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Axiomatic Architecture of Scientific Theories

HABILITATION THESIS

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Contents:

Abstract

The received concepts of axiomatic theory and axiomatic method, which stem from David Hilbert, need a systematic revision in view of more recent mathematical and scientific axiomatic practices, which do not fully follow in Hilbert's steps and re-establish some older historical patterns of axiomatic thinking in unexpected new forms. In this work I motivate, formulate and justify such a revised concept of axiomatic theory, which for a variety of reasons I call constructive, and then argue that it can better serve as a formal representational tool in mathematics and science than the received concept.

Introduction:

The modern notion of the axiomatic method of theory building was formed in the first half of the 20th century in works by David Hilbert (beginning with his *Foundations of Geometry*, first published in 1899 [115]) and his followers. In Russia the new axiomatic method was pioneered by Veniamin Fedorovitch Kagan (1869-1953) who defended his master's thesis entitled "The Problem of Foundation of Geometry in the Modern Setting" in 1907 at Odessa University [133],[134]. Hilbert's contribution to mathematical logic and the foundations of mathematics allows us today to see him as a founding father of a new formal mathematical approach in logic, which changed dramatically the shape of the discipline and led to its booming continuing development, on equal footing with Gotlob Frege and Bertrand Russell. Importantly, Hilbert was not a logician in the narrow sense of the word; his scientific interests spread much wider, so his research in logic and the foundations of mathematics was included in a larger scientific context, which included pure and applied mathematics as well as mathematical physics. This is why the notion of axiomatic method stemming from Hilbert involves not only a set of formal logical techniques but also a general approach to applications of such techniques in any given area of science and an epistemologically grounded view on the place and the role of axiomatised theories in scientific research and scientific education.

A wide philosophical discussion related to Hilbert's axiomatic approach has been triggered by limiting theorems (conventionally called the *Incompleteness* theorems) obtained by Kurt Gödel in the 1930s, which showed that Hilbert's program of axiomatic grounding of mathematics could not be realised in its strong original form (and which later were followed by a number of other similar limiting results). Without trying to downplay the philosophical significance of this continuing discussion, we would like to stress that it leaves aside some other epistemological questions about the axiomatic method, which are at least as significant. Notice that Gödel's Incompleteness theorems are primarily mathematical statements, which have general epistemological implications only insofar as the mathematical constructions related to these statements are interpreted as general mathematical models of mathematical and scientific theories. Hence the question, which plays the central role in the present study: Are the formal axiomatic theories built by Hilbert's receipt in fact adequate to their real prototypes, i.e., to various mathematical theories developed by working mathematicians not specifically concerned with logical and foundational issues as well as to scientific theories beyond the pure mathematics?

As usual in the philosophy of science we talk here about the *adequacy* of a formal model of a theory to its real prototype in a double sense, which combines normative and descriptive aspects of the issue. On the one hand, we assume after Hilbert that a logically and epistemologically grounded notion of formal axiomatic theory can perform a normative function, i.e., to represent the general formal structure of a well-formed contentful theory. On the other hand, we also assume that the normative notion of well-formed theory cannot be grounded by philosophical speculation alone but must be based on certain samples of our contemporary scientific knowledge, which are judged to be pieces of the best available science by the scientific community and by the epistemologist herself on some informal grounds. Clearly, any judgement to such an effect can be a subject of controversy.

A popular answer to the worry about the apparent (in)adequacy of the standard axiomatic method to the current scientific practice is as follows. Surely, so the argument goes, formal axiomatic theories are highly idealised schematic images of real scientific theories and don't account for certain significant informal aspects of the scientific practice. The formal axiomatic approach provides for a logical analysis of accomplished mathematical and scientific theories but it is not useful for any other scientific purpose. Informal aspects of scientific practice as well as formal aspects of scientific theories can be a matter of philosophical reflection and of epistemological study. However a confusion of these two aspects cannot be helpful and is not justified.

In our view such an answer is not fully satisfactory because it takes it for granted that the notion of formal axiomatic theory and its relationships with scientific practice is fixed once and for all. However, this assumption is not justified. The new axiomatic method designed by Hilbert in the beginning of the 20th century implements some contemporary logical and epistemological ideas, which in their turn generalise upon the contemporary scientific and mathematical practice. Hilbert's approach to building axiomatic theories differs drastically from earlier axiomatic approaches such as Euclid's. However in the 20th century logic and mathematics did not stagnate but, on the contrary, rapidly developed. There is no reason to assume that these developments should leave the core 20th century conception of axiomatic method untouched and provide only for its technical improvement. As we show in what follows, today the standard Hilbert's axiomatic architecture of theories is no longer unique. An analysis of some recent mathematical practice helps us to specify certain alternative formal architectures and alternative conceptions of axiomatic theory-building.

Brief overview of the content of this work:

In the first chapter we stress and analyse differences between the Hilbert-style axiomatic method and more traditional axiomatic approaches in mathematics. Toward this end, we compare the axiomatic theories of elementary geometry by Euclid (Section **1.1**) and by Hilbert (**1.2**), and show that these theories are essentially different even though they share the same intuitive content. We pay special attention to an accurate historical reconstruction of the axiomatic architecture of geometrical theory presented in Euclid's *Elements*. This historical example, along with some examples of recent mathematical theories, serves us as a motivation for our proposed concept of constructive axiomatic theory. In the same chapter we consider in historical and theoretical perspectives, following Vladimir Smirnov [266], the concept of *genetic* method of theory-building, and compare this method with the standard Hilbert-style axiomatic method (**1.3**).

In the second chapter we provide a critical overview of the 20th century scientific and mathematical practices, which involve use of the standard Hilbert-style axiomatic method. This covers pure mathematics, the natural sciences and computer science. We start with an analysis of axiomatic set theory, where this approach has been realised to a fuller extent than in any other area of mathematics (Section **2.1**). We then consider the attempt to introduce the axiomatic method into broader mathematical practice, which is associated with the (pseudo-)name of Nicolas Bourbaki [26], [29] and stress the specific model-based character of Bourbaki's axiomatic approach (**2.2**). In the last Section of this chapter (**2.3**) we analyse attempts to use the Hilbert-style axiomatic method in the sciences and show that to date they haven't been fully successful.

In the <u>third</u> chapter we consider some alternative axiomatic approaches in the mathematics of the 20th and 21st centuries.

In the first Section of this chapter (**3.1**) we analyse the philosophical motivations for and conceptual foundations of Categorical logic and category-theoretic foundations of mathematics in the works of William Lawvere. In this context, we consider the axiomatic topos theory (aka theory of elementary topos) first published by Lawvere in 1970 [162]. Lawvere's axiomatic treatment of topos theory on the basis of general category theory allowed for a significant simplification of this theory and boosted its further development. Even if Lawvere did not aim at a revision of the received concept of axiomatic theory stemming from Hilbert, we show that Lawvere's axiomatic approach was essentially different.

The second Section of this chapter (**3.2**) covers the Homotopy type theory and the related project of building new foundations of mathematics, which by Vladimir Voevodsky's suggestion are called today the Univalent Foundations [95]. Here we also pay attention to philosophical motivations and epistemological implications of Voevodsky's research program. The standard version of Univalent Foundations involves the formal language of constructive Type theory (with dependent types) due to Martin-Löf, which is given an intuitive spatial (to wit homotopical) semantics and allows for computer implementation and thus supports an automated form of proof-checking. The rule-based Gentzen-style formal architecture of this theory and its proof-theoretic semantics motivate (along with the aforementioned historical examples) our proposed concept of *constructive* axiomatic method, which generalises and extends the received concept of axiomatic method, stemming from Hilbert.

In the concluding fourth chapter we summarise our results and set further research plans. After giving a summary of results (Section **4.1**) we systematically present our proposed concepts of constructive axiomatic theory and constructive axiomatic method (**4.2**) and finally describe a constructive approach to the formal reconstruction of scientific theories and a strategy for further development of this approach (**4.3**).

Claims presented to the defence:

1. A new feature, which distinguishes Hilbert's axiomatic method from the more traditional forms of axiomatics including Euclid's is a sharp distinction between the constructive deductive aspect (syntax) and the "existential" objectual aspect (semantics) of a given mathematical theory. Such a two-level formal construction allows for an effective analysis of the logical and semantic structure of a given theory by mathematical means (meta-mathematics) within the corresponding theoretical limits (imposed, in particular, by Gödel Incompleteness). However, existing experience of applications of the standard axiomatic method in 20th-century science makes it evident that this method of formal representation does not effectively support many significant routine tasks including formal proof-checking, which prevents wider use of this method in the mathematical, scientific and industrial practices.

2. The axiomatic theory of elementary topos first published by Lawvere in 1970 connects the constructive deductive part of the theory and its objectual part in a new way via the new concept of the *internal logic* of a given category. Even if the theory of elementary topos can be represented as a standard Hilbert-style axiomatic theory its logical and epistemological underpinnings are very different. Lawvere's axiomatic approach involves, in particular, a non-standard notion of semantic interpretation (functorial semantics).

3. The continuing project of building new *Univalent* foundations of mathematics initiated by Vladimir Voevodsky in 2006 uses a non-standard constructive Gentzen-style axiomatic architecture of theories, which better meets the needs of today's scientific practice than the standard axiomatic architecture; in particular, it supports formal proof-checking by computers.

> The constructive axiomatic method combines capacities of the received axiomatic method and the more traditional "genetic" method of theory-building. The constructive axiomatic method allows for the representation of propositional knowledge along with procedural knowledge, and can be used in digital systems of knowledge representation (KR).

By default, all non-English sources are quoted in the author's translations into English.

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1 From Euclid to Hilbert [1]

In the *Introduction* to his *Foundations of Geometry* of 1899 [115] Hilbert states that:

> "Geometry, like arithmetic, requires for its logical development only a small number of simple, fundamental principles. These fundamental principles are called the axioms of geometry. The choice of the axioms and the investigation of their relations to one another is a problem which, since the time of Euclid, has been discussed in numerous excellent memoirs to be found in the mathematical literature. This problem is tantamount to the logical analysis of our intuition of space." (Hereafter [115] is quoted in English translation [107])

Notice Euclid's name in the above quote. Evidently Hilbert had in mind Euclid's *Elements* when he prepared his *Foundations of Geometry* for publication. Hilbert aims at developing Euclidean geometry on a wholly new conceptual basis. In this sense Hilbert's *Foundations* of 1899 qualifies as a fairly revolutionary work. However one should not forget that rewriting geometrical chapters of Euclid's *Elements* in new terms is itself an old and well-establish tradition in the history of mathematical thought. Hilbert's *Foundations of Geometry* as well as Bourbaki's open-ended *Elements of Mathematics* produced later in the 20th century [26],[29] form part of this long tradition, and can be compared with such groundbreaking works of earlier generations as, for example, *Restored Euclid* by Borelli (1658) [25], *New Elements of Geometry* by Arnauld (1667)[7] and *Euclid Freed from All Flaws* by Saccheri (1733)[83]. Thus the Hilbertian revolution that still strongly influences today's mathematical practice is certainly not the first revolution of this sort and hopefully not the last one.

1.1 Euclid: Doing and Showing

Reading older mathematical texts always involves a hermeneutical dilemma: in order to make sense of the mathematical content of a given old

[1] This chapter is based on [223], [235], [233, Ch. 2-3] and [243].

text one wants to interpret it in modern terms; in order to see the difference between the modern mathematical thinking and older ways of mathematical thinking one wants to avoid anachronisms and understand the old text on its own terms [287]. Any scholar studying older mathematics needs to find a way between the Scylla of "antiquarianism" that seeks the scholar's conversion into a person living during a different historical epoch, and the Charybdis of radical "presentism" that finds in older texts nothing but a minor part of today's standard mathematical curricula and wholly ignores the historical change of basic patterns of mathematical thinking [53].

My way through the channel is the following. We read Euclid's text verbatim (relying on Heiberg's edition of the original Greek [65] and using Fitzpatrick's new English translation [64]), consider its most important modern interpretations (including overtly anachronistic ones), criticize some of these interpretations on the basis of textual evidence, and finally suggest some alternative interpretations. In order to prevent the risk of losing the main argument behind the historical details we formulate now our conclusion. Contrary to popular opinion Euclid's geometry is not a system of propositions some of which have the special status of axioms while some others are derived from the axioms according to certain rules of logical inference. Rather, it can be described after Friedman as "a form of rational argument" [77, p.94] where certain non-propositional principles play a major role. We share the opinion of Müller who claimed back in 1974 that no system of modern logic adequately accounts for Euclid's form of geometrical reasoning [198], see also [196]. However, we also specify in what follows (**3.2**) certain features of the new axiomatic architecture developed within the Univalent Foundations project, which share certain features with the axiomatic architecture of Euclid's *Elements*.

1.1.1 Demonstration and "Monstration"

All Propositions of Euclid's *Elements* (with few easily understandable exceptions) fit into the scheme described by Proclus in his *Commentary* [215] as follows:

"Every Problem and every Theorem that is furnished with all its parts

> should contain the following elements: an *enunciation*, an *exposition*, a *specification*, a *construction*, a *proof*, and a *conclusion*. Of these *enunciation* states what is given and what is being sought from it, a perfect *enunciation* consists of both these parts. The *exposition* takes separately what is given and prepares it in advance for use in the investigation. The *specification* takes separately the thing that is sought and makes clear precisely what it is. The *construction* adds what is lacking in the given for finding what is sought. The *proof* draws the proposed inference by reasoning scientifically from the propositions that have been admitted. The *conclusion* reverts to the *enunciation*, confirming what has been proved." [215, p.203] (italic added)

It is appropriate to notice here that the term "proposition", which is traditionally used in translations as a common name of Euclid's problems and theorems, is not found in the original text of the *Elements*: Euclid numerates these things throughout each Book without naming them by any common name. (The reader will shortly see why this detail is important.) The difference between problems and theorems is explained in **1.4** below. Let's now show how Proclus' scheme applies to Proposition 5 of the First Book (Theorem 1.5), which is a well-known theorem about the angles of the isosceles triangle. References in square brackets are added by the translator; some of them will be discussed later on. Words in round brackets are added by the translator for stylistic reason. Words in angle brackets are borrowed from Proclus' quote above. Throughout this chapter we write these words in italics when we use them in Proclus's specific sense.

[*enunciation:*]

> For isosceles triangles, the angles at the base are equal to one another, and if the equal straight lines are produced then the angles under the base will be equal to one another.

[*exposition*]:

> Let ABC be an isosceles triangle having the side AB equal to the side AC; and let the straight lines BD and CE have been produced further

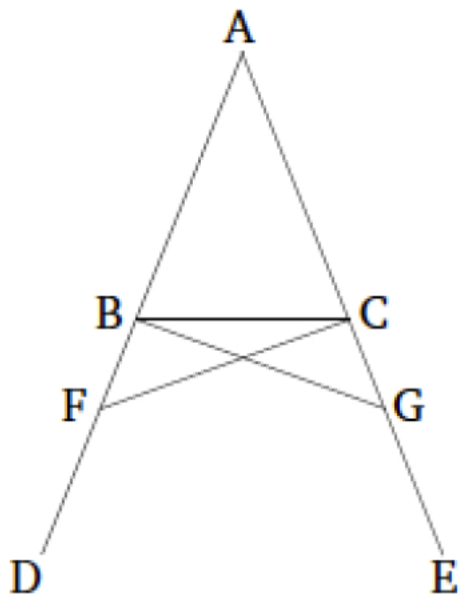

Fig. 1: Theorem 1.5 of Euclid's *Elements*

in a straight line with AB and AC (respectively). [Post. 2].

[*specification*:]

I say that the angle ABC is equal to ACB, and (angle) CBD to BCE.

[*construction*:]

For let a point F be taken somewhere on BD, and let AG have been cut off from the greater AE, equal to the lesser AF [Prop. 1.3]. Also, let the straight lines FC, GB have been joined. [Post. 1]

[*proof*:]

In fact, since AF is equal to AG, and AB to AC, the two (straight lines) FA, AC are equal to the two (straight lines) GA, AB, respectively. They also encompass a common angle FAG. Thus, the base FC is equal to the base GB, and the triangle AFC will be equal to the triangle AGB, and the remaining angles subtended by the equal sides will be equal to the corresponding remaining angles [Prop. 1.4]. (That is) ACF to

ABG, and AFC to AGB. And since the whole of AF is equal to the whole of AG, within which AB is equal to AC, the remainder BF is thus equal to the remainder CG [Ax.3]. But FC was also shown (to be) equal to GB. So the two (straight lines) BF, FC are equal to the two (straight lines) CG, GB respectively, and the angle BFC (is) equal to the angle CGB, while the base BC is common to them. Thus the triangle BFC will be equal to the triangle CGB, and the remaining angles subtended by the equal sides will be equal to the corresponding remaining angles [Prop. 1.4]. Thus FBC is equal to GCB, and BCF to CBG. Therefore, since the whole angle ABG was shown (to be) equal to the whole angle ACF, within which CBG is equal to BCF, the remainder ABC is thus equal to the remainder ACB [Ax. 3]. And they are at the base of triangle ABC. And FBC was also shown (to be) equal to GCB. And they are under the base.

[*conclusion*:]

Thus, for isosceles triangles, the angles at the base are equal to one another, and if the equal sides are produced then the angles under the base will be equal to one another. (Which is) the very thing it was required to show.

An obvious difference between Proclus' analysis of the above theorem and its usual modern analysis is the following. For a modern reader the proof of this theorem begins with Proclus' *exposition* and includes Proclus' *specification*, *construction* and *proof*. Thus for Proclus the *proof* is only a part of what we call today the proof of this theorem. Also notice that Euclid's theorems conclude with the words "which ... was required to *show*" (as correctly translates Fitzpatrick) but not with the words "what it was required to *prove*" (as inaccurately translates Heath [100]). The standard Latin translation of this Euclid's formula as *quod erat demonstrandum* is also inaccurate. These inaccurate translations conflate two different Greek verbs: "apodeiknumi" (English "to prove", Latin "demonstrare") and "deiknumi" (English "to show", Latin "monstrare"). The difference between the two verbs can be clearly seen in the two Aristotle's *Analytics*: Aristotle uses the verb "apodeiknumi" and the derived noun "apodeixis" (proof) as technical terms in his syllogistic logic, and he uses the verb "deiknumi" in a broader and more informal sense when he discusses epistemological issues (mostly in the *Second Analytics*). Without trying to trace here the history of Greek logical and mathematical terminology and speculate about possible influences of some Greek writers on some other writers, we would like to stress the remarkable fact that Aristotle's use of the verbs "deiknumi" and "apodeiknumi" agrees with Euclid's and Proclus'. In ourview this fact alone provides sufficient motivation for taking the difference between the two verbs seriously and distinguishing between *proof* and "showing" (or otherwise between *demonstration* and *monstration*).

The question of the *logical significance* of the *exposition*, the *specification* and the *construction* in Euclid's geometry has been discussed in the literature; in what follows we shall briefly describe some tentative answers to it. However before doing this we would like to stress that this question may be ill-posed to begin with. As far as one assumes, first, that the theory of Euclid's *Elements* is (by and large) sound and, second, that any sound mathematical theory is an axiomatic theory in the modern sense, then, in order to make these two assumptions mutually compatible, one has to describe the *exposition*, the *specification* and the *construction* of each of Euclid's theorems as parts of the proof of this theorem and specify their logical role and their logical status. We shall not challenge the usual assumption according to which Euclid's mathematics is by and large sound. However we shall challenge the other assumption according to which any sound mathematical theory is an axiomatic theory in the modern sense. Since we do not take this latter assumption for granted we do not assume from the outset that the problematic elements of Euclid's reasoning (the *exposition*, the *specification* and the *construction*) play some *logical* role, which only needs to be made explicit and appropriately understood. In what follows we describe how these elements work without making any additional assumptions about them, and only then decide whether the role of these elements qualifies as logical or not.

1.1.2 Are Euclid's Proofs Logical?

Let's look at Euclid's Theorem 1.5 more attentively. We begin its analysis with its *proof*. Among the premises of this *proof*, one may easily identify Axiom (Common Notion) 3 according to which

> (Ax.3): If equal things are subtracted from equal things then the remainders are equal

and the preceding Theorem 1.4 according to which

> (Prop.1.4): If two triangles have two corresponding sides equal, and have the angles enclosed by the equal sides equal, then they will also have equal bases, and the two triangles will be equal, and the remaining angles subtended by the equal sides will be equal to the corresponding remaining angles.

We shall not comment on the role of Theorem 1.4 in this *proof* (which seems to be clear) but will say few things about the role of the Axiom 3.

Here is how exactly the Axiom (Common Notion) 3 is used in Euclid's *proof* above. First, *by construction* we have

Con1: $BF \equiv AF - AB$ and **Con2**: $CG \equiv AG - AC$

which is tantamount to saying that point B lays between points A, F and point C lays between points A, G. Second, *by hypothesis* we have

Hyp: $AB = AC$

and once again *by construction*

Con3: $AF = AG$

Now we see that we have got the situation described in Ax.3: equal things are subtracted from equal things. Using this Axiom we conclude that $BF = CG$.

Notice that Ax.3 applies to all "things" (mathematical objects), for which the relation of *equality* and the operation of *subtraction* make sense. In Euclid's mathematics this relation and this operation apply not only to straight segments and numbers but also to geometrical objects of various sorts including *figures*, angles and solids. Since Euclid's equality is not interchangeable with identity we use for the two relations two different symbols: namely, we use the usual symbol for Euclid's equality (even if this equality is not quite usual), and use symbol $\equiv$ for identity. My use of symbols $+$ and $-$ is self-explanatory[2].

The other four of Euclid's Axioms (not to be confused with Postulates!) have the same character. This makes Euclid's Axioms in general, and Ax.3 in particular, very unlike premises like **Con1-3** and **Hyp**, so one may wonder whether the very idea of treating these things on equal footing (as different *premises* of the same inference) makes sense. More precisely we have here the following choice. One option is to interpret Ax.3 as the following implication:

$$\{(a \equiv b - c)\&(d \equiv e - f)\&(b = d)\&(c = f)\} \to (a = b)$$

and then use it along with **Con1-3** and **Hyp** for getting the desired conclusion through *modus ponens* and other appropriate rules. This standard analysis involves a fundamental distinction between premises and conclusion, on the one hand, and rules of inference, on the other hand. It assumes that in spite of the fact that Euclid remains silent about logic (as most of other mathematicians of all times), his reasoning nevertheless follows some implicit logical rules. The purpose of logical analysis in this case is to make this "underlying logic" (as some philosophers like to call it) explicit.

The other option that we have in mind is to interpret Ax.3 itself as a rule rather than as a premiss. Following this rule, which can be pictures as follows:

$$\frac{(a \equiv b - c), (d \equiv e - f), (b = d), (c = f)}{a = b} \tag{1}$$

one derives from **Con1-3** and **Hyp** the desired conclusion. So interpreted Ax.3 hardly qualifies as a *logical* rule because it applies only to propositions of a

[2]The *difference* $A - B$ of two figures A, B is a figure obtained through "cutting" B out of A; the *sum* $A + B$ is the result of *concatenation* of A and B. These operations are not defined up to *congruence* of figures (for there are, generally speaking, many possible ways, in which one may cut out one figure from another) but, according to Euclid's Axioms, these operations are defined up to Euclid's *equality*. This shows that Euclid's *equality* is weaker than *congruence*: according to Axiom 4 congruent objects are equal but, generally, the converse does not hold. In the case of (plane) figures Euclid's equality is equivalent to the equality (in the modern sense) of their air.

particular sort (namely, of the form $X = Y$ where X, Y are *mathematical* objects of appropriate types). This Axiom cannot help one to prove that Socrates is mortal. Nevertheless the domain of application of this rule is quite vast and covers the whole of Euclid's mathematics. An important advantage of this analysis is that it doesn't require one to make any assumption about hidden features of Euclid's thinking: unlike the distinction between logical rules and instances of applications of these rules the distinction between axioms and premises like **Con1-3** and **Hyp** is explicit in Euclid's *Elements*.

There is also a historical reason to prefer the latter reading of Euclid's Common Notions. Aristotle uses the word "axiom" interchangeably with the expressions "common notions", "common opinions" or simply "commons" for what we call today logical laws or logical principles but not for what we call today axioms. Moreover in this context he systematically draws an analogy between mathematical common notions and his proposed logical principles (laws of logic). This among other things provides an important historical justification for calling Euclid's Common Notions by the name of Axioms. It is obvious that mathematics in general and mathematical common notions (axioms) in particular serve for Aristotle as an important source for developing the very idea of logic. Roughly speaking Aristotle's thinking, as we understand it, is this: behind the basic principles of mathematical reasoning spelled out through mathematical common notions (axioms) there are other yet more general principles relevant to reasoning about all sorts of beings and not only about mathematical objects. The fact that Euclid, according to the established chronology, is younger than Aristotle by some 25 years (Euclid's dates unlike Aristotle's are only approximate) shouldn't confuse one. While there is no strong evidence of the influence of Aristotle's work on Euclid, the influence on Aristotle of the same mathematical tradition, on which Euclid elaborated, is clearly documented in Aristotle's writings themselves. In particular, Aristotle quotes Euclid's Ax.3 (which, of course, Aristotle could know from another source) almost verbatim [3].

[3] Here are some quotes:

> "By first principles of proof [as distinguished from first principles in general] I mean the common opinions on which all men base their demonstrations, e.g. that one of two contradictories must be true, that it is impossible for the same thing both be and not to be, and all other propositions of

However important Aristotle's argument may be in the history of Western thought, there is no reason to take it for granted today every time when we try to interpret Euclid's *Elements* or any other old mathematical text. Whatever one's philosophical stance concerning the place of logical principles in human reasoning, one can see what kind of harm can be done if Aristotle's assumption about the primacy of logical and ontological principles is taken straightforwardly and uncritically: one treats Euclid's Axioms on equal footing with premisses like **Con1-3** and **Hyp** and so misses the law-like character of the Axioms. Missing this feature doesn't allow one to see the relationships between Greek logic and Greek mathematics, which we have just sketched.

Having said that, we would like to repeat that Euclid's *proof* (apodeixis) is the part of Euclid's theorems, which more resembles what we today call proof (in

---

this kind." (Met. 996b27-32, Heath's translation, corrected)

Here Aristotle refers to a logical principle as "common opinion". In the next quote he compares mathematical and logical axioms:

> "We have now to say whether it is up to the same science or to different sciences to inquire into what in mathematics is called axioms and into [the general issue of] essence. Clearly the inquiry into these things is up to the same science, namely, to the science of the philosopher. For axioms hold of everything that [there] is but not of some particular genus apart from others. Everyone makes use of them because they concern being qua being, and each genus is. But men use them just so far as is sufficient for their purpose, that is, within the limits of the genus relevant to their proofs. Since axioms clearly hold for all things qua being (for being is what all things share in common) one who studies being qua being also inquires into the axioms. This is why one who observes things partly [=who inquires into a special domain] like a geometer or a arithmetician never tries to say whether the axioms are true or false." (Met. 1005a19-28, our translation)

Here is the last quote where Aristotle refers to Ax.3 explicitly:

> "Since the mathematician too uses common [axioms] only on the case-by-case basis, it must be the business of the first philosophy to investigate their fundamentals. For that, when equals are subtracted from equals, the remainders are equal is common to all quantities, but mathematics singles out and investigates some portion of its proper matter, as e.g. lines or angles or numbers, or some other sort of quantity, not however qua being, but as [..] continuous." (Met. 1061b, our translation)

The "science of philosopher" otherwise called the "first philosophy" is Aristotle's logic, which in his understanding is closely related to (if not indistinguishable from) what we call today ontology. After Alexandrian librarians we today call the relevant collection of Aristotle's texts by the name of *metaphysics* and also use this name for a relevant philosophical discipline.

logic) than other parts of Euclid's theorems. For this reason, in what follows we shall call those inferences in Euclid's *proofs* which are based on Axioms *protological* inferences, and distinguish them from inferences of another type that we shall call *geometrical* inferences. This analysis is not incompatible with the idea (going back to Aristotle) that behind Euclid's protological and geometrical inferences there are inferences of a more fundamental sort, that can be called *logical* in the proper sense of the word. However we claim that Euclid's text as it stands provides us with no evidence in favor of this strong assumption. One can learn Euclid's mathematics and fully appreciate its rigor without knowing anything about logic, just like Moliere's M. Jourdain could express himself well long before he learned anything about prose!

Whether or not the science of logic really helps one to improve on mathematical rigor — or it is rather the mathematical rigor that helps one to do logic rigorously — is a controversial question that we shall discuss further in this work. The purpose of our present reading of Euclid is at the same time more modest and more ambitious than the purpose of logical analysis. It is more modest because this reading doesn't purport to assess Euclid's reasoning from the viewpoint of today's mathematics and logic but aims at reconstructing this reasoning in its authentic archaic form. It is more ambitious because it doesn't take today's viewpoint for granted but aims at reconsidering this viewpoint by bringing it into a historical perspective.

1.1.3 Instantiation, Objecthood and Objectivity

Let us now see where the premises **Hyp** and **Con 1-3** come from. As we have already mentioned they actually come from two different sources: **Hyp** is assumed *by hypothesis* while **Con 1-3** are assumed *by construction*. Here I shall consider these two cases one after the other.

The notion of hypothetic reasoning is an important extension of the core notion of axiomatic theory outlined above; it is well-treated in the literature and we shall not cover it here in full. We shall consider only one particular aspect of hypothetical reasoning as it is present in Euclid. The hypothesis that validates **Hyp**, informally speaking, amounts to the fact that Theorem 1.5 tells us something about isosceles triangles (rather than about objects of another sort). The corresponding definition (Definition 1.20) tells us that two sides of the isosceles triangle are equal. However to get from here to **Hyp** one needs yet another step. The *enunciation* of Theorem 1.5 refers to isosceles triangles *in general.* But **Hyp** that is involved in the *proof* of this Theorem concerns only the *particular* triangle ABC. Notice also that the *proof* concludes with the propositions $ABC = ACB$ and $FBC = GCB$ (where ABC, ACB, FBC and GCB are angles), which also concern only the *particular* triangle ABC. This conclusion differs from the following *conclusion* (of the whole Theorem), which almost verbatim repeats the *enunciation* and once again refers to isosceles triangles and their angles in general terms.

The wanted step that allows Euclid to proceed from the *enunciation* to **Hyp** is made in the *exposition* of this Theorem, which introduces triangle ABC as an "arbitrary representative" of isosceles triangles (in general). In terms of modern logic this step can be described as the *universal instantiation* :

$$\forall x P(x) \Longrightarrow P(a/x)$$

where $P(a/x)$ is the result of the substitution of the individual constant a at the place of all free occurrences of variable x in $P(x)$. The same notion of universal instantiation allows us to interpret Euclid's *specification* in the obvious way. The reciprocal backward step that allows Euclid to obtain the *conclusion* of the Theorem from the conclusion of the *proof* can be similarly described as the *universal generalization* :

$$P(a) \Longrightarrow \forall x P(x)$$

(which is a valid rule only under certain conditions that we skip here).

