.

I shall not treat here the history of Hilbert's research in foundations systematically[1] but try to reconstruct the core dialectics of Hilbert's ideas, which is crucial for my analysis of today's state of affairs given in the next Chapter. I shall refer in this present Chapter to three Hilbert's texts: first, *Foundations of Geometry* of 1899, second, his address *Axiomatic Thought* of 1917 (Hilbert 1918) and, finally, his paper *Foundations of Mathematics* of 1927 (Hilbert 1927), which makes explicit the philosophical background behind the monumental two-volume work (Hilbert and Bernays 1934–1939) co-authored with Bernays and published in 1934–1939 under the same title. We shall see that although the Axiomatic Method as presented in *Foundations of Geometry* of 1899 and in *Foundations of Mathematics* in both cases qualifies as (or at least is commonly called by Hilbert's contemporaries) formal, the sense of being formal is not the same in the two cases.

[1]For the question of historical origins of Hilbert's Axiomatic Method see Toepell (1986) and Corry (2006).

A. Rodin, *Axiomatic Method and Category Theory*, Synthese Library 364,
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3.1 *Foundations* of 1899: Logical Form and Mathematical Intuition

In the last Section I stressed Hilbert's assumption according to which the deduction of mathematical theorems from axioms is purely *logical* and then argued that the geometrical theory of Euclid's *Elements* prima facie falsifies this assumption. However this assumption is not specific for Hilbert's approach. Frege, who sharply criticizes Hilbert's *Foundations* of 1899 on a different ground (that I shortly explain), wholly agrees with Hilbert on this general point. A major difference between Frege's and (early) Hilbert's versions of the Axiomatic Method, which led to a controversy between the two thinkers (Frege 1971), was the following. Frege assumes as a matter of course that all terms involved into axioms and theorems of a given theory are meaningful and that their meanings are specified in advance and rigidly fixed once and for all (at least within the given theory). Hilbert in his turn allows certain terms of a given to change their meanings and be considered without any fixed meaning at all. A theory of this latter sort Frege and some other Hilbert's contemporaries call *formal*. For a mathematically educated reader (let alone logician) this "informal" notion of formal theory is, of course, very familiar. Nevertheless for my purpose it is useful to present it here in an explicit form. Then I shall explain the sense in which Frege et al. call such a theory formal.

The first paragraph of the Foundations of 1899 reads:

> Let us consider three distinct systems of things. The things composing the first system, we will call points and designate them by the letters $A, B, C, \ldots$; those of the second, we will call straight lines and designate them by the letters $a, b, c, \ldots$; and those of the third system, we will call planes and designate them by the Greek letters α, β, γ. [..] We think of these points, straight lines, and planes as having certain mutual relations, which we indicate by means of such words as "are situated", "between"; "parallel", "congruent", "continuous", etc. The complete and exact description of these relations follows as a consequence of the axioms of geometry. These axioms [..] express certain related fundamental facts of our intuition.

The idea is this. The purpose of foundations of geometry is to develop geometry ab ovo. This means that "fundamental facts of our [geometrical] intuition" cannot be here tacitly taken for granted (as this is done in non-foundational geometrical studies) but must be explicitly described and postulated. The proposed method of describing these facts is the following. First, one identifies a list of *types of objects*, which are *primitive* in the sense that they are not defined in terms of some other (types of) objects; they are introduced without any definition. Second, one identifies a list of *primitive relations* between primitive objects; these primitive relations are also introduced without definitions. Finally, one makes up a list of *axioms*, i.e., propositions, which involve only primitive objects and primitive relations between these objects. Every consequence of these axioms qualifies as a geometrical theorem. (I shall specify a relevant notion of consequence in what follows; we shall see that there are in fact two different notions of consequence, which are here in play.)

Hilbert's Axiomatic Method does *not* assume that primitive objects and primitive relations are given through the usual linguistic meanings of words "point", "between", etc. Primitive objects are assumed instead to be bare "things" (possibly of several different *types*), which are called points, straight lines and the like by a merely linguistic convention having no theoretical significance. Primitive relations are treated similarly. Thus Hilbert's list of types of primitive objects and of primitive relations given in the above quote does not say us anything except that the given axiomatic theory involves three different types of primitive objects and several different relations between these objects. All the relevant information about these objects and these relations is supposed to be captured by axioms, which specify certain facts about these objects and these relations without using any assumption as to *what* are these objects and these relations.

To see how it works consider the First Axiom of Hilbert's *Foundations* of 1899:

(A1.0) Two distinct points A and B always completely determine a straight line a (op.cit., p. 2).

and remind that words "points" and "straight line" should not be read here in the usual sense. Notice also a relation between the points and the line, which is expressed by saying that the points determine the line; there is more than one way to translate this expression in terms of relations but Hilbert uses here the binary relation of *incidence* between a given straight line and a given point, which can be also informally expressed by saying that the given point *lies* at the given straight line (or equivalently by saying that the given straight line *goes through* the given point). This semantic hygiene leaves us with the following *formal* reading of A1.0

(A1.1) Given two different primitive objects A, B of basic type P ("points") there exist a unique primitive object a of another basic type L ("straight lines"), such that each of A, B and a hold a primitive relation R ("incidence").

Although A1.1 may seem to be not very informative it presents what Hilbert's First Axiom "really says" more accurately than A1.0. The idea of Hilbert's Axiomatic Method is that a system of propositions like A1.1 provided with an appropriate system of logic may completely determine (in a sense that I try to clarify further in what follows) what the Euclidean (or some other) geometry "really is". The same method of theory-building is supposed to apply in various domains of the theoretical inquiry both within and outside the pure mathematics. Whatever is the domain of application of the Axiomatic Method the axioms always involve only abstract objects and abstract relations. What is specific for Euclidean geometry from Hilbert's axiomatic viewpoint is the list of its axioms rather than any particular subject-matter like *space* or *extension*.

Suppose a non-experienced reader looks at A1.1 and asks what this proposition has to do with the Euclidean geometry. An appropriate explanation can be given by translation A1.1 back to A1.0 followed by the "naive" reading of A1.0, which turns it into a proposition similar to Euclid's First Postulate. This naive reading of A1.0 refers to a "fundamental fact of our intuition", which, by Hilbert's word, this axiom "expresses". However in the given context this "fundamental fact of intuition" does not *ground* the corresponding axiom A1.0 but merely motivates it. We shall shortly see, however, that in a different version of his Axiomatic Method presented in the *Foundations* of 1927 Hilbert grants a fundamental role to the geometrical intuition of a *special* sort.

How a proposition like A1.1 may qualify as an axiom? In his letter to Frege Hilbert says:

> [A]s soon as I posited an axiom it will exist and be "true". [..] If the arbitrarily posited axioms together with all their consequences do not contradict each other, then they are true and the things defined by these axioms exist. For me, this is the criterion of truth and existence. (Frege 1971, p. 12)

Some comments are here in order.

1. Unlike Frege, Hilbert does *not* think about mathematical axioms as self-evident truths. In the above quote Hilbert speaks of axioms as sheer stipulations, which are "true" in virtue of the fact that they are posited by someone. The only rule restricting the positing of new axioms is the rule according to which each axiom must be self-consistent and any set of such axioms (belonging to the same theory) may contain only mutually consistent axioms. As Hilbert puts this in the above passage "If the arbitrarily posited axioms [..] do not contradict each other, then they are true". One may remark (as did Frege) that given a set of true propositions it is impossible to infer from them a contradiction anyway. However this observation does not make Hilbert's rule redundant because being true does not have its usual meaning. Since being true reduces to being stipulated the question Which stipulations are allowed and which are not? must be treated independently. Thus the consistency condition must be checked before "axioms become true", i.e. before one stipulates that a given set of expressions represents a set of mathematical truths. Such a checking requires a special notion of consistency, which applies to linguistic expressions having no definite truth-values. At the time of writing his letter to Frege Hilbert did not formulate yet the appropriate notion of consistency rigorously; we shall shortly see how he tried to solve this problem afterwards.
2. Notice the peculiar form of Hilbert's axioms, which involves terms with variable meaning. An expression of this form turns into a proposition only when the meaning of all its terms becomes determined. So in order to stipulate that a set of axiom-like expressions represents a set of axioms, Hilbert needs to assume that there exist "things defined by these axioms", which (a) make all terms in these axioms meaningful and (b) which make these axioms true. In the above quote Hilbert states that the existence of such things is always granted when the corresponding set of axioms is consistent. ("If the arbitrarily posited axioms [..] do not contradict each other, then [..] the things defined by these axioms exist".) Notice that the existence of these things has no other prerequisites except consistency. Whence there arise two mutually related questions: *What* are the things "defined by axioms"? and *How* the axioms "define" them? Let me consider these two questions in turn.

The former question has at least three different answers. The first general answer is this: given an expression like A1.1, which bears on "bare things" and "bare relations" of multiple types one instantiates these things and these relations in one's mind and so get what Hilbert after Kant calls *objects of thought* or *thought-things* (*Gedankendinge* in German), which are related by corresponding *thought-relations*.[2] These thought-things and thought-relations exist merely in virtue of the fact that one thinks of them consistently. They may be or be not supported by some sensual intuitions; the sensual intuition is a separate issue which must not be confused with the capacity to instantiate objects and relations between objects as such. This latter capacity can be also called intuition – not in the sense of Kant's *Transcendental Aesthetics* but exclusively in the sense of Kant's *Doctrine of Method* (Kant 1999). Hintikka (1997a) quite rightly stresses the fundamental role of this restricted notion of intuition in Hilbert's Axiomatic Method. Even when we think of mathematical objects as "bare things" without associating with these things anything over and above the relations stipulated through axioms like A1.1 we think about these objects, by Kant's word, *in concreto* (which shows, by the way, that the usual characterization of such object as *abstract* is somewhat misleading). The mathematical intuition in the relevant restricted sense of the term is the capacity to think concretely about objects and relations between objects without associating to these objects and these relations any additional qualities.

The second answer concerns the role of sensual intuition. Remind that in the introductory part of his *Foundations* of 1899 Hilbert says that his geometrical axioms "express certain related fundamental facts of our intuition". Earlier in 1891 he made the following remark:

> Geometry is the science that deals with the properties of space. [..] I can never penetrate the properties of space by pure reflection, much as I can never recognize the basic laws of mechanics, the law of gravitation or any other physical law in this way. Space is not a product of my reflections. Rather, it is given to me through the senses. (quoted after Corry 2000, p. 44)

In 1894 Hilbert develops this view on geometry:

> Among the appearances or facts of experience manifest to us in the observation of nature, there is a peculiar type, namely, those facts concerning the outer shape of things, Geometry deals with these facts [..]. Geometry is a science whose essentials are developed to such a degree, that all its facts can already be logically deduced from earlier ones. Much different is the case with the theory of electricity or with optics, in which still many new facts are being discovered. Nevertheless, with regards to its origins, geometry is a natural science (ibid. p. 45)

> [A]ll other sciences-above all mechanics, but subsequently also optics, the theory of electricity, etc.- should be treated according to the model set forth in geometry. (ibid. p. 45)

What Hilbert says here about the empirical character of geometry prima facie is not compatible with his notion of geometry as a free creation of mind expressed in his letter to Frege quoted above. It is not impossible, of course, that during this period of time Hilbert had conflicting ideas about the nature of geometry and could contradict himself. However it seems me suggestive to try to reconcile the two notions of geometry. As a part of the pure mathematics geometry is treated as a free creation of mind; the fundamental question here is whether or not the given set

[2] Compare with Orwell's *thoughtcrimes*.

of geometrical axioms is consistent while the question where those axioms come from is irrelevant. As a natural science geometry seeks to express properties of the physical space through an appropriate set of axioms, then "logically deduce" from these axioms some further geometrical propositions and finally check these deduced propositions against properties of the physical space. So the two geometries well fit together: the physical geometry takes care about choosing axioms properly while the mathematical geometry takes care about the consistency of any proposed set of geometrical axioms, about the deduction of new theorems from these axioms and some other relevant problems. This epistemological model is applicable to all natural sciences; what makes geometry "more mathematical" than say, the theory of electricity, is the fact that geometry easier allows for an axiomatic treatment because its "essentials" are better developed.

So we may consider geometry in a larger sense, which combines the axiomatic mathematical geometry, on the one hand, and the empirical physical geometry, on the other hand. Objects of this combined geometry are no longer bare individuals but spatial physical bodies, light rays, etc. Interestingly, the traditional notion according to which geometry presents properties of the physical space in an idealized form is irrelevant to Hilbert's axiomatic setting. Geometrical objects are thought of here either as bare individuals detached from any sensual intuition or as physical bodies as they are perceived by senses; Hilbert's epistemic scheme, which we reconstruct on the basis of the above passages, does not include any intermediate "ideal" element between the axiomatic logical reasoning and the sensual perception. We shall see, however, that in his later works Hilbert introduces such ideal elements (Sect. **3.4**).

The third answer to the question about Hilbert's mathematical "things" and their existence concerns the possibility of interpreting axioms of a given axiomatic theory in terms of another mathematical theory. For example with the help of standard tools of Analytic Geometry A1.0 and other Hilbert's axioms translate into true propositions about real numbers. An interpretation M that translates all axioms of a given axiomatic theory A into true propositions of another theory T is called a *model* of A in T; one says also that axioms of A are true in model M. Suppose we know which proposition of T is true and which is false. This allows one to reverse the order of ideas about A. Observe that in order to check whether or not axioms of A are true in M one does not need to establish consistency of this set of axioms in advance. Moreover, if axioms of A are true in M (i.e., if M is indeed a model of A) then one may conclude that A is consistent! Remind Hilbert's remark according to which any consistent set of propositions can be made by a fiat into a system of axioms, which are true and meaningful. Now we proceed the other way round: we first check that our axioms are true and meaningful in some model[3] and on this basis conclude that the given set of axioms is consistent. However this conclusion is not valid unless

[3]By meaningfulness of a given axiom in a given model I mean the bare fact that this axiom translates into a meaningful proposition. Saying that every axiom of a given theory is true and meaningful in a model of this theory is, of course, pleonastic.

T, which is the background theory of M, is consistent in its turn. So what the above argument really proves is not the absolute but only the *relative* consistency of A, i.e., the proposition of the form "if T is consistent then A is also consistent".

From a mathematical point of view this third way of interpreting Hilbert-style axioms turns to be the most productive. Already in his *Foundations* of 1899 Hilbert applies this method systematically; in the course of twentieth century this method develops into the modern *model theory*, which remains today an active field of mathematical research still having some philosophical flavor. I would like to stress here that interpreting a Hilbert-style axiomatic theory in terms of another mathematical theory and interpreting such a theory in some intuitive terms *directly* are two very different issues. Since both procedures go under the same title of "interpretation" they are too often confused in the current debates. The idea that Hilbert's axiomatic theory of Euclidean geometry can be either interpreted "as usual", i.e., by associating with the terms "point", "straight line", "between", etc. their "usual" intuitive meanings, or alternatively, be interpreted arithmetically by identifying points with pairs of numbers, etc., is plainly misleading because it puts under the same title of interpretation two procedures, which do not belong to the same general type.

3. Let us finally discuss Hibert's view according to which axioms of a given mathematical theory "define" objects of this theory. Since Hilbert's axioms refer only to bare "things" and bare relations and since, according to Hilbert, any consistent set of such axioms allows one to produce a "system of things" S satisfying these axioms by a fiat (or more precisely by the very fact that one forms consistent thoughts "about" certain things), such S can be thought of as the "definiendum" of the axioms. One may ask however whether a given consistent set of axioms defines the corresponding system S *uniquely*. Here is what Hilbert says about this in the same letter to Frege:

> You say that my concepts, e.g. "point", "between", are not unequivocally fixed [..]. But surely it is self-evident that every theory is merely a framework or schema of concepts together with their necessary relations to one another, and that basic elements can be construed as one pleases. If I think of my points as some system or other of things, e.g. the system of love, of law, or of chimney sweeps [..] and then conceive of all my axioms as relations between these things, then my theorems, e.g. the Pythagorean one, will hold of these things as well. In other words, each and every theory can always be applied to infinitely many systems of basic elements. For one merely has to apply a univocal and invertible one-to-one transformation and stipulate that the axioms for the transformed things be correspondingly similar. (cit. by Frege 1971, p. 13).

There are two important ideas in this passage. First Hilbert stresses here once again that in his axiomatic setting primitive geometrical terms have no intrinsic meaning: any system of things (i.e., model) satisfying Hilbert's axioms counts as an Euclidean space. This point has been already discussed earlier in this Chapter and I shall not return to it. Then follows this crucial observation: given a model M of a given axiomatic theory one can always get another model M' of the same theory through a one-to-one transformation of elements of M into elements of the new model M' in such a way that relations between elements of M' also satisfy the axioms of the given theory. In the modern language the kind of transformation described here by Hilbert is called *isomorphism*. Apparently Hilbert thinks here about an axiomatic theory that determines its models *up to isomorphism*, i.e., such that all its models are *isomorphic*, i.e., are transformable into each other by some isomorphisms. Such theories are called today *categorical*. (Beware that *that* sense of being categorical has nothing to do with the category theory!) Isomorphic models can be seen as "equal" and representing the same *structure*, which is invariant under transformations between these models. This leads to a philosophical view on mathematics known as *mathematical structuralism*; according to this view structures are basic mathematical objects. I consider the mathematical structuralism and its significance for the Axiomatic Method in Chap. **9**. The idea of the "replacement of equality by isomorphism" is also discussed in Chaps. **6** and **7**.

Precipitating this further discussion I would like only to stress here that not every Hilbert-style axiomatic theory is categorical. In fact this is a rather strong property that most of useful axiomatic theories do not enjoy. Apparently Hilbert didn't see this problem before he first published his *Foundations* in 1899; however in his lecture *On the Concept of Number* (Hilbert 1900) delivered in the same year 1899 and published in 1900 Hilbert already introduces an "axiom of completeness" (Vollstandigkeitsaxiom), which requires from any model of a given theory (this time it was arithmetic) this maximal property: any model M of the given theory extended with some new elements is no longer a model. Then he proves that among all models of his theory (without the completeness axiom) there is only one model (up to isomorphism, of course!), which also satisfies the completeness axiom, see Corry (2004, p. 160) for details. The second edition of Hilbert's *Foundations of Geometry* appeared in 1903 already contains a geometrical axiom of completeness.

Let me now return to the question about the sense of being formal. Frege and his contemporaries called Hilbert's axioms for geometry *formal* extending the sense of being formal used by Trendelenburg (1862) when he made popular the expression "formal logic". Formal logic in Trendelenburg's sense is by and large what Kant calls *general* logic as distinguished from his transcendental logic: the formal or general logic takes into account only the form of reasoning and is neutral with respect to its content (Kant's transcendental logic is not wholly neutral with respect to the content of reasoning because it takes into account the difference between objects of possible experience and thought-objects of other sorts). The modern usual sense of "formal logic" retains this older sense but does not reduce to it because it also includes the idea of symbolic mathematical presentation of logical form, see Sect. **3.3** below. Although Hilbert's axioms are not logical tautologies that hold for all objects and all relations whatsoever, they represent *logical forms* of propositions obtained through the usual contentual reading of the same axioms and allow for alternative instantiations of this logical form (i.e., for alternative models). For example, by reading words "point" and "straight line" in Hilbert's First Axiom A.1.0 naively, i.e., by associating with these words their usual linguistic meanings, one gets an universal contentual proposition about (all) points and (all) straight lines (in the relevant domain where this axiom applies). However the intended meaning of this axiom expressed more explicitly in A1.1 specifies only a property of abstract relation between abstract objects of two different types. This description is purely formal in the sense that it fixes no domain of objects and no concrete relation. It specifies a specific form of relation between objects but specifies no particular relation and no particular object and no particular type of objects.

Today we would qualify the theory of Hilbert's *Foundations* of 1899 as informal or semiformal at best. This is because this theory is formulated in the natural German with the help of some symbols like any typical introductory mathematical text. Today's paradigmatic examples of formal theories is given by axiomatic theories of sets and of arithmetic like ZF and PA. These latter theories differ from the theory of Hilbert's *Foundations* of 1899 first of all by their symbolic syntax. In Sect. **3.3** we shall see how the idea of using such a symbolic syntax combines with Hilbert's earlier approach described in this Section. We shall see that the symbolic approach involves some epistemological ideas, which do not make part of the traditional notion of being formal relevant to Hilbert's *Foundations* of 1899. So being formal, semiformal and informal should not be thought of only as a matter of degree.

3.2 *Foundations* of 1899: Logicality and Logicism

Consider once again the Hilbert's First Axiom A1.0

> Any two distinct points of a straight line completely determine that line

and remind that certain words in this sentence including the words "points" and "straight line", are *not* supposed to be understood in their usual sense. Now remark that some other words like "any" and "two" *are* supposed to be understood in the usual sense. Clearly this second category of words plays in Hilbert's *Foundations* of 1899 an essential role: unless at least some words in these axioms are meaningful the axioms reduce to an abracadabra! In the last Section I elaborated on words of the former category, now let us look more attentively on words of the latter category. First of all let us see how exactly words are sorted into two sorts here. Words of the first sort refer to primitive geometrical concepts like point, straight line and between (whether these primitive concepts are understood traditionally or in the sophisticated formal way explained above). What about words of the second category?

In order to answer this question it is helpful to paraphrase A1.0 as follows:

> If different points A, B belong to straight line a and to straight line b then a is identical to b

Now leaving out *geometrical* words and expressions "points A, B", "straight line a", "straight line b", "belong to" we get this list: "if", "different", "and", "then", "is", and "identical to". So the last paraphrase helps us to see that the words belonging to the second list stand for *logical* notions.

How to distinguish between logical and non-logical terms more formally? There exist in the literature two main approaches to defining the notion of *logicality*: one develops the idea of logic being *content-free* (so that logical signs are understood as the "punctuation marks") and the other that describes itself as *semantic* develops the idea of logic being *content-invariant* (Bonnay 2006). This later approach dates back to Tarski's proposal (1986) to identify logical notions with invariants of all permutations of elements of some given set.[4]

This latter approach obviously better squares with Hilbert's Axiomatic Method; the idea here is to make the fixity of meanings of logical terms and the variability of meanings of non-logical terms into a formal criterion allowing one to distinguish between these two sorts of terms. Tarski accounts for this fixity as invariance under permutations of elements of a given set of individuals (which represents here a certain universe of discourse). This approach to logicality is motivated by Klein's *Erlangen Program* in geometry (Klein 1872); it establishes a conceptual link between Klein's and Hilbert's works in foundations of geometry, which is both conceptually significant and historically plausible. I discuss it in Sect. **9.3** below.

Now I would like only to stress Hilbert's fundamental assumption behind his Axiomatic Method (as presented in his *Foundations* of 1899) according to which logic is the ground layer foundation of all theories built axiomatically. As Hintikka puts this, for Hilbert

> The basic clarified form of mathematical theorizing is a purely logical axiom system. (Hintikka 1997a, p. 20)

This does not mean, of course, that Hilbert like Russell in (1903) tries to *reduce* mathematics to logic. This later version of logicism is certainly not Hilbert's. In this book I shall use the term "mathematical logicism" in a broader sense of epistemic primacy of logic over mathematics. In this broader sense Hilbert's view on mathematics qualifies is another version of logicism.

As I have already mentioned in Sect. **2.4** the idea of logic and metaphysics as a foundation of all science dates back to Aristotle. This idea had apparently little or no influence on Greek mathematics (that followed Euclid rather than Aristotle) but later became quite influential in the medieval Scholasticism. The Early Modern mathematically-laden science that triumphed with Newton's *Principia* largely rejected the old scholastic pattern of theory-building and developed a very different notion of scientific theory that was described in general terms by Kant in his *Critique of the Pure Reason*. The Kantian philosophy of science and mathematics remained the mainstream until the beginning of the twentieth century when the old scholastic pattern of theory-building kicked back under the new name of modern Axiomatic Method.

In Kant's view the *objectivity* of pure mathematics (which underlies the objectivity of the mathematically-laden empirical science) roots in its *objecthood*, i.e., in the universal schemata according to which one constructs mathematical *objects* – but not just in the general character of mathematical concepts. Making difference

[4] Bonnay (2006) formulates Tarski's Thesis as follows:

> Given a set M, an operation Q_M acting on M is logical iff it is invariant under all permutations

between the mathematical objectivity and the universal logical validity, according to Kant, is crucial for differentiating between the mathematical reasoning and the philosophical speculation. Here is the famous passage:

> Give a philosopher the concept of triangle and let him try to find out in his way how the sum of its angles might be related to a right angle. He has nothing but the concept of figure enclosed by three straight lines, and in it the concept of equally many angles. Now he may reflect on his concept as long as he wants, yet he will never produce anything new. He can analyze and make distinct the concept of a straight line, or of an angle, or of the number three, but he will not come upon any other properties that do not already lie in these concepts. But now let the geometer take up this question. He begins at once to construct a triangle. Since he knows that two right angles together are exactly equal to all of the adjacent angles that can be drawn at one point on a straight line, he extends one side of his triangle and obtains two adjacent angles that together are equal to the two right ones. [..] In such a way through a chain of inferences that is always guided by intuition, he arrives at a fully illuminated and at the same time general solution of the question. (*Critique of Pure Reason* Kant 1999, A 716 / B 744)

Kant's philosophy of mathematics and mathematically-laden science is based upon an analysis of his best contemporary science as represented by Newton's *Principia* (Friedman 1992). This does not mean, of course, that Kant derives his philosophical principles from the principles of Newtonian physics; Kant's *critical* philosophy rather aims at explaining how the type of knowledge best represented by the Newtonian physics is possible (as an objectively valid knowledge). Anyway this method of philosophical work makes Kant's philosophy strongly dependent on the contemporary mathematics and science. Cohen, Natorp and other neo-Kantians who wished to sustain the Kantian project of critical philosophy in the nineteenth century realized this fact very clearly and made efforts to supply the Kantian philosophy with a historical dimension allowing one to keep track of the progress in sciences and mathematics (Heis 2007). It was not quite clear in the nineteenth century and it still remains a matter of controversy today which (if any) features of Kant's original approach remain sustainable in the context of the current science and mathematics, and which features of this original approach are hopelessly outdated. More radically one may wonder if there is anything at all in Kant's analysis that survives all the dramatic changes in science and pure mathematics that have happened since Kant's own time.

In spite of a number of interesting attempts of upgrading the Kantian philosophy of mathematics in order to account for new mathematical developments (like the invention of non-Euclidean geometries) at certain point the Kantian line in the philosophy of mathematics has been largely abandoned. Bertrand Russell's intellectual development is representative in this sense: after publishing in 1897 his Kantian *Essay on Foundations of Geometry* (Russell 1897) and a short romance with Hegel (Hylton 1990) Russell learns in 1900 about new works in mathematical logic, publishes during the same year an essay on Leibniz (Russell 1900), who now becomes the right philosophical ancestor, and already in 1903 publishes the *Principles of Mathematics* (Russell 1903) where the subject is developed on new logicist grounds. In the *Introduction* to this book Russell explains his attitude to the Kantian line of thought as follows:

> It seemed plain that mathematics consists of deductions, and yet the orthodox accounts of deduction were largely or wholly inapplicable to existing mathematics. Not only the Aristotelian syllogistic theory, but also the modem doctrines of Symbolic Logic, were either theoretically inadequate to mathematical reasoning, or at any rate required such artificial forms of statement that they could not be practically applied. In this fact lay the strength of the Kantian view, which asserted that mathematical reasoning is not strictly formal, but always uses intuitions, i.e. the a priori knowledge of space and time. Thanks to the progress of Symbolic Logic, especially as treated by Professor Peano, this part of the Kantian philosophy is now capable of a final and irrevocable refutation. By the help of ten principles of deduction and ten other premisses of a general logical nature (e.g. "implication is a relation"), all mathematics can be strictly and formally deduced. [..]
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> The general doctrine that all mathematics is deduction by logical principles from logical principles was strongly advocated by Leibniz... But owing partly to a faulty logic, partly to belief in the logical necessity of Euclidean Geometry, he was led into hopeless errors in the endeavour to carry out in detail a view which, in its general outline, is now known to be correct. The actual propositions of Euclid, for example, do not follow from the principles of logic alone; and the perception of this fact led Kant to his innovations in the theory of knowledge. But since the growth of non-Euclidean Geometry, it has appeared that pure mathematics has no concern with the question whether the axioms and propositions of Euclid hold of actual space or not ..... What pure mathematics asserts is merely that the Euclidean propositions follow from the Euclidean axioms, i.e., it asserts an implication..... We assert always in mathematics that if a certain assertion p is true of any entity x or of any set of entities $x, y, z \ldots$, then some other assertion q is true of those entities; but we do not assert either p or q separately of our entities.

The above argument, which is supposed to refute Kant, obviously begs the question. From the outset Russell takes it for granted that "mathematics consists of deductions" and his following remarks make it clear that by deduction Russell means here a *logical* deduction, i.e. a deduction of propositions from certain other propositions according to some general rules, which are not specific for mathematics. This statement overtly contradicts what Kant says about mathematics, and the following Russell's argument only provides this first statement with some additional details but does not constitute any philosophical objection to Kant. Kant's own objection to the Leibnizian view on mathematics, to which Russell adheres here, is this. From a *formal* point of view (i.e. as far as only *logical form* of sentences is taken into consideration) mathematics is no different from a mere metaphysical speculation; a speculative metaphysical theory can be developed on an axiomatic basis just like any mathematical theory (think about Spinoza's *Ethics* for example). What makes the crucial difference between mathematics and speculation is the fact that mathematics constructs its *objects* according to certain rules, while speculation proceeds with concepts without being involved in any similar constructive activity. The fact that the speculative thought may also posit some entities falling under these concepts from the Kantian viewpoint does not constitute an objection because such stipulated entities doesn't qualify as *objects* in the strong Kantian sense of the term. Behind an *object* there is a procedure (governed by a certain *rule*) that constructs it; speculative entities are stipulated as mere *thought-things* falling under given descriptions by a fiat. This is the reason why the pure mathematics is *objective* in the sense in which the pure speculation is not. What makes the pure mathematics objective is the rule-like character of object-construction. The formal logical consistency is a necessary but not sufficient condition for claiming that a given axiomatic theory is objectively valid. This objective character of mathematics, according to Kant, allows for application of mathematics in natural sciences (I leave however this further point aside). Russell's critique of Kant in the *Principles of Mathematics* simply does not take into account the Kantian problem of separation of the pure mathematics from the pure speculation. In this respect Russell's Leibnizian approach to mathematics is more traditional than Kant's and in Kantian terms qualifies as dogmatic. Not surprisingly Russell provides his philosophy of mathematics with a metaphysical doctrine that he calls the *logical atomism*. This is how he describes the relation of this doctrine to logic and mathematics in the *Introduction* to his (Russell 1918):

> As I have attempted to prove in *The Principles of Mathematics*, when we analyse mathematics we bring it all back to logic. It all comes back to logic in the strictest and most formal sense. In the present lectures, I shall try to set forth in a sort of outline, rather briefly and rather unsatisfactorily, a kind of logical doctrine which seems to me to result from the philosophy of mathematics – not exactly logically, but as what emerges as one reflects: a certain kind of logical doctrine, and on the basis of this a certain kind of metaphysic.

As a recent biographer describes Russell's work during this early period of his career

> From August 1900 until the completion of Principia Mathematica in 1910 Russell was both a metaphysician and a working logician. The two are completely intertwined in his work: metaphysics was to provide the basis for logic; logic and logicism were to be the basis for arguments for the metaphysics. (Hylton 1990, pp. 7–8)

Thus we can see how an older pattern of intellectual work, which many people in the nineteenth century believed to be definitely sublated by Kant's critical philosophy and other developments, reemerged in the beginning of the twentieth century in the context of new mathematics and new symbolic logic. An attempt to describe the general intellectual context of that time would obviously lead me too far but it is my understanding that Russell's case in an extreme form represents a more general intellectual tendency. Even more important is the fact that this tendency towards the revival of the traditional alliance between logical and metaphysical thinking is still very much alive today, and in fact since 1900 this intellectual project has firmly established itself in the philosophical school known as *Analytic Philosophy* (as well as in some other branches of today's philosophy). So my critique of Russell of early 1900s and, in particular, my attempts to revendicate the Kantian and the Hegelian (see Sect. **5.8** below) lines of philosophical thought in the context of recent mathematics of our own times, aims primarily at the modern proponents of this traditional alliance.

Russell's interpretation of Kant's work in the philosophy of mathematics as an attempt to fill logical gaps appearing when one tries to reconstruct Euclid's geometry with Aristotle's syllogistic logic hardly correctly describes Kant's intention. However these Russell's words are helpful for a better understanding of his own project. Russell suggests two independent reasons why there are such logical gaps: first, because Euclid's geometry is logically imperfect and, second, because Aristotle's logic is not appropriate for doing mathematics. However the new mathematics (including non-Euclidean geometries) and the new symbolic logic taken together, according to Russell, wholly fix the problem making Russell's Leibnizian dream real. What Russell's *Principles of Mathematics* aim at is made clear by the following lines that I take from the *Preface* to this work:

> The second volume, in which I have had the great good fortune to secure the collaboration of Mr A. N. Whitehead, will be addressed exclusively to mathematicians; it will contain chains of deductions, from the premisses of symbolic logic through Arithmetic, finite and infinite, to Geometry, in an order similar to that adopted in the present volume; it will also contain various original developments, in which the method of Professor Peano, as supplemented by the Logic of Relations, has shown itself a powerful instrument of mathematical investigation.

(The planned second volume of *The Principles of Mathematics* appeared later as a co-authored independent three-volume work Russell and Whitehead 1910–1913.)

Thus we can see that in early 1900s Hilbert was not alone who thought about logic as the ground layer foundation of mathematics. (This is in spite of the fact that unlike Russell Hilbert in his *Foundations* of 1899 officially sticks to Kant! – notice the Kant's quote used as the epigraph in this book. We shall shortly see (Sect. **3.4**) that Hilbert's later version of Axiomatic Method better fits the Kantian view on mathematics than this earlier version.) However there were also strong opposing voices during the same period of time. Among prominent critics of logical approaches in foundations of mathematics were Poincaré (Detlefsen 1992) and Brouwer. Consider, for example, this Brouwer's passage written in 1907:

> About mathematical reasoning, I show in the beginning of the chapter that it is no logical reasoning, that it uses the connectives of logic only because of the poverty of language, and thus may perhaps keeps alive the language accompaniment even after the human intellect has already long ago outgrown the logical argument itself. For, far from the fact that it would be a "strange company" that does not reason logically, I believe that it is only a matter of inertia, that the words that go with it [i.e., logic] as yet still exist in modern languages. A pure use of these words hardly occurs, and [in] impure [form] they are used in daily life, where they have led to all kinds of misunderstanding and dogmatism [..]. Those misconceptions arose, not because of insufficient mathematical insight, but because mathematics, lacking a pure language, makes do with the language of logical reasoning, although its thoughts reason not logically, but mathematically, which is something totally different. (quoted after van Dalen 2000, pp. 128–129)

I warn the reader that Brouwer's concern expressed in the above quote is not met by using the formal *intuitionistic logic* instead of classical logic in foundations of mathematics because this replacement of logic leaves untouched the assumption about the primacy of logic over mathematics; such a replacement only translates (through Heyting's formalization of the intuitionistic logic (Heyting 1956)) some Brouwer's ideas into a logicist foundational framework. However the very fact that today we have more than one candidate logic for building foundations of mathematics is remarkable. The logicist view on mathematics was particularly appealing in the beginning of the twentieth century because at that time the traditional geometry was already split into its Euclidean and multiple non-Euclidean versions but logic still preserved its traditional unity. Today we live in a very different environment. Here how Gabbay describes it as for 1994:

> In recent years we have witnessed a very strong and fruitful interaction between traditional logic on the one hand and computer science and Artificial intelligence on the other. As a result, there was urgent need for logic to evolve. New systems were developed to cater for the needs of applications. Old concepts were changed and modified and new concepts came into prominence. The community became divided. Many expressed themselves strongly, both for and against, the new ideas. Papers were rejected or accepted on ideological grounds, as well as technical substance. In this atmosphere, it seemed necessary to clarify the basic concepts underlying logic and computation, especially the very notion of a logical system. [..] The views among members of the community are varied and in many cases, very strongly held. There is at one extreme the pluralistic view, expressed to me once in a meeting by a distinguished colleague who said something like "we use logics like we use computer languages". At the other end of the spectrum there is the view of those who believe there is only one true logic, and all the rest is nonsense. Of course there exist several proposals for this true logic with their respective bands of followers. (Gabbay 1994, *Preface*, p. v)

So anyone who holds today a logicist view on mathematics (in the broad sense of "logicist" explained above) needs, first, to specify *which* is his or her favorite logic used in foundations, second, to explain *why* this particular logic is the most appropriate for the purpose[5] and, third (which is perhaps the hardest task), to explain why and in which sense one's favorite logic qualifies as logic. This problematic character of modern logic does not imply that the logicist view on mathematics is no longer tenable but it certainly shows that this view can not and should not be taken for granted. In Chap. **10** we shall see how the New Axiomatic Methods deals with this new degree of freedom of today's axiomatic thought.

A more detailed argument against the mathematical logicism has been given in 1907 by Cassirer who continued to push the Kantian line taking into account newest mathematical developments of his time. Referring to Russell (1903) and new formal logical methods under the name of "logistics" Cassirer says:

> Here rises a problem that lies wholly outside the scope of "logistics" [..] All empirical judgements belong to their domain: they must respect the limits of experience. What logistics develops is a system of hypothetical assumptions about which we cannot know, whether they are actually established in experience or whether they allow for some immediate or non-immediate concrete application. According to Russell even the general notion of magnitude does not belong to the domain of pure mathematics and logic but has an empirical element, which can be grasped only through a sensual perception. From the standpoint of logistics the task of thought ends when it manages to establish a strict deductive link between all its constructions and productions. Thus the worry about laws governing the world of objects is left wholly to the direct observation, which alone, within its proper very narrow limits, is supposed to tell us whether we find here certain rules or a pure chaos. [According to Russell] logic and mathematics deal only with the order of concepts and should not care about the order or disorder of objects. As long as one follows this line of conceptual analysis the empirical entity always escapes one's rational

[5] Alternatively one may consider a variety of "Non-Classical Mathematics" each based on its proper logic (Vasyukov 2009).