As long as the *exposition* and the *specification* are interpreted in terms of the universal instantiation these operations are understood as logical inferences and, accordingly, as elements of a proof in the modern sense of the word. A somewhat different - albeit not wholly incompatible - interpretation of Euclid's *exposition* and *specification* can be straightforwardly given in terms of Kant's *transcendental aesthetics* and *transcendental logic* developed in his *Critique of Pure Reason* [136]. Kant thinks of the traditional geometrical *exposition* not as a logical inference of one proposition from another but as a "general procedure of the imagination for providing a concept with its image"; a representation of such a general procedure Kant calls a *schema* of the given concept (A140). Thus for Kant any individual mathematical object (like triangle ABC) always comes with a specific *rule* that one follows in constructing this object in one's imagination, and that provides a link between this object and its corresponding concept (the concept of isosceles triangle in our example). According to Kant the representation of general concepts by imaginary individual objects (which Kant also describes as "construction of concepts" for short) is the principal distinctive feature of mathematical thinking, which distinguishes it from philosophical speculation.

> "Philosophical cognition is rational cognition from concepts, mathematical cognition is that from the construction of concepts. But to construct a concept means to exhibit a priori the intuition corresponding to it. For the construction of a concept, therefore, a non-empirical intuition is required, which consequently, as intuition, is an individual object, but that must nevertheless, as the construction of a concept (of a general representation), express in the representation universal validity for all possible intuitions that belong under the same concept, either through mere imagination, in pure intuition, or on paper, in empirical intuition. [*dots*] The individual drawn figure is empirical, and nevertheless serves to express the concept without damage to its universality, for in the case of this empirical intuition we have taken account only of the action of constructing the concept, to which many determinations, e.g., those of the magnitude of the sides and the angles, are entirely indifferent, and thus we have abstracted from these differences, which do not alter the concept of the triangle.
> Philosophical cognition thus considers the particular only in the universal, but mathematical cognition considers the universal in the particular, indeed even in the individual." (KRV, A713-4/B741-2).

[135], [136]

Kant's account can be understood as a further explanation of what the instantiation of mathematical concepts amounts to; then one may claim that the Kantian interpretation of Euclid's *exposition* and *specification* is compatible with its interpretation as universal instantiation in the modern sense. However the Kantian interpretation doesn't on its own suggest the instantiation should be interpreted as a logical procedure in a narrow sense, i.e., as an inference of a proposition from another proposition. As the above quote makes clear, Kant describes the instantiation as a cognitive procedure of a different sort.

Now coming back to Euclid, we must first of all admit that the *exposition* and the *specification* of Theorem 1.5 as they stand are too concise to justify preferring one philosophical interpretation rather than another. Euclid introduces an isosceles triangle through Definition 1.20 providing no rule for constructing such a thing. (This example may serve as evidence against the often-repeated claim that every geometrical object considered by Euclid is supposed to be constructed on the basis of Postulates beforehand.) Nevertheless given the important role of constructions in Euclid's geometry, which we explain in the next Section, the idea that every geometrical object in Euclid has an associated construction rule appears very plausible. There is also another interesting textual feature of Euclid's *specification* that in ourview makes the Kantian interpretation more plausible.

Notice the use of the first person in the *specification* of Theorem 1.5 : "I say that ....". In *Elements* Euclid uses this expression systematically in the *specification* of every theorem. Interpreting the *specification* in terms of universal instantiation one should, of course, disregard this feature as merely rhetorical. However it may be taken into account through the following consideration. While the *enunciation* of a theorem is a general proposition that can be best understood á la Frege in abstraction from any human or inhuman thinker, i.e., independently of any thinking *subject* who might believe this proposition, assert it, refute it, or do anything else about it, the core of Euclid's theorems (beginning with their *exposition*) involves an individual thinker (individual subject) that cannot and should not be wholly abstracted away in this context. When Euclid *enunciates* a theorem this *enunciation* does not involve - or at least is not supposed to involve

- any particularities of Euclid's individual thinking; the less this *enunciation* is affected by Euclid's (or anyone else's) individual writing and speaking style the better. However the *exposition* and the*specification* of the given theorem essentially involve an *arbitrary* choice of notation ("Let ABC be an isosceles triangle..."), which is an individual choice made by an individual mathematician (namely, made by Euclid on the occasion of writing his *Elements*). This individual choice of notation goes on par with what we have earlier described as *instantiation*, i.e. the choice of one individual triangle (triangle ABC) of the given type, which serves Euclid for proving the general theorem about *all* triangles of this type. The *exposition* can also be naturally accompanied by drawing a diagram, which in its turn involves the choice of a particular shape (provided this shape is of the appropriate type), leaving aside the choices of its further features like color, etc.

Thus when in the *specification* of Theorem 1.5 we read "I say that the angle ABC is equal to ACB" we indeed do have good reason to take Euclid's wording seriously. For the sentence "angle ABC is equal to ACB" unlike the sentence "for isosceles triangles, the angles at the base are equal to one another" has a feature that is relevant only to one particular presentation (and to one particular diagram if any), namely the use of letters A, B, C rather than some others [4]. The words "I say that ..." in the given context stress this situational character of the following sentence "angle ABC is equal to ACB". What matters in these words is, of course, not Euclid's personality but the reference to a particular act of speech and cognition of an individual mathematician. Proving the same theorem on a different occasion Euclid or anybody else could use other letters and another diagram of the appropriate type.

A competent reader of Euclid is supposed to know that the choice of letters in Euclid's notation is arbitrary and that Euclid's reasoning does not depend on this choice. The arbitrary character of this notation should be distinguished from the general arbitrariness of linguistic symbols in natural languages. What is specific for the case of *exposition* and *specification* is the fact that here the arbitrary elements of reasoning (like notation) are sharply distinguished from its invariant elements. To use Kant's term we can say that behind the notion

[4]Although the choice of letters in Euclid's notation is arbitrary the *system* of this notation is not. This traditional geometrical notation has a relatively stable and rather sophisticated syntax.

according to which the choice of Euclid's notation is arbitrary (at least to the degree that letters used in this notation are permutable) and according to which the same reasoning may work equally well with different diagrams (provided all of them belong to the same appropriate type) there is a certain invariant *schema* that sharply limits such possible choices. This schema not only *allows* for making some arbitrary choices but *requires* every possible choice in the given reasoning to be wholly arbitrary. This requirement is tantamount to saying that subjective reasons behind choices made by an individual mathematician for presenting a given mathematical argument are strictly irrelevant to the "argument itself" (in spite of the fact that the argument cannot be formulated without making such choices). In general talks in natural languages there is no similar sharp distinction between arbitrary and invariant elements. When I write this text I can certainly change some of the wording without changing the sense of our argument, but I am not in a position to describe precisely the scope of such possible changes and identify the intended "sense" of my argument with mathematical rigour. This is because the present study is philosophical and historical, not purely mathematical.

Thus Euclid's *exposition* serves for the formulation of a given universal proposition in terms, which are suitable for a particular act of mathematical cognition made by an individual mathematician. This aspect of the *exposition* is not accounted for by the modern notion of universal instantiation. It may be argued that this aspect of the *exposition* needs not be addressed in a *logical* analysis of Euclid's mathematics that aims at explication of the *objective meaning* of Euclid's reasoning and may well leave aside cognitive aspects of this reasoning. We agree that this latter issue lies out of the scope of logical analysis in the usual sense of the term but we disagree that the objective meaning of Euclid's reasoning can be properly understood without addressing this issue. Euclid's mathematical reasoning is *objective* due to a mechanism that allows one to make universally valid inferences through one's individual thinking. Whatever the "objective meaning" might consist of this mechanism must be taken into account.

1.1.4 Proto-Logical Deduction and Geometrical Production

Recall that the *proof* of Euclid's Theorem 1.5 uses not only premiss **Hyp** assumed "by hypothesis" but also premisses **Con 1-3** (as well as a number of other premisses of the same type) assumed "by construction". We turn now to the question about the role of Euclid's *constructions* (which, but the way, are ubiquitous not only in geometrical but also in arithmetical Books of the *Elements*) and more specifically consider the question how these *constructions* support certain premisses that are used in following *proofs*.

As is well known, Euclid's geometrical constructions are supposed to be realised "by ruler and compass". In the *Elements* this condition is expressed in the *Elements* through the following three

> Postulates:
> 1. Let it have been postulated to draw a straight-line from any point to any point.
> 2. And to produce a finite straight-line continuously in a straight-line.
> 3. And to draw a circle with any center and radius.

(We leave out of the present discussion Euclid's two further Postulates including the problematic Fifth Postulate.)

Before we consider popular interpretations of these Postulates and suggest our own interpretation let us briefly discuss the very term "postulate", which is traditionally used in English translations of Euclid's *Elements*. Fitzpatrick translates Euclid's verb "aitein" by the English verb "to postulate" following the long tradition of Latin translations, where this Greek verb is translated by the Latin verb "postulare". However, according to today's standard dictionaries the modern English verb "to postulate" does not translate the Greek verb "aitein" and the Latin verb "postulare" in general contexts: the modern dictionaries translate these verbs into "to demand" or "to ask for". This shows clearly that the meaning of the English verb "to postulate" that derives from Latin "postulare" changed during its lifetime[5].

[5] I reproduce here Fitzpatrick's footnote about Euclid's expression "let it be postulated":

Aristotle describes a postulate (aitema) as what "is assumed when the learner either has no opinion on the subject or is of a contrary opinion" (*An. Post.* 76b); further he draws a contrast between postulates and *hypotheses* saying that the latter appear more plausible to the learner than the former (*ibid.*). It is unnecessary for our present purpose to go any further into this semantical analysis, trying to reconstruct an epistemic attitude that Euclid might have had in mind "demanding" the reader to take his Postulates for granted. The purpose of the above philological remark is rather to warn the reader that the modern meaning of the English word "postulate" can easily mislead when one tries to interpret Euclid's Postulates adequately. So we suggest reading Euclid's Postulates as they stand and trying to reconstruct their meaning from their context, forgetting for a while what one has learned about the meaning of the term "postulate" from modern sources.

Euclid's Postulates are usually interpreted as propositions of a certain type and on this basis are qualified as axioms in the modern sense of the term. There are at least two different ways of rendering Postulates in a propositional form. We shall demonstrate them at the example of Postulate 1. This Postulate can be interpreted either as the following *modal* proposition:

(PM1): given two different points it is always possible to draw a (segment of) straight-line between these points

or as the following *existential* proposition:

(PE1): for any two different points there exists a (segment of) straight-line lying between these points.

Propositional interpretations of Euclid's Postulates allow one to present Euclid's geometry as an axiomatic theory in the modern sense of the word and, more specifically, to present Euclid's geometrical constructions as parts of proofs of his theorems. Even before the modern notion of axiomatic theory was strictly

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"The Greek present perfect tense indicates a past action with present significance. Hence, the 3rd-person present perfect imperative *Hitesthw* could be translated as "let it be postulated", in the sense "let it stand as postulated", but not "let the postulate be now brought forward". The literal translation "let it have been postulated" sounds awkward in English, but more accurately captures the meaning of the Greek."

defined in formal terms, many translators and commentators of Euclid's *Elements* tended to think about his theory in this way and interpreted Euclid's Postulates in the modal sense. Later a number of authors ([119], [127]) proposed different formal reconstructions of Euclid's geometry based on the existential reading of Postulates. According to Hintikka and Remes

> "[R]eliance on auxiliary construction does not take us outside the axiomatic framework of geometry. Auxiliary constructions are in fact little more than ancient counterparts to applications of modern instantiation rules." [120, p. 270]

The instantiation rules work in this context as follows. First, through *universal instantiation* (which under this reading corresponds to Euclid's *exposition* and *specification*) one gets some propositions like **Hyp** about certain particular objects (individuals) like AB and AC. Then one uses Postulates 1-3 reading them as existential axioms according to which the existence of certain geometrical objects implies the existence of certain further geometrical objects, and so proves the (hypothetical) existence of such further objects of interest. Finally one uses another instantiation rule called the rule of *existential instantiation*:

$$\frac{\forall x P(x)}{P(a)} \tag{2}$$

and thus "gets" these new objects. Under this interpretation Euclid's *constructions* turn into logical inferences of a sort. As Hintikka and Remes emphasise in their paper, the principal distinctive feature of Euclid's *constructions* (under their interpretation) is that these constructions introduce some *new* individuals; they call such individuals "new" in the sense that these individuals are not (and cannot be) introduced through the universal instantiation of hypotheses forming part of the *enunciation* of the given theorem.

The propositional interpretations of Euclid's Postulates are illuminating because they allow for the analysis of traditional geometrical constructions in modern logical terms. However they require a paraphrasing of Euclid's wording, which from a logical point of view is far from being innocent. In order to see this let us leave aside the epistemic attitude expressed by the verb "postulate" and focus on the question of *what* Euclid postulates in his Postulates 1-3. Literally, he postulates the following:

P1: to draw a straight-line from any point to any point.
P2: to produce a finite straight-line continuously in a straight-line.
P3: to draw a circle with any center and radius.

As they stand expressions P1-3 don't qualify as propositions; they rather describe certain *operations*! And making up a proposition from something which is not a proposition is not an innocent step. My following analysis is based on the idea that Postulates are *not* primitive truths from which one may derive some further truths but are primitive operations that can be combined with each other and so produce some further operations. In order to make our reading clear we paraphrase P1-3 as follows:

(OP1): drawing a (segment of) straight-line between its given endpoints
(OP2): continuing a segment of given straight-line indefinitely ("in a straight-line)"
(OP3): drawing a circle by given radius (a segment of straight-line) and center (which is supposed to be one of the two endpoints of the given radius).

Noticeably none of OP1-3 allows for producing geometrical constructions out of nothing; each of these fundamental operations produces a geometrical object out of some other objects, which are supposed to be *given* in advance. The table below specifies inputs (operands) and outputs (results) of OP1-3:

operation	input	output
OP1	two (different) points	straight segment
OP2	straight segment	(bigger) straight segment
OP3	straight segment and one of its endpoints	circle

PE1 as it stands does not imply that there exists at least one point or at least one line in Euclid's geometrical universe. If there are no points then there are no lines either. Similar remarks can be made about the existential interpretation of Euclid's other Postulates. Thus the existential interpretation of Postulates by itself does not turn these Postulates into existential axioms that guarantee that Euclid's universe is non-empty and contains all geometrical objects constructible by ruler and compass. To meet this purpose one also needs to postulate the existence of at least two different points — and then argue that the absence of any counterpart of such an axiom in Euclid is due to Euclid's logical incompetence. The proposed reading of Postulates 1-3 as operations doesn't require such ad hoc stipulations and thus is more faithful to Euclid's text [6].

Hintikka and Remes describe Euclid's geometrical constructions as *auxiliary*. Such a description may be adequate to the role of geometrical constructions in today's practice of teaching elementary geometry, but not to the role of constructions in Euclid's *Elements*. Recall that Euclid's so-called Propositions are of two types: some of them are Theorems while others are Problems (see again the above quotation from Proclus' *Commentary*). In the *Elements*, Problems are at least as important as Theorems and arguably even more important: in fact the *Elements* begin and end with a Problem but not with a Theorem. As we shall now see, when a given *construction* forms part of a problem rather than a theorem, it cannot be described as auxiliary in any appropriate sense. We shall also see the modern title "proposition" is not really appropriate when we talk about Euclid's Problems: while *enunciations* of Theorems do qualify as propositions in the modern logical sense of the term, *enunciations* of Problems do not.

We shall demonstrate these features using the well-known example of Problem 1.1 that opens Euclid's *Elements*; our notational conventions remain the same as in the example of Theorem 1.5.

[*enunciation:*]

[6] Since the concepts of infinite straight line and infinite half-line (ray) are absent from Euclid's geometry, the result of OP2 is always a finite straight segment. However this result is obviously not fully determined by its single operand. This shows that OP2 doesn't quite fit today's usual notion of algebraic operation.

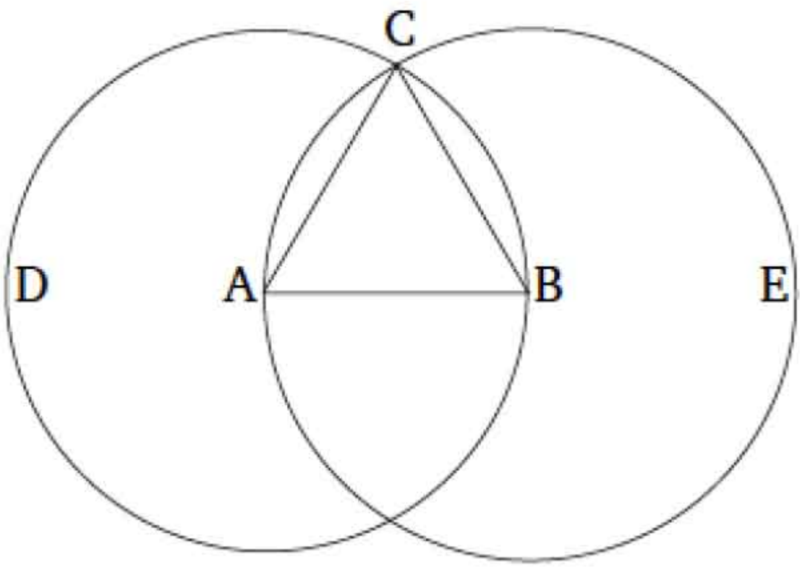

Fig. 2: Problem 1.1 of Euclid's *Elements*

To construct an equilateral triangle on a given finite straight-line.

[*exposition*:]

Let AB be the given finite straight-line.

[*specification*:]

So it is required to construct an equilateral triangle on the straight-line AB.

[*construction*:]

Let the circle BCD with center A and radius AB have been drawn [Post. 3], and again let the circle ACE with center B and radius BA have been drawn [Post. 3]. And let the straight-lines CA and CB have been joined from the point C, where the circles cut one another, to the points A and B [Post. 1].

[*proof*:]

> And since the point A is the center of the circle CDB, AC is equal to AB [Def. 1.15]. Again, since the point B is the center of the circle CAE, BC is equal to BA [Def. 1.15]. But CA was also shown (to be) equal to AB. Thus, CA and CB are each equal to AB. But things equal to the same thing are also equal to one another [Axiom 1]. Thus, CA is also equal to CB. Thus, the three (straight-lines) CA, AB, and BC are equal to one another.

[*conclusion*:]

> Thus, the triangle ABC is equilateral, and has been constructed on the given finite straight-line AB. (Which is) the very thing it was required to do.

As one can see in this example, *enunciations* of Problems are expressed in the same grammatical form as Postulates 1-3, namely in the form of infinitive verbal expressions. We read these expressions in the same straightforward way, in which we read the Postulates: as descriptions of certain geometrical *operations*. The obvious difference between (*enunciations* of) Problems and Postulates is this: while Postulates introduce basic operations taken for granted (drawing by ruler and compass) Problems describe complex operations, which in the last analysis reduce to these basic operations. Such reduction is made through a *construction* of a given Problem: it performs the complex operation described in the *enunciation* of the problem through combining basic operations OP1-3 (and possibly some complex operations performed earlier). The procedure that allows for performing complex operations by combining a small number of repeatable basic operations we shall call a *geometrical production.* In Problem 1.1 the construction of the regular triangle is (geometrically) *produced* from drawing the straight-line between two given points (Postulate 1) and drawing a circle by given center and radius (Postulate 3). This is, of course, just another way of saying that the regular triangle is constructed by ruler and compass; the unusual terminology helps us to describe Euclid's geometrical constructions more precisely.

Let us see in more detail how Euclid's geometrical production works. Basic operations OP1-3 like other (complex) operations need to be *performed*: in order to produce an output they have to be fed some input. This input is provided through the *exposition* of the given Problem (the straight line AB in the above example). OP1-3 are composed in the usual way well-known from today's algebra: outputs of earlier performed operations are used as inputs for further operations[7].

Just like Postulates 1-3, *enunciations* of Problems can be read as modal or existential propositions (in the modern logical sense of the term). Then the (modified) *enunciation* of Problem 1.1 reads:

> (1.1.M) it is possible to construct a regular triangle on a given finite straight-line:

or

> (1.1.E) for any finite straight-line there exists a regular triangle on this line.

As soon as the *enunciations* of Euclid's Problems are rendered into the propositional form, the Problems turn into theorems of a special sort. In the case of existential interpretation, Problems turn into *existential* theorems that state (under certain hypotheses) that there exist certain objects having certain desired properties. However this is not what we find in Euclid's text as it stands. Every one of Euclid's Problems ends with the formula "the very thing it was required to do", not "to show" or "to prove". We can see no evidence in the *Elements* that justifies the idea that in Euclid's mathematics *doing* is less significant than *showing* and that the former is in some sense reducible to the latter.

According to another popular reading Euclid's Problems are tasks or questions of a sort. This version of a modal propositional interpretation of Euclid's Problems involves a deontic modality rather than a possibility modality:

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[7]Problem 1.1 involves a difficulty that has been widely discussed in the literature: Euclid does not provide any principle that may allow him to construct a point of intersection of the two circles involved in the *construction* of this Problem. This flaw is usually described as a *logical* flaw. In our view it is more appropriate to describe this flaw as properly*geometrical*, and fill the gap in the reasoning with the following additional postulate (rather than an additional axiom):

> Let it have been postulated to produce a point of intersection of two circles with a common radius.

Even if this additional postulates is introduced here purely ad hoc, the way in which it is introduced gives an idea of how Euclid's Postulates could emerge in the real history.

(1.1.D) it is required to construct a regular triangle on a given finite straight-line:

Indeed geometrical problems similar to Euclid's Problems can be found in today's Elementary Geometry textbooks as exercises. However the analogy between Euclid's Problems and school problems on construction by ruler and compass is not quite precise. *Enunciations* of Euclid's Problems just like the *enunciations* of Euclid's Theorems prima facie express no modality whatsoever. A deontic expression appears only in the *exposition* of the given Problem ("it is required to construct an equilateral triangle on the straight-line AB"). We don't think that this fact justifies the deontic reading of the *enunciation* because, as we have already explained above, the *exposition* describes the reasoning of an individual mathematician. That every complex construction must be performed through Postulates and earlier performed constructions is an epistemic requirement, which is on par with the requirement according to which every theorem must be proved rather than simply stated. Recall that the *expositions* of Euclid's Theorems have the form "I say that...". This indeed provides an apparent contrast with the *expositions* of Problems that have the form "it is required to ....". However this contrast doesn't seem to us to be really sharp. Euclid's expression "I say that..." in the given context is interchangeable with the expression "it is required to show that...", which matches the closing formula of Theorems, "(this is) the very thing it was required to show". Euclid's expression "it is required to..." that he uses in the *expositions* of Problems similarly matches the closing formula of Problems, "(this is) the very thing it was required to do". The requirement according to which every Theorem must be "shown" or "monstrated" doesn't imply, of course, that the *enunciation* (statement) of this Theorem has a deontic meaning. Nor does the requirement according to which every Problem must be "done" imply that the *enunciation* of this Problem has something to do with deontic modalities.

The analogy between axioms and theorems, on the one hand, and postulates and problems, on the other hand, may suggest that Euclid's geometry splits into two independent parts, one of which is ruled by (proto)logical deduction while the other is ruled by geometrical production. However, this doesn't happen, and in fact problems and theorems turn out to be mutually dependent elements of the same theory. The above example of Problem 1.1 and Theorem 1.5 show how the intertwining of problems and theorems works. Theorems, generally, involve *constructions* (called in this case auxiliary), which may depend (in the order of geometrical production) on earlier treated problems (as the *construction* of Theorem 1.5 depends on Problem 1.3.) Problems in their turn always involve appropriate *proofs* that prove that the *construction* of the given theorem indeed performs the operation described in the *enunciation* of this theorem (rather than some other operation). Such *proofs*, generally, depend (in the order of the protological deduction) on certain earlier treated theorems (just as in the case of *proofs* of theorems).

Although this mechanism linking problems with theorems may look unproblematic, it gives rise to the following puzzle. Geometrical production produces geometrical objects from other objects. Protological deduction deduces certain propositions from other propositions. How then it may happen that the geometrical production has an impact on the protological deduction? In particular, how may the geometrical production justify premises assumed "by construction", so that these premises are used in following *proofs*?

In order to answer this question, let's come back to the premise **Con3** ($AF = AG$) from Theorem 1.5 and see what if anything makes it true. $AF = AG$ because Euclid or anybody else following Euclid's instructions constructs this pair of straight segments in this way. How do we know that by following these instructions one indeed gets the desired result? It is because the *construction* of Problem 1.3 that contains the appropriate instruction is followed by a *proof* that proves that this *construction* does exactly what it is required to do. *Construction* 1.3 in its turn uses *construction* 1.2 while *construction* 1.2 uses *construction* 1.1 quoted above. In other words *construction* 1.1 (geometrically) produces *construction* 1.2 and *construction* 1.2 in its turn produces *construction* 1.3. This geometrical production produces the relevant part of *construction* 1.5 (the construction of equal straight segments AF and AG) from first principles, namely from Postulates 1-3. In order to get the corresponding protological deduction of premise **Con3** from first principles we should now look at *proofs* 1.1, 1.2 and 1.3 and then combine these three proofs into a single chain. To save space, we leave the details to the reader and report only what we get in the end. The result is somewhat surprising from the point of view of modern logical analysis. The chain of *constructions* leading to *construction* 1.5 involves a number of circles (through Postulate 3). Radii of a given circle are equal by definition (Definition 1.15). Thus by constructing a circle and its two radii, say, X and Y one gets a primitive (not supposed to be proved) premise $X = Y$. Having at hand a number of premises of this form and using Axioms as inference rules (but not as premises!) one gets the desired deduction of **Con3**. The fact that first principles of the protological deduction of **Con3** appear to be partly provided by a definition helps to explain why Euclid places his definitions among other first principles such as postulates and axioms.

A logical analysis of Euclid's geometry that involves a propositional (in particular existential) reading of postulates aims at replacing these two sets of rules by a single set of rules called *logical.* We would like to stress again that the results of our proposed analysis do not exclude the possibility of logical analysis. Such a replacement may be a good idea or not, but in any event logical rules are not made explicit in the Euclid's text, and we do not see much point in saying that he uses rules of this sort implicitly. The fact that *we* can today use modern logic to interpret Euclid is a completely different issue. An interpretation of Euclid's geometry by means of logical analysis can indeed be illuminating, but one should not confuse oneself by believing that Euclid already had a grasp of modern logic even if he could not formulate principles of this logic explicitly. The fact that Euclid's Axioms and Postulates can be easily reformulated and expressed in modern formal languages does not mean that such a reformulation can be easily extended to the whole of Euclid's theory of geometry. As has been demonstrated by Hilbert [115], such a logical reconstruction of Euclid's theory involves a dramatic change of its basic architecture.

1.2 Hilbert: Making It Formal

1.2.1 Thought-things and thought-relations

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,. . . ; those of the second, we will call straight lines and designate them by the letters a, b, c,..; 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 tacitly taken for granted here (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 like "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 tell 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* these objects and relations are.

To see how this 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 recall that the 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 also be 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 not seem to be 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 we 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 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 translating 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, in Hilbert's words, 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 [108] Hilbert grants a fundamental role to geometrical intuition of a *special* sort.

How may a proposition like A1.1 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." ([75, p. 12]

Some comments are here in order.

(1) Unlike Frege [75], 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. 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 had not yet formulated 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 is 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* do the axioms "define" them? Let us 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 gets what Hilbert, after Kant, calls *objects of thought* or *thought-things* (*Gedankendinge* in German), which are related by corresponding *thought-relations.* These thought-things and thought-relations exist merely in virtue of the fact that one thinks of them consistently. They may or may not be 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* [136]. Hintikka [116] 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, in Kant's terms, *in concreto* (which shows, by the way, that the usual characterization of such object as *abstract* is somewhat misleading). Mathematical intuition in the relevant restricted sense of the term is the capacity to think concretely about objects and relations between objects without associating these objects and these relations with any additional qualities.

The second answer concerns the role of sensual intuition. Recall that in the introductory part of his *Foundations* of 1899 Hilbert says that his geometrical axioms "express certain related fundamental facts of our intuition". In 1894 Hilbert explains his view on the nature of geometry as follows:

> "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" (quoted after Corry [49, 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." (*ib.* 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 to us suggestive to try to reconcile the two notions of Geometry. As a part of pure mathematics Geometry is treated as a free creation of mind; the fundamental question here is whether or not the given set 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 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 fit well 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, and about the deduction of new theorems from these axioms. 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 more easily 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 shortly see, however, that in his later works Hilbert introduces such ideal elements.

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 Hilbert's other 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 axioms of A are true in M, one does not need to establish the consistency of this set of axioms in advance. Moreover, if the 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. Recall Hilbert's remark according to which any consistent set of propositions can be made by 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 and on this basis conclude that the given set of axioms is consistent. However this conclusion is not valid unless 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 out to be the most productive. Already in his *Foundations* of 1899 Hilbert applies this method systematically; in the course of the 20th century this method develops into the modern *model theory*, which remains today an active field of mathematical research still having some philosophical flavour. We 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 by the name "interpretation", they are too often confused in 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 groups under the common title "interpretation" two procedures which do not belong to the same general type.

(3) Let us finally discuss Hibert's view according to which the axioms of a given mathematical theory "define" the 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 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, however, ask 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 [75], 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 a Euclidean space. This point has already been discussed earlier in this chapter and we 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 is thinking 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* that we shall discuss in what follows (**2.2.2**, **3.2.5**).

Precipitating this further discussion, we 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 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* [103] 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 [50, p. 160] for details. The second edition of Hilbert's *Foundations of Geometry* which appeared in 1903 [105] already contains a geometrical axiom of completeness.

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 are 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.

1.2.2 Logicism and Objectivity

Consider once again Hilbert's First Axiom A1.0

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

and recall 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 an essential role in Hilbert's *Foundations* of 1899: unless at least some words in these axioms are meaningful the axioms reduce to an abracadabra! In the last Section we elaborated on words of the former category, now let us look more attentively at 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 can one 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 "punctuation marks") and the other, which describes itself as *semantic*, develops the idea of logic being *content-invariant* [23]. This later approach dates back to Tarski's proposal [279] to identify logical notions with invariants of all permutations of elements of some given set [8].

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 [141]; it establishes a conceptual link between Klein's and Hilbert's works in the foundations of geometry, which is both conceptually significant and historically plausible.