> understanding. The more mathematical deduction demonstrates us its virtue and its power, the less we can understand the crucial role of deduction in the theoretical natural sciences. (Cassirer 1907, p. 43)

So, according to Cassirer, what the formal logical foundations of mathematics can *not* possibly provide (whatever system of formal logic is one's favorite) are the notions of objecthood and objectivity appropriate for doing the modern mathematically-laden empirical science (i.e., the *Galilean* science as I called it Sect. **4.3** above). The popular idea to equate the notion of object with that of logical individual, which stems from Frege (Parsons 2008), not only leaves this problem open and but also hides it by eliminating a useful terminological distinction, which helps Kant to distinguish objects of possible experience from thought-things of other sorts. Although Cassirer does not provide any concrete solution of this problem he stresses the relevance of Kantian approach to the modern science in the following words (the second phrase I used as an epigraph to this book):

> The principle according to which our concepts should be sourced in intuitions means that they should be sourced in the mathematical physics and should prove effective in this field. Logical and mathematical concepts must no longer produce instruments for building a metaphysical "world of thought": their proper function and their proper application is only within the empirical science itself. (Cassirer 1907, pp. 43–44)

The fact that the modern logic indeed tends to create "metaphysical worlds of thought" rather than make itself into a part of empirical science, and that today's mainstream philosophy of logic encourages and justifies this overtly metaphysical tendency (usually by presenting it as an innocent intellectual game), appears to me very worrying. Although the hostile attitude towards logic and its mainstream philosophy, which is widely spread in mathematical circles, demonstrates a healthy intellectual reaction, such a negative reaction by itself does not solve the problem. In Chap. **5** I come back to this Cassirer's argument and after Lawvere point to a way out (Sect. **5.8**).

3.3 Axiomatization of Logic: Logical Form Versus Symbolic Form

In his address of 1917 already quoted above Hilbert says among other things the following:

> [I]t appears necessary to axiomatize logic itself and to prove that number theory and set theory are only parts of logic. This method was prepared long ago (not least by Frege's profound investigations); it has been most successfully explained by the acute mathematician and logician Russell. One could regard the completion of this magnificent Russellian enterprise of the axiomatization of logic as the crowning achievement of the work of axiomatization as a whole. (Hilbert 1918, p. 1113)

Leaving now aside the purported reduction of number theory (arithmetic) and set theory to logic let us focus on the idea of *axiomatization of logic*. By calling the axiomatization of logic the "crowning achievement of the work of axiomatization as a whole" Hilbert suggests that the axiomatization of logic is a continuous extension of the axiomatization of geometry, arithmetic and of any other part of mathematics or natural science. However the notion of axiomatization, which I have tried to reconstruct above on the basis of Hilbert's *Foundations* of 1899 does not immediately allow for such an extension. In the nutshell the axiomatization in the sense of *Foundations* of 1899 works like this: using some fixed logical vocabulary one produces a finite list of axioms, which refer only to abstract objects and abstract relations; an intended "naive" interpretation of these axioms and of all theorems derivable from these axioms is supposed to capture the content of the corresponding informal theory in a more precise and "logically clear" form. Notice that this whole procedure applies logic as a tool; an axiomatizer needs to have this tool in a ready-made form just like a carpenter needs a ready-made hammer for putting down a nail. So if the above reconstruction of Axiomatic Method is correct in order to axiomatize logic one needs to use logic. How this may possibly work?

Instead of speculating further on this matter let us see how Hilbert axiomatizes logic in his course on *Theoretical Logic* (Hilbert and Ackermann 1928) co-authored with Ackermann and first published in 1928. This work is greatly influenced by earlier works by Frege and Russell; I shall not however trace here these influences but consider Hilbert's and Ackermann's book on its own rights. The *Introduction* to this book opens with the following words:

> *Mathematical logic*, also called *symbolic logic* or *logistic*, is an extension of the formal method of mathematics to the field of logic. It employs for logic a symbolic language like that which has long been in use to express mathematical relations. In mathematics it would nowadays be considered Utopian to think of using only ordinary language in constructing a mathematical discipline. The great advances in mathematics since antiquity, for instance in algebra, have been dependent to a large extent upon success in finding a usable and efficient symbolism. (quoted after English translation Hilbert and Ackermann 1950, p. 1)

We see that from the very beginning Hilbert and Ackermann introduce here a new kind of logic, which they call mathematical or symbolic.[6] As we shall shortly see Hilbert's notion of axiomatization of logic makes sense *only* in a symbolic setting. The following description of mathematical (symbolic) logic as an "extension of the formal method of mathematics to the field of logic" is puzzling. If by formal method one understands the Axiomatic Method in the sense of Hilbert's *Foundations* of 1899 then it is unclear how this application can make logic symbolic. Indeed, Hilbert's *Foundations* of 1899 is written with the usual mixture of informal prose, geometrical diagrams and the traditional algebraic and geometrical

[6] Saying that symbolic logic is a "new" kind of logic I mean that this kind of logic is new with respect to the "informal" logic used in Hilbert's *Foundations* of 1899; I don't mean, of course, that symbolic logic first appears in Hilbert and Ackermann's book. In a part of the *Introduction* to this book, which I do not quote here, the authors provide a brief historical sketch of symbolic logic tracing its history back to Leibniz.

symbols; Hilbert's *formal* approach developed in this book is no more symbolic than the approach taken in any other elementary geometry textbook published in the nineteenth century.

Notice also that in the above passage Hilbert talks about application of the "formal method of mathematics" in logic. So he thinks here about the formal method of mathematics as something established independently from logic and then suggests to "extend" this method to the new field of logic. However the formal method of *Foundations* of 1899 is certainly not independent of logic. So talking about "formal method" in the above quote Hilbert and Ackermann mean something different. What is then this other formal method?

Hilbert's reference to *symbolic algebra* provides an important hint. Unlike the notion of logical form that can be understood after Trendelenburg (1862) quite independently of any symbolic representation, the notion of *algebraic form* was intimately connected to mathematical symbolism throughout the Modern history of algebra. Descartes' *Geometry* of 1637 (Descartes 1886) which first established algebra as a field of theoretical research at the same time produced what Serfati recently called a "symbolic revolution" in mathematics (Serfati 2005). The key idea of Descartes algebra is the following: the same syntactic operations with symbols may represent geometrical operations with straight segments and arithmetical operations with numbers. In this sense algebraic operations are general *forms* of operations shared by certain arithmetical and geometrical operations.[7] The role of symbolism is crucial here. Although an abstract notion of operation with abstract non-specified operanda may make logical sense it can hardly make *mathematical* sense. The algebraic symbolism invented by Descartes allowed for thinking of abstract operations (applicable both in geometry and arithmetics) in terms of concrete syntactic operations. This idea was used by Boole and other pioneers of symbolic logic. We shall shortly see how Hilbert uses this idea in his new approach to foundations of mathematics.

So at least one thing that Hilbert most certainly had in mind talking in the above passage about the "formal method of mathematics" and suggesting an application of this method in logic is the method of (symbolic) algebra. However in the above passage he describes algebra only as a special case. This is why we cannot derive the wanted sense of being formal from the notion of algebraic form. A more general notion of form, which turns to be appropriate in this case, is Cassirer's notion of *symbolic form* (Cassirer 1923–1925). I shall not develop it here in its full generality but focus only on its mathematical version relevant to Hilbert's work.

[7]Beware that this interpretation of algebra is anachronistic. Arnauld in his *New Elements of Geometry* (Arnauld 1683) suggests that algebraic operations are operations with general mathematical magnitudes; his notion of magnitude generalizes upon geometrical magnitudes and arithmetical magnitudes aka numbers. The modern "abstract" approach in algebra, which is behind the anachronistic reading of Descartes, has been first systematically developed by van der Waerden in the early 1930s (van der Waerden 1930–1931).

The passage quoted above continues as follows:

> The purpose of the symbolic language in mathematical logic is to achieve in logic what it has achieved in mathematics, namely, an exact scientific treatment of its subject-matter. The logical relations which hold with regard to judgments, concepts, etc., are represented by formulas whose interpretation is free from the ambiguities so common in ordinary language. The transition from statements to their logical consequences, as occurs in the drawing of conclusions, is analyzed into its primitive elements, and appears as a formal transformation of the initial formulas in accordance with certain rules, similar to the rules of algebra; logical thinking is reflected in a logical calculus. This calculus makes possible a successful attack on problems whose nature precludes their solution by purely contentual [*inhaltlische*] logical thinking. Among these, for instance, is the problem of characterizing those statements which can be deduced from given premises. (Hilbert and Ackermann 1950, p. 1)

The first sentence of this passage clearly shows that Hilbert considers here an application of mathematics to logic as a way to improve on logic with mathematics. Hilbert and Ackermann claim here that by using the symbolic methods mathematics achieves "an exact scientific treatment of its subject-matter"; using this evidence the authors suggest that these methods may equally allow for an exact scientific treatment of logic. This project should be certainly distinguished from the idea of improving on mathematics through the clarification of its logical structure purported by Hilbert in his *Foundations* of 1899. Nevertheless Hilbert tends to describe both projects in similar terms, namely in terms of formalization and axiomatization. Notice the expression "contentual logical thinking" that appears in the above passage. *Contentual* logical thinking in this context is opposed to *formal* logical thinking. Interestingly, Hilbert and Ackermann themselves do not use the expression "formal logic" in this context and talk instead of mathematical logic and symbolic logic. This can be perhaps explained by the fact that at that time the expression "formal logic" was still more commonly used in Trendelenburg's sense, which does not imply that formal logic should always involve symbolic methods. However when Hilbert and Ackermann oppose what they call "contentual" logic to symbolic logic the qualification of this symbolic logic as *formal* anyway immediately suggests itself. This is a different sense of being formal, which is more familiar to us today. What Hilbert and Ackermann call "contentual" logic would be described today as "informal" logic (even if this informal logic qualifies as formal in the weaker Trendelenburg's sense).

Thus Hilbert and Ackermann's formalization and axiomatization of logic amounts to providing the given system of logic with a new *symbolic* form, not to the specification of the "logical form of logic". Even if the authors describe the axiomatization of logic as "the crowning achievement of the work of axiomatization" this "crowing achievement" is not a simple continuation of the axiomatization in the sense of Hilbert's Foundations of 1899. Blurring the distinction between axiomatization of mathematical theories, on the one hand, and the axiomatization of logic, on the other hand, also blurs the distinction between theories and logic, which is fundamental for the 1899 approach. Hilbert and Ackermann's distinction between the "contentual logic" and the formal (symbolic) logic opens the new possibility of non-standard interpretation of logical signs on equal footing with non-logical signs. In Sect. **4.4** we shall see how this new possibility is realized in Tarski's topological model of intuitionistic propositional calculus; in Sect. 5.4 I show how a further exploration of this possibility leads to a significant change of the Axiomatic Method as presented in Hilbert's *Foundations* of 1899. But the conceptual problem that leads to this further development can be seen already at this early stage of the history of Axiomatic Method.

A fundamental idea behind the Axiomatic Method in the sense of 1899 is a reduction of mathematical reasoning to the form of "purely logical axiom system" (Hintikka). But since the distinction between logical systems and mathematical theories is blurred such a reduction becomes senseless because there is no longer any clear sense in which a given axiom system can be called purely logical rather than simply mathematical. One can however clearly distinguish in this new symbolic setting between formal (to wit symbolic) and contentual theories, and like in 1899 think of formal theories as a foundation for the corresponding contentual theories. Given such a contentual theory T one can design its formal counterpart F and then find another contentual interpretation T' of F. Although it is natural to describe this latter procedure as formalization and axiomatization (assuming that F is built axiomatically) this latter kind of formalization and axiomatization is quite unlike the formalization and axiomatization in the sense of 1899. While the formalization in the sense of 1899 involves the notion of logical form the symbolic formalization just described involves, generally, only the notion of symbolic form.

In Hilbert's thinking both kinds of formalization are merged together, so he hardly distinguishes between them clearly. He apparently assumes that a formal symbolic logical system unlike a formal symbolic mathematical theory does not allow (and does not call for) for multiple alternative contentual interpretations but instead simply clarifies and purifies common vague contentual logical notions expressed in the natural language. This additional assumption apparently allows for systems of formal symbolic logic with a fixed semantics of logical terms. But in fact this assumption produces a tacit shift in the meaning of being formal. If the given symbolic logical system pins down the precise sense of logical notions, which outside the symbolic setting don't have any clear meaning, then the logical symbols used in this logical system are used as proper names of corresponding logical concepts (like the symbol and conventionally used for denoting the logical conjunction) rather than variables that may acquire different interpretations.[8] In mathematics symbols are used in this way when, for example, number π is denoted by symbol "π" and number 1 is denoted by symbol "1". So the only notion of symbolic form, which is relevant in this case, is very unspecific and applies to any natural or artificial language. A notion of symbolic form, which is more specific and more relevant to logic and mathematics comes into the play *only* when a formal mathematical theory presented by symbolic means allows for alternative contentual interpretations: in this case one can say that the formal symbolic theory grasps

[8] I am now talking about variables in the general non-technical sense. Non-logical constants of a given formal theory (in the usual technical sense of the term) also count as variables in this general sense because such constants are differently interpreted in different models of this theory.

the symbolic form shared by all such interpretations. In the case of the system of symbolic logic proposed by Hilbert and Ackermann (see below) this latter specific sense of being formal does not apply. Apparently Hilbert and Ackermann continue to think of their logic as being formal in the traditional Trendelenburg's sense, and do not pay attention to the fact that this traditional sense of being formal does not square with the formal vs. contentual distinction relevant to mathematical theories.

In Sect. 9.3 we shall see that Tarski's analysis of logicality mentioned in the last Section allows for a reconstruction of Hilbert's thinking, which is more coherent that the above reconstruction based on Hilbert's own words. It is hard to say which of the two reconstructions is more historically correct and I leave this question for a further study. (I would like the Tarski-based structuralist interpretation to be correct but I lack sufficient textual evidences.) It seems me likely that Hilbert was indeed driven by structuralist geometrical ideas described in Sect. 9.3 but since he had to apply a more traditional conceptual apparatus for talking about logic, this led him to incoherences that we noticed in the above quote.

Let us now see what kind of axiomatic system of logic Hilbert and Ackermann offer in their book. In fact the authors offer several such systems; for our analysis it is sufficient to consider the simplest one that the authors call *sentential calculus* (Aussagenkalkül) and that is usually called today *propositional calculus*. The given axiomatic presentation of this system of symbolic logic consists of four elements (this analysis into elements is due to myself but not to the authors):

1. Specification of *symbols*, types of symbols, and of the intended interpretations of these symbols. Hilbert and Ackermann distinguish symbols of three types: (i) capital letters standing for propositional variables, (ii) logical symbols, (iii) auxiliary symbols like parentheses and commas, which serve for separating and grouping other symbols.
2. Specification of *syntactic rules* according to which well-formed *formulas* (strings of symbols) are constructed from the specified symbols. These formulas stand for propositions obtained from some given propositions (represented by singular symbols) with a help of logical connectives (also represented by corresponding symbols). So linking propositions by logical connectives is represented here by concatenation of the corresponding symbols into a single string and inserting appropriate auxiliary symbols in a way similar to which this is done with the usual linear alphabetic writing.[9]
3. *Axioms* of this logic, which are distinguished formulas interpreted as logical tautologies, i.e., propositions, which are true for all possible values of variables.
4. Specification of *logical rules*, which allow one to construct certain formulas (representing propositions) from some other formulas (representing other propositions). This construction is interpreted as logical deduction. The set of formulas

[9]Calling the linear alphabetic writing "usual" I mean, of course, that it is usual in the geographic area where I live.

deducible from the axioms according to logical rules 3 is interpreted as the set of (all) logical tautologies.

Let us now see whether or not this "axiomatization" of propositional logic is similar to the axiomatization of Euclidean geometry in the *Foundations* of 1899. Is this the same notion of axiomatization that is at work in both cases? A common feature is this: in both cases we have a generic set of propositions called axioms and certain rules that allow one to deduce some other propositions from these axioms. Here however the analogy between the axiomatic geometry and the axiomatic logic stops. The axiomatic theory of geometry has a logical part and a properly geometrical part. The properly geometrical part is the geometrical axioms and geometrical theorems deduced from these axioms. All the rest (including the rules of deduction) is logic. The considered axiomatic theory of logic consists of logical axioms (generic tautologies), all other tautologies (deducible from the axioms) *and* rules of two sorts plus the specification of symbols. If logical truths (tautologies) are thought of as universal truths about everything and geometrical truths are thought of as truths about some specific kind of things called geometrical objects then the comparison between the axiomatic geometry and the axiomatic logic may make sense.

It is appropriate to notice here that the specification of certain tautologies as "logical axioms" used by Hilbert and Ackerman's in their axiomatization of logic is not necessary for a formal specification of a formal logical system. As has been shown by Gentzen (1934, 1935) a reasonable formal logical system may have no axiom at the expense of having a bigger number of rules. But there is no logical system that has axioms and has no rules. In that sense rules is a more essential element of a logical system than logical axioms. This fact emphasizes my point that the "axiomatization of logic" should not be taken on equal footing with the axiomatization of geometry or axiomatization of any other special theory. What really matters in Hilbert's "axiomatization of logic" is developing logic by symbolic means. As I have just mentioned this can be done without using axioms.