[8]Bonnay [23] 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

Now we 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." [116, p.20]

This does not mean, of course, that Hilbert, like Russell in [251], tries to *reduce* mathematics to logic. This later version of logicism is certainly not Hilbert's. In this work we shall use the term "mathematical logicism" in a broader sense indicating the epistemic primacy of logic over mathematics. In this broader sense of the term Hilbert's view on mathematics does qualify as a version of logicism.

In the early 1900s Hilbert was not alone thinking about logic as the ground layer foundation of mathematics. However there were also strong opposing voices during the same period of time. Among prominent critics of logical approaches in the foundations of mathematics were Henri Poincaré [55] and Luitzen Brouwer. Consider, for example, this passage from Brouwer written in 1907:

> "[M]athematical reasoning [. . .] is no logical reasoning [. . . ;] it uses the connectives of logic only because of the poverty of language, and thus may perhaps keep 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." (letter to Kroteweg 21.01.1907 quoted after [289, p. 128-129])

The controversy between Hilbert and Brouwer is a founding event of the 20th century philosophy of mathematics. It was Brouwer who in 1912 first formulated the philosophical opposition between Hilbert's *Formalism* and his own philosophical view that he called *Intuitionism* [190]. Brouwer's intuitionism stems from Kant's philosophy of mathematics and comprises a general philosophical background, which will not be analysed in the present work in detail. Hereafter we focus only on some key aspects of the Hilbert-Brouwer controversy including the different views on relationships between logic and mathematics held by the two thinkers. It should be borne in mind that this issue includes the question about boundaries between the two disciplines (if any), which as we have already seen, even today doesn't have a commonly accepted solution. Nevertheless the difference between Hilbert's and Brouwer's understanding of the function of logic in mathematics is obvious and has important consequences, some of which are discussed in what follows.

Nowadays the notion of mathematical intuitionism is usually understood via works of Brouwer's student Arend Heyting who expressed some important aspects of Brouwer's intuitionism in the form of *intuitionistic logic* [102]. According to a popular opinion Heyting in his seminal work distracted from Brouwer's philosophy, which the author himself described as a form of mysticism, a rational core that triggered further important developments. We don't want to diminish the importance of Heyting's work and reject the whole strategy of reconciling Hilbert's and Brouwer's approach [190] that Heyting's achievements made possible. We don't want either to deny that certain aspects of Brouwer's philosophy of mathematics have little or no relevance to logic and mathematics as these disciplines are practiced today. Nevertheless we claim that some of Brouwer's insights, which have been left aside by Heyting's formalisation, are relevant and significant in the context of recent developments. This concerns Brouwer's view on the relationships between logic and mathematics. Notice that the sheer replacement of Classical logic by the *Intuitionistic logic* in the standard architecture of mathematical theories and, more generally, in the standard foundations of mathematics, is compatible with the (weak) mathematical logicism, which Brouwer definitely refuses. It can be argued that this weak form of logicism is simply indispensable in any science and in any rational thinking.

This argument, in our view, is plainly wrong; instead of trying to meet it here with general epistemological arguments, we point in what follows to some problematic aspects of the weak logicism and then describe certain mathematical and logical approaches which are motivated by and support different views on the relationships between logic and mathematics.

An important argument against Russell's mathematical logicism was given in the same year (1907) by Ernest Cassirer [41], apparently quite independently of Brouwer at this point. Referring to Russell [251] 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 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. [...] 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." [41, pp. 43-44]

So, according to Cassirer, what the formal logical foundations of mathematics can *not* possibly provide (whatever system of formal logic is one's favourite) are the notions of objecthood and objectivity appropriate for doing modern mathematically-laden empirical science — as opposed to the traditional Aristotle-style metaphysics. The popular idea of equating the notion of object with that of logical individual, which stems from Frege [205], not only leaves this problem open and but also hides it by eliminating a useful terminological distinction. Even if Cassirer directs this arguments against the strong form of mathematical logicism represented by Russell, it also applies to Hilbert's weaker form of mathematical logicism. An axiom system in Hilbert's sense indeed qualifies as a "system of hypothetical assumptions", and what Cassirer says in the above quote about the "strict deductive link" (between axioms and theorems of a given theory) justly describes Hilbert's idea of formal mathematical proof. The problem of "laws governing the world of objects" in Hilbert's formal mathematics remains wholly open just as in Russell's case

Like Russell, Cassirer believes that by 1900 "logic and mathematics have been fused into a true, henceforth indissoluble unity" [41, p. 4]. This allows Heis to qualify Cassirer's views on mathematics in [41] as a form of logicism [101]. However Heis also rightly remarks that unlike Russell, Cassirer does not qualify formal logic as an independent foundation of mathematics. Here we use the term "logicism" in a sense that *implies* what Heis calls the "foundationalist ambition". From an epistemological point of view this is a crucial aspect of relationships between logic and mathematics. The moral is that such general terms as "logicism" or any other similar "-ism" have a limited usefulness in philosophy, and in each particular case one needs to explicate their contents.

[9] .

[9]

"Cassirer's logicism has no foundationalist ambitions. For many logicists, including Russell, the appeal of logicism is that mathematics, whose certainty might otherwise be in doubt, gets to

One may object that unlike Russell's project Hilbert's project of the axiomatisation of mathematics assumes that every formal mathematical theory comes with a certain class of its models which includes one or more *intended* models, and that models and their elements qualify as *objects* of the given theory. However, in our view, the standard concept of model of an axiomatic theory does not solve the problem of objecthood stressed by Cassirer in the above quote. Even if models and their elements indeed qualify as mathematical objects treated by the corresponding formal theory, the only candidate for "laws governing the world of objects" in this case are rules of formal deduction, which by default are interpreted as laws of logical inference applied to propositions rather than as constructive rules applied to these objects themselves.

Traditional Euclid-style geometry solves this problem by stipulating a system of rules for object-building, i.e., geometrical construction, which Euclid calls *postulates*. Kant famously provides a thorough analysis of the epistemic significance of these rules and explains, having Newton's *Principia* in his mind, how these geometrical rules contribute to theories of physics [77]. Cassirer is fully aware of the fact that Kant's philosophy of mathematics and his account of the epistemic role of mathematics in the natural sciences is in 1907 hopelessly outdated and needs a profound revision. However he doesn't accept the solution offered by the mathematical logicism because he, quite rightly in our view, sees in it a revival of a Scholastic pattern of doing science where logic and metaphysics are seen as the ultimate foundations of all empirical science [237], [231].

Indeed as early as 1918 Russell supplements his philosophy of mathematics with a metaphysical doctrine that he calls the *logical atomism*. This is how he

inherit the privileged epistemological status enjoyed by logic. Cassirer rejects this contention, because he does not believe that formal logic has a place of "honor and security" not shared by mathematics or natural science. On his view, the most fundamental kind of logic is transcendental logic, the investigation of the preconditions of science. Since there is no epistemological route to "formal logic" except through an analysis of our best current science, taken as a fact, any attempt to ground the certainty of the latter in terms of the former is a fool's errand. Cassirer's logicism is thus a "transcendental" logicism: mathematics is a branch of transcendental logic — the science of the a priori principles that make (mathematical natural scientific) knowledge possible." [101, p.128] describes the relation of this doctrine to logic and mathematics in the *Introduction* to his [252]:

> "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 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." [126, pp. 7-8]

Thus an older pattern of intellectual work, which many people in the 19th century believed to be definitely sublated by Kant's critical philosophy and other new developments, reemerged in the beginning of the 20th century in the context of new mathematics and new logic. 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.* Nowadays *Analytic Metaphysics* is a recognised academic discipline taught at many philosophy departments, which claims a scientific status. The fact that modern logic led by this school of philosophy indeed tends to create "metaphysical worlds of thought" (in particular, under the name of "possible worlds") rather than make itself into a part of empirical science, appears to us very unfortunate and worrisome. Even if so far this intellectual trend didn't significantly affect science itself, it certainly widened the gap between logic and logically-laden philosophy, on the one hand, and science and mathematics, on the other hand [238], [239], [288]. We'll come back to this issue in **3.2** and then show how some recent developments in logic and mathematics may help to meet Cassirer's concerns .

1.2.3 "Axiomatisation of Logic": Intuition Strikes Back

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

> "[I]t appears necessary to axiomatise 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 axiomatisation of logic as the crowning achievement of the work of axiomatisation as a whole." [106, p. 1113]

Leaving aside the purported reduction of number theory (arithmetic) and set theory to logic let us focus on the idea of the *axiomatisation of logic*. By calling the axiomatisation of logic the "crowning achievement of the work of axiomatisation as a whole" Hilbert suggests that the axiomatisation of logic is a continuous extension of the axiomatisation of geometry, arithmetic and of any other part of mathematics or natural science. However the notion of axiomatisation, which we have tried to reconstruct above on the basis of Hilbert's *Foundations* of 1899 does not immediately allow for such an extension. In a nutshell, axiomatisation in the sense of the *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 axiomatiser 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 axiomatise logic one needs to use logic. How may this possibly work?

Let us see how Hilbert axiomatises logic in his course on *Theoretical Logic* [111] co-authored with Ackermann and first published in 1928. 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 [112, p. 1])

From the very beginning Hilbert and Ackermann introduce here a new kind of logic, which they call mathematical or symbolic[10]. As we shall shortly see Hilbert's notion of the axiomatisation 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 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 19th century.

Notice also that in the above passage Hilbert talks about the application of the "formal method of mathematics" in logic. So he thinks here about the formal

[10] In saying that symbolic logic is a "new" kind of logic we mean that this kind of logic is new with respect to the "informal" logic used in Hilbert's *Foundations* of 1899; we 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 we do not quote here, the authors provide a brief historical sketch of symbolic logic tracing its history back to Leibniz.

method of mathematics as something established independently of logic and then suggests to "extend" this method to the new field of logic. However the formal method of the *Foundations* of 1899 is certainly not independent of logic. So in talking about a "formal method" in the above quote, Hilbert and Ackermann mean something different. What then is this other formal method?

The authors' reference to *symbolic algebra* provides an important hint. However in the above passage they describe 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 out to be appropriate in this case, is Cassirer's notion of *symbolic form* [42]. We shall not develop it here in its full generality but focus only on its mathematical version, relevant to Hilbert's work.

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 analysed 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 contentful [*inhaltlische*] logical thinking. Among these, for instance, is the problem of characterising those statements which can be deduced from given premises." [112, 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, purported by Hilbert in his *Foundations* of 1899, of improving on mathematics through the clarification of its logical structure. Nevertheless Hilbert tends to describe both projects in similar terms, namely in terms of formalisation and axiomatisation.

In Hilbert's thinking both kinds of formalisation and axiomatisation 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 multiple alternative contentful interpretations but instead simply clarifies and purifies common vague contentful logical notions expressed in the natural language. This additional assumption apparently allows for systems of formal symbolic logic with a fixed semantics for the 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 "&" conventionally used for denoting the logical conjunction) rather than as variables that may acquire different interpretations.

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 the axiomatic method and the foundations of mathematics. In the beginning of his paper *Foundations of Mathematics* [108] that was delivered in July 1927 at the Hamburg Mathematical Seminar, Hilbert describes his new 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 extra-logical 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." [108, 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 the 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 extra-logical concrete objects that are intuitively present as immediate experience prior to all thought" and then specifies that as far as mathematics is concerned those "extra-logical 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 rather be understood as pure logic provided with certain "extra-logical" (to wit symbolic) means. According to Hilbert's new view, the immediate intuitive givenness 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 the foundations of mathematics. For further references we 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 "pure" (non-mathematical) logic and some axioms formulated in terms of this logic. In 1927 Hilbert no longer relies on the "pure" informal logic but stresses the foundational impact of symbolic intuition. Here Hilbert explicitly describes symbols as "extra-logical"; the following explanation does not allow one to reduce the notion of intuition pertaining to these extra-logical 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 abandoned 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 contentful 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 theoretical research." [108, p. 467]

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

What has been said allows us to specify Hilbert's epistemological view on the foundations of mathematics, which we qualified above (**1.2.2**) as a form of logicism. Since Hilbert brought methods of symbolic logic into his project of building new foundations of mathematics, he, as we can judge after his remarks, did not change his core understanding of the epistemic function of logic in mathematics and elsewhere. However, because of the involvement of symbolic methods, Hilbert's conception of logic gained important new features. In his [108], Hilbert remarks that his new approach in the foundations of mathematics amounts to "extending the formal point of view of algebra to all of mathematic" (p. 470), which provides a ground for the usual qualification of his view as *formalism*. But in fact the goal of Hilbert's new project is not to make the formal symbolic algebra into a foundation of mathematics, but to use symbolic algebraic methods in logic, keeping untouched the basic axiomatic architecture of mathematical theories as it is presented semi-formally already in the *Foundations* of 1899.

Toward this end Hilbert uses a feature of algebra that plays no special role in earlier works in mathematical logic: we mean the algebraic method of "ideal elements" like -1 or $\sqrt{-1}$. After the introductory remarks quoted above he first introduces a system of symbolic logic similar to one presented in [111] 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 indeed 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". If we understand Hilbert correctly here, 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 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 the advantage of being simpler and more convenient, while the latter has the advantage of making explicit the logical structure of reasoning). The new proposed formalism is, however, 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." [108, 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 (see (**1.2.1**) above). 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 the Hilbert's *Foundations* of 1927 is an overkill in the case of the finitary arithmetic, its expected advantage is that it allows for a uniform treatment of the whole of mathematics by means similar to those used in the finitary arithmetic.

The possibility of checking consistency is evidently crucial for Hilbert's project. Although in 1927 Hilbert offers no general solution to this problem he suggests that this problem is relatively easy and "fundamentally lies within the province of intuition just as much as does in contentful number theory the task, say, of proving the irrationality of $\sqrt{2}$" [108, 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 the 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 a 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 realises, 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 [113] Hilbert calls by the name of *meta-mathematics*. However since the whole of meta-mathematics "fundamentally lies within the province of intuition just as much as does in contentful number theory" this remark does not lead to an infinite regress in foundations. Thus, intuitive proof theory aka meta-mathematics (in Hilbert's original sense of this term) in Hilbert's view of 1927 becomes a foundation for the rest of mathematics. As many scholars observed, Hilbert's project in this mature form shares certain features with Brouwer's approach since it equally assumes the epistemic reliability of mathematical intuition (in the case of finitary syntactic constructions). However Brouwer's approach, and the constructive approach in mathematics more generally, does not involve anything like Hilbert's division of mathematics into its "real" and its "ideal" parts. This difference between the two approaches remains significant.

The most advanced version of Hilbert's project is presented in two volumes [113] published by Hilbert in collaboration with Paul Bernays in 1934-1939. Here the authors provide a sketch of a new formal theory of plane Euclidean geometry, which this time involves a symbolic logical calculus. There is a significant difference between this latter theory and the theory of the *Foundations* of 1899 [115], which cannot be fully analysed in terms of "higher precision and explicitness" due to the application of new symbolic methods. Recall that in the *Foundations* of 1899 Hilbert introduces three different "systems of things", which are intended to be interpreted, respectively, as points, straight lines and planes. The new version of the theory involves a single type of primitive object called *points*. Using a primitive ternary relation $Gr(x, y, z)$ informally interpreted as *points* x, y, z *lie on the same straight line* (Gr stands for German *Gerade*), the first axiom of the theory now reads:

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

and translates into the prose as:

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

(parentheses (x) around a variable stand in Hilbert's notation for the universal quantifier).

The reduction of distinction between several *types* of primitive objects is not innocent from a logical and foundational points of view. Arguably the type distinction is fundamental in logic and mathematics and should be formally expressed in any (theory of the) foundations of mathematics. We shall see in what follows that this problem is equally present in the standard set-theoretic foundations of mathematics and also how the recent Univalent Foundations solve it (**2.2.1**, **3.2**).

As we all know well today in 1927 Hilbert severely underestimated the 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 [304]. However even if Hilbert's foundational project as described in the *Foundations* of 1927 indeed failed, his axiomatic method constituting part of this program certainly survived and until today remains standard. Hilbert's idea according to which an appropriate symbolic logical calculus provided with certain elementary and intuitively transparent finitary mathematical methods of reasoning may allow one to establish all needed meta-theoretical results, including consistency proofs, of all consistent formal axiomatic theories, can be a sufficient foundation for all contemporary mathematics, after Gödel's results belongs to history as a utopian project. But Hilbert's notion of proof theory as a (meta-)mathematical study of logical symbolic calculi survived and further advanced via the application of more advanced meta-mathematical methods. The title of "The Mathematics of Metamathematics" appearing in 1969 [219] perfectly illustrates this shift, which has relaxed the boundary between mathematics and meta-mathematics. As Yu. Manin puts it "good metamathematics is good mathematics rather than shackles on good mathematics" [182, p.3]. Hilbert's distinction between the "real" and the "ideal" mathematics does not reflect current mathematical practice and belongs to history.

1.3 Axiomatic Method versus Genetic Method

In his popular lecture "Axiomatic Thought" [106], [109] Hilbert praises Euclid as the founding farther of the axiomatic method. But Hilbert is also well aware that his new version of this method differs essentially from Euclid's. In this Section we briefly summarise Hilbert's views on how his novel axiomatic method relates to more traditional approaches in theory-building, including Euclid's.

1.3.1 Genetic and Axiomatic Methods in the Theoretical Arithmetic (1900)

In his early 1900 paper on the concept of number [103], [110] Hilbert attempts to extend his novel axiomatic method of the *Foundations* of 1899 from geometry to arithmetic. For this purpose he introduces a distinction between what he calls the *genetic* and the axiomatic methods of introducing new theoretical concepts. The genetic method is exemplified here by the well-known construals of real numbers from rational numbers due to Cauchy and Didekind (Cauchy Sequences and Dedekind Cuts) along with construals of rational numbers and integers from natural numbers, which today still remain standard. In such cases a new mathematical concept (e.g. that of real number) is, in Hilbert's word, "produced" [erzeugt] by another concept (e.g. that of rational number). Without trying to analyse this notion of "production" (cf. the concept of geometrical production introduced in **1.1.4** above) in modern logical terms Hilbert proposes replacing the traditional genetic theory of arithmetic by a formal axiomatic theory.

Comparing the two approaches Hilbert states without further ado that

> "[d]espite the high pedagogic and heuristic value of the genetic method, for the final presentation and the complete logical grounding of our knowledge the axiomatic method deserves the first rank." [110, p. 1093]

This remark clearly demonstrates the epistemological motivation behind Hilbert's new axiomatic method, but does not explain details of its philosophical background. The notion of *weak logicism* that we attribute to Hilbert on the basis of his contemporary and later writings provides a tentative reconstruction of this background.

In the same paper [103] Hilbert makes a historical remark according to which, traditionally, the genetic method has been reserved for Arithmetic while Geometry has used the axiomatic method since Euclid. As we shall shortly see, in 1934 in his joint work with Bernays [113, v.1] (English translation [114, v.1]), Hilbert presents a different view on this matter and, in particular, associates the genetic method with Euclid's geometric theory. The difference between Hilbert's 1900 and 1934 views on this matter, in our opinion, marks important progress in Hilbert's understanding of axiomatic method and its history during his career. For this reason, in what follows we do not further elaborate on this controversial part of Hilbert's 1900 paper.

The notion of the genetic method as an alternative to the formal axiomatic method has been discussed in the later literature, see [43], [208] [11], [52], [266], [154]. Jean Cavailles, Jean Piaget, Sergei S. Demidov, and Elaine Landry follow Hilbert's 1900 line, presenting the genetic method as a genuine alternative to the formal axiomatic method and taking it for granted that the two methods are theoretically incompatible even if they can be jointly used in practice (for different epistemic purposes). Vladimir Smirnov takes a different line, which is closer to Hilbert's 1934 approach. He shows that genetic constructions allow for an effective formal symbolic representation, and that such a formalisation of the genetic approach can be direct rather than roundabout (via the standard axiomatic approach).

That the notion of genetic method in its original form may be confusing in spite of its intuitive appeal is clearly seen in Landry's 2013 paper [154]. Landry uses the notion of genetic method in her argument against Feferman's claim, according to which axiomatic theories of *categories* (such as EM, ETCS and CCAF, see [194] and **3.1.2** below) cannot be an independent foundations of category theory and of the rest of mathematics because these theories allegedly use prior notions of

[11] Piaget calls the genetic method *operational*.

collection (or class) and operation [67]. Landry argues back that these notions are used in this context only for an heuristic *genetic* underpinning of category theory but play no role in its logical axiomatic foundation. Instead of defining a category in terms of *classes* of things called "objects" and "morphisms" Landry proposes thinking of axiomatic theories of categories as follows:

> "[T]he category-theoretic structuralist simply *begins* by assuming the existence of a system of two sorts of things (namely "objects" and "morphisms") and then brings these things into relationships with one another by means of certain axioms." (op. cit. p. 44)

In a following footnote Landry explains that

> "What is doing the work here is neither the notion of a system nor the notion of an abstract system, rather it is the Hilbertian idea of a theory as a *schema* for concepts that, themselves, are *implicitly* defined by the axioms. Thus we don't need a "fixed domain of elements" [..], we do not need a system as a "collection" of elements [..]" (op. cit. p. 44)

For reasons that are explained in what follows (see **3.1**), we share Landry's optimism about the possibility of axiomatic category theory independent of any *logically* prior notion of class. However we are not convinced by her argument and do not think that first-order theories such as EM, ETCS and CCAF achieve this goal. Our objection to Landry is this. (To save space we shall refer to EM; to ETCS and CCAF the same argument applies similarly.) We assume after Hintikka [118]) that EM requires a notion of semantic consequence that forms part of its underlying logic. In order to construe the relation of semantic consequence properly one needs to fix some formal semantical framework (again as a part of the logical machinery). When one uses a Tarski-style model-theoretic semantics to this end, it *does* involve some notion of class (collection, universe) of individuals. In this case thinking about categories introduced through EM in terms of classes is *not* just a convenient way of representing categories, which has certain heuristic and pedagogical values, but a proper part of the very axiomatic construction of EM, which belongs to its logical machinery.

Thus the distinction between the genetic and the axiomatic methods of theory- and concept-building is less clear-cut than it might seem. Whether the class-based definition of category qualifies as axiomatic or genetic depends on one's general conception of logic and logical semantics. If one opts for the Tarski-style logical semantics then the class-based definition of category qualifies as genuinely axiomatic. However if Landry's "category-theoretic structuralist" opts for a different logical semantics that does not involve the prior notion of collection (which Landry leaves unspecified and which is not immediately available on the existing logical market), she may justly conceive of the same definition as merely genetic.

1.3.2 Revendication of the Genetic Method

Let us now turn to Hilbert's late discussion on the genetic method in his 1934 monograph co-authored with Paul Bernays [113, v.1]. Here is a relevant passage:

> "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 idealising 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." [114, pp.1a-2a]

Hilbert and Bernays present in the above passage a more complex picture of the relationships between genetic and axiomatic methods than one finds in Hilbert's 1900 paper [103]. The authors refer here to three closely related concepts:

1. being axiomatic in the "broadest sense";
2. being axiomatic in the "narrowest sense" aka being *formally* axiomatic;
3. being constructive aka genetic.

According to this account a theory qualifies as *broadly* axiomatic (1) when it is built on first principles while the rest of its content is obtained from these principles via certain logical procedure, which in this case is not specified. The following examples, which include the geometrical theory of Euclid's *Elements*, helpfully clarify the intended meaning of the authors' term. Here is another quote from the same source in which Hilbert and Bernays tell us more about Euclid:

"Euclid's axiomatics was intended to be contentful 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." (*ib.*, p. 20a)

Hilbert's idea of formal axiomatic method (2) has already been discussed above. However in the above passage Hilbert and Bernays emphasise a distinctive feature of this approach, which needs a further comment. Their concept of "existential form" of axiomatics can be best understood by comparing the formal axiomatic method (2) with the genetic method (3). While a genetic theory *constructs* (or in the words of Hilbert 1900 "produces") its objects from simple elements via the recursive application of certain operations (such as constructions by the ruler and compass in Euclid's geometry), a formal axiomatic theory involves the notion of interpretation (model) which, in its turn, comes with an assumption that a model of the given theory *exists* in some appropriate sense of the word. So by the "existential form" of axioms one should understand in this context not the special character of existential axioms in the usual today's sense, i.e., of first-order axioms with existential quantifiers, but the character of all axioms of Hilbert-style formal axiomatic theories as distinguished from Euclid-style constructive postulates.

Hilbert and Bernays emphasise that the assumption according to which a given axiomatic theory has a model "properly augments the assumptions formulated in the axioms" of this theory, and further elaborate on this point later in the same Introduction to [114]. The authors consider the case when the given theory has a finite model to be unproblematic (as indicated in the above quote). But the case of an infinite model in the author's view constitutes a hard problem. They rule out the idea that the existence of such a model can be justified on empirical or phenomenological grounds. They stress that the existence of a model implies consistency and then turn the tables and formulate in this context what can be called, anachronistically, the main idea of the Hilbert Program: to develop a theory that would allow one to prove or disprove the consistency of a given axiomatic theory by finitary *genetic* (constructive) means; in case the given theory is provably consistent one is in a position to stipulate for it a default abstract model in terms of "thought-things and thought-relations", as it has already been explained in **1.2.1** above. An overview of the following development of the Hilbert Program, which has been challenged by Gödel's Incompleteness Theorems, and of the related research in logic, mathematics and philosophy, is out of the scope of this present work.

The last quote makes it clear that Hilbert and Bernays now qualify Euclid's theory as genetic aka constructive. The genetic (aka constructive) (3) and formal axiomatic (2) methods of theory-building both now fall under the concept of being axiomatic "in the broadest sense" (1). The authors do not elaborate on their generalised concept of being axiomatic (1) but their pointing to such a concept suggests the possibility of unification of (2) and (3) within a single theoretical framework.

In fact, the mature version of the formal axiomatic method presented by Hilbert and Bernays in 1934 provides such a unification in a specific form. Recall that in the symbolic setting mathematical proofs are construed as (properly interpreted) chains of formulas built according to certain fixed syntactic rules. In such a framework to *prove* a proposition expressed by formula F is to *construct* an appropriate chain of formulas that ends up with F. Thus doing mathematics in Hilbert's formal axiomatic setting in its mature symbolic form does not reduce contentful genetic constructions in mathematics altogether but only reduces all such genetic constructions to constructions of a special sort, namely, to the finitary symbolic constructions. For a mathematical study of such symbolic construction (that may eventually bring a proof of the consistency of a given formal axiomatic theory) Hilbert and Bernays reserve a special theoretical domain of *meta-mathematics*. In order to avoid a vicious circle in their foundational reasoning the author avoid using the formal axiomatic method in metamathematics and reserve for this area of mathematical research the traditional genetic aka constructive method. Bearing in mind the foundational role that meta-mathematics plays in this theoretical framework it is fair to say that the genetic method in this particular finitary application plays a crucial role in the mature version of Hilbert's formal axiomatic method.

Recall also that Hilbert's special treatment of finitary symbolic constructions among all other mathematical constructions (including traditional geometrical constructions) is underpinned by Hilbert's metaphysically-laden and epistemologically relevant distinction between *real* and *ideal* mathematical objects: according to this account only finitary symbolic constructions are real and hence operational. Lifting or modifying this controversial philosophically-laden distinction — without ignoring its properly mathematical content — opens room for new ways of combining genetic and axiomatic approaches in mathematical reasoning. Some such new approaches are described in what follows.

1.4 Conclusion of Chapter 1

Let us summarise. Euclid's geometrical theory is based on rules rather than axioms in today's sense of the term. These rules are of two different sorts. One set of rules concerns equalities; these rules allow one to derive new equalities from given equalities. Euclid calls such rules *Common Notions*; after Aristotle these rules are also commonly called *Axioms* even if they do not qualify as axioms in the modern sense of the term stemming from Hilbert. We describe these rules as being *proto-logical*, bearing in mind that they are analogous to rules of truth-preserving logical deduction; the only difference being that Euclid's "axioms" do not apply to all propositions but to mathematical propositions of a specific form, namely, to equalities (of figures or numbers).

The other set of rules Euclid calls *Postulates*; these rules apply to geometrical objects themselves (rather than propositions *about* the geometrical objects). A recursive use of these rules provides for geometrical constructions called in school mathematics *constructions by ruler and compass* [12]. In order to distinguish a result of rule-based geometrical construction from the procedural aspect of this construction and at the same time stress an analogy between derivation of equalities via Common Notions, we call this latter geometrical procedure by the name of *geometrical production.*

Proto-logical deduction and geometrical production are closely intertwined in Euclid's geometrical reasoning: the results of relevant proto-logical deduction are used in geometrical production and vice versa. Deduction and production jointly generate a system of geometrical objects with certain required properties (via solving *problems*) along with a system of justified propositions about some geometrical objects (via proving appropriate *theorems*). This twofold theoretical construction we conveniently call a geometrical theory. Even if the

[12]The Postulates are not sufficient for supporting these constructions since Euclid's reasoning also involves a number of tacit postulates that, in particular, allow him to determine the point of intersection of circles in Proposition 1.1 of the *Elements*. This constructive incompleteness of Euclid's theory has no direct bearing on our present argument, however.

above description of this notion of theory involves some modern logical concepts such as that of existential instantiation, it does not qualify as a systematic reconstruction of Euclid's theory in today's logical terms. In particular, it leaves without a detailed analysis the nature of the relationships between proto-logical inferences and geometrical constructions. Our experience of interpreting Euclid in the context of today's mathematics and logic confirms Ian Müller's conclusion according to which no system of modern logic accounts properly for Euclid's form of geometrical reasoning [198], [196]. However in what follows we describe some recent logical and mathematical approaches, which are seemingly analogous to Euclid's approach. The notion of Curry-Howard Correspondence and its specific rendering in the Homotopy Type theory (HoTT) allows for a precise formal description of how propositional and non-propositional elements are related in geometrical reasoning (see **3.2**). Without trying to develop a more precise formal reconstruction of Euclid's geometry in today's logical terms we shall use this theory in what follows as a useful historical example motivating our further considerations about today's axiomatic approaches.

Unlike Euclid, Hilbert develops an axiomatic theory of Euclidean geometry in the convenient modern sense of the term. Leaving aside details (some of which have been considered above) this approach can be described as follows. (Uninterpreted) theory T is a set of formal sentences aka propositional forms with a distinguished subset A of such forms called *axioms*; all other forms of T are called *theorems* and formally derivable from the axioms according to some fixed syntactic rules. The so-called *Hilbert-style* formal theories are characterised by the fact that the number of such derivation rules is minimal (Hilbert himself uses only *modus ponens* and substitution) while the number of axioms (and possibly axiom schemes) is large. A key concept of this framework is that of *interpretation*, which is an assignment of certain semantic values to elements of propositional forms; an interpretation "fills the forms with a content" and thus make these symbolic forms into contentful sentences with definite truth-values. The talks of "formal sentences" and "propositional forms" refers to such an intended interpretation (or more precisely, to a class of such possible interpretations because the same formal sentence under different interpretations can bring about different contentful sentences having different truth-values).