3.4 *Foundations* of 1927: Intuition Strikes Back

As we have seen the formalization of logic (that Hilbert calls it "axiomatization") is not an innocent procedure which merely makes logical notions clearer and more explicit; whether it has this effect or not the formalization allows for studying and developing logic by mathematical means. Not surprisingly, the replacement of the traditional non-mathematical "informal" logic by the mathematical symbolic logic has a very significant impact upon Hilbert's ideas about Axiomatic Method and foundations of mathematics. Let us now see how his new foundational project looks like. In the beginning of his paper *Foundations of Mathematics* (Hilbert 1927) that has been delivered in July 1927 at the Hamburg Mathematical Seminar, Hilbert describes this project in the following words:

> With this new way of providing a foundation for mathematics, which we may appropriately call a proof theory, I pursue a significant goal, for I should like to eliminate once and for all the questions regarding the foundations of mathematics in the form in which they are now posed, by turning every mathematical proposition into a formula that can be concretely exhibited and strictly derived, thus recasting mathematical definitions and inferences in such a way that they are unshakable and yet provide an adequate picture of the whole science. I believe that I can attain this goal completely with my proof theory, even if a great deal of work must still be done before it is fully developed.
>
> No more than any other science can mathematics be founded by logic alone; rather, as a condition for the use of logical inferences and the performance of logical operations, something must already be given to us in our faculty of representation, certain extralogical concrete objects that are intuitively present as immediate experience prior to all thought. If logical inference is to be reliable, it must be possible to survey these objects completely in all their parts, and the fact that they occur, that they differ from one another, and that they follow each other, or are concatenated, is immediately given intuitively, together with the objects, as something that neither can be reduced to anything else nor requires reduction. This is the basic philosophical position that I regard as requisite for mathematics and, in general, for all scientific thinking, understanding, and communication. And in mathematics, in particular, what we consider is the concrete signs themselves, whose shape, according to the conception we have adopted, is immediately clear and recognizable. This is the very least that must be presupposed; no scientific thinker can dispense with it, and therefore everyone must maintain it, consciously or not. (Hilbert 1927, pp. 464–465)

When Hilbert says that mathematics cannot be "founded by logic alone" a modern reader acquainted with Hilbert's Axiomatic Method readily agrees: of course, for doing mathematics one needs in addition to principles of logic some specific mathematical axioms like axioms of set theory! As we shall shortly see Hilbert indeed uses such specific axioms in his *Foundations* of 1927. But in the above passage he refers to something completely different! He states here that no logical inference is possible without "certain extralogical concrete objects that are intuitively present as immediate experience prior to all thought" and then specifies that as far as mathematics is concerned those "extralogical concrete objects" are "the concrete signs themselves, whose shape, according to the conception we have adopted, is immediately clear and recognizable". Since mathematical symbolic logic does use concrete signs (symbols) Hilbert's "logic alone" cannot be mathematical; in the given context the *mathematical* logic should be rather understood as the pure logic provided with certain "extralogical" (to wit symbolic) means. According to the new Hilbert's view the immediate intuitive giveness of the "concrete signs", which allows one to acknowledge "the fact that they occur, that they differ from one another, and that they follow each other, or are concatenated" is an indispensable ingredient of foundations of mathematics. For further references I shall call this specific sort of mathematical intuition, which allows one to manipulate and calculate with mathematical symbols, the *symbolic* intuition.

Let us compare Hilbert's view on foundations expressed in the above passage with his earlier views expressed in his comments on his *Foundations* of 1899. In 1899 he founds geometry on the "pure" (non-mathematical) logic and some axioms formulated in terms of this logic. The only notion of intuition, which is still indispensable in this setting, is the intuition in the sense of Kant's *Doctrine* *of Method*; this *minimal* intuition can be accounted for by the logical notion of *existential instantiation* and be considered itself as a part of logic (see Sect. **2.3** above). In 1927 Hilbert no longer relies on the "pure" informal logic but stresses the foundational impact of symbolic intuition. Hilbert explicitly describes here symbols as "extralogical"; the following explanation does not allow one to reduce the notion of intuition related to these extralogical objects to the minimal "logical" intuition. Indeed, while the mere fact that these objects "occur" and "differ from one another" does not yet make them extralogical, the fact that "they follow each other, or are concatenated" certainly does! Thus Hilbert's new foundational proposal of 1927 unlike that of 1899 essentially involves a non-logical notion of symbolic intuition.

This does not mean however that by 1927 Hilbert abandon his earlier idea according to which all mathematical theories require a logical background. He rather upgrades this idea as follows: a system of logic, which is appropriate for founding mathematics, is not a system of "pure" (non-mathematical) logic but a system of symbolic mathematical logic (which includes an extra-logical symbolic aspect). Here is how Hilbert describes this upgrade himself:

> [I]n my theory contentual inference is replaced by manipulation of signs [ausseres Handeln] according to rules; in this way the axiomatic method attains that reliability and perfection that it can and must reach if it is to become the basic instrument of all research. (Hilbert 1927, p. 467)

The replacement of the "contentual inference" by the manipulation of signs involves two ways of formalization, which work here together but nevertheless can and should be carefully distinguished. The formalization in the sense of 1899 remains here at work, so the manipulation of signs presents here the *logical* form of the given contentual inference. Simultaneously the manipulation with signs presents the *symbolic* form of the same contentual inference.

Since Hilbert makes it clear that his new method amounts to "extending the formal point of view of algebra to all of mathematic" (ibid. p. 470) we may safely identify the symbolic form with the algebraic form in the given context. However Hilbert's proposal does not reduce to the algebraization of logic the same sense, in which one can speak of the algebraization of logic in earlier works by Boole, Morgan and others. For Hilbert uses a feature of algebra that plays no special role in earlier works in mathematical logic: I mean the algebraic method of "ideal elements" like -1 or $\sqrt{-1}$. Leaving a more detailed discussion on this algebraic method until Sects. **3.6** and **8.4** let me first describe Hilbert's proposal. After the introductory remarks quoted above he first introduces a system of symbolic logic similar to one presented in Hilbert and Ackermann (1928) and, second, adds two further groups of axioms, which he describes as "specifically mathematical", namely "axioms of equality" and "axioms of number". Then Hilbert shows how this apparatus allows one to do the finitary arithmetic. One may wonder if doing the finitary arithmetics with this heavy logical machinery provides any epistemic advantage over doing it in the traditional way. Hilbert's answer is: No, it does not! As far as the finitary arithmetic is concerned this machinery allows one at best to "impart information".[10] If I understand here Hilbert correctly his thinking is this: since usual arithmetical manipulations with natural numbers represented by strings of strokes or by the standard Arabic numerals are just as intuitively clear as the manipulation of symbols and formulas in the Hilbert's symbolic system, from the foundational viewpoint the difference between the two formalisms is after all not essential (notwithstanding the fact that the former formalism has an advantage of being simpler and more convenient, while the latter formalism has an advantage of making explicit the logical structure of reasoning). However the new proposed formalism is advantageous as soon as one goes beyond the finitary arithmetic. Hilbert suggests thinking about such an extension after the pattern of algebraic extension:

> Just as, for example, the negative numbers are indispensable in elementary number theory and just as modern number theory and algebra become possible only through the Kummer-Dedekind ideals, so scientific mathematics becomes possible only through the introduction of ideal propositions. (Hilbert 1927, p. 471)

An "ideal proposition" is any proposition that is not provable from Hilbert's logical and arithmetical axioms, i.e., any proposition, which is not a proposition of the finitary arithmetic. So any additional axiom and any formal proposition obtained as a formal consequence of the extended axiom system (which includes the same logical and arithmetical axioms plus the new axiom) qualifies as ideal. The only requirement that limits such possible extensions is the requirement according to which the extended system of axioms must be *consistent*. As soon as the consistency is granted one may safely think of "ideal" objects and "ideal" relations involved into the given ideal proposition as *existent* along the same pattern of thinking, which we have already explained talking about the *Foundations* of 1899 (remind from Sect. **3.1** of thought-things and thought-relations). And in fact one can do more. Since these ideal objects and relations are represented by symbols and strings of symbols, which (unlike the bare thought-things and thought-relations) are bone fide mathematical objects on their own, any further interpretation of these ideal things is an option but not a necessary requirement. In the new symbolic setting these ideal things are concretely represented to begin with, and one may work with them just like in algebra people work with $\sqrt{-1}$. Crucially, working with ideal objects and relations involves the same type of syntactic manipulations as calculating with natural numbers. So even if Hilbert's *Foundations* of 1927 is an overkill in the case of the finitary arithmetic it allows for a uniform treatment of the whole of mathematics by means similar to those used in the finitely arithmetic.

The possibility of checking consistency is evidently crucial for Hilbert's project. Although in 1927 Hilbert offers no general solution of this problem he suggests that this problem is relatively easy and "fundamentally lies within the province of intuition just as much as does in contentual number theory the task, say, of proving

10 If we now begin to construct mathematics, we shall first set our sights upon elementary number theory; we recognize that we can obtain and prove its truths through contentual intuitive considerations. The formulas that we encounter when we take this approach are used only to impart information. Letters stand for numerals, and an equation informs us of the fact that two signs stand for the same thing. (Hilbert 1927, p. 469)

the irrationality of $\sqrt{2}$" (Hilbert 1927, p. 471). In the formal symbolic setting where a proof is represented by a string of symbols and formulas are constructed according to precise syntactic rules the proof of consistency of a given set of axioms amounts to a proof showing that there is no string of formulas that ends up with a formula expressing contradiction like $0 \neq 0$ (a simple argument shows that if $0 \neq 0$ cannot be formally proved no other contradiction can be proved either). Hilbert realizes, of course, that such a consistency proof will not itself qualify as *formal* but will belong to his *proof theory*, which in a different place (Hilbert and Bernays 1934–1939) Hilbert calls by the name of *metamathematics*. However since the whole of metamathematics "fundamentally lies within the province of intuition just as much as does in contentual number theory" this remark does not lead to the infinite regress in foundations. Thus the intuitive proof theory aka metamathematics (in Hilbert's original sense of this term) in Hilbert's view of 1927 becomes a foundation for the rest of mathematics. Beware that so far this is a declaration of intent, not yet an accomplished project. A more advanced version of the same project is presented in two volumes (Hilbert and Bernays 1934–1939) published by Hilbert with a cooperation with Bernays in 1934–1939.

As we all well know today in 1927 Hilbert severely underestimated potential difficulties of his proof theory; Gödel's famous incompleteness theorems and all the following work in the area convinced many people that Hilbert's Program failed (Zach 2003). However even if Hilbert's foundational project as described in the *Foundations* of 1927 indeed failed, his Axiomatic Method making part of this program certainly survived and until today remains standard.

3.5 Symbolic Logic and Diagrammatic Logic

What kind of things qualify as symbols? Should they always be the convenient letters written from the left to the right or other systems of writing can be used to the same effect? Is the conceptual structure of the symbolic logic neutral with respect to its symbolic presentation or it is at some degree determined by this presentation? Can one open new logical possibilities by modifying this standard syntax? As every user of LaTeX perfectly knows various sorts of complicated non-standard multi-level symbols and diagrams can be encoded into the convenient strings of letters. Tolstoy's printed novels communicate us all sort of things using the same type of linear coding. But this useful feature of the standard symbolic syntax hardly justifies by itself Hilbert's far-reaching idea of the ultimate epistemic reduction of mathematical intuition to the symbolic intuition associated with the symbolic syntax of this particular sort (moreover if one takes into consideration the failure of Hilbert's program in its original form). Instead of trying to answer the above questions directly I propose now to look at some other syntactic possibilities known from the history of symbolic logic. Namely, I shall talk about logical diagrams after Venn's of 1881 where this subject is treated systematically. Then I come back to Hilbert and make some further comments about his idea of formal symbolic mathematics.

Here is how Venn in 1881 describes the complementary roles of symbols and diagrams in logic talking about "subdivisions of classes":

> For one thing, we can of course always represent the products of such a subdivision in the language of common [non-symbolic] Logic, or even in that of common life, if we choose to do so. They do not readily offer themselves for this purpose, but when pressed will consent, though failing sadly in the desired symmetry and compactness. The relative cumbousness of such a mode of expression is obviously the real measure of our need for a reformed or symbolic language. [..] The reader will see at once how conveniently and briefly we can thus indicate any desired combination of class terms, and, by consequence, any desired proposition. [..]
>
> That such a scheme is complete there can be no doubt. But unfortunately, owing to this very completeness, it is apt to prove terribly lengthy. [..] This then is the state of thing which a reformed scheme of diagrammatic notation has to meet. It must correspond in all essential respects to that regular system of class subdivision which has just been referred to under its verbal and its literal or symbolic aspect. Theoretically, as we shall see, this is perfectly attainable. (Venn 1881, pp. 102–103)

As we can see Venn discusses here the use of symbols and diagrams in logic from a practical rather than theoretical viewpoint. Unlike Hilbert Venn does not try to distinguish his symbolic logic from the "pure" logic; instead he distinguishes between the symbolic logic and the "common" logic meaning the "informal" logic that applies only the natural language without helping itself with special symbols. Then Venn stresses that diagrams become helpful when the corresponding symbolic expressions turn to be "terribly lengthy". Does the length of symbolic expression play any role from a theoretical or foundational viewpoint? Hilbert could argue that the length of symbolic expressions is irrelevant to the *theoretical* logic and to the foundations of mathematics (as far as this length remains finite) even if it matters practically. I am not convinced by this argument. Hilbert's project aims at reduction of the "ideal" objects and propositions to their "real" counterparts, i.e., to their corresponding symbolic expressions. The idea that a long symbolic calculation is just as reliable as a short one is obviously a simplifying idealization. Even if this idealization is acceptable for certain purposes there is no reason to disregard a more realistic picture in a theoretical study. If Venn is right that diagrams help to tackle with the complexity of symbolic logical operations the diagrammatic notation must be taken as seriously as the symbolic notation.

It may be argued that symbols unlike diagrams do not involve the idea of *resemblance* to what these things stand for, and that this fact makes symbols appropriate and diagrams inappropriate in foundations of mathematics. In old good times, so the argument goes, when mathematics dealt with circles, triangles and the like people could picture these things with diagrams and use these diagrams in their proofs. However since the modern mathematics involves highly abstract concepts, which do not allow for such a straightforward intuitive representation, diagrams become irrelevant. If such abstract mathematical concepts allow for any intuitive representation at all such representations are purely *symbolic* and do not involve any relation of resemblance between symbolic constructions their corresponding mathematical objects.

This argument is based on several misunderstandings. The idea that traditional geometrical diagrams in some sense *resemble* certain ideal objects is a Platonic interpretation of the traditional geometrical practice that can and must be distinguished from this practice itself. One does not need this interpretation for doing traditional geometry with traditional geometrical diagrams. Notice that Venn's logical diagrams just like Venn's and Hilbert's logical symbols are not supposed to resemble anything at all.

The idea that symbolic constructions do *not* resemble anything should be also taken critically. In my view the relation of resemblance between mathematical objects and their material representations is ill-construed to begin with. We can rather establish such a relations between different material representations of the same object. For example an Euclidean circle can be represented both with the traditional diagram (found in Sect. **7.2** in this book) and also symbolically with a single letter. Then one may observe that each of these two representations is unlike the other and further observe that a letter used for denoting the circle is unlike any other letter that can be used for the same purpose. Here we can see the difference. The capacity to read diagrams and the capacity to read letters equally require the capacity to distinguish between graphical shapes and to identify tokens of the same shape. But in the case of diagrams this graphical typing reflects the typing of corresponding mathematical objects while in the case of letters it does not. In that particular respect diagrams are more informative than letter symbols (although letters, of course, have other epistemic advantages). However I cannot see any general philosophical justification of the idea that using graphical types in one way rather than in another way is more appropriate in mathematics and logic. Which one is more appropriate in a given context is rather a technical question.

Let us read again more attentively what Hilbert says in the passage quoted in the last Section:

> [A]s a condition for the use of logical inferences and the performance of logical operations, something must already be given to us in our faculty of representation, certain extralogical concrete objects that are intuitively present as immediate experience prior to all thought. If logical inference is to be reliable, it must be possible to survey these objects completely in all their parts, and the fact that they occur, that they differ from one another, and that they follow each other, or are concatenated, is immediately given intuitively, together with the objects, as something that neither can be reduced to anything else nor requires reduction. (Hilbert 1927, pp. 464–465)

Hilbert describes the above claim as his "basic philosophical position that I regard as requisite for mathematics and, in general, for all scientific thinking, understanding, and communication". Yet one may remark that in this claim Hilbert mentions objects that "follow each other" and "are concatenated" which is hardly appropriate in a claim aiming at the full philosophical generality because the notions of order ("following each other") and concatenation are applicable to objects of some sorts but may be not applicable to objects of some other sorts. If Hilbert would like to claim here that the order and the concatenation of the "extralogical concrete objects" are indeed *necessary* in "all scientific thinking, understanding, and communication" he would need to provide an additional argument justifying this latter claim. The following lines make it clear that Hilbert has here in mind nothing else but the familiar symbolic algebraic notation:

> And in mathematics, in particular, what we consider is the concrete signs themselves, whose shape, according to the conception we have adopted, is immediately clear and recognizable. This is the very least that must be presupposed; no scientific thinker can dispense with it, and therefore everyone must maintain it, consciously or not. (Hilbert 1927, p. 465)

Why signs rather than diagrams? Why the concatenations of signs rather than geometric constructions of other sorts? Well, Hilbert's project aims at showing that the manipulation with signs (symbols) is sufficient for all mathematical purposes. Even if this foundational project works out it does not close the possibility of a different foundation with uses a different intuitive background and a different basic geometry. I shall show in Sect. **4.3** that Hilbert's formal mathematics is not quite appropriate for applications in natural sciences and try to explain why. As soon as the application of mathematics in science counts as a purpose of doing mathematics this argument shows that the manipulation with signs in the way suggested by Hilbert is not sufficient for all mathematical purposes.

In order to see a possibility of Axiomatic Method, which involves other mathematical intuitions than symbolic, it is instructive to look back at Euclid's *Elements* from a particular point of view suggested by Friedman (1992). Friedman suggest this point of view as his reconstruction of Kant's view but this does not matter in the given context:

> Euclidean geometry [..] is not to be compared with Hilbert's axiomatization [of Euclidean geometry in his Hilbert 1899], say, but rather with Frege's *Begriffsschrift*. It is not a substantive doctrine, but a form of rational representation: a form of rational argument and inference. Accordingly, its propositions are established, not by quasi-perceptual acquaintance with some particular subject matter, but, as far as possible, by the most rigorous methods of proof – by the proof-procedures of Euclid, Book I, for example. There remains a serious question about Euclid's axioms, of course; when pressed, Kant would most likely claim that they represent the most general conditions under which alone a concept of extended magnitude – and therefore a rigorous conception of an external world – is possible (see A163/B204). And, of course, we now know that Kant is fundamentally mistaken here. (Friedman 1992, pp. 94–95)

Euclidean geometry can be equally compared in this sense with Hilbert's symbolic logic: "when pressed" Hilbert like Kant could say that unless the symbolic syntax of his logic is taken for granted one, strictly speaking, cannot reason mathematically. Or perhaps Hilbert's mathematical genius and the common sense would win in this case over his philosophical ideas and he would take a more flexible attitude claiming only a particular way of building foundations for mathematics and asking his opponent to do this better.

Although Euclidean geometry cannot any longer serve us as an organon of scientific reasoning I can see no reason why a theory capable to play this role today should be anything like a symbolic logical calculus. Moreover, I believe that if such a scientific organon is possible at all it must reflect objective features of the physical world learned through experience (as Euclidean geometry does it at a limited degree) rather than be based on speculative ideas about the correct thinking underpinned by some metaphysical theories. In Chap. 5 I present a tentative organon of this sort, which is Lawvere's categorical logic. (I shall tell more about Venn's diagrammatic logic during this discussion.) In the end of Chap. 7 I consider Voevodsky's Univalent Foundations as another candidate organon of the same sort. Now let us return back to Hilbert and see how his foundational project of 1927 develops in his later work.

3.6 *Foundations* of 1934–1939: Doing Is Showing?