An important further standard feature of this approach, which is often taken for granted and left unnoticed, is the distinction between logical and non-logical syntactic elements (symbols) of formal sentences. This distinction is clearly semantic; moreover it assumes a notion of logicality according to which logical and extra-logical concepts are distinguished. Yet, this fundamental distinction is usually introduced in textbooks as a part of uninterpreted theories as if it would belong to the row syntax itself. The procedure of semantic interpretation just described usually applies solely to the non-logical part of the syntax while the meaning of logical symbols (i.e., meaning of logical constants) is supposed to be fixed independently beforehand. Even if Hilbert and his co-authors don't have a developed theory of formal logical semantics the idea that any mathematical theory comprises a "purely logical" core is crucial for Hilbert since his *Foundations* of 1899. According to this idea all formal syntactic derivations in a given theory are interpretable as purely *logical* inferences, which do not depend on details of (non-logical) interpretations of its axioms and theorems. This distinctive feature of Hilbert's conception of a formal theory is underpinned by his epistemological view, which we have described above as a weak form of logicism (see **1.2.2**).

Let us stress once again that the present Section does not aim at tracing the historical development of axiomatic method from Euclid to Hilbert taking into consideration all or some intermediate stages of this long route. Only some fragments of this historical rout have so far been studied, so any narrative covering the period of more than two millennia that separates Hilbert from Euclid would lack the historical and theoretical precision that I'm trying to achieve in the present study. This is why the above is nothing but a comparative historical and theoretical analysis of Hilbert's axiomatic method, which today is still commonly called "modern", with Euclid's method. Even such a limited use of a historical approach allows us to think of the axiomatic method in a historical perspective, and avoid considering its contemporary standard form as final and immune to possible revisions and further developments and modifications. In addition to its historical significance the above reconstruction of Euclid's axiomatic method has a theoretical value because it helps us to analyse and describe some recent trends of axiomatic reasoning in the following parts of this work.

2 The Axiomatic Method at Work in Mathematical and Scientific Practice [13]

Hilbert's work in the foundations of mathematics has been seminal in a number of ways. As it has been already mentioned in the last chapter, Hilbert's Program along with Gödel's Incompleteness Theorems boosted a large research program that determined the mainstream development of the foundation of mathematics over the course of the 20th century, and the opinion of some researchers still occupies this distinguished position today. Today's *proof theory*, *set theory* (in its modern axiomatic form), the research program of *Reverse Mathematics*, and a number of related areas of research at the crossroads of logic, mathematics and philosophy, stem from Hilbert's relevant works and constitute a research field known as FOM (for foundations of mathematics). However, Hilbert's project in the foundations of mathematics was in fact even more ambitious. It aimed to profoundly change contemporary research practices in mathematics and mathematically-laden science by making the formal axiomatic method, in Hilbert's own words, into a "basic instrument of all theoretical research" [108, p. 467]. The present chapter focuses on this latter "practical" aspect of the axiomatic method.

As we shall see, the axiomatic approach indeed had significant impact on mathematics and mathematical education of the 20th century. The case of science is different: in spite of continuing attempts to apply axiomatic approaches in science, some of which are overviewed below (see **2.3**) , these attempts so far did not have a comparable effect. But even the case of pure mathematics is less obvious than it may first appear. Outside axiomatic set theory, proof theory and other mathematical disciplines constituting part of mainstream FOM research, the formal symbolic version of Hilbert's axiomatic method has no application at all. What is commonly used in mainstream contemporary mathematics and mathematical education is a semi-formal version of axiomatic method modelled after Hilbert's *Foundations of Geometry* of 1899 and supported by Cantor-style informal (so-called "naive") set theory (see **2.2** below). In addition, in the

[13] This Section includes material from [233, Ch. 4] and [250].

mathematics and science of the 20th century we find interesting examples of axiomatic approaches, which qualify as axiomatic in Hilbert's "broadest sense" of 1934 but at the same time significantly diverge from Hilbert's conception of formal axiomatic method even in its semi-formal version. Such non-standard axiomatic approaches are particularly interesting for the present study and will be treated separately (Ch. **3** below).

Some researchers (including many FOM researchers) explain the fact that FOM is relatively isolated from the mainstream of today's mathematics by appeal to a natural division of labor. They assume (i) that an ordinary working mathematician needs not master the details of the foundations of her discipline and (ii) that research in FOM, just like any other special mathematical research, requires a very specific professional training and qualification (along with an understanding of relevant philosophical matters), which cannot possibly be provided for all working mathematicians. This argument may sound convincing and be easily accepted by those working mathematicians who do not see a need to care about the foundations of their discipline. Notice however that the argument uses a notion of the foundations of mathematics that differs essentially from the more traditional notion according to which the foundations of a theoretical discipline comprise a core part of the content of this discipline known to all its practitioners (whatever their areas of specialisation within the discipline) and thus serves for its theoretical unification. In *this* traditional sense Euclid's *Elements* qualify as foundations of the Geometry and theoretical Arithmetic of his time, and Newton's *Principia* qualify as foundations of his then-novel theoretical physics [14].

Some working mathematicians among those who *do* care about foundations are dissatisfied with the received conception of FOM and believe that the traditional notion is more appropriate. Here is a harsh statement to this effect from Lawvere and Rosebrugh:

"A foundation makes explicit the essential general features, ingredients,

[14] In Russian there is a tradition to referring to foundations in the logical and theoretical sense related to FOM and to foundations in the traditional sense using two different words: "osnovania" and "osnovy". This terminological distinction is helpful in many situations but we are not going to use it in what follows, first, because it cannot be applied in English without a terminological invention and, second, because our general theoretical strategy, as it will be explained below, aims at convergence rather than divergence of the two notions.

> 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.” [166, p.235]

Some researchers try to make a peaceful agreement between the two concepts of foundations by reserving the name of *practical foundations* for the traditional notion [280]. In a parallel (albeit chronologically more recent) development in the philosophical camp a group of philosophers attempt to constitute the *philosophy of mathematical practice* as a subfield of the philosophy of mathematics, which takes into account mathematics as it is actually practiced (today and in the past) outside FOM [181], and considers relevant philosophical issues beyond the foundational issues. Since pure mathematics is a theoretical discipline *par excellence*, the grounds of the relevant distinction between mathematical theory and mathematical practice are far from clear, moreover so that philosophers of mathematical practice usually refer to mathematical theories as such rather than only to applications of these theories and historical details of their emergence. The popular wisdom according to which “in theory there is no difference between theory and practice, while in practice, there is” perfectly applies to this case. Practically speaking, it is clearly the case that philosophers who associate themselves with mainstream FOM research and philosophers of mathematical practice are doing different research and form different communities. But from a theoretical point of view it is not clear why these lines of philosophical inquiry should be so different.

Without further exploring the distinction between mathematical theories and mathematical practices, let us briefly formulate our take on the above methodological problem. After Euclid, Hilbert and Lawvere, we do believe that foundations of mathematics and science in the *traditional* sense of the term are important both practically and theoretically. We also believe that the foundations of mathematics in *this* sense, as well as foundations of any other scientific discipline, need permanent revision and renewal along with the development of the corresponding discipline itself. However important and valuable are the results achieved in mainstream FOM studies, we consider the fact that they don't help to build foundations of mathematics in the traditional sense of the term to be an open problem that needs urgent solution.

One can think of two types of solutions: (i) reforming current mathematical practice according to the theoretical standard established by the received FOM and (ii) revising the theoretical fundamentals of FOM and designing on this basis new foundations, which take into account the mathematical practice of the last century and that can be better implemented in practice. In the present chapter we analyse some attempts to apply strategy (i) and show that the success of this strategy has so far been very limited. In chapter **3** we explore strategy (ii) including the ongoing project aiming at new *univalent* foundations of mathematics [95].

2.1 Set Theory

Recall the notion of *system of things* used by Hilbert in an early description of his formal axiomatic method [115]. 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 of axiomatising set theory explains why and how the later development of Hilbert's formal approach to the foundations of mathematics involved not only the formal axiomatic method itself but also the axiomatic theory of sets [15].

Since Zermelo's pioneering works in axiomatic set theory, mainstream research in set theory has 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 of any foundational claims made about this theory. To be more

[15] Zermelo's principal motivation for axiomatising set theory was saving Cantor's so-called "naive" set theory from paradoxes [206]

concrete, let us consider Cantor's Continuum Hypothesis (CH), which in 1900 was listed by Hilbert [104] as the number one among 23 open mathematical problems that Hilbert at that time judged to be the most important[16].

CH is a very peculiar example of a 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 [87] discovered that ZF (which is an improved version of Zermelo's axiomatic theory of sets so called after the names of Zermelo and Fraenkel [71]) is consistent with CH by building a model of ZF in which CH holds. In 1963 Cohen [47] 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 [149].

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 [17]. There are obvious trivial "solutions" of this sort such as considering CH itself as an axiom. Then, however, it remains to show that the system of axioms for set theory that solves the CH problem is a "right" one, so that the proposed solution is "genuine". We 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

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[16]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). The 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

[17]In particular, this is the view of Hugh Woodin, a leading expert in set theory from Harvard University (personal communication).

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 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 19th century, it contrasts sharply with mainstream mathematics, which still manages to provide yes-no answers to many well-posed questions.

It may be argued that the formal axiomatic framework makes explicit the 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. We cannot see how such a deductive relativism (or "if-thenism") about mathematics could be sustainable. It is incompatible not only with 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 (which 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 a certain axiomatic theory. So a consistent if-thenist would not hold without further qualification that CH is independent of the axioms of ZF, but would rather say that it depends on one's assumptions.

Thus, in spite of the fact that 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-formalised proofs. What is specific for 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 ourunderstanding, an open question whether or not such a roundabout way of studying sets has indeed proven effective.

2.2 Bourbaki

2.2.1 Semantic Version of the Formal Axiomatic Method

The multi-volumed *Elements of Mathematics* [26], [29] produced by a group of (mainly French) mathematicians using the pseudoname *Nicolas Bourbaki* since 1939 (the year in which the first volume of *Elements* came out) is the most recent serious attempt to write a self-contained compendium of the core contemporary mathematics after Euclid's example [18] . Although Hilbert's *Foundations* of 1899 fall under the same description the two works differ in their purpose. Hilbert's work of 1899 is focused on Euclidean geometry, which in the end of the 19th century was already only a relatively small part of what was commonly known under the name of geometry in the mathematical community. Here, Hilbert rebuilt an old theory with his new axiomatic method, clarified the

[18]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 could be the *Elements of Mathematic*. This unusual singular form of the word is supposed to stressed Bourbaki's aim of the unification of mathematics. The Bourbaki group was formed in 1935, see [18] for an account of its early history. Bourbaki's volumes have been published and republished by several publishers. The first series of the *Elements*' volumes was published by Hermann (Paris) [26]; after a conflict between the Bourbaki group and the publisher the rights to publish were given to Springer (via a number of intermediate publishers) [29]. After a long break that continued from 1998 to 2016 Springer published a new original volume of Bourbaki's *Elements* on Algebraic Topology [30]; in 2019 a new edition of an older volume in the same series came out. So the project is officially still in progress. To date Bourbaki's *Elements* comprises 11 original volumes (some of these in several books), which exist in multiple editions and are mostly translated into English and Russian. Updated information about the Bourbaki project can be found at the website of *Association of Nicolas Bourbaki's Collaborators* at https://www.bourbaki.fr .

logical structure of this old theory, and left it to other people to do a similar job for more recent theories including, in particular, Riemanian geometry [294]. Unlike Euclid, Hilbert does not attempt to introduce all 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 to 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 of the grounding ideas behind it, was published in 1950 as a separate programmatic article entitled the "Architecture of Mathematics" [27].

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 [35, 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." [27, 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 and another aspect, which can be called *structural*; in Bourbaki's view it 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 the axiomatic method rather than Hilbert's formal axiomatic method in its original form:

> "[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 [35, 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." [27, p. 223].

In order to illustrate his point Bourbaki uses the example of the "abstract" group theory; the author describes this theory as an axiomatic theory construed after the pattern of Hilbert's *Foundations* of 1899 [115] as a *system of things*, which are subject to the following three axioms (modulo a slight change in 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. The above axiomatic theory of groups (which we denote **GT** for further reference) have various interpretations, many of which which were known and studied before the emergence of axiomatic group theory: by interpreting variables x, y, z, as invertible geometrical transformations (like motion) and interpreting the operation $\circ$ as composition of these transformations one gets the notion of a group of geometrical transformations; 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. But until group theory was axiomatically formulated and thus brought about the precise *general* notion of group, those examples could not be understood as instances and special cases of one and the same general concept, and the links between these different groups, which were eventually guessed by some smart mathematicians, looked unsystematic and sometimes even mysterious.

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

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

which follows from **G1** - **G3** almost immediately [27, p. 225]. We claim that the simplicity of this example does not allow it to correctly represent Bourbaki's axiomatic method. Notice that among the *objects* of **GT** there are no (abstract) groups, just like among the 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; the objects of this theory are *elements* of the given group but not this group itself. But Bourbaki's theory of (abstract) groups (see [26] vol. 2, Chapter 1, Section 6), like any other presentation of this theory, does treat groups as its objects, distinguishes between different groups, classifies 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 into 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 to a *definition* of a traditional mathematical object like the triangle; theorems of **GT** like **P** are to be compared with propositions like "all triangles have three angles" implied by the definition of a triangle.

Let's now see what kind of representation of the abstract concept of group is used by Bourbaki in their volume on algebra, which includes group theory. As in other similar cases they use a *set-theoretic* representation for it; the relevant set theory is developed in the first volume of Bourbaki's *Elements*. One easily 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 the Cartesian product of sets which reduces it to the primitive set-theoretic relation of membership. However, such a model of **GT** is just one particular group G, not a domain where *all* groups accounted for by group theory live! So 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. This conception of group theory certainly better squares with what is known in mathematics under this name. Notice that even such an elementary concept of group theory as that of *subgroup* (and hence, say, the Lagrange theorem) requires considering two different groups, which from a formal point of view qualify as two different models of **GT**. What is special in Bourbaki's axiomatic presentation of group theory is the fact that all Bourbaki's groups live in the same 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 of Bourbaki's mathematics.

The Bourbaki-style axiomatic approach can be more precisely described using the syntactic concept of *signature*. From the logical point of view a signature can be described as a list of non-logical symbols (or shortcuts to such symbols and symbolic expressions such as the symbol $\circ$ for algebraic operation) of a formal theory, which are apt to multiple interpretations that provide models of the given theory. But to a working mathematician who reads a signature using its default interpretation, the signature appears as a specification of the basic elements of the corresponding mathematical concept, which is apt to multiple instantiations and possibility some further specifications. For example, the signature for the group concept construed as above

$$< G, \circ >$$

tells one that a group comprises an "underlying" set G and binary algebraic operation $\circ$ defined on this set [19] ; in order to complete the introduction of the

[19]The above is a very basic and incomplete form of signature for groups which however is appropriate for the

group concept, it remains to be postulated that set G and operation $\circ$ satisfy axioms **GT**. Set G with operation $\circ$, i.e., a group, which by abuse of notation is usually also denoted as G, can now be thought of as an *arbitrary* group that can be instantiated by various "concrete" and more specific examples. This notation is similar to the traditional geometrical notation where ABC may denote an arbitrary general triangle; notice that this traditional notation also expresses important information about the concept of a triangle, reflecting the fact that a triangle is uniquely determined by its three vertexes A, B, C. Thus Bourbaki-style mathematical reasoning squares with the traditional Euclidean pattern being at the same time evidentially translatable, at least *in principle*, into the Hilbert-style formal reasoning. In this way Bourbaki's version of the axiomatic method can be seen as a practical compromise between the requirements of Hilbert-style formalism and the traditional Euclidean pattern of contentful mathematical reasoning, which very much remains alive in the 20th century and in today's mathematics (see **2.2.4**).

Explaining away Bourbaki's deviation from Hilbert's formal axiomatic method by referring to the practical needs of working mathematicians or mathematical educators does not do full justice to Bourbaki. Since the signature $< G, \circ >$ and axioms **GT** are read contentfully as a set with group operation satisfying the axioms, which indeed is their default reading, the given axiomatic presentation is contentful rather than formal. However, unlike traditional contentful reasoning which associates with linguistic and symbolic expressions certain intuitions and concepts defined in terms of these very expressions, Bourbaki uses for semantic purposes a special theory formally developed in the first volume of their *Elements*, namely their version of set theory. This allows one to qualify the default semantics of Bourbaki's group theory (and all other similarly presented mathematical theories) as a *formal* semantics[20]. As we shall now see, this formal semantic character of Bourbaki's axiomatic style has an

usual Bourbaki-style presentation of group theory in standard mathematical university courses. It is incomplete because, among other things, it does not specify the arity of the group operation. The precise complete form of signature depends on the underlying logical calculus.

[20]The notion of formal semantics is present in an explicit form neither in Hilbert nor in Bourbaki, and we use it here anachronistically.

epistemic significance beyond merely "practical" issues of mathematical education and research.

Bourbaki's presentation of mathematical theories does not provide details of their underlying formal languages. A version of such a formal language is presented only in the first volume of *Elements* on set theory. It guarantees the translatability of Bourbaki's mathematics into a Hilbert-style formalism *in principle* — albeit hardly in practice because of the length of the required symbolic expressions and other feasibility reasons. So "officially" Bourbaki's notation that they use for presenting group theory and the remaining mathematical content of their *Elements* can be understood as a shorthand. However as their remarks some of which are quoted below make clear, this "shortened" semantic notation also expresses the idea that those logical and syntactic details are not essential and not relevant to the mathematical contents expressed with this notation, and that these theoretical contents are sufficiently robust and invariant to allow for different strictly formal representations. We shall shortly see how this idea is further developed outside pure mathematics by Patric Suppes and other proponents of the *semantic view of theories* (**2.3** below).

2.2.2 Mathematical Structure according to Bourbaki

In fact, Bourbaki's view on the semantics of their theories, expressed in the 1950 manifesto, is even more complex:

> "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* [my emphasis - A.R.] become, properly speaking, the only "objects" of mathematics." [27, p. 225-226]

Let us comment on this interesting passage in some detail. The concept of mathematical structure is informally described in the same 1950 paper [27] and demonstrated with the example of group structure presented above. According to this description a structure is determined with a set with certain relations between elements of this set. This informal description can be compared with a formal notion of structure given in Bourbaki's volume on set theory, Ch.4. This comparison shows that the informal description of structure concept given in the 1950 manifesto is not only simplified but severely oversimplified and for this reason misses some essential points. What in the 1950 paper Bourbaki calls a structure, they qualify in the 1939 volume as a *type of structures*. Axioms of group theory **GT** determine such a structure type, viz., that of groups. What then is an "individual" structure, in the given example an "individual" group? This question is tricky and does not have a definite answer. One the one hand, the identity criterion for groups and other alike structures can be borrowed from the underlying set theory; it that sense two groups G, G' are the same only if their underlying sets are the same (beware that this necessary condition is not sufficient). On the other hand, such a conception of identical structures is at odds with the existing practice of identification of *isomorphic* groups and isomorphic structures of all other types. Groups $< G, \circ >, < G', \odot >$ are called isomorphic if there is a bijective map f between their underlying sets

$$f : G \xrightarrow{\sim} G'$$

such that for all g_1, g_2 from G we have $f(g_1 \circ g_2) = f(g_1) \odot f(g_2)$ or, to put it in words, the group operations defined on sets G and G' mutually agree. In this latter sense mathematicians conveniently talk about *the* symmetric group S_3 (the group of permutations of three elements), etc. This convenient conception of being identical *up to isomorphism* also squares with Hilbert's idea according to which two isomorphic models of the same formal theory are essentially the same.

This ambiguous identity criterion of mathematical structures (and in particular, models of formal theories) can hardly be ignored in any discussion of Bourbaki's concept of mathematical structure, however informal. It plays a major role in continuing philosophical debates over *mathematical structuralism* and *set-theoretic substantialism* [20]. As we shall see in **3.2.5** the *Univalence Axiom* resolves this ambiguity in an original and fruitful way.

An interesting point that Bourbaki do emphasise in their informal description of the structure concept in the 1950 paper is the abstract character of the involved set concept, which, on the one hand, allows one to reason at a high level of abstraction and generality and, on the other hand, instantiate a given structure by many "concrete" examples known in advance. An abstract group construed à la Bourbaki can be exemplified in this way by groups of permutation, groups of geometrical transformations and all other groups known before the advance of Bourbaki-style abstract mathematics. As we have already stressed this feature of Bourbaki's mathematics supports its continuity with more traditional patters of mathematical reasoning and thus contributes essentially to its (however limited, see below) success.

Since Bourbaki's *Elements* is intended as a compendium of contemporary mathematics like Euclid's *Elements*, rather than as a philosophical or logical treatise, the author's unwillingness to explore the related philosophical and logical issues expressed in the above quote is understandable. Bourbaki's intention to use the concept of set, leaving aside problematic and polemical details of its logical foundations, is also clear. However the author's words about the "disappearance of sets in the light of recent work on logical formalism" are puzzling. The two references given in the following sentence provide a general overview of the contemporary logical approaches in the foundations of mathematics but contain no original material and thus don't quite clarify the situation. My guess is that in mentioning the "recent work on logical formalism", Bourbaki point here to the whole body of works in the foundations of mathematics motivated by Hilbert's general conception of a formal theory as a "schema of concepts". Such an emphasis on the formal aspect of mathematics is apparently at odds with the above observation concerning the semantic character of Bourbaki's axiomatic approach. However Bourbaki make a specific practical combination of these two conflicting ideas, which drive their research. Let us see how it works.

The talk of "disappearance of sets" reveals the controversial epistemic status of the concept of set in Bourbaki's conception of mathematics. The same problematic epistemic status of sets shows up in the conventional description of algebraic groups and other mathematical objects construed à la Bourbaki as "sets with additional structure". In the case of groups the "additional structure" is the group operation satisfying the axioms of **GT**. This conventional talk is at odds with the above definition of a group as a structure that *comprises* a set and an operation defined on this set. Is the underlying set a proper part of the structure or it is a mere external background that helps one to represent a structure? Here once again we come to the controversy between the *mathematical structuralism* and *set-theoretic substantialism*, which is the sort of philosophical controversy that Bourbaki deliberately avoid discussing. However, when in the above quote they talk about the "disappearance of sets", they definitely point to the latter *structuralist* option. The intended notion of "self-standing structure" (without an underlying set) is nowhere rigorously defined in Bourbaki's *Elements*. But as a matter of practical implementation of this intended notion Bourbaki systematically disregard details of their own set theory and its formal logical machinery — notwithstanding the fact that this machinery constitutes an "official" logical foundation of all of Bourbaki's mathematics. Accordingly, they feel free to describe the group concept and many other mathematical concepts using the "language of sets" without specifying the details of this language, and thus combine the "semantic approach" with an emphasis on formal mathematical structures. This pragmatic compromise is described by Bourbaki in an unpublished draft in the following words [21]:

> "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

[21] The exact date of the draft is missing but it apparently dates back to 1938-1939 as a part of preparation of the first volume of *Elements*.

> 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." [28].

Thus the notion of self-standing mathematical structure independent of any set-theoretic background remains in Bourbaki's *Elements* wishful thinking or, on a more charitable interpretation, their *regulative idea* in the Kantian sense of the word. Hilbert's notion of theory as conceptual schema motivates Bourbaki's structuralism but does not help the authors to implement this idea.

The informal structuralist notion of "mathematical reasoning up to isomorphism" can be formulated more precisely (albeit anachronistically, see [2] for historical details) in the form of the following *isomorphism equivalence principle* (IEP):

Let G, H be two mathematical structures of the same type T (in Bourbaki's sense) and let G, H be isomorphic. Then for any *structural* property P it is the case that structure G has property P if and only if H has property P. In symbols:

$$\forall P. (G \cong H) \Rightarrow (P(G) \leftrightarrow P(H))$$

Another conventional way to express IEP verbally is by saying that *structural* properties of T-structures (unlike their non-structural properties) are *invariant* under T-isomrophisms (i.e., isomorphisms between structures of type T).

Reasoning up to isomorphism about T-structures is tantamount to taking into consideration only isomorphism-invariant properties of these structures, i.e., only T-structural properties. Other properties of these structures belong to their set-theoretic foundation and/or their informal presentation, not to the theory of T-structures in the desired sense of the term. Beware that we are now talking about theories of T-structures not as axiomatic theories in Hilbert's sense but as theories built with Bourbaki's semantic method (which are theories of set-theoretic models of the corresponding axiomatic theories).

Here is how IEP applies in group theory. Consider two isomorphic groups: the full group of permutations of letters a, b, c, let's denote it Γ, and the group Δ of symmetries of Euclidean regular triangle. IEP implies that only structural (i.e., isomorphism-invariant) properties of these groups fall under the scope of group theory, while all the information about the letters a, b, c and geometrical properties of the regular triangle do not — however important and relevant these details can be in other mathematical and non-mathematical contexts. That is why in the context of group theory Γ and Δ are presentations of the same symmetric group S_3. IEP applies similarly to all Bourbaki's other types of structures.

IEP has various pragmatic, mathematical, scientific and philosophical grounds and motivations, some of which we discuss in **3.2.5**. At this point it suffices to stress that IEP is *constitutive* for Bourbaki-style mathematics because it specifies its central subject of study: mathematical structures up to isomorphism. In **3.1.2** we shall consider a more general equivalence principle used in category-theoretic mathematics.

2.2.3 Bourbaki and Mathematics Education

Bourbaki's *Elements* is the most systematic and far-reaching project that belongs to a larger body of contemporary works aiming at a radical renewal of contemporary mathematics and mathematical education, including school mathematics and university textbooks of all levels. Some of these works have been influenced by Bourbaki to some degree, while others followed the same trend independently. This large body of work cannot by fully reviewed here; below we describe only some key points of relevant developments in school mathematics.

Early attempts to reform mathematical education in line with Hilbert's axiomatic approach were made in the beginning of the 20-th century. In the US a key figure in this development was George Bruce Halsted (1853-1922), who translated Bolyai's and Lobachevsky's works into English and in 1904 published a geometry textbook entitled *Rational Geometry* based on Hilbert's *Foundations of Geometry* of 1899 [97]. In a parallel development Veniamin Fedorovitch Kagan published his version of axiomatic foundations of geometry in Russia in 1905, which was also in line with Hilbert 1899, but also took into account other contemporary axiomatic approaches in Geometry including works by Peri and other members of Peano's circle [133]; the second volume of this work, published in 1907, contains a useful historical overview of axiomatic geometry from Euclid to Hilbert [134]. Applications of these novel approaches in contemporary school and university mathematics education require a special historical study, but it is clear that at this point in history such applications were rather exceptional, and in any event remained very limited.

A major attempt at reforming school mathematical education on the national level according to the new standards known as the *New Math* was made in the USA at the end of 1950s as a key element of the American response to the Soviet launch of the first Sputnik in 1957, that aimed at the reinforcement of national technological and intellectual power [207]. This educational reform, which promoted the Bourbaki-style (rather than the original Hilbert-style) approach even at the elementary school level, was predictably opposed by the majority of school teachers and pupils' parents. More importantly, this reform was also severely criticised by certain leading mathematicians and scientists, including such a prominent figure of the time as Richard Feynman [68]. The opinion that the *New Math* was a pedagogical error prevailed already in the early 1970s [142], and this controversial reform was soon largely abandoned.

Ironically or not, in the Soviet Union a similar radical reform of mathematical education was started almost simultaneously with the American *New Math*. In 1959 Boltyansky, Vilenkin and Yaglom published a programmatic paper [296] where they urged for renewal of standard school mathematical curriculum. This early 1959 proposal did not include elements of set theory and Bourbaki-style axiomatics but when in the late 1960s the reform of mathematical education was supported by Soviet authorities and soon developed into a project of national scale, a Bourbaki-style version of school mathematical curriculum was chosen for implementation in school textbooks, developed by a group of leading mathematicians and mathematical educators under the general supervision of Andrey N. Kolmogorov, see for details and further references [200]. The fate of this Soviet reform was similar to that of the American *New Math*: in the late 1970s under the pressure of complaining school teachers, pupils' parents and harsh critique of some mathematicians, this reform was abandoned and new mathematical textbooks were again replaced by more traditional ones.

It is important to stress that both aforementioned national reforms of school mathematics did not proceed in isolation; they were parts of an international trend in mathematics education where the leading role belonged, not surprisingly, to French mathematicians including some members of the Bourbaki group. Bourbaki member André Lichnerowitcz, who was the key figure of the contemporary Bourbaki-style pedagogical reform in France (that developed along the same pattern), was also the Head of the International Commission on Mathematical Instruction of the International Mathematical Union in 1963-1966. At its final preparatory stage the Kolmogorov reform was strongly influenced, in particular, by discussions during the educational section of the International Congress of Mathematicians held in 1966 in Moscow.

The rise and fall of the Bourbaki-style approach in mathematics education (which also involved the university education) had many reasons and consequences that cannot be discussed in the present work. At least one aspect of this story is relevant to our present research, however. The proponents and critics of the Bourbaki-style approach in mathematics and mathematics education agree that it is not helpful for pointing to, illuminating and supporting applications of mathematics in science and technology [268], [68], [8]. The difference between the proponents and the opponents is in their evaluations of this fact, which depend on one's understanding of the nature of mathematics and its relationships with science. Marshall Stone, a prominent mathematician who was a leading figure of the *New Math* movement, expresses his understanding of this general issue in his programmatic 1961 paper entitled *The Revolution in Mathematics* with the following strong claim:

> "While several important changes have taken place since 1900 in our conception of mathematics or in our points of view concerning it, the one which truly involves a revolution in ideas is the discovery that mathematics is entirely independent of the physical world." [268, p.716].

Stone's talk of "our" conception of mathematics is obviously rhetorical. We don't know if he realised that the (in)dependence of mathematics on/from the physical world is not a mathematical conjecture that can be proved or disproved by mathematical methods alone. But he was certainly aware of the fact that a significant part of his fellow mathematicians and scientists disagreed with his view. Let us quote a renowned harsh Russian critic of Bourbaki, Vladimir Arnold:

> "Mathematics is a part of Physics. Physics is an experimental empirical science, a part of Natural Science. Mathematics is a part of Physics where experiments are cheap." [8, p. 229].

Notice that Arnold's views on mathematics as a proper part of physics square with Cassirer's aforementioned view according to which the "proper function and proper application" of logical and mathematical concepts is "only within the empirical science itself" while "building a metaphysical world of thought" with these tools is their misuse (see **1.2.2** above).