Foundations of Mathematics (Hilbert and Bernays 1934–1939) published by Hilbert and Bernays in two volumes in 1934 (first volume) and 1939 (second volume) is a systematic technical development of the project outlined in Hilbert's paper of 1927. Leaving this technical development wholly aside I shall now focus only on the authors' discussion about "formal axiomatics" in the beginning of the first volume. Here is the passage of my interest:

> The term axiomatic will be used partly in a broader and partly in a narrower sense.We will call the development of a theory axiomatic in the broadest sense if the basic notions and presuppositions are stated first, and then the further content of the theory is logically derived with the help of definitions and proofs. In this sense, Euclid provided an axiomatic grounding for geometry, Newton for mechanics, and Clausius for thermodynamics.
>
> In Hilbert's *Foundations of Geometry* [of 1899] the axiomatic standpoint received a sharpening regarding the axiomatic development of a theory: From the factual and conceptual subject matter that gives rise to the basic notions of the theory, we retain only the essence that is formulated in the axioms, and ignore all other content. Finally, for axiomatics in the narrowest sense, the *existential form* comes in as an additional factor. This marks the difference between the *axiomatic method* and the *constructive* or *genetic* method of grounding a theory. While the constructive method introduces the objects of a theory only as a *genus* of things, an axiomatic theory refers to a fixed system of things (or several such systems), and for all predicates of the propositions of the theory, this fixed system of things constitutes a *delimited domain of subjects*, about which hold propositions of the given theory.
>
> There is the assumption that the domain of individuals is given as a whole. Except for the trivial cases where the theory deals only with a finite and fixed set of things, this is an idealizing assumption that properly augments the assumptions formulated in the axioms.
>
> We will call this sharpened form of axiomatics (where the subject matter is ignored and the existential form comes in) *formal axiomatics* for short. (quoted after bilingual edition Hilbert and Bernays 2010, pp. 1a–2a)

We have already discussed the distinction between formal and contentual axioms and I shall not return to it now but comment on the authors' distinction between the constructive (genetic) and the axiomatic methods of theory-building. My first comment concerns Euclid. As we can see in the above passage Hilbert and Bernays qualify Euclid's method as axiomatic in a "broader" sense. This "broader" sense of being axiomatic includes what Hilbert and Bernays call constructive or genetic method. This is clear from another description of Euclid's method that Hilbert and Bernays provide later in their book:

> Euclid's axiomatics was intended to be contentual and intuitive, and the intuitive meaning of the figures is not ignored in it. Furthermore, its axioms are not in existential form either: Euclid does not presuppose that points or lines constitute any fixed domain of individuals. Therefore, he does not state any existence axioms either, but only construction postulates. (ibid., p. 20a)

The above quote clearly shows that Hilbert is well aware about the difference between his and Euclid's approaches to theory-building, which I have emphasized in Sect. **2.2**.[11] The reference to *Foundations* of 1899 in the work of 1934 suggests that in 1934 Hilbert retains his earlier notion of Axiomatic Method. But as a matter of fact in 1934 Hilbert and Bernays use the *symbolic* version of this method. As we shall now see this fact has important consequences for the author's distinction between constructive (genetic) and axiomatic (in the narrow sense) ways of building mathematical theories.

Remind that in the symbolic setting mathematical proofs are chains of formulas constructed according to certain fixed rules. In this setting to *prove* (a proposition expressed by formula F) is to *construct* (a chain of formulas that ends up with F). Thus doing mathematics in Hilbert's formal axiomatic setting does not reduce contentual constructions in mathematics altogether but reduces all such constructions to constructions of a special sort, namely, to the finite symbolic constructions. Notice however that this reduction does not concern the *metamathematical* proofs including the proofs of consistency (of a given set of formal axiom). In the metamathematics everything works like in the traditional contentual mathematics developed by "genetic" methods. One not only performs here certain constructions (chains of formulas) but also makes certain judgements about these constructions, including judgements of the form "such-and-such construction is impossible" (remind that in order to prove the consistency of a set of axioms it is sufficient to show that formula $0 \neq 0$ is not derivable from these axioms). I would like to stress that this new turn of the dialectics of "doing" and "showing" is relevant only to the symbolic version of Hilbert's formal Axiomatic Method but not to the "informal" version of this method presented in Hilbert's *Foundations* 1899. Although all main features of *Foundations* of 1899 are present in the new *Foundations* of 1934, these new *Foundations* have some new features which significantly change the whole picture.

In this context I would like to consider more attentively Hilbert's algebraic motivation explained in his *Foundations* of 1927. Remind that Hilbert suggests thinking of his *ideal propositions* (represented by formulas) after the pattern of algebraic "ideal objects" like $\sqrt{-1}$. In Sect. **3.4** I have tried to explain the analogy (after Hilbert) but now I would like to stress a point where it fails: while in algebra manipulations with symbols represent manipulations with ideal objects in Hilbert's

[11]Nevertheless Hilbert and Bernays in a different place describe Euclid's (as well as Newton's and Clausius') method in this way: "the basic notions and presuppositions are stated first, and then the further content of the theory is *logically* derived with the help of definitions and proofs" (ibid., p. 1, my emphasis). As we have shown in Chap. **2** this description of Euclid's "genetic" method is incorrect. The fact that this method amounts to building certain non-logical objects like triangles from given basic objects according to certain rules is not a sufficient reason for calling it logical.

formal setting manipulations with symbols represent logical inferences and other logical operation, which allows one to *say* different things about ideal objects but not manipulate with them.

Let me demonstrate this point with two historical examples. Consider, first, the following interesting passage from Arnauld's *New Elements* of 1683:

> What cannot be multiplied by its nature can be multiplied through a mental fiction where the truth presents itself as certainly as in a real multiplication. In order to learn the distance covered during 10 hours by one who covers 24 lieu per 8 hours I multiply through a mental fiction 10 hours by 24 lieu that gives me the imaginary product of 240 hour times lieu, which I divide then by 8 hours and get 30 lieu. By the same mental fiction one multiplies a surface by another surface even if the product has 4 dimensions and cannot exist in nature. One may discover many truths through multiplications of this sort.
>
> People say that this is because the imaginary products can be reduced to lines. [..] But there is no evidence that [relevant] proofs depend on those lines, which are in fact wholly alien to them. (Arnauld 1683 pp. 38–39, my translation from French)

Traditionally (in particular, in the early Arab algebra) the product of two straight lines is construed as a rectangle having these given lines as its sides; the product of three lines is a solid but in order to form products of four and more linear factors in this geometrical way one needs higher dimensions, which according to Arnauld "cannot exist in nature". Nevertheless he is ready to consider such higher-dimensional geometrical constructions as useful fictions on equal footing with products of distances by time intervals, which don't have any immediate physical interpretation either but are demonstratively useful for calculations. In the last quoted paragraph Arnauld refers to Descartes' proposal to construct the multiplication of geometrical magnitudes differently, so the product of straight lines is again a straight line; in modern words Descartes' definition of multiplication of straight lines makes this operation algebraically closed. (Descartes uses an auxiliary line 1 as a unit and then considers similar triangles with sides $1, a, b, c$ such that $\frac{1}{a} = \frac{b}{c}$, which gives him the wanted definition of $c = ab$, see Descartes 1886) Arnauld finds this trick artificial and unnecessary. What makes him confident about higher-dimensional geometrical products and quasi-physical units like hour times lieu is the symbolic algebraic calculus supporting these otherwise problematics notions. Using this symbolic calculus one forms the product of four factors $p = abcd$ as easily as any product of two or three factors. One cannot easily imagine the product of two surfaces (just like one cannot give a physical sense to hours times lieu) but one can easily concatenate the string ab and the string cd and think of this operation as multiplication of two surfaces. Descartes' alternative definition of the geometrical product aims at providing a "clear and distinct" intuitive underpinning of this operation that avoids the talk of higher dimensions. Arnauld finds Descartes' construction of product unnecessary because in his eyes the symbolic calculus provides such an intuitive underpinning by itself. The "proofs" that Arnauld mentions in this context are nothing but symbolic calculations.

Now consider this passage from MacLaurin's *Treatise of Fluxions* of 1742 where the author shows how the symbolic algebra allows for operating with Newton's *fluxions* in a precise way in spite of the "obstruse" intuitive nature of those things.

> The improvement that have been made by it [the doctrine of fluxions] [..] are in a great measure owing to a facility, conciseness, and great extend of the method of computation, or algebraic part. It is for the sake of these advantages that so many symbols are employed in algebra. [..] [Algebra] may have been employed to cover, under a complication of symbols, obstruse doctrines, that could not bear the light so well in a plain geometrical form; but, without doubt, obscurity may be avoided in this art as well as in geometry, by defining clearly the import and use of the symbols, and proceeding with care afterwards. (quoted by Cajori 1929, v2, p. 330)

The above passages from Arnauld and MacLaurin well illustrate Hilbert's remarks in his *Foundations* of 1927 where he stresses an epistemic impact of the "formal method of albera" (see above). Let us however compare the above historical examples with Hilbert and Bernay's new symbolic axioms for plane Euclidean geometry found in the *Foundations* of 1934. This new set of axioms involves a single type of primitive objects called *points* (instead of two types specified in the *Foundations* of 1899) and two primitive ternary relations $Gr(x,y,z)$ and $Zw(x,y,z)$, which are informally interpreted as *points* x,y,z *lie on the same straight line* (*Gr* stands for German *Gerade*) and *point y lies between point x and point z* (*Zw* stands for German *zwischen*). The First Axiom reads:

$$(x)(y)Gr(x,x,y)$$

that translates into the prose as follows:

all points x,y *lie on the same line*

(prefix (x) in Hilbert's notation reads "for all x"). Using this axiom, other geometrical axioms expressed similarly, logical axioms and, finally, the appropriate deductive rules (which in the given symbolic setting are syntactic rules allowing for building new formulas from some given formulas) one may formally derive certain *theorems* (which are some new formulas generated from the axioms by the given rules). However this formal deduction does not allow for constructing any new *geometrical* object: remind that "an axiomatic theory refers to a fixed system of things (or several such systems), and for all predicates of the propositions of the theory, this fixed system of things constitutes a *delimited domain of subjects*, about which hold propositions of the given theory".[12] The only way of producing new *ideal objects* within the formal axiomatic framework is to suggest a new system of axioms and assure its consistency. This is very unlike the way, in which the *ideal objects* are produced in the traditional symbolic algebra. Arnauld first takes it for granted that the product of three straight lines is a parallelepiped; next he represents the straight lines by symbols a,b,c and represents their product (i.e.,

[12] One may suggest that this method allows for introducing new objects defined as classes (or sets) of points satisfying holding certain relations definable in terms of the primitive relations; for example a straight line can be tentatively defined as a class of points such that any three points x,y,z of this class hold relation $Gr(x,y,z)$. However in fact the formal deduction cannot be interpreted in this way because in the given setting a *class* of things does not count as a new thing. So one needs a background set theory for following this suggestion.

the parallelepiped) by the string *abc* (I modernize his notation slightly but this is not essential here). Then he considers string *abcd*, thinks how to interpret it geometrically and gets a vague idea of a 4-dimensional object that "cannot exist in nature". Since the algebraic rules of multiplication do not limit the number of factors 4-element strings turn to be just as well-manageable as 3-elements strings. Thus one may safely operate with 4-dimensional and *n*-dimensional parallelepipeds symbolically, no matter how "ideal" such things appear to the geometrical intuition. A symbolic construction, namely the concatenating of four primitive symbols (letters), represents here an ideal geometrical construction (the construction of 4-dimensional parallelepiped).

Hilbert's symbolic constructions unlike Arnauld's and MacLaurin's symbolic constructions represent not ideal constructions themselves but propositions and systems of propositions "about" ideal objects. This makes Hilbert's ideal objects (i.e., all mathematical objects except syntactic objects) fundamentally non-constructive like Plato's "ideal numbers" (see Sect. **6.4** below). One may stipulate their existence by adopting appropriate axioms (keeping in mind the consistency requirement) but one cannot construct them from simpler elements. In Sect. **4.3** I argue that this feature of Hilbert's formalism makes it inappropriate for building mathematical theories useful in sciences.

For further references I shall call the Axiomatic Method as it is described in Hilbert's *Foundations* of 1927 and implemented in the *Foundations* of 1934–1939 (Hilbert and Bernays 1934–1939) by the name of *Formal* Axiomatic Method and call the procedure of reconstruction of mathematics with this method the *formalization* of mathematics. It is essential for what follows not to confuse this specific notion of formalization with any other notion that can be occasionally called by the same name.

A modernized version of Formal Axiomatic Method, which includes basics of the modern model theory, is presented (together with a philosophical underpinning) in Tarski's textbook (1941). A more detailed historical study of the Axiomatic Method would require an analysis of this and many other similar works but this task is out of the scope of the present book. I focus my attention on Hilbert's work because for my purposes it is essential to analyze how ideas emerge and less essential to study how they solidify and become an orthodoxy.

Chapter 4
Formal Axiomatic Method and the Twentieth Century Mathematics

The Formal Axiomatic Method has been proposed by Hilbert about a century ago and it is appropriate to ask how it performed during the past century. It appears to me that its impact is somewhat controversial. On the one hand, during this time period the Formal Axiomatic Method was and still remains *the* standard method of theory-building in eyes of logicians and logically-minded mathematicians, physicists, biologists and philosophers. On the same side of the scale I put the progress in the logico-mathematical investigations (some of which use the title of *foundations of mathematics*), which apply this method in some form. Finally, in some form this method is widely used in the current educational practice. (I specify the relevant forms of the Axiomatic Method below in this Chapter.) But on the other hand, one can also observe that by this date the Formal Axiomatic Method had no significant effect either in the mainstream mathematics or in natural sciences, which remained largely "informal"; none of recent significant advances in mathematics (like the recent proof of Poincaré conjecture) used formal logical methods.

So the today's situation is somewhat schizophrenic. When Hilbert in his *Foundations* of 1927 suggested his Formal Axiomatic Method as the "basic instrument of all research" (Hilbert 1927, p. 467) he really meant it, and this clearly did not happen. At the same time most mathematicians and logicians (as well as many philosophers and a few physicists) agree that the Axiomatic Method is useful; some of these people also believe that this method is indispensable in their discipline. When they are asked what they really mean by Axiomatic Method they likely refer to the modern axiomatic set theory or to another example of axiomatic theory built by Hilbert's method. If we now ask mathematicians and physicists how it is possible that the Axiomatic Method plays an important role in their discipline without having any clearly visible effect on it the most popular answer will be likely this: the Axiomatic Method matters only in the *foundations* of science while the mainstream science cares very little about its own foundations. At that point philosophers and logicians join the discussion and argue like this. It is, of course, not normal that the foundations of your science are not properly taken care of. But this is understandable because you, guys, have a lot of other things to do. And perhaps you are not quite

A. Rodin, *Axiomatic Method and Category Theory*, Synthese Library 364,
DOI 10.1007/978-3-319-00404-4_4,

qualified for the job because the foundations is a somewhat philosophical and logical subject. So forget about foundations and leave this subject to us and give us in return some jobs at mathematical and physical departments. We shall take care about the foundations and you will do the rest.

I am not satisfied by this division of labor (even if I have a philosophical interest to mathematics and don't mind a job at a mathematical department) because, in my view, it produces a wrong notion of foundation of a discipline, which allows a foundation to be wholly detached from the discipline itself. A notion of foundation that seems me satisfactory is described by Lawvere and Rosebrugh in the following words:

> A foundation makes explicit the essential general features, ingredients, and operations of a science, as well as its origins and generals laws of development. The purpose of making these explicit is to provide a guide to the learning, use, and further development of the science. A "pure" foundation that forgets this purpose and pursues a speculative "foundations" for its own sake is clearly a nonfoundation. (Lawvere and Rosebrugh 2003, p. 235)

Having this Lawvere's notion of foundations of mathematics in mind I cannot reserve for Hilbert's Axiomatic Method a place in foundations of today's mathematics until I can see more clearly the role of this method in a broader mathematical context. In the following Section I consider the case of "speculative foundations", i.e., the twentieth century research of formal axiomatic set theories, and in the next Section the case of Bourbaki's mathematics, which is not wholly "speculative" in that sense because it has an important continuing impact on mathematical education and reflects some important features (even if ignores some other important features) of the mainstream mathematics of the second half of the twentieth century. We shall see that although in both cases the Formal Axiomatic Method plays a central role, in neither of these case this method is used as intended by Hilbert. In the second half of this Chapter I shall do two other things: first, put forward an argument explaining why the Formal Axiomatic Method is not quite appropriate for natural sciences and, second, discuss an unusual application of this method by Tarski, which I consider as a step towards the rethinking of Hilbert's Axiomatic Method in Lawvere's work.

4.1 Set Theory

Remind the notion of *system of things* required by the Formal Axiomatic Method. Can one provide a formal axiomatic theory of such *systems*? Unless one assumes that a system U of systems of things is an element of itself this project does not involve a circularity but provides a somewhat *restricted* notion of system of things (as an element of U) that can serve for developing various formal axiomatic theories on the top of the formal theory of U. This anachronistic description of Zermelo's idea to axiomatize set theory explains why and how the later development of Hilbert's formal approach to foundations of mathematics involved not only the Formal Axiomatic Method itself but also the axiomatic theory of sets.[1]

Since Zermelo's pioneering works in the axiomatic set theory the mainstream research in set theory focused on studies of various formal theories of sets and models of such theories. This makes set theory a rare and arguably the most important example of a modern mathematical theory developed wholly within a formal axiomatic setting. So in order to see how the Formal Axiomatic Method works in today's mathematics it is useful to consider the case of set theory quite independently from any foundational claims made about this theory. For being more concrete let us consider Cantor's Continuum Hypothesis (CH), which in 1900 has been listed by Hilbert (1902) as the number one among 23 open mathematical problems that Hilbert at that time judged to be the most important.[2]

CH is a very peculiar example of mathematical problem because today there is still no common opinion as to whether this problem is solved or still remains open! And this peculiar situation is obviously due to the fact that the modern set theory unlike (almost) the rest of mathematics is developed in a formal axiomatic setting. The story in brief is the following. In 1938 Gödel discovered that ZF (which is an improved version of Zermelo's axiomatic theory of sets so called after the names of Zermelo and Fraenkel (Bar-Hillel et al. 1973)) is consistent with CH by building a model of ZF in which CH holds. In 1963 Cohen discovered that ZF is also consistent with the negation of CH by building a model of ZF in which CH does not hold. So it is well established today that neither CH nor its negation can be derived from the axioms of ZF (Kunen 1980). What remains controversial is whether or not this independence result provides a definite answer to the original question by allowing one to claim that the original question is ill-posed. An additional axiom – or some wholly new system of axioms for set theory – may eventually help, of course, to settle the problem in the sense that CH or its negation can be deduced from the new system of axioms. There are obvious trivial "solutions" of this sort like considering CH itself as an axiom. Then, however, it remains to show that the system of axiom for set theory solving the CH problem is a "right" one, and so the proposed solution is "genuine". I cannot see how this can be done on purely mathematical grounds; any possible argument to the effect that one system of axioms for set theory is "more natural" than some other has a speculative nature and lacks any objective validity. Even if one gets some non-trivial proof of CH from some system of axioms that appear to be in some sense natural one can hardly claim that this system of axioms is the "right one" solely because it solves the CH problem and because such a proposed

[1]Zermelo's principal motivation for axiomatizing set theory was saving Cantor's so-called "naive" set theory from paradoxes (Peckhaus 2008).