Since applications of mathematical research are usually expected by the larger scientific community, industry, and taxpayers, the issue of the relationships between mathematics, science and technology has an obvious pragmatic aspect. However, as the above quotes clearly demonstrate, it also has an epistemological aspect. It is remarkable that both of the above claims were made not in the course of a philosophical dispute about the nature of mathematics, as one could imagine in reading these words without their context, but in the course of continuing debates about mathematics education and the possible role of the Bourbaki-style approach in it. One may wonder if these philosophical remarks made by prominent mathematicians are really relevant, since Bourbaki's *Elements* is a mathematical work and as Bourbaki clearly state in their 1950 manifesto, they don't want to be involved in philosophical disputes around it. We believe that they are relevant, but some methodological care is indeed needed in order to treat these philosophical remarks correctly. Here is our methodological take on this problem.

True, Bourbaki's axiomatic architecture of mathematics has a purely mathematical and logical content that can, and in some situations definitely should, be understood in full abstraction from all motivating and otherwise related epistemological and philosophical ideas associated with this architecture — notwithstanding the fact that such a separation of mathematical contents from the related philosophical matters in the field of logical foundations is always problematic and by itself constitutes a difficult epistemological problem. In this purified mathematical form, Bourbaki's formal architecture does not imply by itself any particular philosophical view on mathematics and is open to different philosophical and epistemological interpretations. However Bourbaki's *Elements*, like any other foundations of mathematics, is not neutral with respect to such possible epistemological interpretations. Evidently it better squares with epistemological interpretations intended by its developers and only reluctantly admits (or does not admit at all) some alternative non-intended interpretations. In *that* sense it remains true that Bourbaki's approach supports and promotes a particular epistemological conception of mathematics, which weakens and underplays the traditional strong conceptual links between mathematics, science and technology.

The precise reasons *why* set-theoretic Bourbaki-style axiomatic mathematics is unfriendly to science and technology will be discussed in **4.3** below. Let us only notice here that the specification of such reasons crucially depends on one's epistemological views of science, which are also a subject of philosophical controversy. As far as mathematics education is concerned, it seems to us clear that a mere rebuking of Bourbaki-style reforms in mathematics education and a return to older educational practices cannot be a final solution. Following Lawvere and many other working mathematicians, we believe that mathematics education and the foundations of mathematics should not fall apart. In that sense we understand and approve of attempts to reform the mathematics education in the 20th century on the basis of the contemporary set-theoretic foundations of mathematics. The fact that these reforms were not successful in spite of all efforts, in ourview, is a reason (one among a number of other) to consider a reconstruction of these foundations.

2.2.4 Bourbaki and Euclid

The axiomatic architecture of Euclid's geometry presented in his *Elements* and analysed in **1.1** above may appear to be an archaic pattern of mathematical thinking that has little to do with today's mathematics. However, this impression is wrong. In fact the Euclidean structure, perhaps in a slightly modified form, is still present in today's mathematics. Consider the following example taken from a standard mathematical textbook ([144, p. 100)]:

Theorem 3:

Any closed subset of a compact space is compact

Proof:

Let F be a closed subset of compact space T and $\{F_\alpha\}$ be an arbitrary centered system of closed subsets of subspace $F \subset T$. Then every F_α is also closed in T, and hence $\{F_\alpha\}$ is a centered system of closed sets in T. Therefore $\cap F_\alpha \neq \emptyset$. By Theorem 1 it follows that F is compact.

Although the above theorem is presented in the form of "proposition-proof" as is usual for today's mathematics, its Euclidean structure can be made explicit without re-interpretations and paraphrasing:

[*enunciation:*]

Any closed subset of a compact space is compact

[*exposition:*]

Let F be a closed subset of compact space T

[*specification*: absent]

[*construction*:]

[Let] $\{F_\alpha\}$ [be] an arbitrary centered system of closed subsets of subspace $F \subset T$.

[*proof*:]

[E]very F_α is also closed in T, and hence $\{F_\alpha\}$ is a centered system of closed sets in T. Therefore $\cap F_\alpha \neq \emptyset$. By Theorem 1 it follows that F is compact.

[*conclusion*: absent] The absent *specification* can be formulated as follows:

I say that F is a compact space

while the absent *conclusion* is supposed to be a literal repetition of the *enunciation* of this theorem. Clearly these latter elements can be dropped for reasons of parsimony. In order to better separate the *construction* and the *proof* of the above theorem, the authors could first construct set $\cap F_\alpha$ and only then prove that it is non-empty. This deviation from the classical Euclidean scheme seems to us rather negligible, however.

2.3 Axiomatic Approaches in Science and in the Philosophy of Science

In this Section we provide a general overview of current axiomatic approaches in the natural sciences, Computer Science and the related developments in the philosophy of science. Considering particular physical or other scientific theories from the axiomatic point of view is out of the scope of the present work.

2.3.1 Physics

At the Second International Congress of Mathematicians held in 1900 in Paris, Hilbert presented his famous list of 23 open mathematical problems [104]; the 6th item on the list is the problem of the axiomatisation of fundamental physical theories. This problem still remains largely open, and there are different and often contrary opinions about its pertinence.

The existing axiomatic presentations of physical theories are of two kinds. Presentations of the first kind apply Hilbert's concept of a formal axiomatic theory. Presentations of the second kind use an informal notion of axiomatic theory, sometimes supported with the idea of non-Classical *Quantum* logic. While presentations of the first kind are developed and used only by logicians and philosophers, presentations of the second kind are due to working physicists themselves. As we shall now see, "axiomatics of logicians and philosophers" and "axiomatics of physicists" found in the recent literature are very different; moreover, a significant part of today's logical and philosophical community simply does not recognise the "axiomatics of physicists" as a logically coherent form of axiomatics. The problem of the adequacy of "axiomatics of logicians and philosophers" to current scientific practice is even more acute in case of physics and other natural sciences than in pure mathematics. However, in our opinion, a convergence of different approaches to axiomatisation of scientific theories is nevertheless possible.

A detailed overview of research on the axiomatisation of physics made before 1972, as well as an interesting discussion of the significance of the axiomatic method in science, are found in [36] and [37]. In the same works Mario Bunge also presents his own axiomatic presentations of some physical theories. Bunge's axiomatic theories of physics as well as most of the works that he reviews belong, according to the above classification, to the "axiomatics of logicians and philosophers". Following Tarski [276], Bunge describes his axiomatic approach to physics in these words:

> "There is a single theory that starts from scratch: mathematical logic [...]. All other theories presuppose at least logic and usually a lot more. More precisely, the least a mathematical or a scientific theory takes for granted is the so-called ordinary [i.e., Classical]r(two-valued) predicate calculus enriched with the microtheory of identity. This theory is necessary and sufficient to analyse the concepts, formulas, and reasonings occuring in mathematics and in science — or rather to analyse their form. In fact, every statement in mathematics or in science is, as far as its form is concerned, a formula of that calculus; and every valid reasoning is an instance of an inference pattern consecrated by that same theory." [37, p. 136]

The author calls his above claim a "platitude" (*ibid.*), pointing to the fact that his point of view on the matter is not his original. However, this doesn't mean that this view is (or ever was) shared by all interested parties including physicists, logicians and philosophers. Before we consider some objections and alternative views, let us stress that Bunge's claim is normative rather than descriptive; it tells us how physicists and other scientists *should* build their theories but not how they actually do it. Bunge's axiomatic physics is physics rebuilt according to a certain conceptual scheme adopted on independent grounds. Even if the intended reconstruction of physics concerns only the logical form of its theories and not the content of these theories, it amounts to an application to physical theories of certain pre-established standards, namely, *formal* standards. Using such standards, Bunge apparently assumes that they wholly belong to the competence of logicians and philosophers and don't need to be evaluated from a physical viewpoint. Moreover, Bunge assumes that the general problem of the logical form of scientific theories has already been fixed with Hilbert's concept of axiomatic theory and the concept of Classical First-Order Logic (CFOL).

In the last chapter we already described some of the historical and philosophical background behind this approach. Let us now point to some objections and alternative views. According to Hilary Putnam, the logical part of a given physical theory is a subject of empirical test on equal grounds with the rest of this theory [217]. Since Quantum Theory (i) has been empirically tested with a high degree of precision and (ii) leads to logical paradoxes it makes sense, according to Putnam, to abandon the ordinary logic and replace it with a new Quantum logic where such paradoxes would not arise. In Putnam's view, to use the old logic in the new fundamental theory of physics is to underestimate the novelty of this new physical theory. A critical evaluation of Putnam's argument and interesting counter-arguments can be found in Michael Dummett's paper [57]. We would like to stress that the standard Hilbert-style axiomatic architecture used by Bunge and many other researchers in the field indeed allows one to replace CFOL by another logical calculus including a version of Quantum logic. However, in our view, such a replacement cannot solve the problem of the adequacy of axiomatic (re)presentations of scientific theories. As a prospective solution to this problem we propose in what follows a modification of this axiomatic architecture itself (see **4.2-3**).

The idea that Quantum Theory may include a specific non-classical logic was put forward by John von Neumann in his 1932 book [301], in which the author made an attempt to present Quantum Theory axiomatically; the concept of Quantum logic was developed in a more systematic way by the same author in his 1936 paper co-authored by G.D. Birkhoff [79]. Von Neumann's axiomatic treatment of Quantum Theory definitely belongs to the "axiomatics of physicists" kind. Unlike works by Bunge and other philosophers, von Neumann's book [301] became a classic of physical literature and had a significant impact on the further development of quantum physics. Bunge's reaction to this seminal work demonstrates well the uneasy relationships between logically-oriented philosophers interested in the contemporary science and scientists interested in logic and philosophy:

> "In his epoch-making book ([301]), which enriched the mathematical framework of the theory, von Neumann is wrongly supposed to have laid down the axiomatic foundations of quantum mechanics. As a matter of fact his exposition lacks all the characteristics of modern axiomatics: it does not disclose the presuppositions, it does not identify the basic concepts of the theory, it does not list all the initial assumptions (axioms), it fails to propose a consistent physical interpretation of the formalism, and it is ridden with inconsistencies and philosophical naivetés. Yet for some strange reason it passes for a model of physical axiomatics." [37, p. 132]

Bunge quite rightly points to the fact that von Neumann's theory does not even approximately fall under the standard "modern" concept of being axiomatic, stemming from Hilbert and later further developed by many logicians and philosophers including Bunge himself. In particular, von Neumann's presentation lacks anything like the distinction between syntax and semantics. It is axiomatic only in the very general sense of being built upon few well-distinguished first principles. Other examples of "axiomatics of physicists" are of the same informal character. In addition to von Neumann's classics we can point here to Macky's work [175] not mentioned by Bunge; for a more recent literature see [44] and [216].

The idea of an axiomatic reconstruction of Quantum Theory on the basis of Quantum logic was also explored (albeit not realised) by Louis de Broglie's student Paulette Destouches-Fevriér [54]. Fevriér's project has been harshly criticised by P. Suppes and J.C.C.McKinsey [193] who argue that this project is unrealistic because it also involves a reconstruction Classical Mathematical (allegedly based on Classical logic). Bas van Fraassenn in his 1974 paper with the telling title "The Labyrinth of Quantum Logics" [290], attempts to reformulate the early informal ideas of von Neumann, H. Reichenbach and some other thinkers according to then current logical standards. The result of this work is not one particular logical calculus but a family of such calculi. In 2004, a similar work was done by M.D.Chiara, R. Giuntini and R. Greech on the basis of more recent literature; the authors come to the expected conclusion according to which "The 'labyrinth of quantum logic' described by van Fraassen (1974) [since then] has become more and more labyrinthine." [44, p. 268-269]. However valuable this research can be from logical and philosophical points of view, it is clear that it has very little to do with today's physics.

In spite of the fact that logical approaches presently do not belong to the mainstream of theoretical physics, research on Hilbert's 6th Problem continues. For a recent case study, see [3] and references therein.

2.3.2 Biology

An early attempt to build biology axiomatically was made in 1937 by J.H. Woodger [303]. Woodger's project was practically oriented in the sense that it aimed at a theory that could be used by working biologists for teaching and research. Preparing his monograph J.H. Woodger collaborated in person with A. Tarski and tried to implement Tarski's conception of deductive science in biology. Just as in the case of physics, Woodger's attempt to apply in biology a formal framework designed by logicians and philosophers was rejected by the contemporary biological community [93]; in addition it was severely criticised by philosophers of biology of the younger generation [201]. Remarkably, both proponents and opponents of axiomatic biology often talk about the axiomatisation of biology as an application in this field of methods and tools already successfully used in mathematics and physics: the proponents argue that this helps to build for biology a more solid theoretical basis and make it more precise and rigorous, while the opponents argue that the specific character of biology and its subject matter do not allow one to borrow for this science the formal tools and standards of rigour used in physics and in mathematics. From a larger perspective explored in this work, such arguments appear to be misleading in both cases, because in fact the application of axiomatic approaches in physics and mathematics in its turn is very far from being unproblematic.

All more recent attempts to build biological theories axiomatically, which we have reviewed, aim at logical and epistemological analysis of these theories and are not designed for biological education and research. For a typical example of such work and for further references see [63].

2.3.3 Semantic View of Theories

An important new development in the axiomatic presentation of physical and other scientific theories, which some authors describe as a “revolution” [66], occurred in the 1950s via works by P. Suppes and his collaborators [129]. The proponents of the new axiomatic approach used the titles “semantic view of theories” and “non-statement view”, and opposed their view of scientific theories to the received “syntactic view”, which they attributed to earlier enthusiasts of the Hilbert-style axiomatic method in science [98]. According to the *semantic view* a scientific theory cannot be identified with any particular set of axioms and theorems expressed with an appropriate symbolic syntax — even if such a formal syntactic structure is provided with a default semantic interpretation or a certain class of such interpretations. (This is how theories are identified from the standard Hilbertian point of view, which the proponents of the new method call “syntactic”.) Instead a scientific theory should by identified with a certain class of its *models*, which, generally, may allow for many different axiomatisations that involve different syntactic choices. Notwithstanding the fact that the term “model” is used in science and in logic in a number of very different ways, Suppes argues that the notion of model due to Tarski, which provides a uniform set-theoretic semantics for CFOL and is commonly used in mathematical Model theory, accounts for all relevant cases [274], [275]. Suppes' idea that scientific theories are in a certain sense invariant with respect to their syntactic axiomatic presentation was strongly motivated by the concept of the role of invariance in geometry and physics. By a physical analogy a chosen syntactic representation of a given theory can be seen as a mathematical representation of physical object or process made in a fixed frame of reference a particular coordinate system; such a representation works properly only if one knows how to distinguish between its invariant features, which don't depend on the given frame and superficial features that vary from one frame to another and don't express any objective physical content [275, p. 99]).

From a more technical point of view, the "revolution in Stanford" that gave rise to the *semantic view* of scientific theories amounts to taking into account the formal semantics and model theory (that have been developed by A. Tarski shortly before this development) and using the Bourbaki-style set-theoretic axiomatic presentation of theories rather than the Hilbert-style axiomatic method in its original form. What has been said above about the differences between Bourbaki's and Hilbert's axiomatic approaches in mathematics (see **2.2.1**) remains relevant to the Bourbaki-style axiomatic approach applied by Suppes and his followers for representing scientific theories. More details concerning the origins and the early history of the *semantic view* as well as a standard presentation of this view can be found in [273, p. 3-5]; for a recent overview see [98].

From the early 1970s onwards the *semantic view* was further developed by J.D. Sneed [262], W. Stegmüller [267], W. Balzer, and U. Moulines [14], [16], [15] who support their Bourbaki-style formal approach with a version of philosophical *structuralism*. The body of works produced within this project arguably clarifies the logical structure of various scientific theories and structural relationships between different theories. However we would like to stress the fact that the obtained "semantic" axiomatic presentations of scientific theories play no role and have no prospective application in current scientific practices including science education at all levels. However controversial the role of Bourbaki in 20th century mathematics and mathematics education, it is a brute historical fact that the Bourbaki-style presentation of mathematical theories was used in many significant developments in pure mathematics, and strongly influenced mathematics education over the passed century. However, the attempt by P. Suppes and his many followers to apply Bourbaki's method in science was not, practically speaking, successful, and did not have a comparable effect. We postpone a more principled discussion on the *semantic view* until **4.3** where we argue that the update of Hilbert's formal axiomatic method proposed by the proponents of this view is not sufficient for an effective and adequate formal representation of scientific theories, and then suggest a remedy.

2.3.4 Computer Science and Engineering

With the rise of Computer Science (CS) and the computer revolution of the second half of the 20th century the axiomatic method and, more generally, formal logical approaches in mathematics and natural science became significant in a new way. The traditional concept of Knowledge Representation (KR) is twofold: on the one hand, it is a routine practical concern of working scientists, scientific editors and university lecturers. On the other hand, it is a research subject for epistemologists and historians of science, theoreticians of education and some other experts. These traditional functions of KR remain important today. However, with the rise of Artificial Intelligence (AI) — first as a theoretical possibility and more recently as an existing information technology — KR also became a research area in CS, a domain of Software Engineering and existing digital technology having industrial applications. The relationships between existing KR technologies and formal logical methods including the standard Hilbert-style axiomatic method are far from simple and straightforward. Early works in Artificial Intelligence (AI) and KR were logically-oriented and aimed at direct computational implementations of standard logical frameworks including Hilbert-style formal axiomatic systems [191]. However it was soon realised that since effective computability constitutes a hard theoretical and practical problem, computer-based AI and KR require further theoretical work and engineering invention. This development boosted very fruitful research in the Computational Logic, with a close interaction with CS [32]. However, even in this special form logic did not remain the only theoretical foundation for KR research and KR engineering: many important ideas came to KR from linguistics and more recently from neurobiology. With the recent emergence of KR technologies based on non-parametric statistical methods such as Machine Learning and Deep Learning, the role and relevance of logical foundations in KR has once again been seriously questioned [145].

Many problems and concerns arising in computer-based KR such as the aforementioned question of the role and place of logic have an obvious philosophical character. In the present work we focus only on issues related to axiomatic approaches in KR. For a more general philosophical discussion of computer-based KR and its logical and epistemological foundations see [178].

Our proposed notion of Constructive Axiomatic Method (see **4.2** below) unlike the received Hilbert-style axiomatic method allows for a direct computational implementation: we design this method on the basis of a certain formal calculus (Homotopy Type theory) that has already been computationally implemented (in AGDA and some other software). Integration of this approach into the existing KR technology is an ongoing project [249], [146].

A form of axiomatics currently used in CS and Engineering is *Axiomatic Design* (AD) [270] [167] [62] [69]. AD is not a computational implementation of the Hilbert-style axiomatic method, but it shares some essential features with it. The idea of AD is to design complex industrial products (say, aircrafts or new composite materials) in a top-down way rather than the traditional bottom-up way: one begins with customer's requirements (aka "axioms"), translates them into appropriate functional requirements, specifies needed ingredients for the production and, finally, designs on this basis the optimal technological process with the desired outcome. The traditional way of designing new technologies works into the opposite direction: one experiments with the ingredients, eventually obtains some new interesting product and then looks for its applications. The analogy between AD and Hilbert's formal axiomatic method can be better seen if this latter method is understood by contrast to the*genetic* method of building mathematical concepts and theories (see **1.3** above). This analogy (and apparently a motivation) is further reinforced by the fact that AD essentially employs the *independence axiom* according to which functional requirements must always be mutually independent. As in the case of von Neumann's "axiomatic" Quantum Theory we see here a fruitful application of Hilbert's axiomatic method as a general idea and insight but not as a ready-made logical technique.

2.4 Conclusion of Chapter 2

In this chapter we made an attempt to describe and evaluate the integral impact of the Hilbert-style axiomatic method on mathematics and science. In Section **2.1** we considered set theory, where the role of the formal axiomatic method is more significant than in any other area of today's mathematics.

However in this and some other related cases this role is very special. In modern axiomatic set-theory (as distinguished from Cantor-style so-called "naive" set theory) axiomatic theories such as ZFC are typically involved as objects of further mathematical study rather than as fragments of established mathematical knowledge. Similar remarks can be made about the standard Proof theory developed after Hilbert's pioneering works in this field. Proofs and conjectures in these mathematical disciplines are typically presented in the same semi-formal mathematical style as in any other area of mathematics — even if people working in FOM usually more tightly control the theoretical resources used in their proofs than mathematicians working in other areas. Thus in set theory, proof theory and other FOM-related mathematical disciplines the Hilbert-style axiomatic representation of theories is used primarily for *meta*-mathematical purposes, namely, as a means for studying the represented theories using various (meta-)mathematical methods, but not for more pedestrian purposes such as storing, communication, dissemination and reproduction of mathematical knowledge.

In Section **2.2** we described and analysed different aspects of the most significant and systematic attempt to introduce the Hilbert-style axiomatic method (as an element of set-theoretic foundations) into a broad mathematical practice, which is associated with the (pseudo-)name of Nicolas Bourbaki. We have stressed the fact that Bourbaki's axiomatic approach is not quite the same as Hilbert's. Unlike Hilbert's original version, Bourbaki's version of the axiomatic method is *semantic*, which means that Bourbaki-style axiomatic theories are equipped with default set-theoretic models, which are, generally, not isomorphic. This important point has never been emphasised by Bourbaki himself. It has, however, been emphasised and widely discussed in a different context by Patrick Suppes and some other philosophers who tried to use Bourbaki's axiomatic approach to represent scientific theories beyond pure mathematics. Proponents of this approach coined the term "semantic view of theories" as a name of the view according to which a theory is essentially characterised by a class of its models rather than by its axioms and theorems in a non-interpreted form. This approach allows one to abstract away from syntactic details, which working mathematicians usually consider to be irrelevant to their object of study, and thus more effectively use this method for representational purposes. A drawback of this approach is that mathematical proofs presented in Bourbaki-style are not formally checkable in practice (beyond trivial cases). Given a valid proof so presented one is always in a position to show that the conclusion of this proof can *in principle* be logically deduced from the axioms of set theory, but typically not in a position to perform such a formal deduction syntactically and check its correctness.

As we have seen, the impact of Bourbaki's continuing project is controversial. Outside a narrow group of enthusiasts who continue today to push Bourbaki's project further forward, there are very few research mathematicians and mathematics educators who are ready to use Bourbaki's volumes in their teaching and research. Attempts to reform elementary mathematics education along Bourbki's line, which were undertaken in many countries from the1950s to the 1970s are almost universally seen today as "pedagogical errors" (see **2.2.3** above). However, it is hardly disputable that Bourbaki succeeded creating a certain vision of mathematics as a unified discipline and producing a semi-formal "set-theoretic language", which is routinely used today in various fields and areas of mathematical research helping their cross-fertilisation. Even if there are serious reasons to regard Bourbaki's set-theoretical FOM as outdated today, its basic ingredients still serve mathematicians and students of mathematics for many everyday purposes.

In Section **2.3** we overviewed some past attempts to implement the Hilbert-style axiomatic architecture in physics, biology, computer science and engineering. We observed that such attempts have so far failed to make axiomatic theory-building into a standard and commonly recognised scientific practice. Already in the 1950s the community of people interested in the application of formal logical methods in science divided into a group of working scientists who tried to make progress in their scientific fields using new logical methods, on the one hand, and a group of logically-minded philosophers who tried to provide the existing scientific theories with new logical foundations and to represent these theory axiomatically according to rigorous logical standards, on the other hand. Some members of the second group quite rightly pointed to the fact that scientists tended to interpret logical methods too liberally and fell short of any reasonable logical standard in their attempts to apply the axiomatic method in science. In spite of recurring attempts to bridge this disciplinary gap between science and the logically-oriented philosophy of science it remains today quite wide.

Some researchers don't see a problem here assuming that a logical analysis of scientific theories is a special philosophical task, which need not be seen as a part of science itself. This point of view is clearly at odds with the idea according to which the philosophy of a scientific discipline should be in close interaction with the ongoing research in this discipline and be well informed by its latest achievements. Proponents of logical approaches in the philosophy of science of the older generation, such as Ernest Nagel and Mario Bunge, spent significant efforts aimed at bridging the gap between science and its philosophy; they hoped that modern logical tools could help them to achieve this goal. However, in retrospect, it appears to us that this goal was never achieved. We believe that the goal is rightly set and attempt in this work to achieve it by reconsidering the received notion of axiomatic method and designing a new conception of this method having in mind its possible applications in science.

3 Novel Axiomatic Approaches

In this chapter we describe two non-standard axiomatic mathematical theories: the theory of elementary topos built by W. Lawvere as a part of his project of developing category-theoretic foundations for mathematics and the Homotopy Type theory built by V. Voevodsky and his collaborators as a part of the project of developing Univalent Foundations for mathematics. We qualify these axiomatic theories as "non-standard" and "novel" because they use axiomatic architectures that differ significantly from Hilbert's axiomatic architecture (which in this context we call "standard"). In the next chapter we formulate on this basis a new version of the axiomatic method that we call "constructive" and show that it is more appropriate for the formal representation of scientific theories than the standard axiomatic method.

3.1 Category-theoretic foundations of mathematics and Topos theory[22]

3.1.1 Language of Categories

The mathematical concept of *category* emerged in the 1940s, its official birth marked by a 1945 paper by S. Eilenberg and S. MacLane [60]. Its motivation and basic content can be explained using Bourbaki's concept of mathematical *structure*, discussed above (see **2.2.2**). Recall the concept of *isomorphic* algebraic groups and observe that

- The *relation* of isomorphism between groups is defined in terms of the existence of an isomorphism as a *map*. Such maps are, generally, many and form a further structure.

- Recall that a group isomorphism

$$f : G \xrightarrow{\sim} G'$$

  is a bijective (aka *invertible*) map such that for all g_1, g_2 from G we have $f(g_1 \circ g_2) = f(g_1) \odot f(g_2)$ where $\circ$ and $\odot$ are group operations in G and G' correspondingly. If we now take f to be any (not necessarily invertible) function and preserve the above condition we get a more general concept of map between algebraic groups, which is known as group *homomorphism.*

Similar observations can be made about other types of Bourbaki-style mathematical structures: algebraic rings, lattices, topological spaces, etc. The moral is that every type of structure is equipped with a special type of map between structures of the given type. In current mathematical argo it is said that such maps "respect" or "preserve" the corresponding structure (even if what is *respected* and *preserved* here is the type of structure and not a singular structure up to isomorphism). It should also be stressed that such maps are not defined automatically for all types of structures: in some cases there is room for different choices. For example, standard maps between topological spaces called continuous transformations do not, strictly speaking, *preserve* the topological structure but *reflect* it : open subsets in the target space are images of open subsets in the

[22]This Section includes some materials from [233, Ch. 5].

source space but open subsets of the source space may also map to closed subsets of the target space. Arguably the concept of a *map* aka *transformation* between structures of the same type is at least as fundamental as the concept of the corresponding structure type itself. Consider again the concept of topological space. It is apparently impossible to explain this concept properly without referring to continuity and continuous transformations. This and similar examples motivate the idea that Bourbaki's definitions of structure types are incomplete, and that an adequate definition should also specify a corresponding type of maps between structures of the given type.

The category of mathematical structures of a certain type T can be informally described as a collection of all structures of the given type together with all maps between these structures that respect it (so that the type of such maps is also specified). The *category of groups* $\mathbb{G}$, which comprises all groups and all homomorphisms between the groups, is an example. Another important example is the *category of sets* $\mathbb{SET}$ that comprises all sets and all functions. Crucially, the maps between structures, which in category theory are called *morphisms*, support a further structure of an algebraic kind: morphisms $f : A \to B$ and $g : B \to C$ induce the composition morphism $h = g \circ f : A \to C$ as it can be also shown in a more geometric way with the following diagram:

$$\begin{tikzcd} & B \arrow[dr, "g"] & \\ A \arrow[ur, "f"] \arrow[rr, "h"'] & & C \end{tikzcd}$$

The equality $h = g \circ f$ translates into the statement that the above diagram *commutes* (or is *commutative*).

Algebraically speaking, the operation of composition $\circ$ defined on morphisms of a given category is *partial* in the sense that it is defined not for all ordered pairs of morphisms found in the given category but only for those pairs of morphisms where the target object of one morphism (called its *codomain*) coincides with the source (*domain*) of the other morphism. Notice that the structure determined by the composition of morphisms in a given category is not precisely a Bourbaki-like structure, because the collection of objects of the given category (sets, groups, topological spaces, etc.) and the collection of all morphisms belonging to this category are, generally, not sets but proper classes as in the case of the category $\mathbb{SET}$ of "all" sets and in the case of the category $\mathbb{G}$ of "all" groups. Nevertheless, categories also admit a natural definition of structure-preserving map that is called *functor*. An easy example of functor is the *forgetful* functor $U : \mathbb{G} \to \mathbb{SET}$ that maps every group to its underlying set "forgetting" about the group operation. The notion of functor allows one to consider categories of categories of various sorts, i.e., categories such that their object are also categories and morphisms are functors. Functors of the form $f : A \to B$ also form categories (called functor categories and denoted $[A, B]$), where morphisms are structure-preserving maps between functors called *natural transformations*.

More formally, a category comprises:

- a class of *objects* $A, B, C, \ldots$;
- for every pair of objects A, B a class (usually a set) of *morphisms* $f, g, h, \ldots$ of form $A \to B$ where A is called the *domain* and B the *codomain* of the given morphism f, in symbols $Dom(f) = A$ and $CoDom(f) = B$;
- the operation of composition $h = g \circ f$ defined for all pairs of morphisms f, g such that $CoDom(f) = Dom(g)$, which is associative, i.e., $h \circ (g \circ f) = (h \circ g) \circ f$ whenever the composition is defined (in what follows we often omit the composition sign $\circ$ and write $h = gf$);
- the *identity morphism* 1_A associated with every object A such that for every morphism f with $CoDom(f) = A$, $1_A \circ f = f$ and for every morphism g with $Dom(g) = A$, $f \circ 1_A$.

The notion of identity morphism or "null transformation" formally introduced here is similar to the notion of unit of algebraic group, but since the operation of composition defined on morphisms is partial, each object of the given category has its own identity morphism. Since the above definition of category implies the uniqueness of identity morphisms for each object of the given category, the notion of object can be formally identified with that of identity morphism.

Categories of various Bourbaki-style mathematical structures are important examples of categories, but they are not the only such examples. The concept of category can be also fruitfully used in a more abstract way. Notice that an algebraic group can be identified (up to isomorphism) with a category with a single object provided with a set of invertible morphisms, i.e., isomorphisms into itself. The morphisms here are elements of the group, the identity morphism of the object is the unit, and the composition of morphisms is the group operation. The invertibility of morphisms grants the existence of inverse elements. In this case the category concept is used not as an abstraction that captures a common structure of independently introduced mathematical concepts and constructions (such as sets with functions, groups with homomorphisms, etc.), but as an autonomous elementary conceptual tool for the construction (or reconstruction) of mathematical concepts.

Category theory (CT) proved to be a useful mathematical *language* that in the second half of the 20th century helped to organise a number of emerging concepts and new results into a stable theoretical form. For early examples that justified the usefulness of CT in the eyes of many mathematicians, see [61], [59], [218]. Some new mathematical concepts and mathematical theories developed during the same period would not even have emerged without CT: in particular, this is true of the concept of topos invented by A. Grothendieck in the 1950s. A number of today's mathematical disciplines, including Algebraic Geometry, Homological Algebra, Functional Analysis, use CT as their basic language [147], [183].

As Yu. Manin puts it, "at the next stage of this historical development [that began with the rise of Set theory at the end of the 19th century], sets gave way to categories" [182, p.7]. This is a robust "fact of science" in Hermann Cohen's sense [45, 119-120], which calls for historical and philosophical reflexion and analysis. Here we mention only one reason why the category-theoretic language was at certain point preferred to the set-theoretic one in a significant number of mathematical disciplines. The concept of map aka transformation (French *application*, German *Abbildung*, Russian *otobrazhenie*) has a geometrical origin; it has been increasingly important in mathematics at least since Gauss' pioneering works on the geometry of curve surfaces in the first half of the 19th century [78]. In particular, it plays a central role in Felix Klein's *Erlangen Program* of 1872, which aims at a characterisation of geometric spaces in terms of groups of their transformations and invariants of these transformations [141]. Cantor's set theory also uses this concept at a fundamental level in the form of one-to-one correspondence between sets, without which the key notion of cardinality of a set cannot be defined. In the 20th century mathematics the importance of map concept in a number of key mathematical disciplines became even more obvious. The fact is strongly evidenced by the successful application of CT in the aforementioned works by H. Cartan, S.Eilenberg, N. Steenrod and D. Quillen, and in many contemporary mathematical works.

The standard axiomatic approach *in* set theory started by E. Zermelo in 1908 [305] uses as its only non-logical primitive predicate the relation of *membership* ($x \in y$ or in words "x is element of set y"), not the concept of map or some of its modifications. This has an effect on how the standard axiomatic approach is used in the Bourbaki-style set-theoretic semantic setting. Surely, maps between mathematical structures, e.g., group homomorphisms, are representable in this setting. But the set-theoretic details of such representation have been perceived by many mathematicians as wholly irrelevant. Recall the aforementioned puzzling fragment of the Bourbaki 1950 manifesto in which the author claimed that "the notion of set itself disappeared ... in the light of the recent work on logical formalism", so that "mathematical structures become, properly speaking, the only 'objects' of mathematics" [27, p. 225-226], see **2.2.2** above. Bourbaki doesn't stress the importance of maps in this article but his dissatisfaction with the standard set-theoretic grounding of mathematical structures is manifest. In this context the success of CT as an alternative language that allowed mathematical structures to be effectively theoretically accounting for in terms of their maps bypassing the redundant set-theoretic details, appeared to many mathematicians, among them some Bourbaki members (including H. Cartan, S. Eilenberg and A. Grothendieck) to be a way to realise Bourbaki's dream about the "disappearance of sets".

Given the fact that CT emerged during the early stage of Bourbaki's project and the fact that such important figures in the early history of CT as S. Eilenberg and A. Grothendieck were at the same time Bourbaki members, one might wonder how it could happen that CT never made its appearance in the Bourbaki's volumes. In fact such plans existed: as evidenced in extant working materials of Bourbaki's in 1956, the group planned to supplement their first volume of *Elements* (Set theory) with an additional chapter on categories and functors, and in 1961 — to produce a separate volume on the Abelian Categories [48]; see also [195] for further historical details. These plans were never realised, however. Different members of the group had contrary opinions about the merits and the usefulness of categorical approaches; as a result the language of categories did not gain Bourbaki's official recognition (in spite of the fact that many category-theoretic ideas and concepts were used in disguise in a number of places of Bourbaki's *Elements*.)

In our view, the absence of CT in Bourbaki's *Elements* is not contingent on, and cannot be fully explained by, an analysis of conflicting views of Bourbaki members on that matter. A deeper reason is that the category-theoretic language, in the form in which it has been proven effective, is not merely a conservative extension of Bourbaki's set-theoretic semantic framework; it could not be be easily integrated into this framework without revising its basic principles. As we have already explained above using the example of group theory, Bourbaki-style set-theoretic presentations of mathematical theories can be analysed and understood in standard logical terms including model theory (see **2.2.1** above). This allows one to think of set theory not only as a mathematical language but also as a foundation of mathematics. It is not immediately clear how this or a similar analysis may proceed in the case of theories presented using the category-theoretic language. The introduction of categories on equal footing with groups, topological spaces and other mathematical structures not only encounters technical difficulties (including the size problem in the case of categories of "all" sets, groups, etc.) but is also at odds with the existing practice of "categorical reasoning", which makes the Bourbaki-style set-theoretic underpinning wholly redundant (at least from the viewpoint of a working mathematician). While we cautiously say that CT "helps to organise" some mathematical contents into a "stable theoretical form" (in order to avoid misunderstanding on the part of logical and philosophical readers), S. Eilenberg and N. Steenrod in the *Preface* to their [61] as well as D. Quillen in [218] boldly claim that they build "axiomatic" theories. Like von Neumann in [301], these authors use the notion of axiomatic theory in the very broad sense of theory built on explicit first principles; they don't formally specify any particular axiomatic architecture and don't provide any other logical details of their axiomatic approach. In the next Section we overview the contribution of W. Lawvere, who since the early 1960s has been working on the transformation of the categorical language into full-fledged foundations of mathematics.

3.1.2 Category Theory as a Foundation

A) Diagrammatic Syntax

The standard definition of category presented above straightforwardly translates into the language of CFOL, provided that objects are identified with their identity morphisms (in order to avoid distinguishing between objects and morphisms as two different types). Such a theory has four primitive terms: "morphism" as a general name of individuals (like "sets" in ZFC) and three primitive predicates, namely, two binary relations of being domain and codomain of a given morphism ($Dom(A, g)$ and $CoDom(B, f)$) and a ternary relation for the composition of morphisms ($Comp(h, g, f)$, which holds when $Dom(A, g)$, $CoDom(B, f)$ and $A = B$ hold). The axioms of such a theory are sometimes referred to in the current literature as Eilenberg-MacLane axioms, and the theory itself is called EM for that reason (see **1.3.1** above). Such a list of axioms for general CT was published in 1963 by W. Lawvere in his Ph.D. thesis [155]. Interestingly, Lawvere introduces in the same move a non-standard syntactic convention that allows one to read a categorical diagram as a logical (first-order) formula:

> "[In a category w]e identify objects with their identity maps and we regard a diagram
>
> $$A \xrightarrow{f} B$$
>
> as a formula which asserts that A is the (identity map of the) domain of f and that B is the (identity map of the) codomain of f. Thus, for example, the following is a universally valid formula
>
> $$A \xrightarrow{f} B \Rightarrow A \xrightarrow{A} A \wedge A \xrightarrow{f} B \wedge B \xrightarrow{B} B \wedge Af = f = fB$$
>
> "

From a formal point of view, this is nothing but an unusual symbolic convention or a shorthand, which does not change the sense of the matter. But in fact it touches upon the core of the standard (Hilbert-style) axiomatic method. Recall from **1.2.3** Hilbert's distinction between "real" and "ideal" mathematical constructions: only symbolic constructions qualify in this sense as real while all their interpretations qualify as ideal. As we have already stressed, this distinction creates a gap between the formalised and the usual non-formalised mathematics because manipulations with symbols in formal theories represent some logical operations but not operations with the "ideal" objects themselves. This is an essential feature of Hilbert-style formal mathematics, which does not disappear if one simply ignores the metaphysical distinction between real and ideal objects and the historical origin of this mathematics. Let us demonstrate this point with a simple example. Hilbert and Bernays in [113] use the symbolic expression $Zw(x, y, z)$ to denote a predicate saying that a given point y lies *between* given points x, z. As soon as values of x, y, z are fixed, $Zw(x, y, z)$ expresses a proposition. Now let us tentatively identify $Zw(x, y, z)$ with a geometrical *object* (construction), which makes the corresponding proposition true, namely with a triple of points $< x, y, z >$ such that y lies *between* points x and z. So we hope that our formula will refer both to a geometrical object (construction) and a true proposition "about" this object — just as in the case of the expression $f : A \to B$, read both as a particular morphism f and, by Lawvere's convention, also as a judgement that A is the domain and B is the codomain of f.

In some special cases such a constructive interpretation of Hilbert's formalism seems to work. Consider the formula

$$Zw(x, y, z) \to Zw(z, y, x)$$

and read it, first, as intended (i.e., as a logical implication) and second, as a description of the geometrical operation, which turns this geometrical construction

$$X \text{——} Y \text{——} Z$$

into that

$$Z \text{——} Y \text{——} X$$

by permuting endpoints X, Y. In this particular case there is indeed a structural similarity between operations with symbols x, y, z in Hilbert's formulas and operations with symbols X, Y, Z forming part of the traditional geometrical notation, used together with traditional geometrical diagrams. However, such a geometrical interpretation of logical formulas obviously does not extend to the whole of Hilbert's formalized Euclidean geometry. Notice that the formula $Zw(x, y, z)$ is meaningful even if it expresses a false proposition; in such cases we still have a symbolic construction but have no corresponding geometrical construction. Further, if we consider a slightly more complex formulas like this one

$$\forall x \forall y \forall z (Zw(x, y, z) \to Zw(z, y, x))$$

(which under the intended interpretation says that $Zw(x, y, z) \to Zw(z, y, x)$ is universally valid) we find no obvious geometrical interpretation for the symbol $\forall$, and no geometrical counterpart of this whole formula.

The use of diagrammatic category-theoretic syntax as a logical syntax (or, equivalently, a logical interpretation of category-theoretic commutative diagrams) proposed by Lawvere does not by itself solve, but only highlights, the problem of mismatch between the logical syntax of formalised theories and the symbolic means used in everyday mathematics. We shall shortly see, however, that Lawvere accomplishes much more toward its solution, in particular, by developing a geometric interpretation of logical quantifiers (see **3.1.4**).

B) <u>Elementary Theory of Category of Sets</u>

An important result obtained by Lawvere in [155], which has also been published as a separate paper [156], is a category-theoretic axiomatic theory of sets known as ETCS (Elementary Theory of Category of Sets), where the word "elementary" refers to the first-order character of this theory. See also [164] for a more detailed exposition of this work. Unlike the aforementioned theories of Homological Algebra and Homotopy [61], [218] that also use the language of categories and are called by their authors "axiomatic", ETCS is an axiomatic first-order theory in the standard sense of the term familiar to logicians and philosophers. At the same time, ETCS has some unusual features, as we shall shortly see. Lawvere's idea is to extend EM with additional axioms to the effect that a category that satisfies the additional axioms is in an appropriate sense (which will be specified shortly) *equivalent* to the "concrete" category of sets $\mathbb{SET}$

built with (all) ZFC-based sets and functions.

Here we provide only a partial description of ETCS's concepts and axioms, which helps one to understand the basics of Lawvere's category-theoretic approach to set theory. First, one needs to reconstruct the primitive set-theoretic concept of membership in the category-theoretic setting. To that end, one postulates the existence of a *terminal object*, which is an object of the given category that has exactly one incoming morphism from every object of this category (including itself). If such an object exists in a category then it is unique up to canonical (unique) isomorphism (exercise). In $\mathbb{SET}$ the terminal object is "the" singleton object (albeit the identity relation used in ZFC distinguishes between different singleton sets). Now an *element* x of a given object (set) A in category $\mathbb{C}$ is identified with arrow $x : T \to A$ from the terminal object T to that object. Elements of objects in a category, so defined, are also colloquially called *points*. Thus the concept of set-theoretic membership as reconstructed in ETCS is no longer primitive but derived. Moreover it is no longer a relation between two sets, but a relatively complex construction that involves all objects of the given category (via the universal property of terminal object).

In addition to the existence of terminal object ETCS postulates the existence of certain other *universal constructions*, which is the defining property of *Cartesian Closed category* or CCC for short (see **3.1.3 C** below). Among these constructions is the *exponent object* B^A, which in $\mathbb{SET}$ represents the set of functions of the form $A \to B$.

Further, ETCS comprises an axiom which describes the special property of $\mathbb{SET}$ that morphisms in this category, i.e., functions, are determined pointwise: if the values of two functions $f, g : A \to B$ coincide on all their arguments, i.e., for all $x \in A$ we have $f(x) = g(x)$, then the two functions are the same: $f = g$. Using the category-theoretic reconstruction of set-theoretic membership, this property can be expressed via the statement that the following diagram commutes for all x:

$$\begin{tikzcd}
 & T \arrow[dl, "x"'] \arrow[dr, "y"] & \\
A \arrow[rr, shift left, "f"] \arrow[rr, shift right, "g"'] & & B
\end{tikzcd}$$

This property is known as *function extensionality* (but Lawvere in [155], [156] does not use this terminology, which is more recent and comes from different sources). In topos theory (see **3.1.4** below), the same property is called *well-pointedness*.

The Axiom of Choice (AC) is expressed in category-theoretic terms as follows. Morphism e is called *epic* aka *epimorphism* if it is right-cancellable in the sense that for all morphisms f, g, $fe = ge$ implies $f = g$ (provided the compositions are well-defined). A morphism $e : A \to B$ is called *split epimorphism* if there is a morphism $s : B \to A$ such that $es = B$. Every split epimorphism is an epimorphism (exercise). The category-theoretic version of AC postulates that all epimorphisms in a given category are split. In $\mathbb{SET}$, epimorphisms are surjective functions. The splitting condition $es = B$ in $\mathbb{SET}$ amounts to saying that given a surjective function $e : A \to B$, there exists a choice function $s : B \to A$ that for every element $b \in B$ picks up a particular $a \in A$ such that $e(a) = b$.

In total Lawvere introduces 8 axioms (on the top of the Eilenberg-MacLane axioms). As in the case of the EM axioms, the translation of these ETCS axioms into CFOL is straightforward; this allows one to qualify ETCS as a first-order formal theory in the standard sense of the term.

The appropriate notion of equivalence used in ETCS for the meta-theoretic purpose indicated above is itself category-theoretic. Consider categories A, B with two functors f, g going into the opposite directions:

$$A \underset{g}{\overset{f}{\rightleftarrows}} B$$

;

consider compositions fg and gf. If $fg = A$ and $gf = B$ then the functors f and g are said to be mutually inverse, being invertible and being isomorphisms, and categories A, B are called isomorphic. The *equivalence of categories* is a weaker property, which amounts to the existence of invertible natural transformations (natural isomorphisms) $\eta : fg \to A$ and $\theta : gf \to B$. Isomorphic categories are equivalent but equivalent categories are not, generally, isomorphic. The equivalence of categories so defined is obviously an equivalence relation in the usual sense of the term.

Why has this particular form of equivalence been used by Lawvere along with ETCS (recall that it is introduced in [156] not as a proper part of ETCS but as a meta-theoretic concept)? Category-theoretic properties such as Cartesian closedness (and all other properties of categories used in Lawvere's axioms that extend EM to ETCS) are invariant not only under the isomorphism of categories (which is a trivial fact) but also under the equivalence of categories (which is not a trivial fact). The idea according to which category theory studies properties of categories, which are invariant under the equivalence of categories, can be understood on equal footing with the idea according to which Bourbaki-style structures are studied up to isomorphism. We have shown in **2.2.2** how this latter idea can be spelled out more precisely using the *isomorphism equivalence principle* (IEP). Category theory applies a more general equivalence principle, namely the *category equivalence principle* (CEP), where the equivalence in question is the equivalence of categories [2]:

Let G, H be two equivalent categories. Then for any *categorical* property P it is the case that category G has property P if and only if category H has property P. In symbols:

$$\forall P. (G \simeq H) \Rightarrow (P(G) \leftrightarrow P(H))$$

CEP is not a part of (and does not follow from) the standard definition of category above, but it plays a major role in category theory. *Small* categories, i.e., categories where classes of objects are sets, can be seen and studied as Bourbaki-style structures. However, the categories *of* Bourbaki-style structures of various types such as $\mathbb{SET}$, $\mathbb{G}$, etc., which arise "naturally" in the context of Bourbaki-style mathematics, are *large* and only *locally small* — in the sense that all collections of morphisms from a fixed object A to fixed object B in such categories are sets of the form $Hom(A, B)$, known as *hom-sets*. Locally small categories are not structures in Bourbaki's sense and their theory cannot be built using Bourbaki's semantic axiomatic method. The invention of category-theoretic constructions with properties invariant under the equivalence of categories (but not only isomorphism-invariant), which form the bulk of basic CT, has been a definite step beyond Bourbaki-style mathematics. Thus CEP provides an additional theoretical argument in support of our above claim that CT could not possibly be integrated into Bourbaki's *Elements* without a very substantial revision of this work (see **3.1.1**).

Just as the set-theoretic foundations of Bourbaki-style mathematics are not isomorphism-invariant all the way down (Benaceraf problem [20]), the lower level of ETCS, namely, the Eilenber-MacLane axioms (EM), are not fully category-invariant (i.e., not invariant under the equivalence of categories). Recall that these axioms include the condition of composability of morphisms f, g expressed in terms of the *equality* of objects (composition $g \circ f$ exists if and only if the domain of g and the codomain of f are the same), which, unlike the isomorphism of objects, is not category-invariant. (The equality of morphisms with shared domain and codomain in locally small categories is category-invariant.) This foundational difficulty was not treated by Lawvere in [156] but was later addressed by Michael Makkai, who used a multi-sorted language that allowed him to formulate the concept of morphism composition without referring to the equality of objects [179]. In **3.2.5** below we describe a more recent approach to equivalence principles and related identity issues based on Homotopy Type theory [2].

Lawvere's "metatheorem" [156, p. 1510] according to which models of ETCS are equivalent to $\mathbb{SET}$ holds under additional completeness conditions that cannot be expressed in the first-order form. In that respect ETCS is similar to standard first-order theories like ZFC and PA, where non-standard models can also be ruled out only via further higher-order assumptions. Thus ETCS satisfies the usual standards of formal axiomatic theories with the following mutually related reservations: (i) the intended model of this theory is not a set model but a proper class model and (ii) the intended model is specified up to category equivalence but not up to isomorphism. However significant these reservations may be, they hardly change the core of Hilbert's axiomatic method. ETCS first demonstrated how category theory could be used to build formal axiomatic theories in the sense of "axiomatic" acceptable both to working mathematicians interested in applications of CT in various mathematical disciplines and to logicians and logically-oriented philosophers interested in foundations.

3.1.3 Categorical Logic[23]

Along with his work on ETCS Lawvere developed a more general project of category-theoretic foundations of mathematics, namely, an axiomatic theory of the Category of Categories (Category of Categories as a Foundation or CCAF for short) [157]. For an analysis of this work see our [233, ch. 5.2]. We turn now to a different, albeit closely related, development in category theory that can be labelled the *internalisation of logic*. We shall argue that the concept of the internal logic of a given category has far-reaching epistemological implications and calls for a more profound revision of standard formal axiomatic approaches. In order to explain this important development, we need some preliminaries.

A) <u>Type theories</u>

The idea of a logical calculus that does not simply apply to different domains of individuals but explicitly distinguishes between different *types* of individuals dates back to Bertrand Russell, who coined the term "theory of types" [251, Appendix B] (we leave aside Russell's motivations for developing this theory). The idea can even be traced further back to Aristotle's distinction between different *genus* of things, and his principle according to which switching between different genus in a piece of reasoning (*metabasis*) is not allowed.

One may remark that the type distinction reflects a feature of our natural languages:

> "Types are inherent in everyday language, for example, when we distinguish between "who" and "what" or between "somebody" and "something"" [171, p.125].

and further remark that distinguishing between different types of objects is tacitly done in mathematical practice:

> "In our mathematical practice we have learned to keep things apart. If we have a rational number and set of points in the Euclidean plane, we cannot even imagine what it means to form the intersection. [. . .] If we think of a set of objects, we usually think of collecting things of a certain type, and set-theoretical operations are to be carried out inside

[23] For a broader overview of the subject see [51].

that type. Some types might be considered as subtypes of some other types, but in other cases two different types have nothing to do with each other. That does not mean that their intersection is empty, but that it would be insane to even *talk* about the intersection.” ([34, p. 31])

The problem stressed in the above quote concerns the fact that the standard set-theoretic foundations of mathematics do not support a formal distinction between different types of objects, and thus do allow for the absurd set-theoretic constructions mentioned in the quote. Since every mathematical object is a set, one may always form an intersection of two objects. Interestingly, the type distinction between points and straight lines, which is made explicit in Hilbert’s *Foundations* of 1899, disappears in his *Foundations* of 1934, where Hilbert’s formal axiomatic method takes its mature symbolic form; in the latter case Hilbert treats only points as primitive objects. This is not surprising because the symbolic logical calculus used by Hilbert is not typed. One could expect that the replacement of the underlying logical calculus by a typed logic may solve the problem without any significant effect on the axiomatic architecture. However it turns out that this is not the case. Attempts to use type theories in the foundations of logic and mathematics gave rise to a novel axiomatic architecture of formal theories, which is quite unlike Hilbert’s.

B) Combinatorial Logic and Curry-Howard Correspondence

In 1924 Moses Schönfinkel published a paper [255], [256] aimed at deepening Hilbert’s formalisation of logic, which in the author’s view did not provide a purely formal treatment of logical concepts such as *proposition* and *variable*. Schönfinkel’s approach was to reduce the logical concepts, which so far were generally seen as basic, to a small number of more fundamental syntactic operations like substitution and permutation of signs [261]. Independently similar ideas inspired Huskell Curry in the1920s (before he first came across Schönfinkel’s paper during the academic year of 1927-1928), who gave to this field of study its current name of *Combinatory Logic* [39], [260]. Here is how Curry describes the aim and the scope of Combinatory Logic in a later co-authored monograph:

“Combinatory logic is a branch of mathematical logic which concerns

> itself with the ultimate foundations. Its purpose is the analysis of certain notions of such basic character that they are ordinarily taken for granted. These include [(i)] the process of substitution, usually indicated by the use of variables; and also [(ii)] the classification of the entities constructed by these processes into types or categories, which in many systems has to be done intuitively before the theory can be applied. It has been observed that these notions, although generally presupposed, are not simple; they constitute a prelogic, so to speak, whose analysis is by no means trivial. " ([96, p. 2])

Purposes (i) and (ii) mentioned by Curry in the above quotes are mutually dependent. Since a formal logical calculus is seen as a bare symbolic calculus where signs do not have any previously assumed meaning, one needs to make explicit certain distinctions between different types of symbolic constructions without which this calculus cannot qualify as logical. This includes, in particular, the distinction between individuals, propositions and logical connectives. The idea of Combinatory Logic as Curry describes it requires making all such distinctions *formal* without appealing to the usual meanings of the words "individual", "proposition", etc. Thus, pushing the formal approach to logic to its extreme shows the necessity of typing. One may argue on this ground that the type distinction is always present in logic whether one describes it explicitly or not. As Bart Jacobs puts this 'A logic is always a logic over a type theory" [128, p.1].

In 1969 William Howard reformulated and extended Curry's results in a note [125] that was first published only in 1980. Instead of using Combinatorial Logic, Howard used the formalism of (simply) typed *lambda calculus* invented by Alonzo Church in the late 1920s [39] and first published in 1933. Curry was, of course, aware about the fact that the formalism of lambda-calculus comes close to that of Combinatory Logic, but he claimed that his formalism provides a deeper foundational analysis [96, p.6-9]. Unlike Curry, Howard did not stress the philosophical motivation and the foundational significance of this result, but formulated it in terms of structural correspondence between two families of formal calculi, namely, the simply typed lambda calculi, on the one hand, and certain Getzen-style deductive systems, on the other hand. Such a presentation has certain pedagogical advantages for mathematical students not interested in foundational issues but it leaves wholly behind the philosophical content of Curry's work and gives a mistaken impression that we are dealing here with an unexpected mathematical fact rather than with a contentful logical principle.

Recall that in **1.4** we described the fact that *problems* and *theorems* in Euclid's *Elements* share a common structure as a form of Curry-Howard correspondence. Now we are in a position to explain this comparison. The Curry-Howard correspondence amounts to the observation that the rules of simply typed lambda calculus can be used *mutatis mutandis* for making (constructive) formal deductions. Lambda calculus (both typed and untyped) is a formal model of computations or, to put it in the language of computer science, of algorithms. Algorithms solve Euclid's geometrical problems. Logical deductions prove theorems. The two types of operations have different kinds of content, but share a common formal structure.

A more recent development in mathematical logic that is conceptually closely related to the Curry-Howard correspondence is the BHK semantics of the intuitionistic propositional calculus, so called after the names of Brouwer, Heyting and Kolmogorov. In his *Calculus of Problems* published in 1932 Andrey Nikolayevitch Kolmogorov uses a version of the syntax of Heyting's intuitionistic propositional calculus but provides it with a different semantics by interpreting this calculus in terms of solving problems or performing computational tasks rather than inferring propositions from other propositions [143].

C) Cartesian Closed Categories and Categorical Logic

By means of category theory, the mathematical structure behind the Curry-Howard correspondence can be presented in an invariant form that does not depend on arbitrary syntactic choices. An appropriate category that in Lawvere's words "serve[s] as a common abstraction of type theory and propositional logic" [160, p.134] is known as the *Cartesian closed* category or CCC for short. CCC first appears under this name in Lawvere's 1969 paper [160]. The concept of CCC itself is, however, already present in Lawvere's dissertation [155] and, as we have already seen, in his 1964 ETCS paper. Lawvere's ideas about the logical relevance of CCC were systematically developed by Joachim Lambek in the late 1960s and early 1970s, see [151], [152], [153]; a systematic analysis of the relationships between

Combinatory Logic, lambda-calculus and Cartesian Closed categories, which also contains some historical notes, is found in Lambek's and Scott's monograph [171]. This work gave rise to the new research field of Categorical Logic, which today also involves different approaches that combine logic with category theory [291], [51].

The aim of Lawvere's 1969 paper [160], as the author described it more recently, was to "demystify the incompleteness theorem of Gödel and the truth-definition theory of Tarski by showing that both are consequences of some very simple algebra in the cartesian-closed setting" [165, p.2]. Lawvere's idea here is to build a minimal conceptual setting that supports the *diagonal proofs* (aka diagonal arguments) so-called after Cantor's famous proof that there exists no one-to-one elementwise correspondence between the set $\mathbb{R}$ of real numbers and the set $\mathbb{N}$ of natural numbers, which involves an infinite matrix of 01-sequences and its inverted diagonal. Similar arguments involving impossible constructions have been used to prove impossibility results (and other related results), in particular, by Gödel (the First Incompleteness theorem), Tarski (the undefinability of arithmetical truth in arithmetics), Russell (Russell Paradox), Brouwer (his Fix Point theorem) and Turing (the undecidability of the Halting Problem over Turing machines). Before Lawvere's 1969 paper, however, no precise notion of generalised diagonal argument covering all such case was known. Lawvere showed that CCC is an appropriate abstract setting in which the argument in its general algebraic form goes through.

As we saw in **3.1.2**, ETCS specifies "the" category of sets as CCC of a special sort. The logical relevance of CCC explained above allows one to think of ETCS in a new way. In the last Section we presented ETCS as a formal first-order axiomatic theory similar to ZFC and PA (with certain reservations). Recall that the standard axiomatic architecture of such theories comprises (i) a background logical calculus equipped with a logical semantics that specifies, in particular, the meaning of logical constants and the concept of logical inference in some form) and (ii) the extra-logical part that involves non-logical constants, concepts (like that of membership in ZFC) and axioms that are not logical tautologies. In ETCS the extra-logical part has two layers: the general category theory EM (the lower layer) and Lawvere's 8 additional axioms and their consequences (the upper layer). Now it turns out that a fragment of this upper layer, namely the theory of CCC, admits a logical semantics and in this sense is logically relevant. This fact perturbs the standard axiomatic order according to which logic fully belongs to the background layer and is shared by many or even all (in case one accepts logical monism, i.e., the thesis that there is only one true Logic) such theories, while all higher extra-logical levels of axiomatic constructions deal with more and more specific features of particular theories. The idea of such a hierarchical theoretical order is central in the standard axiomatic architecture as described, for example, in [276], and is incompatible with the idea that Lawvere's 8 axioms may have a bearing on the logical part of ETCS along with its extra-logical part. As we shall shortly see, Lawvere does take this latter idea very seriously and it leads him eventually to the concept of the *internal logic* of a given category. In this present work we make an attempt to develop this idea more systematically and propose on this basis a novel view on the axiomatic method.

The idea that the relationships between logic and set theory are more intimate and more specific than the standard axiomatic view suggests was around long before the emergence of Categorical Logic and before the emergence of modern axiomatics and set theory itself. Thinking of propositions in terms of extensions of concepts was a key feature of George Boole's pioneering mathematical approach in logic [24]. This extensional approach in logic, where the concepts of *universe of discourse* and logical *class* of individuals are central, was later further developed by John Venn [295], widely known today thanks to his useful logical diagrams. Boolean algebra in its present form is a mathematical structure that is shared by Classical Propositional logic with standard connectives and the powerset of a given set with standard set-theoretic operations. Lawvere's discovery that CCC is a categorical structure shared by $\mathbb{SET}$, simply typed lambda-calculus, and the constructive fragment of Natural Deduction calculus, continues the same line of research in the mathematical logic. Before we discuss the epistemological implications of this discovery, let us describe another of Lawvere's achievements that demonstrates that an analysis of basic logical concepts in category-theoretic terms is indeed illuminating and non-trivial.

D) Quantifiers as Adjoints

An *adjoint situation* (called also an *adjunction*) is a pair of categories A, B with two functors f, g going in opposite directions:

$$A \underset{g}{\overset{f}{\rightleftarrows}} B$$

and natural transformations $\alpha : A \to gf$ and $\beta : fg \to B$ such that $(f\beta)(\alpha g) = g$ and $(\beta g)(f\alpha) = f$ provided that the following two triangles commute:

$$\begin{tikzcd} g \arrow[r, "\alpha g"] \arrow[dr, "1_g"'] & gfg \arrow[d, "g\beta"] \\ & g \end{tikzcd}$$

$$\begin{tikzcd} f \arrow[r, "f\alpha"] \arrow[dr, "1_f"'] & fgf \arrow[d, "\beta f"] \\ & f \end{tikzcd}$$

(As above we do not distinguish in categories between objects and their identity morphisms.) Given an adjoint situation as above functor g is called *left adjoint* to functor f and functor f is called *right adjoint* to functor g, in symbols $g \dashv f$. A given functor has at most one (up to unique isomorphism) left adjoint and one right adjoint (exercise).