[2]The Continuum Hypothesis conjectured by Cantor states that there is no cardinal number strictly bigger than the minimal infinite cardinal number $\aleph_0$ (which can be described as the "number of all natural numbers") and strictly smaller that the cardinal number $2^{\aleph_0}$ of the set of all subsets of some set having the cardinal number ω (for example, the set of all series of natural numbers, including infinite series). Number $2^{\aleph_0}$ has been identified by Cantor with the number of points on a given continuous line or surface; hence the name of this conjecture.

solution is smart and elegant. Although this situation is not unprecedented and may be compared, in particular, with the fate of the *Problem of Parallels* in geometry of the nineteenth century (see Sect. 8.3 below) it makes a sharp contrast with the mainstream mathematics that still manages to provide yes-no answers to many well-posed questions.

It may be argued that the formal axiomatic framework makes explicit a relativistic nature of mathematics, which we should learn to live with; according to this viewpoint it is pointless to ask whether CH is true or false without further qualifications, and all that mathematicians can do is to study which axioms do imply CH (modulo some specified rules of inference), which axioms imply its negation, and which do neither (like the axioms of ZF). More generally, the only thing that mathematics can do according to this point of view is to provide true propositions of the *if – then* form: *if* such-and-such propositions are true *then* certain other propositions are also true. I cannot see how such a deductive relativism (or "if-thenism") about mathematics can be sustainable. It is incompatible not only with the common mathematical practice but also, more specifically, with the current practice of studying formal axiomatic systems. Denote S the proposition saying that CH is independent from the axioms of ZF (in the sense that neither S nor its negation can be derived from these axioms). S is commonly seen as an established theorem on a par with any other firmly established mathematical theorem. However S is not expressed in the *if – then* form; it is expressed as an "absolute" mathematical truth about ZF and CH, which does not refer to any particular formal framework. The proof of S (that comprises the construction of Gödel's model L verifying CH and Cohen's forcing construction falsifying CH) is a piece of rather sophisticated "usual" or "informal" mathematics but not a formal inference within certain axiomatic theory. So a consistent if-thenist would not hold without further qualifications that CH is independent from the axioms of ZF but rather say that it depends of one's assumptions.

Remind that Hilbert's foundational project of 1927 was supported by the hypothesis according to which all metamathematical questions concerning the consistency and the independency of axioms expressed in a formal language were trivial or, to put it more precisely, treatable by finitary means. And we know today this hypothesis is false. When people suggest today ZF as a foundation of mathematics they no longer hope to prove the consistency of this formal theory (since such a proof requires a stronger metatheory) but rather take a pragmatic attitude according to which this theory can be used unless one eventually discovers that it is contradictory; if this happens there are always ways of modifying the axioms of ZF that may block the eventual contradiction. As I have already said the known proofs of S are far from being trivial or finitary.

Thus in spite of the fact that the modern set theory no longer considers sets naively but works instead with various formal axiomatic theories of sets this modern theory like any other modern mathematical theory relies on non-formalized proofs. What is specific for the modern set theory is its *object* rather than its method. Instead of studying sets "directly" in the same way in which, say, group-theorists study groups, set-theorists study formal axiomatic theories of sets. However the *methods* used by modern set-theorists are not essentially different from methods used in other parts of today's mathematics. It remains in my sense an open question whether or not such a roundabout way of studying sets has indeed proved effective. True, at the present there is no clear alternative to it. However it is not inconceivable that in the near future the mathematical community may bring about an improved "naive" concept of set that would allow one to study sets like groups. It is not inconceivable that such an old-fashioned way of thinking about sets could after all allow for a real progress in the CH problem. In any event it seems me important to keep such a possibility open and not try to take it out of the table using philosophical arguments.

The distinction between a *theory* and *metatheory* (and, more generally, between *mathematics* and *metamathematics*), which dates back to Hilbert, is helpful for making things clearer. In modern set theory a *theory* is ZF or another formal axiomatic theory while proofs of independence of CH from the axioms of ZF and similar results belong to a *metatheory* that tell us important things about formal axiomatic theories. I would like, however, to stress here the fact that this terminology, which remains standard in the community of people working in *foundations* of mathematics, is heavily philosophically-laden and overtly clashes with the language in which the wider mathematical community usually describes its own activities. Namely, the distinction between mathematics and metamathematics implies the view according to which formal axiomatic theories are "usual" mathematical theories while metatheories belong to a special domain of metamathematics that lays somehow "beyond" the usual mathematics and has some philosophical flavor. But if we leave now philosophy aside and describe the same subject-matter in the language of mathematicians we come to a different view: formal axiomatic theories are *not* mathematical theories in the usual sense of the word while their corresponding metatheories are comparable with "usual" theories from any other area of mathematics! This linguistic confusion reflects a gap between what today's mathematics *is* and what in the opinion of certain people it *must* be. In any event it seems me essential to fix it by suggesting a more neutral language. By the analogy with the distinction between an *object language* and a *metalanguage* in formal semantics I suggest to use the term "object-theory" for what in formal axiomatic studies (but not in the rest of mathematics) is usually called simply a "theory".[3]

One may argue as follows. True, any formal object-theory requires some supporting informal metatheory. True, metatheories are, generally, sophisticated. But

[3]Remind from Sect. 3.4 that Hilbert's distinction between real and ideal mathematical objects translates into Hilbert's distinction between (contentual) mathematics and *metamathematics* as follows: mathematics studies ideal objects with a help of real syntactic constructions; metamathematics studies real syntactic constructions without using anything ideal. This, remind, was Hilbert's original idea supposed to help mathematics to "get real" without leaving the ideal "Cantor's Paradise". When in the light of Gödel's incompleteness theorems and other developments it became clear that metamathematics cannot do solely with finitary means, some limited "ideal content" – i.e., some more advanced *mathematical* content – was allowed in it. The title of "The Mathematics of Metamathematics" appeared in 1969 (Rasiowa and Sikorski 1963) perfectly illustrates this shift, which has relaxed the boundary between mathematics and metamathematics.

metatheories can be formalized in their turn and studied with the metametatheories. Although this regress is potentially infinite and is not going to lead us to any ultimate self-evident ground it nevertheless deepens our understanding of foundations of mathematics. Although informal instruments cannot be then wholly taken away like the Wittgenstein's ladder they can be viewed as a part of general philosophical underpinning of mathematics rather than as a part of mathematics proper.[4]

Once again I claim that the above view is based on an a priori idea about mathematics and its foundations that is not justified neither by the old nor by the recent mathematical practice. First of all it confuses firm (meta)mathematical results like the independence of CH with a general philosophical discussion. It reflects an existing trend in the Analytic philosophy of mixing mathematical and speculative arguments indiscriminately, which is rejected by the majority of mathematicians who do not want to allow for philosophical speculation in mathematical papers. I support this rejection from the philosophers' side and insist that although philosophical speculation and mathematics may indeed fertilize each other they must be carefully distinguished and kept apart. The fact that metamathematical results are obtained "informally" does not preclude them to be firm and mathematically valid; such results must be sharply distinguished from their philosophical interpretations, which do not have and cannot possibly have any objective validity. Second of all the above view once again takes it for granted that a formalized object-theory is a self-standing mathematical theory. This seems me very dubious. Metamathematical results concerning ZF and its likes make the core content of the modern set theory rather than only a foundation of this theory, which can be left aside unless one has a special interest in studying foundations (while *philosophical* interpretations can and should be left aside when one works in set theory as a mathematician).

Thus we can see that even in set theory where formal methods are applied systematically the (metamathematical) informal methods remain essential. Let us now see how the Axiomatic Method applies in the modern mathematics outside set theory and mathematical logic.

4.2 Bourbaki

The multi-volumed *Elements of Mathematics* produced in 1939–1988 by a group of (mainly French) mathematicians using the pseudoname *Nicolas Bourbaki* is the most recent serious attempt to write a self-contained compendium of the core contemporary mathematics after the Euclid's example (interpreted liberally).[5]

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[4]A similar view is developed by Shapiro in (1971).

[5]The original French title is *Eléments de mathématique*, which uses the unusual singular form "mathématique" (while the usual French word for mathematics is "mathématiques"). So a more accurate English translation of the title is *Elements of Mathematic*. This unusual singular form of the word is supposed to stressed Bourbaki's aim of the unification of mathematics.

Although Hilbert's *Foundations* of 1899 fall under the same description the two works differ in their purpose. Remind that Hilbert's work of 1899 is focused on the Euclidean geometry, which in the end of the nineteenth century was already only a relatively small part of what was commonly known under the name of geometry in the mathematical community. Hilbert rebuilt here an old theory with a new Axiomatic Method, clarified the logical structure of this old theory, and left it to other people to do a similar job for more recent theories like the Riemanian geometry (see for example Veblen and Whitehead 1932). So in his *Foundations* of 1899 Hilbert presented a *method* allegedly applicable everywhere in mathematics and beyond, but unlike Euclid he did not try in this work to account for basic mathematical *concepts* sufficient for developing the rest of his contemporary mathematics. Bourbaki in his turn like Euclid aims at providing a genuine self-contained introduction into the contemporary mathematics, which systematically presents not only its method but also its basic content. A concise general description of this project, which makes explicit some grounding ideas behind it, has been published in 1950 as a separate article (Bourbaki 1950).

The section of this article named "Logical Formalism and the Axiomatic Method" begins as follows:

> After more or less evident bankruptcy of the different systems [..] it looked, at the beginning of the present [20th] century as if the attempt had just about been abandoned to conceive of mathematics as a science characterized by a definitely specified purpose and method; instead there was a tendency to look upon mathematics as a "collection of disciplines based on particular, exactly specified concepts", interrelated by "a thousand roads of communications" [..] [quoted by the author from Brunschvicg (1912, p. 447)] Today, we believe however that the internal evolution of mathematical science has, in spite of appearance, brought about a closer unity among its different parts, so as to create something like a central nucleus that is more coherent than it has ever been. The essential aspect of this evolution has been the systematic study of the relation existing between different mathematical theories, and which has led to what is generally known as the "axiomatic method." (Bourbaki 1950, p. 222)

After this recognition of the unifying power of the Axiomatic Method Bourbaki makes an interesting move by distinguishing between the *logical* aspect of the Axiomatic Method from another aspect, which can be called *structural* (see Chap. **9**); in Bourbaki's view this is the latter rather than former aspect that makes the Axiomatic Method a powerful instrument of the unification; as we shall now see Bourbaki points here to his proper version of Axiomatic Method rather than Hilbert's Formal Axiomatic Method in its original form as described in the last Chapter:

> [E]very mathematical theory is a concatenation of propositions, each one derived from the preceding ones in conformity with the rules of a logical system [..] It is therefore a meaningless truism to say that this "deductive reasoning" is a unifying principle for mathematics. [..] [I]t is the external form which the mathematician gives to his thought, the vehicle which makes it accessible to others, in short, the language suited to mathematicians; this is all, no further significance should be attached to it. What the axiomatic method sets as its essential aim, is exactly that which logical formalism by itself cannot supply, namely the profound intelligibility of mathematics. [..] Where the superficial observer sees only two, or several, quite distinct theories, lending one another "unexpected support" [quoted by

> the author from Brunschvicg (1912, p. 446)] through the intervention of a mathematician of genius, the axiomatic method teaches us to look for the deep-lying reasons for such a discovery, to find the common ideas of these theories, buried under the accumulation of details properly belonging to each of them, to bring these ideas forward and to put them in their proper light. (Bourbaki 1950, p. 223)

In order to illustrate his point Bourbaki uses the example of the "abstract" group theory; the author describes here this theory as an axiomatic theory construed after the pattern of Hilbert's *Foundations* of 1899 as a *system of things*, which are subject to the following three axioms (modulo a slight change Bourbaki's original notion).

G1: $x \circ (y \circ z) = (x \circ y) \circ z$ (associativity of $\circ$)
G2: There exists an item *1* (called *unit*) such that for all x $x \circ 1 = 1 \circ x = x$
G3: For all x there exists x^{-1} (called *inverse* of x) such that $x \circ x^{-1} = x^{-1} \circ x = 1$.

A system of things satisfying these axioms is called a *group*. Expression $x \circ y = z$ stands here for an abstract binary *algebraic operation*, which in the given context is to be understood as a (uninterpreted) logical ternary *relation* $R(x,y,z)$ having this special property: if $x = x'$ and $y = y'$ then $R(x,y,z) \leftrightarrow R(x',y',z)$ (which means informally that the output of the operation is uniquely determined by its input).

The above axiomatic theory of groups (which I shall denote **GT** for further references) have various interpretations, which were known and studied before the rise of Hilbert's Axiomatic Method: by interpreting variables x,y,z, as invertible geometrical transformations (like motion) and interpreting the operation $\circ$ as composition of these transformation one gets the notion of group of geometrical transformation; by interpreting variables x,y,z, as whole numbers and interpreting $\circ$ as $+$ (addition of whole numbers) one gets the additive group of whole numbers, etc. Such examples belong to different domains of mathematics and many of them play some significant role in their proper domains. But until **GT** was axiomatically formulated as above [6] and until it brought about the precise *general* notion of group those examples could not be understood as instances and special cases of one and the same thing (for no such thing was known yet!) and the links between these different groups, which were eventually guessed by some smart mathematicians, looked as unsystematic and sometimes even mysterious. Thus in this case the Axiomatic Method helps to bring about the new powerful mathematical concept of a group and develop the corresponding general theory, which unifies a large spectrum of significant mathematical results from different areas of mathematics.

As we see Bourbaki points here on a general phenomenon, which is not specific to the Axiomatic Method (and moreover to Hilbert's Formal Axiomatic Method), namely to the mathematical concept-building and its unifying role. Such basic mathematical concepts as *number*, *figure* and the like can be similarly seen as abstractions generalizing upon more specific examples like sets of dots or sets of strokes (for number) and circles, polygons, etc. (for figures). Although the

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[6] As Bourbaki notices here **GT** can be defined through different axioms. What determines the identity of this theory is its set of true propositions (including both axioms and theorems inferred from these axioms) but not a given set of axioms.

generalization upon and abstraction from specific features of previously known examples is not the only way in which emerge new mathematical concepts this way of emergence is a major one. And in Bourbaki's example it works through the Axiomatic Method. Let us see how it works more attentively.

First of all we need to distinguish between two ways in which an axiomatic theory *unifies* its content. When a set of contentual propositions is logically deduced from certain propositions belonging to the same set and chosen as axioms this unifies all these propositions into a single (contentual) theory. As we have seen Bourbaki recognizes this fact but in the last quote he clearly points to a different way of unification, which is equally made possible by the Axiomatic Method. This different way of unification is made possible by the *formal* character of Axiomatic Method, where "formal" is to be understood in the sense of Hilbert's *Foundations* of 1899 rather than in the sense of his *Foundations* of 1927 (which is referred to in the above quote in the expression "logical formalism"). This second axiomatic unification amounts to the following: a *formal* (as opposed to contentual) axiomatic theory unifies its interpretations (models) by identifying certain common features of these interpretations and abstracting from all other specific features of those interpretations. The usual talk of interpretation of a given formal theory takes it for granted that the formal theory is given first and interpreted next. Now we reverse the perspective and consider the (would-be) interpretations as given (as contentual mathematical theories and fragments of such theories) and then think how to make up a formal theory, which captures common features of these things and thus unifies them. The example of **GT** is used by Bourbaki to illustrate the latter but not the former way of unification.

Thus Bourbaki shows – in my view quite correctly – that the Formal Axiomatic Method has a unifying capacity, which is absent from contentual versions of the Axiomatic Method. However Bourbaki's version of Axiomatic Method is not identical to Hilbert's! Let me now describe the difference.

As an example of a *theorem* of **GT** Bourbaki mentions this proposition **P**:

$$\text{For all } x, y, z \text{ if } x \circ y = x \circ z \text{ then } y = z$$

which follows from **G1–G3** almost immediately. I claim that the simplicity of this example does not allow it to represent correctly Bourbaki's Axiomatic Method. Notice that among *objects* of **GT** there is no (abstract) groups just like among objects of Euclidean (3D) geometry developed in Hilbert's *Foundations* of 1899 there is no such thing as (3D) Euclidean space. As a *system of things* (model) of **GT** any group is a domain where all axioms and theorems of **GT** hold; objects of this theory are *elements* of the given group but not this group itself. But Bourbaki's theory of (abstract) groups (see Bourbaki 1939–1988, vol. 2, Chap. 1, Sect. 6) like any other presentation of this theory does treat groups as its objects, distinguishes between different groups, classify them and makes various constructions with them. Notice that **GT** by itself does not allow one even to formulate the notion of *subgroup*! Take also in consideration that **GT** is not *categorical* (in the usual model-theoretic sense of the term), which simply amounts to saying that not all groups are isomorphic. So axioms **G1–G3** provide nothing but the general notion of abstract group and in this sense can be compared with a *definition* of some traditional mathematical object like triangle; theorems of **GT** like **P** are to be compared with propositions like "all triangles have three angles" implied by the definition of triangle. Remind the famous passage from Kant's *Critique of Pure Reason* that has been already quoted above

> Give a philosopher the concept of triangle and let him try to find out in his way how the sum of its angles might be related to a right angle. He has nothing but the concept of figure enclosed by three straight lines, and in it the concept of equally many angles. Now he may reflect on his concept as long as he wants, yet he will never produce anything new. He can analyze and make distinct the concept of a straight line, or of an angle, or of the number three, but he will not come upon any other properties that do not already lie in these concepts. But now let the geometer take up this question. He begins at once to construct a triangle .... In such a way through a chain of inferences that is always guided by intuition, he arrives at a fully illuminated and at the same time general solution of the question. (*Critique of Pure Reason* Kant (1999), A 716 / B 744)

Kant's point applies to the case of group theory too. Although there are some general truths like **P** which logically follow from the concept of group (i.e., from axioms **G1–G3**) those truths are of little mathematical interest. The genuine mathematical work in the (abstract) group theory begins when groups are conceived of (or "constructed") as individual objects. In this way mathematicians prove non-trivial facts about groups that do not follow from **G1–G3** alone (the mathematical reader may think of Lagrange's theorem for a simple example). I do not mean to suggest here that Kant's philosophy of mathematics in its original form fully applies to the modern mathematics in general and to the modern group theory in particular. So we need to study more precisely *how* groups and other modern mathematical concepts are constructed as objects; this issue will be thoroughly discussed in Chap. 9. However already at this point it is clear that **GT** by itself no more deserves the name of group theory than a definition of triangle deserves the name of a *theory* of triangle. Kant's point about the proper geometrical study of triangles and a "philsophical" study of triangles that seeks to get the relevant knowledge directly from definitions (see Sect. 2.3 above) is perfectly relevant in the case of group theory!