Let us now for simplicity think of the given category as $\mathbb{SET}$ but have in mind that the described construction in a slightly more abstract form can be done in more general categories (in particular, in toposes, as we shall shortly see in **3.1.4**). Suppose that we have a one-place predicate (a property) P, which is meaningful on set Y, so that there is a subset P_Y of Y (in symbols $P_Y \subseteq Y$) such that for all $y \in Y$ $P(y)$ is true just in case $y \in P_Y$. Now using these data and morphism $f : X \to Y$ we can define a new predicate R on X as follows: we say that for all $x \in X$ $R(x)$ is true when $f(x) \in P_Y$ and false otherwise. So we get the subset $R_X \subseteq X$ such that for all $x \in X$, $R(x)$ is true just in case $x \in R_X$. Let us assume in addition that every subset P_Y of Y is determined by some predicate P meaningful on Y. Then given morphism f as above, we can associate with every subset P_Y a subset R_X and, correspondingly, a way to associate with every predicate P meaningful on Y a certain predicate R meaningful on X. Since subsets of a given set Y form a Boolean algebra $B(Y)$, we thus get a map between Boolean algebras (notice the change of direction!):

$$f^* : B(Y) \longrightarrow B(X)$$

Since Boolean algebras themselves are categories (with objects subsets and maps inclusions of subsets), f^* is a functor. For every proposition of the form $P(y)$, where $y \in Y$, functor f^* takes some $x \in X$ such that $y = f(x)$ and produces a new proposition $P(f(x)) = R(x)$ (for a single given y it may produce a set of different propositions of this form). Since it replaces y in $P(y)$ by $f(x) = y$ it is appropriate to call f^* a *substitution* functor.

The *left* adjoint to the substitution functor f^* is the functor

$$\exists_f : B(X) \longrightarrow B(Y)$$

which sends every $R \in B(X)$ (i.e. every subset of X) into $P \in B(Y)$ (subset of Y) consisting of elements $y \in Y$, such that *there exists* some $x \in R$ such that $y = f(x)$; in (some more) symbols

$$\exists_f(R) = \{y|\exists x(y = f(x) \wedge x \in R)\}$$

In other words, $\exists_f$ sends R into its *image* P under f. Now if (as above) we think of R as a property $R(x)$ meaningful on X and think of P as a property $P(y)$ meaningful on Y we can describe $\exists_f$ by saying that it transformes $R(x)$ into $P(y) = \exists_f x P'(x, y)$ and interpret $\exists_f$ as the usual existential quantifier.

The *right* adjoint to the substitution functor f^* is functor

$$\forall_f : B(X) \longrightarrow B(Y)$$

which sends every subset R of X into subset P of Y defined as follows:

$$\forall_f(R) = \{y|\forall x(y = f(x) \Rightarrow x \in R)\}$$

and thus transforms $R(X)$ into $P(y) = \forall_f x P'(x, y)$. Notice that functors $\exists_f$ and $\forall_f$ are defined here as adjoints to substitution functor f^*.

The fact that in this setting quantifiers arise "naturally" through functorial adjunction is remarkable from a mathematical point of view. According to Marquis and Reyes "[t]his was a key observation that convinced many mathematicians that this was the right analysis of quantifiers" [184, p.710].

The idea of quantifiers as adjoints to substitution was first mentioned by Lawvere in [158] and then fully elaborated in the *Dialectica* paper [159] with the help of the notion of CCC; the categorical construction, which supports quantification and in fact the full first-order logic, Lawvere calls a *hyperdoctrine* [161]. Along with first-order logic, Lawvere's hyperdoctrines internalise the equality relation. For an analysis of this work of Lawvere see [233, ch. 5.5]. Let us only mention here an interesting historical detail. When Lawvere discusses the internalised equality relation in hypodoctrines, he mention the possibility to think about it in terms of homotopy theory but does not develop this idea further [161, p. 3-4]. This is a clear precipitation of the equality concept in Homotopy Type theory as it was developed in the late 2000s, see **3.2** below.

Lawvere and Rosenbourgh provide an interesting conceptual explanation of the construal of logical quantifiers as adjoint functors presented above [166, p.193-194]. They develop a view according to which the usual idea that the same logical formalism can be repeatedly applied to various *universes of discourse* arbitrary chosen on pragmatic or other external grounds (defended, in particular, by Venn [295]), gives only a local and essentially incomplete picture of how logic works. According to Lawvere and Rosenbourgh, the multiplicity of universes represented by objects of a given base category, and the structure of "passages" of translations between these universes, which are represented by morphisms of this category, essentially determine the (internal) logic of this category. Thus the category-theoretic approach in logic makes the global picture explicit and, in particular, shows where logical quantifiers come from. It goes without saying that the view on logic developed by Lawvere and Rosenbourgh also rules out the Fregean view according to which there exists just one universe of discourse, which is the actual world itself [117, p.x-xi]. Notice that the view on logic developed by Lawvere and Rosenbourgh can be reformulated in more technical terms if multiple universes of discourse are understood as multiple *types* in the sense of Type theory. A contemporary systematic analysis of basic Categorical logic from a type-theoretic point of view (before the emergence of Homotopy type theory) can be found in [128].

E) <u>Objective and Subjective Logic according to Lawvere</u>

Lawvere's thinking about logic and the foundations of mathematics was strongly motivated by Hegel's philosophy, particularly by Hegel's distinction between *objective* and *subjective* logic. Some details concerning Lawvere's reading of Hegel and application of Hegelian ideas in Lawvere's mathematical work can be found in [233, ch.5.8] and [232]. Here we leave these details aside and explain Lawvere's approach in its own terms without referring to Hegel. As we shall now see the distinction between objective and subjective logic made by Lawvere (after Hegel) has not only philosophical but also a technical mathematical content. This is how Lawvere draws this distinction in his 1994 paper [163]:

> "Arising [...] from the needs of geometry, category theory has developed such notions as adjoint functor, topos, fibration, closed category, 2-category, etc. in order to provide (i) a guide to the complex, but very non-arbitrary constructions of the concepts and their interactions which grow out of the study of space and quantity. It was only the relentless adherence to the needs of that basic subject that made category theory so well-determined yet powerful. [...] If we replace "space and quantity" in (i) above by "any serious object of study", then (i) becomes my working definition of *objective logic*. Of course, when taken in a philosophically proper sense, space and quantity do pervade any serious field of study. [..] Category theory has also objectified as a special case (ii) the *subjective logic* of inference between statements. Here statements are of interest only for their potential to describe the objects which concretize the concepts." [163, p. 16]

Lawvere's *subjective* logic is logic in a familiar sense of the word, which accounts for inferences between statements or judgements and can be formally construed as CFOL or another similar calculus. However in Lawvere's view it is a serious philosophical mistake to think of such logical calculi as self-sustained and use them as foundations of mathematical and scientific theories as the standard axiomatic method requires . Lawvere argues that logic in this familiar sense itself needs a grounding in theories of "space and quantity", i.e., in geometrical and arithmetical theories. Thus Lawvere's view of subjective logic, i.e., of logic in the usual sense of the word, reverses the order of ideas assumed in the standard axiomatic architecture as described in [276].

It can be argued that standard logical calculi including CFOL meet this requirement since they involve at the fundamental level some mathematical concepts (which "grow out of the study of space and quantity") such as the concept of function. It can be also argued that the standard axiomatic architecture is compatible with the idea of the ontological grounding of logic, many versions of which are found in the old and the current philosophical literature. Considering Lawvere's Hegelian views of logic in such broader contexts makes perfect sense, but at present we would like to stress that Lawvere's philosophical views of logic are tightly connected with his mathematical contribution to this discipline. Lawvere's claim that (subjective) logic is a "special case" of a more general structure that he calls *objective* logic has a precise mathematical sense. Consider the concept of adjoint functor explained above. It has a fundamental role in category theory and hence — assuming that CT provides a foundation of mathematics — in all of mathematics [159]. This is why the concept of adjoint functor qualifies as an element of *objective* logic in Lawvere's sense. *In particular*, the concept of adjoint functor allows for representation (and arguably also for a deep conceptual analysis) of logical quantifiers, which are elements of usual *subjective* logic. So the (subjective) logical structure of a given theory appears as an integral part of a wider mathematical and conceptual structure; this "subjective" part of the theoretical structure can be abstracted away and studied independently, but it should not be thought of as a self-sustained independent foundation of this structure. In the next Section we shall see how this view on logic and its place in mathematical theories is further developed in Lawvere's axiomatic theory of topos.

3.1.4 Toposes and their Internal Logic

A) <u>Elementary topos</u>

The mathematical notion of *topos* first appeared in the circle of Alexandre Grothendieck around 1960 as a twofold upgrade of the notion of topological space. The first upgrade amounts to considering a given topological space T together with the *sheaves* of functions from open subsets of this space to some target sets; a sheaf respects *gluing conditions*, which allow for seeing the target sets as "momentary images" of the same *continually variable* set varying over T. If the target sets are provided with an extra structure, say, with the group structure, one may similarly think of groups continuously varying over a given topological space. In order to get from the notion of sheaf to that of topos, one needs first to render the former notion in category-theoretic terms. Think of T as a category with objects open subsets of T (*opens* for short) and morphisms set-theoretic inclusions of these subsets, so in the resulting category there is at most one morphism going from one given object to another. (For any pair of opens U, V we either do or do not have $U \subseteq V$; categories with at most one morphism with a fixed domain and a fixed codomain are called partial orders). Then a sheaf can be defined as a functor $T^{op} \to S$ from the category T^{op} obtained from T by the "reversal of arrows" to the category of sets S, which satisfies gluing conditions assuring that the target variable set varies continuously with respect to T . The fact that the arrows must be reversed in this case was difficult to understand without using the category-theoretical notion of functor; this was a major difficulty for earlier attempts to develop a "topology without points" [131]. One gets a basic example of topos by considering the category of all sheafs on a given topological space together with maps between those things; since sheafs are functors the maps are natural transformations.

The second upgrade amounts to a generalization of the usual notion of topology. Given topological space T one may always associate with a given open U its *covering family* C_U, which is a collection of opens V_i such that their union contains U :

$$\bigcup_i V_i \supseteq U$$

, i.e., each point of U belongs to at least one of V_i. In particular, T itself is always covered by at least one collection of its opens. Grothendieck observed that the notion of covering family makes sense not only for partial orders but also for categories of a more general sort, and defined a covering family of a given object to be a collection of incoming morphisms closed under certain operations. This led him to a more general notion of topology called *Grothendieck topology*, defined by distinguishing among all collections of morphisms sharing a codomain those which count as covering families of this given object. A category C provided with a Grothendieck topology J is called a *site* (C, J). A sheaf over a site is defined just as in the case of topological space. The Grothendieck topos is a category of sheaves over some given site. For a systematic introduction see [177, ch. 2-3].

The notion of topos invented by Grothendieck and developed by his collaborators was not originally supposed to have any special relevance to logic; the discovery of such a special relevance is wholly due to Lawvere. In the beginning of his seminal 1970 paper [162] Lawvere provides his definition of topos usually called today the definition of *elementary* topos ; the title "elementary" reflects the fact that Lawvere's definition, unlike Grothendieck's original construction, almost straightforwardly translates into the standard first-order formal language [194]. According to this definition an (elementary) topos T is CCC with a *subobject classifier*, which plays in a general topos the role similar to that played by $\mathbf{2}$ (the two-point set) in $\mathbb{SET}$, which equally qualifies as an elementary topos. $\mathbf{2}$ classifies subsets of a given set S in the sense that if one asks whether a given element $p \in S$ belongs to subset $U \subseteq S$ there are just 2 possible answers: yes and no; this allows for identifying every subset U with a particular function $u : S \to \mathbf{2}$, which sends every p belonging to U to "yes" and every p not belonging to U to "no". Correspondingly the set 2^S of all such functions is identified with the set of all subsets (the powerset) of set S.

Given two objects A, B of CCC the exponential object A^B always exists but in order to get a distinguished object Ω playing the role of "object of truth values", so that for all A Ω^A represents the space of subobjects of A, one needs an additional postulate. By a subobject of A one means here any incoming *monomorphism* f, i.e., such f that for all g, h $f \circ g = f \circ h$ implies $g = h$ (cancelability from the left). Given two subobjects f_1, f_2 of the same object A consider morphism h such that $f_1 = f_2 \circ h$; according to the above definition of subobject there is at most one such morphism. This shows that subobjects of a given object are partially ordered. In $\mathbb{SET}$ the partial order of subobjects is the complete Boolean lattice while in the general case the lattice is Heyting. Evidently Lawvere's earlier work on ETCS helped him to formulate the axioms for elementary topos. It was Lawvere who first thought of sheaves as *continuously variable sets* and observed that the category of such things shares a number of basic properties with the category of usual "static" sets. For a systematic presentation of topos theory from the elementary viewpoint see [194], [132, p. 68-119].

The concept of elementary topos is more general than that of Grothendieck topos : there is a class of elementary toposes, which are not Grothendieck. In particular, the category $FinS$ of (all) finite sets is an elementary topos but not a Grothendieck topos because $FinS$ lacks infinite limits. Another important example of a non-Grothendieck topos is the *effective* topos which can be thought of as a set-theoretic-like universe where all (total) functions from natural numbers to natural numbers are recursive. An exact necessary and sufficient condition under which a given elementary topos is Grothendieck topos was found in 1972 by Grothendieck's student Jean Giraud [86].

Lawvere's axioms for elementary topos helped many people outside the community of experts in Algebraic Geometry enter this field and conduct a fruitful research in it. Everyone who learns the topos theory today begins with Lawvere's axioms for elementary topos. This makes Lawvere's axiomatisation of topos theory a true success story of the axiomatic method in 20th century mathematics. On the one hand, the theory of elementary topos has the same pragmatic advantages as Quillen's "axiomatic" homotopy theory [218] and the "axiomatic" theory of Homological Algebra by Eilenberg and Steenrod [61] :like these theories, it dramatically simplifies and clarifies a difficult mathematical subject, and boosts its further research and development. On the other hand, unlike the aforementioned theories by Quillen, Eilenberg and Steenrod, Lawvere's theory of elementary topos along with ETCS meets the standard of logical rigour that allows one to qualify this theory as axiomatic without reservations (or with only minor reservations that we have made above in the case of ETCS). However, this theory also has another unusual feature, to an analysis of which we now turn.

B) Internal logic of topos; Logic and geometry

Lawvere's 1970 paper "Quantifiers and Sheaves" where the axioms for elementary topos first appear in press, begins as follows:

> "The unity of opposites in the title is essentially that between logic and geometry, and there are compelling reasons for maintaining that

> geometry is the leading aspect. At the same time [...] there are important influences in the other direction: a Grothendieck "topology" appears most naturally as a modal operator, of the nature "it is locally the case that", the usual logical operators, such as $\forall$, $\exists$, $\Rightarrow$ have natural analogues which apply to families of geometrical objects rather than to propositional functions, and an important technique is to lift constructions first understood for "the" category $\underline{S}$ of abstract sets to an arbitrary topos . We first sum up the principle contradictions of the Grothendieck-Giraud-Verdier theory of topos in terms of four or five adjoint functors [...] enabling one to claim that in a sense logic is a special case of geometry." [162, p. 329]

"The unity of opposites in the title" of this paper is that between logic and geometry because the concept of quantifier is logical while that of sheaf is geometrical. Leaving aside the Hegelean notion of unity of opposites we shall describe here the unity of geometry and logic in toposes in mathematical terms. Lawvere's axioms for elementary topos are motivated by his idea to look at toposes as generalised "variable" sets; the resulting axiomatic theory is a generalized version of ETCS that admits other subobject-classifiers than **2**. What was said above in **3.1.2** about the internal logical structure of $\mathbb{SET}$ also applies to the theory of elementary topos. Moreover, in the topos theory the concept of internal logic allows for rigorous formalisation. To this end one associates with a topos a symbolic calculus called the Mitchell-Bénabou language or the *internal language* of the given topos and then provides this calculus with a formal logical semantics (called the Kripke-Joyal semantics) which is fully determined by the structure of that given topos; the internal language so construed is sound and complete with respect to this semantics, see [177, p. 296-318] for details. The construction of internal language implements mathematically Lawvere's notion of objective logic as follows: the (subjective) logical structure of a topos, i.e., its internal language, is determined by its ambient topos structure, which has an objective geometrical (and possibly physical) content. As Lawvere puts it, logic turns out to be a special case of geometry.

If topos theory is built with the standard axiomatic architecture that assumes that the logical part of a given theory is self-sustained and fixed in advance, then the internal logic of a topos appears as an additional structure on top of the given topos. This standard axiomatic approach in Topos theory is used by Colin McLarty in [194]. After introducing categories and toposes in the usual semi-formal way, McLarty comes to the internal logic of toposes [194, ch. 14] and finally in [194, ch. 16] entitled "From the Internal Language to the Topos", shows how a given topos can be re-described *internally* with its own internal language. The internal description of a topos does not, generally, fully coincide with its external description because the external language always has greater expressive power (for otherwise it could not be used for the specification of the internal language), and so certain details discernible "from outside" can be missing in the internal description.

This interesting effect can be compared with a similar effect in Riemanian Geometry, where details of the embedding of a manifold into an outer space are not reflected in intrinsic geometrical descriptions of this manifold. Draw a straight line L on a sheet of paper and then fold the paper. Extrinsically L is no longer straight but intrinsically it has not changed. The distinction between intrinsic and extrinsic geometrical properties was first described in precise mathematical terms by Carl Friedrich Gauss in his 1828 dissertation on curve surfaces [78] (the result is commonly known today under its original Latin name of *Theorema Egregium*). Bernhard Riemann took a further step suggesting that a curve surface or a higher-dimensional manifold determined only in terms of its intrinsic properties can be thought of and studied as a geometrical object in its own right [222]. Today this "intrinsic" approach in geometry is standard; let us mention that it also plays a major role in General Relativity theory, which accounts for physical space-time in intrinsic geometrical terms, which in this case have a physical meaning; see our [224] and [226].

Lawvere's idea, according to which the logical part of Topos theory needs to be grounded by and be an integral part of a geometrical structure (which can be a part of a physical theory [227]), squares with the notion of the internal logic of a topos, but at the same time is at odds with the idea that the Topos theory, as any other axiomatic theory, also needs some sort of underlying base logic in its foundation. In Lawvere's view, as we understand it, the internal logic of topos is the only "true" logic of Topos theory. Bearing in mind the above geometrical analogy, it is suggestive to think of the possibility of specifying a topos by means of its own internal language without using any external logical resources. In a simple form this possibility can be realised as follows. Instead of construing the internal language L_T of topos T as an additional structure on T , one introduces the syntax of L_T independently, provides it first with some preliminary intuitive semantics, and then step-by-step upgrades this semantics to the full-fledged formal topos Kripke-Joyal semantics.

This is how John Bell develops what he calls the *local set theory* (LST) [19], which provides a "view on topos from the inside". From this internal viewpoint a topos appears as a set-like universe where "sets" are "local" in the sense that "some of the set-theoretic operations, e.g. intersection and union, may only be performed on sets of the same type, [... m]oreover, variables are constrained to range only over given types" [19, p.99]. Bell shows that LST-sets and LST-functions form a topos (like classical ZFC-sets) and then, after tackling soundness and completeness issues, proves a fundamental *equivalence theorem*, according to which :
For any topos $\mathbf{E}$ and its internal language L_E the category (topos) $\mathbf{C}(LST_E)$ of LST-sets built with L_E is category equivalent to $\mathbf{E}$, in symbols,

$$\mathbf{E} \simeq \mathbf{C}(LST_E)$$

.

Topos $\mathbf{C}(LST_E)$ is conventionally called *linguistic* (since it is built with an essential use of formal language L_E). Bell's equivalence theorem can be succinctly expressed in words by saying that every topos is equivalent to its linguistic topos [19, p. 105-113].

The equivalence theorem along with the soundness and completeness theorems proved by Bell, according to which truth in the linguistic topos $\mathbf{C}(LST_E)$ corresponds precisely to derivability in LST_E, show that a topos is indeed uniquely (up to category equivalence) determined by LST, based on the internal language of this topos. Does this fact allow one to qualify LST as an axiomatic construction of Topos theory? Topos $\mathbf{C}(LST_E)$ is not one of the local sets treated by LST_E just as category (topos) $\mathbb{SET}$ is not a set with additional structure. From this point of view it is fair to say that LST does <u>not</u> qualify as an axiomatic theory of topos, just as ZFC does not qualify as an axiomatic theory of $\mathbb{SET}$. At the same time, LST provides a precise formal internal description of topos, which shows that

> "Any topos may be regarded as a mathematical domain of discourse or 'world' in which mathematical concepts can be interpreted and mathematical constructions performed." [19, p.238].

So this formal theory certainly provides at least a useful logical perspective on topos. Peter Johnstone in his encyclopaedic work on Topos theory entitled "Sketches of an Elephant" [132] compares a topos with an elephant from a popular Indian tale about three blind men feeling different parts of this animal and giving each other very different reports. Johnstone calls Bell's internal logical perspective on topos "toposes as theories" view on the "elephant". Johnstone provides an accurate systematic presentation of this logical approach, along with and independently from the geometrical approach that he calls "toposes as spaces"; both these approaches are preceded by the "toposes as categories" approach, which remains basic in Johnstone's presentation, at least in the pedagogical sense.

Johnstone's work makes it clear that the reduction of geometrical contents to logical contents in the vein of Hilbert's *Foundations of Geometry* of 1899 [115] does not go through in Topos theory; a major reason for this is that the axiomatic architecture designed by Hilbert in that work does not do justice to the concept of internal logic that arises in Topos theory. This standard axiomatic architecture can be compared with geometry before Lobachevsky and Riemann, that takes Euclidean space for granted as a universal stage where all geometrical reasoning and geometrical construction takes place. Hilbert's axiomatic method allows for designing multiple geometrical stages using a universal "logical stage", i.e., a fixed background logic. If one is a logical pluralist one may apply the same theoretical scheme but use more than one background logic and justify the proliferation of mathematical and non-mathematical theories that may emerge in this way [292]. However, this doesn't help one to make sense of the concept of internal logic in Topos theory, which quite evidently plays a fundamental role in its logical foundations.

Indeed, in spite of the fact that the Mitchell-Bénabou language and the Kripke-Joyal topos semantics are rigorous mathematical constructions, the very concept of internal logic of a given category still needs more conceptual clarity. This clarity can hardly be achieved without a revision of the closely related logical concepts of *semantics* and *model* in the context of categorical logic. Given the internal language L of category C, people often refer to C as an element of the semantics for L (categorical semantics) or as a model of L. This depends on the chosen point of view. In **3.2.5** we consider an interesting suggestion due to Vladimir Voevodsky as to how these fundamental logical concepts can be disentangled in the new context.

Even if Topos theory in its existing form does not provide a clear-cut alternative axiomatic architecture, it effectively demonstrates problems of the standard axiomatic architecture. In the next Section we consider a more recent formal approach in the foundations of mathematics known as the *Univalent Foundations*, which is essentially intrinsic and supports a distinction between logical and geometrical properties at the formal level. This latter approach will give us a clearer picture of alternative axiomatic architecture of theories. Before we introduce the Univalent Foundations, we discuss some relevant logical, mathematical and historical issues.

3.2 Homotopy Type theory and Univalent Foundations[24]

3.2.1 Rules versus Axioms. Hilbert-style and Gentzen-style formal systems

Hilbert and Tarski after him conceive of a theory T as a set of formal sentences with an additional structure induced by the derivability relation, which is satisfied by the class of its intended models and, ideally, not satisfied by any non-intended interpretation. (The latter is a desideratum rather than a definite requirement.) An interpretation of a given sentence s in this context is an assignment of certain semantic values to all <u>non-logical</u> symbols of the base formal language that are found in s. Thus this approach assumes that one distinguishes in advance between logical and non-logical symbols of the given alphabet. This requirement reflects the epistemological assumption according to which logic is epistemologically prior to all theories, which are "based" on this logic. The standard version of axiomatic method described by Tarski in [276]

[24]This Section includes material from [233, Ch. 6-7], [240], [297] and [244].

explicitly requires that one first fixes logical calculus L and then applies it in an axiomatic presentation of some particular non-logical theory T. All existing approaches to the formal representation of scientific theories and the standard axiomatic approach in mathematics use this familiar axiomatic architecture. If L is the CFOL with identity then Suppes talks about a "standard formalization" of the given theory [275, p. 24].

The above formal architecture of theories is not unique, however. Back in 1935, Hilbert's associate Gerhard Gentzen argued that

> "The formalization of logical deduction, especially as it has been developed by Frege, Russell, and Hilbert, is rather far removed from the forms of deduction used in practice in mathematical proofs." [81, p.68]

and proposed an alternative approach to the syntactic presentation of deductive systems, which involved relatively complex systems of rules and didn't use logical tautologies as axioms. In [80], [81] Gentzen builds in this way two formal calculi, known today as Natural Deduction and Sequent Calculus.

Gentzen's remark quoted above constitutes a pragmatic argument but hardly points to a specific epistemological view on logic and axiomatic method. However his further remark that

> "The introductions [i.e. the introduction rules for logical symbols] represent, as it were, the 'definitions' of [semantic values of] the symbol concerned." [81, p.80]

is seen today by some authors as an origin of an alternative non-Tarskian conception of logical semantics, which was developed in an explicit form only in the late 1990s and is known today under the name of *proof-theoretic* semantics (PTS) [257, 209]. We shall say more on PTS in the next Section.

Recall that Hilbert's axiomatic method is based on the idea that a theory conceived of as a potentially infinite set of sentences T can be formally presented via a subset (preferably finite and as short as possible) $A \subseteq T$ of these sentences called axioms, and a set of logical rules that allow one to deduce all other T-sentences from the axioms. It is further assumed that the logical rules are shared by all theories (logical monism) or at least by a large class of theories (logical pluralism). This familiar picture of formal theory invites two independent objections, which are partly motivated by the historical study of axiomatic method presented in chapters **1-2** of the present work.

First, the familiar notion according to which a theory (scientific or mathematical) can be safely identified with a set of sentences (provided that a sentence is understood as alinguistic expression that expresses a logical proposition or judgement) is more problematic that it may appear. As we have seen, Euclid's geometrical theory does not fall under this conception of theory : its first principles are rules rather than sentences and in addition to *theorems*, which are sentences, it comprises *problems*, which are not (**1.1.4** above). To put it in modern terms, Euclid's geometry in its original form is a Gentzen-style theory but not a Hilbert-style theory. Recall that a version of the *non-statement view* of theories has been put forward and defended by Patrick Suppes and other enthusiasts of the semantic approach to the formal representation of scientific theories (**2.3.3** above). Let us point to the fact that the non-statement view has a strong consequence for logic. As we shall see in the next Section Tarski's model-theoretic logical semantics supports the view according to which logical rules are essentially syntactic while logical contents expressed with these rules have a propositional form. An alternative approach in logical semantics, which in our view is more adequate to the task of the formal representation of theories, will also be considered in the next Section.

Second, the assumption according to which all inferences of theorems from axioms should in all cases be *logical* inferences, is equally problematic. This is a strong epistemic normative principle that motivated Hilbert to pursue his projects in axiomatic mathematics. Obviously, this principle has a definite content only when one uses it along with a criterion for distinguishing logical from non-logical inferences, i.e., a criterion of *logicality*. Hilbert did not use any explicit criterion of logicality but took his conception of logic for granted.

Criteria of logicality should not be confused with criteria of the *validity* of a given inference. It is a trivial remark that certain non-logical inferences are valid as, for example, certain of Euclid's geometrical inferences. Once the body temperature of a human individual is higher than 37°C, one concludes that the individual is sick. This inference rule is valid but it is not logical because the domain of its applicability is limited. As we have already mentioned above, the problem of logicality is difficult and cannot be discussed systematically in this work (**1.2.2** above). We use here only a weak necessary criterion of logicality that in the contrapositive form can be formulated as follows: if an inference rule is valid only in a limited theoretical domain and not in any other domain, it does not qualify as a logical rule. Euclid's *Axioms* and *Postulates* are examples of non-logical rules. While the *Postulates* apply to geometrical constructions, Euclid's *Axioms* apply to propositions just like logical rules. However the *Axioms* are not logical because they apply only to propositions about numbers and geometrical magnitudes but not to propositions of other sorts. Various examples of useful non-logical rules of propositional inference are found in today's computer science and information technology: computer systems implementing such rules are known under the name of *expert systems*, which are systems of automated reasoning about certain limited domain such as human health and disease, car traffic, etc. [31].

The claim that every valid inference rule is analysable into a *logical* inference rule and a set of specific assumptions is a strong epistemic thesis. If a necessary and sufficient criterion of logicality is fixed, this principle still allows for two different readings. The *normative* reading is that a system of reasoning qualifies as a theory only if all its inference rules are logical. The *descriptive* reading is that all scientific reasoning is analysable in this way. In an actual situation in which criteria of logicality are debated, the normative thesis is, in our view, too strong. In addition, it is hard to make sense of this thesis unless one accepts logical monism in some form. As for the descriptive thesis, in simple cases it appears plausible. Let $T(p)$ be "the body temperature of person p is higher than 37°C" and $S(p)$ be "person p is sick". Then rule

$$\frac{T(p)}{S(p)} \tag{3}$$

can be replaced by axiom $T(p) \to S(p)$ and an inference relying on the logical rule of *modus ponens* to the same effect:

$$\frac{T(p); T(p) \to S(p)}{S(p)} \tag{4}$$

However there is no theoretical justification for the claim that a similar reduction of non-logical rules to rules that may qualify as logical according to a reasonable logicality criterion is possible in all cases of interest. A similar replacement of any specific inference rule

$$\frac{\Gamma, A}{B} \tag{5}$$

by axiom $A \to B$ used along with *modus ponens*

$$\frac{A; A \to B}{B} \tag{6}$$

that allows for conversion of Gentzen-style formal proofs into Hilbert-style axiomatic proofs is possible for any formal theory T that has the following *Deduction Propertry* (aka Deduction Theorem for T) [140]:

If in T formula B is derivable from formula A in context Γ then the implication $A \to B$ is also derivable in T in the same context. In symbols:

$\Gamma, A \vdash_T B$ entails $\Gamma \vdash_T A \to B$ for all Γ, A and B in T.