Before I make explicit the way in which groups and other mathematical concepts are constructed as (mathematical) objects I need to clarify a widespread terminological ambiguity. Bourbaki call abstract groups *abstract* by the contrast with such "concrete" examples of groups as the additive group of whole numbers, the group of Euclidean motions and the like. This way of distinguishing between the abstract and the concrete in mathematics is specific for the given mathematical context and should not be confused to the general distinction between abstract concepts and concrete instants of those concepts, which is applicable everywhere in mathematics and beyond. The former distinction describes a way of obtaining some mathematical concepts from some others: given the concept of the additive group, the concept of group of Euclidean motions, the concept of group of permutations, etc., one forms the general concept of group through abstraction from certain specific features of each of these examples. The obtained concept of so-called *abstract* group like any other mathematical concept may be thought of both *in abstracto*, i.e., as a self-standing non-represented concept, and in *in concreto* as represented by an individual object or a set of such objects. The confusion between the two senses of “abstract” and “concrete” in mathematics is not without a reason; it can be explained by the fact that at certain points of history certain concepts obtained through abstraction from earlier known mathematical concepts cannot be immediately represented *in concreto*, and so remain speculative rather than genuinely mathematical until they get supported by newly developed corresponding modes of representation. In Chap. **9** below I show more precisely how this dialectics of abstract and concrete works in the history of mathematics. Notice that the title of “abstract group theory” used by Bourbaki in 1950 today is an anachronism: what Bourbaki in 1950 calls an “abstract group” today is commonly called simply a group.

Let’s now see what kind of representation of the abstract group concept is used by Bourbaki in their volume on algebra, which includes the group theory. Like in other similar cases they use for it a *set-theoretic* representation; the relevant set theory is developed in the first volume of Bourbaki’s *Elements* (I shall tell more about it shortly.) Crucially, the set-theoretic representation does not reduce to interpreting **GT** in set theory. One gets a set-theoretic model of **GT** by interpreting variables x, y, z, as elements of some set G and interpreting the group operation $\circ$ in terms of Cartesian product of sets which reduces it to the primitive set-theoretic relation of membership. However such a model of **GT** is nothing but one particular group G but not a domain where live *all* groups accounted for by group theory! So the group theory as developed in Bourbaki’s *Elements* is not just an interpreted version of **GT** but a theory of *set-theoretic models* of **GT** developed on the basis of (Bourbaki’s) set theory. Thus all Bourbaki’s groups live in an universe of sets (that interprets their set theory). The same is the case for objects (structures) of other sorts treated in Bourbaki’s *Elements*. In this sense set theory qualifies as a foundation of all Bourbaki’s mathematics.

We see that Hilbert’s Axiomatic Method is used by Bourbaki in a very peculiar way, which is not made clear by Bourbaki’s remark about the Axiomatic Method in his paper of 1950. Namely, the Hilbert-style axiomatic theory of groups **GT** is used by Bourbaki for defining *objects* of their theory of groups, which is not an informal version of **GT** but something quite different! Objects of this sort Bourbaki call *structures*. Formal Axiomatic Method involves the idea that *isomorphic* structures are essentially the same. Thus Bourbaki’s structures have a double identity criterion: in one sense the identity of a given structure is given by the identity of its underlying set (which is formally treated in the corresponding set theory) and in a different sense the identity of structures is their isomorphism. In Chap. **9** we shall see how this conceptual tension translates into a philosophical controversy between *structuralism* and *set-theoretic substantialism*. The issue of identity of structures will be also discussed in Part **II** of this book. Here I would like to stress that the peculiar way, in which Bourbaki applies Hilbert’s Axiomatic Method, allows Bourbaki’s mathematics to fit (by and large) the pattern of traditional mathematics, where mathematical concepts are represented by individual objects.

Let us now look at Bourbaki's set theory presented in the first volume of his *Elements* (Bourbaki 1939–1988). This theory follows the formal method of Hilbert's *Foundations* of 1927 closer than any other known to me axiomatic exposition of set theory. The First Chapter of the volume, which has the title *Description of Formal Mathematics*, begins with an account of *signs* and *assemblies* (strings) of signs provided with a definition of mathematical theory according to which a theory

> ...contains rules which allow us to assert that certain assemblies of signs are *terms* or *relations* of the theory, and other rules which allow us to assert that certain assemblies are *theorems* of the theory. (quoted by English translation Bourbaki (1968, p. 16))

Then follows a description of operations that allow for constructing new assemblies of signs from some given assemblies; the simplest operation of this sort is the *concatenation* of two given assemblies A, B into a new assembly AB. On such a purely syntactic basis Bourbaki introduces some logical concepts necessary for a formal axiomatic treatment of set theory. Although Bourbaki's version of axiomatic set theory is not identical to ZF it is similar in its character; the differences between the two ways of formalizing set theory are not relevant to the present discussion and I leave them aside. The general Bourbaki's strategy in set theory is to develop formally all set-theoretic concepts used in various specific theories treated in other volumes after the pattern of group theory, so that all these specific theories appear as (or can be translated into) fragments of the formal Set-theory.

As a matter of fact the formal notation used in the first volume never reappears in the following volumes, so the possibility of translation of specific theories into the formal set theory remains theoretical and is never explicitly explored. Instead the formal set theory presented in the first volume Bourbaki everywhere else uses its informal version, which is systematically exposed in an unpublished draft of the first volume of the *Elements* (Bourbaki 1935–1939). After a philosophical introduction the author introduces (informally) the concept of *fundamental set* and the relation of membership between sets and their elements. (The author calls a set *fundamental* in order to distinguish a well-defined set concept from a more general notion of collection.) Then the author introduces (with the usual informal notation) the concept of *subset*, which is the subject to the following axiom:

> Any predicate of type A defines a subset of A; any subset of A can be defined through a predicate of type A. (Bourbaki 1935–1939, p. 7, hereafter my translation from French)

(Predicate of type A is a predicate P such that for every element a of set A $P(a)$ has a definite truth-value. The subset S of set A defined by P consists of such and only of such a for which $P(a)$ is true.)

Next Bourbaki introduces the concept of *complement* of a given subset, of *powerset* $P(A)$ of a given set A (i.e., the set of all subsets of A); of *union*, *intersection* and *cartesian product* of sets (described as *operations* on sets), of *relation* and *function* between sets. Having these basic concepts in his disposal the author says:

> In any mathematical theory one begins with a number of fundamental sets, each of which consists of elements of a certain type that needs to be considered. Then on the basis of types

that are already known one introduces new types of elements (for example, the subsets of a set of elements, pairs of elements) and for each of those new types of elements one introduces sets of elements of those types.

So one forms a family of sets constructed from the fundamental sets. Those constructions are the following:

1) given set A, which is already constructed, take the set $P(A)$ of the subsets of A;
2) given sets A, B, which are already constructed, take the cartesian product AxB of these sets.

The sets of objects, which are constructed in this way, are introduced into a theory step by step when it is needed. Each proof involves only a finite number of sets. We call such sets *types* of the given theory; their infinite hierarchy constitutes a *scale of types*. (Bourbaki 1935–1939, pp. 43–44)

On this basis the author describes the (informal) concept of *structure* as follows:

We begin with a number of fundamental sets: $A, B, C, \ldots, L$ that we call *base sets*. To be given a *structure* on this base amounts to this:

1) be given properties of elements of these sets; 2) be given relations between elements of these sets; 3) be given a number of types making part of the scale of types constructed on this base; 4) be given relations between elements of certain types constructed on this base; 5) assume as true a number of mutually consistent propositions about these properties and these relations. (Bourbaki 1935–1939, pp. 44–45)

What I want to stress is the fact that principles of building mathematical theory described in the Bourbaki's draft are not so different from Euclid's: Bourbaki like Euclid begins with principles of building mathematical objects but not with axioms. Axioms (in the modern sense of the term) appear only in the very end of the above list (the 5th item) and as I have already argued they play the role of definitions. While for Euclid the basic data is a finite family of *points* and the rest of the geometrical universe is constructed from these points by Postulates for Bourbaki the basic data is a finite family of *sets* and the rest is constructed as just described. While for Euclid the basic type of geometrical object is a *figure* for Bourbaki the basic type of mathematical object is a *structure*. In both cases the constructed objects come with certain propositions that can be asserted about these objects without proofs because they immediately follow from corresponding definitions. In both cases the construction of objects is a subject of certain rules but not the matter of a mere stipulation. In order to continue this analogy one may compare the notion of *isomorphism* of structures in Bourbaki with Euclid's notion of *congruence* of figures.

We see that Bourbaki's mathematics has two different layers. On the ground layer it has a formal set theory built with Formal Axiomatic Method. But the rest of this mathematics is developed with an informal notion of set described through constructive postulates like "take the cartesian product of given sets", which are similar to Euclid's Postulates. Calling such postulates constructive I mean not that they involve only finitary operations (they are obviously infinitary) but that they are not existential propositions. However the formal theory of sets provides a theoretical possibility of translating all these informal set-theoretic postulates into formal existential axioms and ultimately – of translating all of Bourbaki's mathematics into strings of symbols operated according to purely syntactic rules, which can be also described as constructive postulates (about operating with strings of symbols). So this translation allows (in principle) for replacement of informal set-theoretic infinitary postulates into finitary syntactic postulates.

It is interesting to observe that the ground set-theoretic layer of Bourbaki's mathematics is seen by the author as undesirable; it looks like Bourbaki would be happy to get rid of it but does not know how to do this exactly. Here is what he says about it in the earlier quoted draft (my translation from French):

> The reader will see that the nature of elements of fundamental sets can be always easily left undetermined and that this point of view is often useful. From here there is only one step to thinking that only structure matters and that the true aim of mathematical theory is a study of structure independently from sets that may represent it. Perhaps it is indeed possible to study structures themselves and forbid oneself to consider fundamental sets. However because of the commodity of language and the invincible habit of mind we take the "ontological" approach, i.e., stipulate fundamental sets for each theory.

and in his manifesto of 1950 Bourbaki says in a footnote:

> We take here a naive point of view and do not deal with the thorny questions, half philosophical, half mathematical, raised by the problem of the "nature" of the mathematical "beings" or "objects". Suffice it to say that the axiomatic studies of the nineteenth and twentieth centuries have gradually replaced the initial pluralism of the mental representation of these "beings" thought of at first as ideal "abstractions" of sense experiences and retaining all their heterogeneity by an unitary concept, gradually reducing all the mathematical notions, first to the concept of the natural number and then, in a second stage, to the notion of set. This latter concept, considered for a long time as "primitive" and "undefinable", has been the object of endless polemics, as a result of its extremely general character and on account of the very vague type of mental representation which it calls forth; the difficulties did not disappear until the notion of set itself disappeared (and with it all the metaphysical pseudo-problems concerning mathematical "beings") in the light of the recent work on logical formalism. From this new point of view, mathematical structures become, properly speaking, the only "objects" of mathematics. (Bourbaki 1950, pp. 225–226)

Since "the recent work on logical formalism" referred to by Bourbaki in the last quote does not contain anything that goes beyond Hilbert's Formal Axiomatic Method these passages are rather puzzling. A way to understand Bourbaki's misgivings about set theory is this. A strictly formal version of the Axiomatic Method like one applied in Bourbaki's volume on set theory does not require set theory developed in advance; however except this volume the set theory (namely its informal version) is used by Bourbaki essentially. So the misgiving may be due to the fact that, in particular, Bourbaki's group theory is not a formal theory like **GT** (which by itself does not require the notion of set) but a theory of set-theoretic models of **GT**. Although this fact may indeed explain Bourbaki's reference to the "logical formalism" the idea of making structures into objects is hardly compatible with the Formal Axiomatic Method (for structures determined by formal axiomatic theories are not objects of their theories). So what Bourbaki really aim at is not a strictly formal axiomatic treatment of group theory and the rest of mathematics but rather finding an appropriate replacement for set theory in its role of universal vehicle carrying all specific mathematical structures. In Sect. 9.5 I shall come back to this issue and consider category theory as such an alternative vehicle.

Coming now back to the question about the role of the Formal Axiomatic Method in the twentieth century mathematics we observe the following. Bourbaki formulated an important part of the contemporary mathematics in set-theoretic terms and thus showed a theoretical possibility to formalize this part of mathematics by reducing it to formal axiomatic set theory. However the Formal Axiomatic Method did not become in Bourbaki's hands the "basic instrument of all research" as Hilbert suggested in 1927 (p. 467); it is used instead only for foundational purposes (notwithstanding the fact that the syntax of Bourbaki's formal set theory lacks the intuitive clearness through which Hilbert hoped to ground the infinitary reasoning in mathematics). As an instrument of research and more importantly as an instrument of presenting a ready-made knowledge in a systematic form Bourbaki uses a different version of Axiomatic Method. Describing this other method as *informal* one should keep in mind that the relevant difference between being formal or informal is not a matter of degree. As we have seen Bourbaki's method involves features, which are not present in Hilbert's method, in particular the idea of using formal axioms for defining objects of a given theory rather than developing this theory itself. Another important difference concerns the doing-showing dilemma. While Formal Axiomatic Method makes mathematical (as distinguished from metamathematical) reasoning into a (syntactic) construction Bourbaki uses informal set-theoretic constructions and proves (informally) theorems about these constructions and in this sense more closely follows Euclid's example. The fact that this new way of doing-and-showing can be in principle represented formally should not be, of course, neglected. However it does not provide any substance to the claim that the Axiomatic Method used by Bourbaki throughout his *Elements* qualifies as formal in Hilbert's sense.

4.3 Galilean Science and Set-Theoretic Foundations of Mathematics

Remind from the last Section Bourbaki's claim according to which "every mathematical theory is a concatenation of propositions, each one derived from the preceding ones in conformity with the rules of a logical system", which the author suggests as self-evident. This is a statement of what I have called above a mathematical logicism in the large sense and argued that it is not self-evident at all. In Chap. 2 I have shown that Euclid's mathematics does not fit the logicist description of mathematics as a "concatenation of propositions" because in addition to theorems it contains *problems*, which aim at constructing objects rather than proving propositions. In Sects. 3.4–3.6 we have seen that Hilbert's Formal Axiomatic Method (in the sense of his *Foundations* of 1927 and later works) employs a very special form of mathematical logicism, which reduces a "concatenation of propositions" to a symbolic construction and in this way assures a fundamental role of mathematical intuition of a special sort. Finally we have observed that

Bourbaki's mathematics, in fact, is not quite unlike Euclid's: it cannot be described as a concatenation of propositions either because it similarly involves constructions of objects of special sort called structures. The idea of reduction of all constructive postulates to existential propositions plays a role *only* at the ground level of the formal set theory, where informal set-theoretic constructions indeed translate into symbolic constructions (or more precisely, a *possibility* of such translation is *shown* albeit no such translation is actually *done*). Now I would like to return to the issue of mathematical logicism and argue that mathematics has a non-propositional constructive aspect, which makes possible application of mathematics in empirical sciences and technology. This view is obviously not original and dates back at least to Kant. Nevertheless it is appropriate to spell it here again in modern terms.

It is hardly controversial that mathematics deals with forms of possible human experience; in its simplest and most general form this claim is simply tantamount to saying that mathematics applies across a wide range of human practices. Today this is even more true than it was in Kant's time: crucial technologies, on which depend our well-being, in many ways depend on mathematical considerations and cannot be sustained and further developed without mathematical expertise; mathematics today makes part of any engineering education. In Kant's time the only properly mathematized science was (Newtonian) mechanics; the following progress of science in the nineteenth century has brought us to the point when every physical theory deserving the name has a mathematical aspect. Today physics and chemistry are mathematized and the mathematization of biology is in progress. Using mathematical models also becomes an usual practice in social sciences.

Let me now be more specific and ask which general forms of experience are relevant to today's science and technology. This question is obviously yet too large and too general to be answered in any detail here. I would like however to stress only the following point. At least since Galileo's times science practices an active intervention of humans into the nature through experiments rather than a passive observation and description of the observed phenomena. So we are talking now about the mathematically-laden science where mathematics serves for guiding human interactions with the environment rather than simply for describing how this environment appears to our senses. As van Fraassen puts this

> The real importance of theory, to the working scientist, is that it is a factor in experimental design. (Fraassen 1980, p. 73)

Thus mathematical forms of possible experience relevant to the modern science are forms of such possible *interactions* with the environment rather than only linguistic and logical forms that allow for spelling out some plausible hypotheses about the world and deriving from them some consequences according to certain rules. The forms of the latter kind may be sufficient for developing a speculative science along the older scholastic pattern but they are certainly not sufficient for developing the modern mathematically-laden science and the modern mathematically-laden technology.

As far as the pure mathematics is conceived as a domain of abstract logical possibilities the fact that mathematics proves "unreasonably effective" Wigner

(1960) in its applications to empirical sciences and technology remains a complete mystery. The mystery is dissolved as soon as one observes that mathematics explores not everything that can possibly *be* the case (which is a hardly observable domain unless one delimit the sense of "possibly" in one way or another) but rather what we can possibly *do* within the limits of our human capacities (which are steadily growing with the progress of science and technology). What are these limits is a tricky question. On the one hand, mathematics systematically ignores certain apparent limits by exaggerating relevant capacities: this is usually called the *mathematical idealization*. For example, mathematicians pretend that they can count up to $10^{10^{10}}$ just as easily as up to 10 or that they can draw a straight line between two stars just as easily as they can draw a line between two points marked on a sheet of paper. This strategy usually works until the point where the empirical constraints become pressing and people invent new mathematics that takes these constraints into account as this, for example, happened when people realized that the old good Euclidean geometry is not appropriate for describing the physical space at large astronomical scales (even if it still works amazingly well at the scale of a planetary system like ours). One the other hand, it also happens that in a real experiment people observe what in terms of the assumed mathematical description of this experiment qualifies as impossible as this happened in the Michelson-Morley experiment supposed to measure parameters of the ether flow around the Earth. In such cases people say that the assumed mathematical description (and hence the corresponding physical theory) is wrong and look for a new one. Sometimes the suitable mathematics can be found in a nearly ready-made form and only used for building a new physical theory but sometimes in order to fix the problem one needs to develop the appropriate mathematics from the outset as this happened in the history of the electro-magnetism, for example. This picture suggests the view on the pure mathematics as a proper part of the modern Galilean science.

Russell's neo-Leibnizian logicism about mathematics promises nothing more and nothing less than that: to make mathematics a part of logic, so that any mathematical form of possible experience turns into the form of a *proposition* (and forms of logical inference of propositions from some other propositions), which may eventually refer to some experience. Russell's view is quite radical in this respect, and many people including Hilbert who were directly involved into reforming mathematics on the basis of new logic in the beginning of the twentieth century didn't share Russell's philosophical views. Anyway, as I have already argued, a weaker form of the neo-Leibnizian approach is intrinsic to Hilbert's Formal Axiomatic Method. Even if forms of possible experience delivered by a formal axiomatic theory do not qualify as *logical* forms in the precise sense of the term they nevertheless are forms of possible empirical *propositions* rather than forms of empirical interactions or anything else. This, in my view, explains the very little success that Hilbert's Axiomatic Method has had so far in physics and other natural sciences.

As far as we want to continue to develop the Galilean science (and the technology connected to this type of science) our mathematics must provide for it forms of possible empirical interaction rather than just forms of propositions. In other words it must provide forms appropriate for *doing* various things in the world but not only forms for talking about this world and *showing* how the world looks like in a mathematical representation. Since formal axiomatic theories are not appropriate for this job we need to learn how to build mathematical theories differently.

As Kant shows in great detail the traditional geometry and his contemporary algebra are useful in the Galilean science because these mathematical theories are *constructive* in the sense that they involve rules for constructing their objects (explicitly or implicitly). Today we cannot hope, of course, to get a new mathematical theory that would allow for identifying a physical object with a mathematical object in the same way, in which one may identify (modulo the mathematical idealization), say, a planet with an Euclidean sphere. Today's physicists describe particles using the mathematical group theory and manipulate with these particles in experiments using a special hi-tech equipment; they don't expect that mathematical manipulations with groups would map their experimental manipulations in a direct way. Nevertheless the constructive character of the mainstream informal mathematical practice, which I have stressed earlier in this Chapter, still helps physicists and other scientists to design their experiments and their equipments. Scientists make up mathematical models of their experimental systems, manipulate both with the models (theoretically) and with the experimental systems (in real experiments) and see whether the manipulations of the two sorts work coherently. This is, of course, an oversimplified picture of the scientific experiment (for more details see Fraassen 1980) but it is sufficient for seeing that the possibility to establish a correlation between mathematical manipulations, on the one hand, and experimental manipulations, on the other hand, remains essential in today's mathematically-laden experimental science.

I cannot see how such a correlation can be possibly established when the only type of mathematical objects available for manipulation are syntactic objects, which represent (logical forms of) propositions related to certain experimental settings but do not represent these experimental settings themselves. Given a proposition expressed in a formal language one may interpret it in terms of physical data and evaluate whether under this interpretation the given proposition is true or false. From a number of so interpreted true propositions one may deduce some further propositions, consider them as physical predictions and finally check these predictions against the available physical data. So far so good but notice that from a methodological viewpoint this way of doing natural science resembles Ptolemean astronomy saving phenomena rather than the Galilean physics intervening into the nature with experiments! In order to design an experiment one needs a mathematical model (representation) of a given physical environment itself but not of formal propositions interpretable in terms of this environment.

Bourbaki's *Elements* present a possible compromise between the formal axiomatic approach and a more traditional constructive approach of doing mathematics. (Beware that I use here the term "constructive" in the sense of Hilbert's distinction between "construction postulates" and propositional "existential form" (Hilbert and Bernays 2010, p. 20). This sense of being constructive does not imply anything about the admissibility of mathematical constructions of a given type. The admissibility of constructions depends on concrete constructive postulates but I am now taking about constructive postulates in general.) So we have in Bourbaki's *Elements* a formal axiomatic theory (of sets) on the ground level, informal set-theoretic constructions on upper levels and the (theoretical) possibility to translate the higher-level mathematics into the formal ground-level terms. Arguably such a translation (however impractical it may be) plays an important justificatory role since it allows for reduction of problematic set-theoretic constructions (like the construction of the powerset of a given infinite set) to formal deductions from axioms of set-theory (including existential axioms) to (long and tedious but anyway finitary) symbolic constructions. So the question of admissibility of problematic set-theoretic constrictions reduces to the question of admissibility of the corresponding symbolic constructions. And in this latter case we have a clear criterion of admissibility: a given formal deduction is admissible if and only if it, first, conforms the explicit rules of deduction and, second, the given formal theory of sets is *consistent*, which is tantamount to saying that from the given axioms the proposition $0 \neq 0$ can *not* be deduced by the aforementioned rules (i.e. that these rules do not allow to build a symbolic construction with the axioms on the one end and $0 \neq 0$ on the other end).

Although having such a device for checking suspicious mathematical constructions and suspicious mathematical proofs seems to be a good idea Bourbaki's attempt to apply this device in practice brings a rather controversial outcome. First of all it does not allow for an effective proof-checking: formal versions of Bourbaki's proofs are too long and cumbersome to be survey able by a human mathematician and at the same they are not adopted for an automatic proof-checking with a computer. This is why the theoretical possibility of translating set-theoretic constructions into symbolic one may serve only as an *epistemic ground* justifying certain basic set-theoretic constructions like the powerset.

It must be however stressed that in Bourbaki's *Elements* the Formal Axiomatic Method does not perform this foundational task as perfectly as Hilbert has imagined it in 1927. There are two fundamental difficulties here that Hilbert did not acknowledge. First, the criterion of formal consistency cannot be used straightforwardly because the proof of consistency of Bourbaki's set theory or any other formal set theory requires using some stronger theory. So we don't really know whether or not the background set theory is consistent. Second, what can and what cannot be proved in a formal theory depends on the chosen background logic and such a choice, as I have already stressed, is a matter of controversy. Given these fundamental difficulties I doubt that formal Set theories can play the foundational role they are supposed to play in Bourbaki or elsewhere in the modern mathematics. The powerset operation is admitted by Bourbaki not *because* its formal existential version (i.e., the formal powerset axiom) is formally consistent with other axioms of set theory but rather on an intuitive ground by an appropriate modification of the intuition relevant to the finite case. The finitary intuition underpinning Euclid's First Postulate is similarly modified in the case of the Second Postulate, which allows for an infinite extension of a given straight segment. Although the infinite powerset operation requires a deeper modification of the finitary intuition this is nevertheless a matter of degree rather than matter of principle. This is why any sharp distinction between "real" and "ideal" objects in mathematics is in my view unjustified. In Chap. 8 I show how in the history of mathematics "ideal" objects become "real" through the development of appropriate intuitions. The power set operation can be, in my view, stipulated directly as a constructive postulate ("constructive" in the above sense!) rather than in the roundabout way through the existential powerset axiom. Having said that I do not deny that the formal logical analysis has helped to clarify some important issues in foundations of set theory and in foundations of mathematics in general. We have learned about certain set-theoretic paradoxes and ways of avoiding them. In particular we learned to distinguish sets from *proper classes* (like the class of all sets, see Sect. 6.8). So even formal axiomatic set theories cannot provide ultimate foundations they show how us how to avoid known contradiction by forbidding certain constructions like the set of all sets.

Using a formal set theory as a foundation and using a set theory informally for building structures are related but still different issues, which we need to distinguish carefully. We have seen that the idea of building mathematical objects as *structured sets* comes from the Formal Axiomatic Method but then lives an independent life within theories, which are nor formal neither axiomatic in Hilbert's sense of these terms. It is appropriate to ask whether the informal "set-theoretical language" is indeed sufficient for doing modern mathematics. We shall see in Chap. 9 that the answer is rather in negative. Here I shall only make a remark concerning possible applications of Bourbaki's structural mathematics in natural sciences. Although the informal Bourbaki's structural mathematics just like Euclid's mathematics involves constructions (but not only logical deductions) the set-theoretic nature of these constructions apparently makes an obstacle for interpreting these constructions in physical terms and applying them in physics and other natural sciences. Consider the case of group theory: in spite of the fact that this theory is widely used in the twentieth century physics physicists normally think of groups as *groups of transformations* (see Sect. 7.1) rather than construe them as structured sets. This fact is hardly surprising since Cantor's notion of infinite set is of metaphysical rather than physical origin and has no physical sense; naively this can be expressed by saying that infinite sets do not exist in nature. The later development of set theory, which eventually led to modern axiomatic theories of sets, clarified the logical aspect of set theory but did not have anything to do with physics either. This makes Bourbaki's structural mathematics quite unlike Euclid's mathematics where the basic mathematical concepts such as number and (geometrical) magnitude are directly relevant to the material practices of counting and measuring, which are even today indispensable in any empirical science. The question, which I want to stress now is the following: is the present detachment of foundations of mathematics from the foundations of natural science indeed an epistemic necessity or rather an outcome of bad epistemic strategy?

It is true that the contemporary mathematics is so sophisticated and the experience relevant to the contemporary fundamental physics is so unlike the everyday human experience that we cannot hope to find a pre-established cognitive mechanism providing a natural link between mathematics and physics at the level of their foundations. However constructing such a link artificially can be an epistemic strategy on its own. As I have already mentioned in Sect. 3.2 the idea to use metaphysics as a guide and formal logic as a tool for building foundations of mathematics is related to the anti-Kantian turn in the philosophy in the very beginning of the twentieth century, which gave a new credit to the traditional pre-Modern and pre-Galilean patterns of doing science. I claim that the epistemic success of Galilean Science in the past is a sufficient reason to continue to keep its basic epistemic strategy untouched and think how to conform new mathematical and physical results with it rather than give up this strategy and replace it by a new form of Scholasticism. This may sound like knocking into an open door in the large scientific and mathematical community but not in the community of people professionally working in the foundations of mathematics, most of which still follows the logicist agenda established by Frege and Russell a century ago and works in an isolation from the mainstream mathematics and physics. I would like to stress that linking foundations of mathematics to foundations of physics is in *not* in my view only a pragmatic question of having better mathematical tools for physics but a fundamental question concerning foundations of the modern mathematized natural science. Although in this book I cannot present a systematic defense of the Galilean Science I claim that the irrelevance of set-theoretic foundations of mathematics to the contemporary physics is a strong reason for rejecting these attempted foundations and looking for a replacement.

4.4 Towards the New Axiomatic Method: Interpreting Logic

In the twentieth century the part Symbolic Logic, which describes itself as "philosophical", went through a booming development; for an overview I refer the reader to the last continuing edition of the *Handbook of Philosophical Logic* edited by Gabbay and Guenthner (2001).[7] Without trying to survey here the recent history of philosophical logic I shall try only to answer this question: Whether or not the development of logic after Hilbert brought about any new notion of Axiomatic Method?

In order to answer this question let me first of all stress that Hilbert's Formal Axiomatic Method is extremely flexible and leaves one the freedom not only for building various axiomatic theories (including incompatible ones) but also for making a choice of the background logic. The mere replacement of (formalized)

[7] If one asks what is specifically "philosophical" about the multiple formal systems presented in this Handbook then, I think, the answer is twofold: all these formal systems are designed with certain philosophical motivations and/or used for treating some philosophical problems. The relevant notion of being philosophical derives from a particular notion of philosophy, which can be roughly identified with the Analytic Philosophy.

Classical logic by the (formalized) Intuitionistic logic or any other system of formal logic does require the replacement of the Formal Axiomatic Method by a new method of theory-building.[8] However there is, in my view, at least one continuing development in logic, which indeed changes the sense of the Formal Axiomatic Method but not only makes a new application of the same method.

Remind from Sect. **3.3** that in their logical textbook of Hilbert and Ackermann (1928) distinguish between "contentual" and formal (i.e., symbolic) logic along with distinguishing between contentual and formal axiomatic mathematical theories. Do Hilbert and Ackermann also mean here a possibility of providing their system of symbolic logic with alternative interpretations (alternative models) along with alternative interpretations of formal axiomatic theories based on this system of logic? As I have already argued this is not the case: the authors rather think of formalization of "contentual", i.e., informal, logical concepts as a way of making these concepts sharper and better manageable with a help of symbolic means. A similar attitude is expressed in 1932 by Gentzen in a footnote:

> If the words 'sentence' ('theorem') and 'proof are used informally as constituents of our language they are of course intended to mean something quite different from the purely formally introduced concepts of 'sentence' ('theorem') and 'proof (and even under an intuitive interpretation the latter concepts are still considerably narrower than the former); the context should make it clear in each case how these concepts are intended. (Gentzen (1932), p. 312)

So in Gentzen's view a formal language allows for a narrower and more precise formulation of logical concepts than the natural language. It is an essential part of this view that each formal logical concepts has certain *intended* interpretation, which can be expressed by formal means more precisely than by the natural language. Tarski is his *Introduction* of 1941 treats logic as a formal theory such that it necessarily makes part of any other (formal) theory and which can be called, in this sense, the *minimal* theory. (He does not discuss in this textbook the issue of multiplicity of logics and talks about *the* logic in singular.) After explaining the notion of model of a given formal theory and providing some examples Tarski remarks:

> For precision it may be added, that the considerations which we sketched here are applicable to any deductive theory in whose construction logic is presupposed, but their application to logic itself brings about certain complications which we would rather not discuss here. (Tarski 1941 p. 119)

Let me now point to such a complication, which is of philosophical rather than mathematical character. In his earlier paper *Sentential Calculus and Topology* (Tarski 1956) first published in 1938 Tarski develops topological interpretations

[8]Although this second degree of freedom (which adds to the free choice of axioms) has been not previewed by Hilbert himself I don't qualify its discovery as a modification of Hilbert's Axiomatics Method. However this new degree of freedom undermines Hilbert's suggested epistemic justification of his method and thus creates new epistemological problems. I shall discuss these problems and suggest a solution in Chap. **10**.

of Classical and Intuitionistic propositional calculi. Under these interpretations the syntax of propositional logic is interpreted in terms of elements (open subsets) of a given topological space and operations with these elements, so a given well-formed formula F designates (not a proposition but) a certain element ϕ of the given space construed from other elements. Then Tarski proves that F is derivable in the given calculus if and only if ϕ has certain property P (which is not the same in the Classical and the Intuitionistic cases).

Let me briefly explain for a non-mathematical reader what is *topology* and an *open set*. Topology is a "gummy geometry", which studies those properties of geometrical objects, which remain invariant under invertible continuous transformations. Think about a circle (more precisely a circumference of a circle) and allow it to change its form however you want but without cutting it and without gluing it to itself. Such a "gummy" object qualifies as a circle (or "one-dimensional sphere") in the *topological* sense of the term (even if it may look like an oval and so not qualify as circle in the usual Euclidean sense of the term). A standard way to define a topological object (usually called a topological *space*) using set theory is the following. For a given set T (of "points") one specifies which subsets $U \subseteq T$ count as *open*; the complement $T \setminus U$ of an open set is called *closed*. The choice of open (sub)sets is restricted by several axioms, which I shall not list here. Consider a circle (or any closed curve) on the Euclidean plane and think of these two things as sets of points. Then the "natural" topology of the plane is one where the interior area of the circle counts as open while the rest (the rest of the plane together with the curve) counts as closed. In this setting a continuous transformation is defined as one that reflects opens, i.e., one, which never transforms closed sets into open.

I shall not reproduce here details of this Tarski's construction and discuss only its problematic significance for the Formal Axiomatic Method. Remind that a pillar of Formal Axiomatic Method is the symbolic logic and that this pillar is twofold: it comprises a symbolic syntax, on the one hand, and a logical content, on the other hand. Each of the two aspects of symbolic logic has its specific epistemic impact: the logical content comprises basic laws of reasoning; the symbolic syntax makes these laws explicit through the symbolic intuition (Sect. **3.4**). This is why the *logical* content of formal theories (built by Hilbrt's Formal Axiomatic Method) unlike their non-logical content is indispensable. Indeed, given a list of mutually consistent axioms one may after Hilbert stipulate *thought-things* and *thought-relations* satisfying these axioms and consider them as mathematical objects; since the axioms are written with a symbolic language this language provides these "ideal" objects with a symbolic representation, which makes it easy to reason about these objects as if they were "real". Further interpretations of the given formal theory can be interesting and useful (and may even constitute the pragmatic raison d'être of the given theory) but, strictly speaking they are not necessary. However this way of construing mathematical objects does not work unless one has the notion of logical consistency in one's disposal. On the syntactic level the logical consistency translates into the impossibility of building a string of formulae, which begins with the axioms and ends up with a distinguished formula like $0 \neq 0$ (provided one follows fixed rules about building strings of formulas). However this syntactic counterpart of the logical consistency is not sufficient by itself to make the Formal Axiomatic Method work. In order to make the method work one must assume, in particular, that the symbolic expression $0 \neq 0$ expresses a contradiction. So the logical content unlike the non-logical content cannot be dispensed with in formal theories. It is always possible, of course, to study systems of symbols on their own rights but the Formal Axiomatic Method does not fall out of such a study unless one brings logical concepts into it. So interpreting logical and non-logical terms in a formal theory are indeed two rather different issues. Understandably Tarski did not want to enter into these details in his introductory chapter on model theory (Tarski 1941).

With Tarski's topological interpretation of the Classical and Intuitionistic propositional calculi we get in addition to the *intended* logical interpretation of the given symbolic calculi a non-intended one. Tarski (1956) uses for this interpretation Kuratowski's semi-formal set-theoretic approach to topology (Kuratowski 1948–1950); a fully formalized version of this latter theory would comprise (i) a background logic, (ii) a formal set theory, and (iii) an axiomatic theory of topological spaces interpreted in the underlying set theory. What sense one can make of the fact that a theory, which involves these three foundational levels (i), (ii), (iii) interprets a logical calculus, which can be a fragment of its own background logic (i)?

The standard notion of model and its usual epistemological underpinning (as developed in Tarski (1941)) hardly allows for thinking about Tarski's topological interpretation as epistemically significant. However, if one sees this construction from a different perspective, which brings one back to Boole and Venn, the same construction appears as a proper part of propositional logic (rather than its interpretation), which accounts (in topological terms) for the *universe of discourse* relevant to this logic . We shall discuss this alternative view on logic in the next Chapter (see Sect. **5.3** below). Let me only mention here that the issue of universe of discourse in Venn's work is closely related to his use of logical diagrams (see Sect. **3.5** above), and that the logical diagrams are topological objects in the sense that their sizes and shapes do not matter but their topological properties (and only such properties) do.

Tarski himself makes the following remark about his proposed topological interpretation of the two propositional calculi (which the translator calls *sentential*):

> The present discussion seems to me to have a certain interest not only from the purely formal point of view; it also throws an interesting light on the content relations between the two systems of the sentential calculus and the intuitions underlying these systems [zugrundeliegende Intuitionen]. (Tarski 1956, p. 421)

Does Tarski claims here seriously that certain topological intuitions underly logical calculi or he rather talks about intuitions here in a vague sense, which does not imply any epistemological commitment on his part? Be it as it may Tarski's topological interpretation of propositional logic points to a connection between geometry and logic, which is very unlike that assumed in Hilbert's *Foundations* of 1899 and other works following the same line of thought. Remind from Sect. **3.3** that in 1899 Hilbert thinks of logic as an ultimate foundation of geometrical theories, which are treated on equal footing with physical theories and theories of other sorts. This way of founding geometry on logic allows Hilbert to avoid in foundations of geometry any appeal to geometrical intuition (or at least this is his intention). When the foundations of Euclidean geometry are remade by Hilbert with a symbolic logical calculus the intuition comes back and its foundational role becomes explicit but this concerns only the *symbolic* rather than the properly geometrical intuition. Now under Tarski's topological interpretation the propositional fragment of logic itself appears to be geometrical in a sense. (The same observation can be made already on the basis of using diagrams in logic.) This suggests that some geometrical (and in particular topological) intuitions may after all play a role in foundations too.