The Deduction Theorem holds for CFOL, for Intuitionistic First-Order logic (in both cases for closed formulas), and for some other logical calculi. The Deduction Theorem fails in von-Neumann's Quantum Logic [180]. This latter fact is well-known, and it can give a mistaken impression that the situation in which a formal calculus doesn't have the Deduction Property is somewhat exotic. In fact, when a Gentzen-style calculus is designed to formally represent some specific form of contentful reasoning rather than represent "logic" as a universal form of reasoning, the Deduction Property can hardly be expected. For an example of such a formal system with the intended semantics in Cryptography, which provably does not have the Deduction Property, see [148]. Notice also that the Deduction Property of a given formal calculus T could possibly help to convert non-logical inferences into logical inferences by *modus ponens* only if the implication symbol that T comprises admits of a logical semantics. If the intended semantics of this symbol in T is not logical, but determined in terms of the intended domain of application of T, then there is no reason to think of inferences using *modus ponens* with T-implication as logical, either. It is clear how the Deduction Property helps one to reduce a set of logical rules to the single logical rule of *modus ponens* and thus convert Gentzen-style logical reasoning into Hilbert-style axiomatic reasoning but it is not clear after all whether it can help to convert non-logical inferences to logical ones. This issue requires further study.

Unlike Hilbert, Gentzen never tried to apply his rule-based approach beyond pure mathematics and even beyond arithmetic. Today, Gentzen-style presentation of formal calculi is well known and popular among logicians (particularly proof theorists), but less known in the broader community of researchers working on applications of formal approaches in science. A study of the potential of Gentzen-style formal systems for representing mathematical and scientific theories has so far been a little explored issue in formal epistemology. An important practical advantage of this approach is that rule-based formal systems are more easily and straightforwardly implementable on computers than are axiom-based Hilbert-style formal theories. In order to demonstrate the potential of Gentzen-style formal approach with a concrete example we consider in what follows a particular Gentzen-style calculus, namely, Martin-Löf Type theory (MLTT), the related Homotopy Type theory (HoTT), and the project of Univalent Foundations of mathematics (**3.2.3-5** below).

To conclude this Section, let us fix a terminological issue. Gentzen-style formal systems unlike Hilbert-style systems are rule-based but not axiom-based. Nevertheless we shall call formal theories formalised in Gentzen-style *axiomatic theories*, on equal footing with Hilbert-style theories. We motivate and justify this terminological choice as follows. It is common to describe the theory of Euclid's *Elements* as an axiomatic theory — disregarding the fact that this is a Gentzen-style, i.e., rule-based, theory but not a Hilbert-style axiomatic theory. Given the historical significance of Euclid for Hilbert's axiomatic project it would be odd to call Euclid's *Elements* otherwise. At the same time we remark that Hilbert uses the traditional logical term "axiom" in a very specific way, namely to refer to hypotheses of a certain form. In the history of logic this term has been used in a number of different senses. In particular, Aristotle uses this term to refer to logical *rules* such as the rule of *perfect syllogism*. Calling a Gentzen-style system (such as MLTT, for example) that has no axioms in Hilbert's standard sense of the term by the name of axiomatic theory may appear to be a strange idea, but from a wider historical perspective it appears to do justice to the long-term Euclidean tradition of rigorous mathematical reasoning from first principles. Hilbert's work on the axiomatic approach in mathematics and science, which began in the 19th century, has today also become a part of history. However important this particular episode in the overall history of axiomatic method may be, it should not be seen as the last word on this chapter of intellectual history, nor as a single foundational moment that cancelled the earlier history and triggered a wholly new development. So, understanding the risks of confusion and mitigating these risks by detailed explanations, we opt for using the name of axiomatic theory in a wider sense than is usual. Doing this, we also have in mind that Hilbert himself conceived of such a broader notion of being axiomatic in his joint work with Bernays [113]; see **1.3.2** above. By calling Getzen-style theories *axiomatic* along with Hilbert-style theories we generalise the received notion of formal axiomatic theory but do not thereby make the notion of being axiomatic less formal or less rigourous.

3.2.2 Model-theoretic and Proof-theoretic Logical Semantics. General Proof theory

Hilbert never explicitly elaborated on the concept of logical inference but it is plausible that in his *Foundations of Geometry* [115] he had in view a prototype of the model-theoretic truth-conditional semantical concept of *logical consequence* later formulated by Alfred Tarski [278]. Tarski's concept of logical consequence belongs to the core of what we call in this work the standard (Hilbert-style) axiomatic method. Along with the standard truth-conditional semantics of propositional connectives and the standard explanation of predicates and quantifiers in terms of their extensions the metatheoretical logical consequence relation belongs to the standard formal semantics of CFOL.

Tarski defines logical consequence as follows:

Propositional form B is a logical consequence of propositional forms $A_1, \dots, A_n$ iff every interpretation I of the given language, which makes

$A_1, \ldots, A_n$ into true propositions $A_1^I, \ldots, A_n^I$ makes B into true proposition B^I, in symbols $A_1, \ldots, A_n \models B$.

Notice that this conception of logical consequence does not involve that of rule. On this view, syntactic rules that regulate derivations of th form $A_1, \ldots, A_n \vdash B$ are viewed (granting their soundness with respect to the given semantics) as a mere symbolic representation of the fundamental relation $A_1, \ldots, A_n \models B$. Gödel's First Incompleteness theorem implies that in the case of consistent and sufficiently strong theories such a symbolic representation of logical consequence cannot be faithful, i.e., semantically complete. The consequence relation $A \models B$ is construed according to the above definition as a meta-theoretical sentence that expresses a fact of the matter. The rule

$$\frac{A}{B} \tag{7}$$

on this view is nothing but an element of theoretical syntax (on equal footing with rules for building well-formed formulas from the given alphabet of symbols), allowing one to express the same fact with the language of a given formal theory.

Until recently, such a semantics of syntactic derivations, due to Tarski, was the only available formal logical semantics found in logic textbooks. Since the late 1990s Peter Shröder-Heister and his colleagues have been developing an alternative *proof-theoretic* logical semantics or PTS for short [257]. Since the rise of PTS, the more familiar Tarski-style logical semantics that includes the standard notion of logical consequence is often referred to as *model-theoretic* semantics.

In order to explain the basic idea of PTS and its relevance to the axiomatic method we need to introduce some more context. *Proof theory* in its modern form stems from the aforementioned work by Hilbert and Bernays [113, vol. 2] where a proof is understood as and identified with a syntactic derivation of theorems from axioms. This notion of formal proof still remains standard today in mainstream proof theory, which can be described as a mathematical study of syntactic derivations in various formal calculi of interest including Peano Arithmetic and its modifications. The fact that such formal proofs don't quite resemble mathematical proofs as they appear in everyday mathematical practice is usually explained away by saying that proof theory as a part of mathematical logic and foundations of mathematics studies what mathematical proofs are *in principle*, while details and styles of proofs as they appear in the actual mathematical practice, however interesting and important they may be for historians, sociologists and philosophers of mathematics, and for working mathematicians themselves, have no logical significance.

Since the early 1970s Dag Prawitz has published a number of papers where he criticises the received conception of formal proof stressing the fact that this conception leaves wholly aside the epistemic aspects of proofs, which in his view are essential; in order to distinguish between proof theory in the standard sense and a broader discipline that includes a study of epistemic and practical aspects Prawitz calls the latter the *General* Proof theory [210], [211], [212], [213]. A formal proof (= a syntactic derivation from axioms) according to its default semantics *preserves the truth* of its premises (axioms): a theorem derived from true axioms is also true. Tarski's notion of logical consequence helps one to make this notion of preservation of truth formal and precise. Prawitz agrees that proofs should preserve truth in this sense, but he argues that this condition is not sufficient:

> "[A] valid argument must preserve truth. But the preservance of truth is clearly not a sufficient condition for validity; nobody would consider e.g. Peano's axioms followed by Fermat's last theorem as a proof, even if in fact Fermat's last theorem follows from these axioms. As every examiner stresses, it is not enough that the steps of a proof happen to follow from the preceding ones, it must also be seen that they follow. " [212, p. 26]

One may argue that one's "seeing" that a given theorem logically follows from the axioms of the corresponding theory is a merely psychological and pedagogical matter that has no properly *logical* relevance and significance. However, this commits one to a very narrow conception of logic that is hardly tenable. Indeed, what Prawitz in the above quote calls "seeing" has not only a cognitive and psychological but also (and primarily) an *epistemic* content: he stresses the fact that anything that counts as a proof functions as a piece of evidence that supports a certain sentence, namely, the one that it is a proof of. Let PA be the Peano axioms, FLT be Fermat's last theorem and $PA \models FLT$

be the meta-theoretical sentence saying that FLT is a logical consequence of PA. At the time of this writing FLT is known to be true (thanks to Andrew Wiles proof), while $PA \models FLT$ remains an open conjecture. Consider a hypothetical situation in which $PA \models FLT$ is proven by proof P. Assuming PA and using the same P, we get a new proof of FLT. However the sentence $PA \models FLT$, whether true or false, cannot possibly qualify by itself as a proof of FLT.

Remarkably, this Prawitz's view on proofs is in accord with Wittgenstein's late view according to which "a mathematical proof must be surveyable" [302, p.75], see [197] for an analysis and discussion.

One who wants to argue that the issue stressed here is out of the scope of logic, and insists that logic deals with truth and falsity but not with what one may *know* or not know about the truth and falsity of certain propositions, needs either to accept that such epistemically-laden concepts as *evidence*, *justification* and *proof* do not belong to logic, or somehow strip away from these concepts their epistemic contents. While saying that the concept of proof does not belong to logic is a sheer absurdity, the standard notion of a formal proof as an idealised syntactic object that represents a derivation of proven formula may look respectable even if its epistemic content, if any, is not specified. Admittedly, a hypothetical syntactic derivation of FLT from PA would comprise more than one step in any reasonable formal proof system. Since formal proofs typically involve the application of syntactic rules such as *modus ponens* that have a standard logical interpretation, it may be argued that formal proofs in fact do have the wanted evidential force and do prove the corresponding sentences in a strong epistemic sense of "prove". However since one agrees that the concept of proof is both logical and essentially epistemic, one wants to be more specific and describe the epistemically-laden proof-theoretic semantics of syntactic proof-objects and syntactic proof-procedures more carefully. This is the principle subject matter of PTS. As Thomas Piecha and Peter Shröder-Heister formulate it, "in contrast to a truth-conditional meaning theory, [in PTS] one should explain the meaning of a sentence in terms of what it is to *know* that the sentence is true, which in mathematics amounts to having a proof of the sentence." [209, p.5-6].

The idea of PTS is partly motivated by a broad philosophical view on meaning (and hence on semantics), which is conventionally called "meaning-as-use". This view on meaning goes back to Wittgenstein and more recently has been defended and further systematically developed by Robert Brandom [33] under the name of *inferentialism*. Since PTS is a *formal* semantical theory, the reference to "use" amounts here to referring to syntactic *rules*, which specify the use of symbols and symbolic expressions in formal calculi. Gentzen's original insight according to which rules of inference provide the semantic value of symbolic expressions is preserved in all the various existing versions of PTS.

For a review of the current state of affairs in PTS see [257], [209], [72] and the literature therein. In this work, we consider more specifically only one family of formal calculi with a PTS-style semantics, namely Martin-Löf Type theory (MLTT) and MLTT-based Homotopy Type theory (HoTT).

Goran Sundholm, in his recent paper [272], traces the historical roots of what he calls the "neglect of epistemic considerations in logic" — meaning by logic mainstream logical research in the 20-th century. We provide here one more historical piece of evidence that shows that epistemic considerations have not only been systematically neglected in mainstream 20th-century logic, but also expected by some authors who tried to apply contemporary logic in science.

In their general introduction to logic, written for scientists and published in 1934 [46], Morris Cohen and Ernest Nagel argue that the most distinctive and valuable feature of science is its method which they identify with logic:

> "[T]he constant and universal feature of science is its general method, which consists in the persisting search for truth, constantly asking: Is it so? To what extent is it so? Why is it so? [...] And this can be seen on reflection to be the demand for the best available evidence, the determination of which we call logic. Scientific method is thus the persistent application of logic as the common feature of all reasoned knowledge." [46, p. 192].

In a similar vein the authors make explicit their general conception of logic as a "study of what constitutes a proof, that is, complete or conclusive evidence" [46, p. 5]. After describing their philosophical understanding of logic and its role in science the authors introduce some elements of the contemporary symbolic logic and the Hilbert-style axiomatic method. Even though the authors are enthusiastic about possible applications of this logical machinery in science, they notice that it does not meet their expectations. This sort of logical machinery certainly does **not** help one to handle and evaluate evidence (including empirical evidence). The authors go as far as to suggest that Hilbert's conception of formal proof is unwarranted, and that a better name for Hilbert's formal "proof" is "deduction" [46, p. 7]. The contemporary symbolic logic exposed in this book is anything but the "study of what constitutes a proof", in the intended sense of "proof" as conclusive evidence.

Surely, the "neglect of epistemic considerations in logic" of the 20th century has never been total. Epistemic considerations played a particularly important role in logical developments motivated by constructive and intuitionistic approaches. MLTT emerged in the1970s against this intellectual background, and was called by its author the '*Intuitionistic* Type theory' [189], [186]. As Per Martin-Löf stresses, his study and work at Moscow State University under the supervision of Andrey Nikolaevitch Kolmogorov in the 1960s, and more specifically Kolmogorov's logical ideas [143] where an important part of the motivation behind MLTT (private communication). Nowadays, the recognition of the epistemic dimension of logic is rapidly growing, as evidenced, in particular by the recently published comprehensive monograph on Justification Logic by Sergei Artemov and Melwin Fitting [9].

3.2.3 MLTT and its Proof-theoretic Semantics

In this Section we explain some basic concepts of MLTT and highlight its special features, which are important for our general theme. For a systematic presentation of MLTT in its original form see [186] and for a modern introduction (which involves the homotopical interpretation absent in the original version) see [95].

MLTT is a Gentzen-style typed calculus that comprises no axiom. Its distinctive feature is the presence of *dependent* types, which are types of the form $B(a) : TYPE$ where $A : TYPE$ and $a : A$; in words: types dependent on base type A is a family of types indexed by terms of the base type. The concept of a dependent type has a pragmatic motivation in computer science and practical programming: think of lists of variable length; this structure can be described in type-theoretic terms as a family of types that represent the lists, which depend on the base type of natural numbers.

The meaning of the syntactic rules and other syntactic elements of MLTT is provided via a special semantic procedure that Martin-Löf calls the *meaning explanation*. In [185] the author compares the meaning explanation with a program compiler, which translates a computer program written in a higher-level programming language into a lower-level command language. According to Martin-Löf a similar appropriate translation of MLTT syntactic rules into elementary logical steps gives these rules their meaning and simultaneously validates them [188]. As we shall now see Martin-Löf's meaning explanation qualifies as an informal version of PTS.

Basic syntactic expressions in MLTT are called *judgements*. This term is widely used today in a technical sense, but unlike some other technical logical and mathematical terms it is not arbitrarily chosen. Behind MLTT Martin-Löf has a systematic philosophical conception of logic that does full justice to the epistemic aspect of logic [187], [188]. According to Martin-Löf, the notion of judgement is logically fundamental, while the notion of proposition is derived and obtained via an analysis of judgement into a proposition and its proof (or proofs).

MLTT comprises the following four basic forms of judgements:

(i) $A : TYPE$;

(ii) $A \equiv_{TYPE} B$;

(iii) $a : A$;

(iv) $a \equiv_A a'$

In words (i) says that A is a type, (ii) that types A and B are the same, (iii) that a is a term of type A and (iv) that a and a' are the same term of type A.

The first 1971 version of Martin-Löf's theory allowed types to be terms of themselves and for this reason did not include the "type of types" TYPE aka type universe aka big type. However in his doctoral thesis published in 1972 Jean-Yves Girard discovered a paradox known today by his name [84], which showed that the presence of judgements of the form $a : a$ makes Martin-Löf's theory inconsistent (just as sets that are members of themselves lead to Russell's paradox). Hence the presence of the judgement forms (i),(ii) in the standard 1984 version of the MLTT. Meaning explanations for (i),(ii) are the same as for (iii), (iv) except that (i),(ii) are placed at the next higher level of the type-theoretic hierarchy. Let us now provide some explanations of (iii) and (iv).

Martin-Löf offers four different readings of (iii) [186, p. 5]:

1. a is an element of set A
2. a is a proof (witness, evidence) of proposition A
3. a is a method of fulfilling (realising) the intention (expectation) A
4. a is a method of solving the problem (doing the task) A

(1) expresses the common idea of extensional thinking about types as collections of their terms; "sets" referred to in this meaning explanation should not be confused with sets of ZFC, which is a type-free theory. (2) expresses the "propositions-as-types paradigm" (as it is called in CS) related to the Curry-Howard correspondence (see **3.1.3** above).

According to this interpretation a proposition is true when it has a proof, i.e., when type A is not empty. This constructive conception of truth is a part of PTS semantics and has an obvious epistemic content (since the relevant concept of proof is epistemic). It should be mentioned, however, that it also admits a realistic interpretation in which proofs are understood as truth-makers [271]. Think, for example, about the event of Socrates's death as a truth-maker of and also a conclusive piece of evidence for the claim that Socrates is mortal.

(3) is Martin-Löf's analysis of judgement in terms of Husserl's Phenomenology and (4) refers to Kolmogorov's *Calculus of Problems* [143] and the BHK semantics of the Intuitionistic propositional calculus.

Martin-Löf argues that the above interpretations of judgement form (iii) not only share a logical form but also are closely conceptually related; in particular the author argues that in the last analysis the concepts of set and proposition are the same:

> "If we take seriously the idea that a proposition is defined by lying down how its canonical proofs are formed [...] and accept that a set is defined by prescribing how its canonical elements are formed, then it is clear that it would only lead to an unnecessary duplication to keep the notions of

proposition and set [...] apart. Instead we simply identify them, that is, treat them as one and the same notion." [186, p. 13]

We shall shortly see how the homotopical interpretation of MLTT modifies, clarifies and systematises the mixture of ideas that form the intended semantics of MLTT in its original 1984 version[25].

We now turn to the meaning explanation of the judgement form (iv). Judgement $a \ \equiv_A \ a'$ asserts that terms a, a' of the same type A are equal. Notice that terms of different types in MLTT are not compared. This is a major difference between MLTT-types interpreted as sets and ZFC-sets: while in ZFC sets can share some elements, this is not the case in MLTT. In some contexts this feature appears to be counter-intuitive, but at the same time it provides desired limitations that allow one to rule out as absurd questions like "Is number pi equal to the Euclidean space?" on formal grounds. Assuming that these mathematical objects are represented by certain ZFC-sets one needs also assume that the above question is meaningful and has a definite answer.

The equality relation $\equiv_A$ that appears in (iv) is called in MLTT *judgemental* or *definitional* equality. Notice that $a \equiv_A a'$ is a (form of) judgement but not a proposition. However the concept of *propositional* equality $a \ =_A \ a'$ is also present in MLTT; it qualifies as a type on its own ($a \ =_A \ a' : TYPE$), which is called an *identity type*. In accordance with reading (2) of the judgement form (iii), a term of identity type is a proof of this proposition. MLTT validates the following rule, according to which a judgemental equality entails the corresponding propositional equality:

$$\frac{a \ \equiv_A \ a'}{refl_a : a \ =_A \ a'} \tag{8}$$

where $refl_x$ is the canonical proof of proposition $a \ =_A \ a'$ called the *reflection* of term a.

The difference between judgemental and propositional equality is reflected in many popular programming languages in which the two relations are expressed by different symbols, and in this form is familiar to anyone with minimal

[25]For a different extension of MLTT intended semantics see [242]

experience in programming. In programming, the judgemental aka definitional equality $A \equiv B$ is usually interpreted as an assignment to symbol A of a value B determined in advance; think of A as a newly defined term (definiendum) and B as its definition (more precisely, its definiens). Clearly, such an equality does not require a proof: it holds by fiat or by definition. By contrast, the propositional equality $A = B$ is, generally, a non-trivial equality that, if it holds, requires a proof. However, for reasons of uniformity, the trivial case in which $A = B$ holds on the ground that $A \equiv B$ also needs to be covered. This explains the above formal rule.

In 1984 Robert Seely showed that MLTT is the internal language of *locally* Cartesian closed categories (LCCC) [259]. In the Type-theoretic community the same result is usually described by saying that LCCC is a model of MLTT[26].

Remarkably, LCCC (taken along with its base category) is also a canonical example of a hyperdoctrine in Lawvere's sense (see **3.1.3 D** above). Thus, Seely's result made explicit the fact that Martin-Löf and Lawvere, working with different formal techniques and different philosophical motivations (that were strong in both cases) discovered the same fundamental logical structure.

LCCC verifies an additional rule called the *Reflection Principle* (RP), which does not constitute part of the core of MLTT:

$$\frac{p : a \ =_A \ a'}{a \ \equiv_A \ a'}$$

RP can be expressed in words by saying that every equality (identity) reduces to equality by definition. Before 1993, when Thomas Streicher published his

[26]Category $\mathbb{C}$ is called locally Cartesian closed if for every object $A \in Ob(\mathbb{C})$ the slice-category $\mathbb{C}/A$ is CCC. $\mathbb{C}/A$-objects are $\mathbb{C}$-arrows of form $C \to A$ (with fixed base object A) and $\mathbb{C}/A$-morphisms are $\mathbb{C}$-morphisms of form $C \to D$ such that triangles

$$\begin{tikzcd} C \arrow[rr] \arrow[dr] & & D \arrow[dl] \\ & A & \end{tikzcd}$$

commute. Composition of morphisms in $\mathbb{C}/A$ is the same as in $\mathbb{C}$ (the closure of $\mathbb{C}/A$-morphisms under the composition in $\mathbb{C}$ is easily checkable).

habilitation thesis [269], all known models of MLTT had the reflection property. It was conjectured that PR, or at least the weaker property that if a propositional equality has a proof then this proof is judgementally unique (abbreviated as UIP for the uniqueness of identity proves), might be a theorem provable in MLTT. Thomas Streicher and Martin Hoffman refuted these conjectures by providing a model of MLTT in which RP does not hold [122], [123]. The main idea of Hoffman and Streicher was to represent types (more precisely, judgements of form $A : TYPE$) as *groupoids* and terms $a : A$ as elements of these groupoids. The identity type $a \quad =_A \quad a'$ is represented in this model by the *arrow groupoid* of groupoid A, which is a functor category of the form $[I, A]$ where I is the connected groupoid having exactly two non-identical objects and a single non-identity isomorphism between these objects (the "generic path"). Clearly this model allows groupoid $a \quad =_A \quad a'$ to be non-empty when terms a and a' are not definitionally equal [27].

Since it has been established that RP is not a theorem of MLTT the version of MLTT that does <u>not</u> comprise this rule is conventionally referred to as the *intensional* MLTT, while the version that comprises this additional principle is called *extensional.* The motivation for using these traditional logical terms in this specific context can be explained by appeal to Frege's popular *Venus* example [73], [74].

Frege considers three different names — *Venus*, *Morning Star* and *Evening Star* — which all refer to the same planet. Frege wonders how it is possible that while the identity statement

[27]The algebraic concept (structure) of groupoid is similar to that of group (see **2.2.1-2** above); the key difference is that unlike the group operation the groupoid operation is partial: for some pairs of elements the result of the operation is determined while for some other pairs of elements it is not. A groupoid can be alternatively described as a category G such that all its morphisms are invertible, i.e., are isomorphisms. By collapsing all objects of G into one object one gets a group. This remark shows that the groupoid structure is, generally, richer than the group structure. As we shall explain in the next Section the Hofmann&Streicher groupoid interpretation of MLTT is a step toward the homotopical interpretation of MLTT discovered independently by Steven Awodey and Vladimir Voevodsky in the mid-2000s; in order to proceed from the former interpretation to the later one should think of Hofmann&Streicher's groupoids as groupoids of *paths* between points of a topological space.

$$\textit{Morning Star} \text{ is } \textit{Morning Star} \tag{9}$$

and the identity statement,

$$\textit{Morning Star} \text{ is } \textit{Venus} \tag{10}$$

which expresses a mere linguistic convention that "Venus" is an alternative name to *Morning Star*, are trivial, the statement

$$\textit{Evening Star} \text{ is } \textit{Morning Star} \tag{11}$$

is a non-obvious astronomical fact that needs an accurate justification, which involves both a solid theoretical background and appropriate observational data. Where does the difference between informative and non-informative identity statements come from? Frege does not provide a full answer to this question but does provide a theoretical framework for answering it. To this end he distinguishes between the *sense* (aka meaning) and the *reference* of any given linguistic expression.

At first glance, Frege's example supports the Reflection Principle: however the identity of Morning Star (MS) and Evening Star (ES) is discovered and justified, MS and ES are merely two different names of the same entity. So it is plausible that the propositional identity $e : MS = ES$ (where e is the evidence that justifies this proposition) should entail the definitional identity $MS \equiv ES$. This point of view is called *extensional* in the sense that different senses (meanings) of the expressions "Morning Star" and "Evening Star" are not taken into account. Only the referent of these two names matters — and the fact that in both cases the referent is the same. So the linguistic expressions 'Morning Star" and "Evening Star" are analysed from this point of view as bare names, on equal footing with the name "Venus", which also refers to the same entity. The linguistic choice of each of these three names has certain historical (or mythological as in case of the name "Venus") motivations but this issue is not a part of logic.

Notice that the above (extensional) point of view assumes that our theory of *Venus* represents facts about *Venus* but leaves it aside how these facts are *known* and, in particular, how known facts about *Venus* are justified. Arguably, this is not what one wants to call a theory. Indeed, scientific and mathematical theories do not only tell us how things are but also support such claims with proofs, evidence and perhaps justifications of other sorts. The presence of such justifications is an essential feature of science. So it is reasonable to conceive of a theory where, using a mathematical metaphor, the identity of the Morning Star and the Evening Star is not an axiom nor a definition but a theorem. The proof of this "theorem" should not necessarily be thought of as a derivation from axioms even if it may have a deductive structure. For the moment let us think of this proof as evidence e that combines theoretical and empirical (observational) elements. In the next Section, we show how it can be construed in terms of HoTT.

So one can conceive of a theory that comprises two different different definitions (descriptions) of MS and ES and the evidenced statement (judgement $e : MS = ES$) that MS and ES are the same planet. This theory violates RP: MS and ES are provably the same but not definitionally/judgementally the same. This theory takes it for granted (i.e., assumes as already known) what the Morning Star is and what the Evening Star is; then it establishes using evidence e that MS and ES are the same planet. The difference between what is known in advance and what is established within the theory matters in this case. In such a situation, it matters what MS and ES *mean*, not only what these expressions refer to. Since MS and ES are the same entity, every property P of MS is also a property of ES and vice versa. However John may happen to *know* that MS has certain property P (for example that it is observable during the morning hours) but not know that ES has the same property. Such contexts in which the meaning (sense) of linguistic expressions, as distinguished from their reference, matters are conventionally called *intensional contexts*.

Thus Frege's *Venus* example helps to explain the terminology used for distinguishing between two versions of MLTT. It should be stressed, however, that using traditional logical terms with a long history and a heavy philosophical load (that does not remain quite stable through the history of logic) in specific technical contexts like that of MLTT creates a risk of confusion. As we shall see in **3.2.5 D**, the Univalence Axiom used along with intensional MLTT (without RP) entails *functional extensionality*, and on this ground can be seen as an extensionality principle (in a different sense). Generally, there is more than one sense in which a logical calculus can be said to be extensional or intensional; it is important to carefully disentangle these different senses.

3.2.4 From MLTT to HoTT

Here we provide a very informal exposition of the mathematical background involved to the homotopical interpretation of MLTT, and provide references to literature in which the same topics are treated systematically.

A) Higher categories

Given abstract category $\mathbb{C}$ and a pair of its objects A, B consider the class $Hom(A, B)$ of morphisms $f, g, ...$ of the form $A \to B$. We turn $Hom(A, B)$ into a new category, formally introducing morphisms of the form $\alpha : f \to g$, that is, morphisms between morphisms of $\mathbb{C}$:

$$\begin{tikzcd} A \arrow[r, bend left, "f"] \arrow[r, bend right, "g"'] \arrow[r, phantom, "\Downarrow"] & B \end{tikzcd}$$

Notice that the new morphisms can be composed in two different ways:

horizontal composition:

$$\begin{tikzcd} A \arrow[r, bend left, "f"] \arrow[r, bend right, "g"'] \arrow[r, phantom, "\Downarrow"] & B \arrow[r, bend left, "h"] \arrow[r, bend right, "i"'] \arrow[r, phantom, "\Downarrow"] & C \longrightarrow A \arrow[r, bend left, "k"] \arrow[r, bend right, "l"'] \arrow[r, phantom, "\Downarrow"] & C \end{tikzcd}$$

where $k = fh$ and $l = gi$

$$\begin{tikzcd} A \arrow[r, bend left, "f"] \arrow[r, bend right, "g"'] \arrow[r, phantom, "\Downarrow"] & B \end{tikzcd}$$

$$\begin{tikzcd} A \arrow[r, bend left, "g"] \arrow[r, bend right, "h"'] \arrow[r, phantom, "\Downarrow"] & B \end{tikzcd}$$

$\downarrow$

vertical composition:

$$\begin{tikzcd} A \arrow[r, bend left, "f"] \arrow[r, bend right, "h"'] \arrow[r, phantom, "\Downarrow"] & B \end{tikzcd}$$

Requiring appropriate equational conditions called *coherence laws*, which are expressed in the form of commutativity of certain diagrams and guarantee that compositions of morphisms at both levels are properly coordinated, one obtains a *2-category* that comprises:

- objects $A, B, \ldots$ (as in $\mathbb{C}$);
- morphisms $f, g, ..$ between objects (as in $\mathbb{C}$) called in this context *1-morphisms* and the operation composition for 1-morphisms (as in $\mathbb{C}$);
- *2-morphisms* $\alpha, \beta, ..$ and two operations of composition for 2-morphisms.

An example of a 2-category which has been around from the very beginning of category theory (though it was not called by this name) is the 2-category $\nvDash - \mathbb{CAT}$ that has all small categories as objects, functors between these categories as 1-morphisms and natural transformations between the functors as 2-morphisms [176].

The "rising of dimension" by introducing higher-level morphisms can be continued, bringing about various concepts of n-category and ω-category [225]. This construction involves a number of different possible technical and conceptual choices, which lead to a proliferation of n-category concept. Such a proliferation concerns, primarily, the general concept of *weak* (as distinguished to *strict*) n-category, which we explain below with the example of the path groupoid (**3.2.4 C**). In his 2002 paper [168], Thomas Leinster overviews and compares ten different concepts of weak n-category. During the following decade, the application of homotopical approaches in this area of mathematical research allowed for a considerable stabilisation of the basic concepts of higher category theory [265].

B) Paths, Homotopies and Higher Homotopies

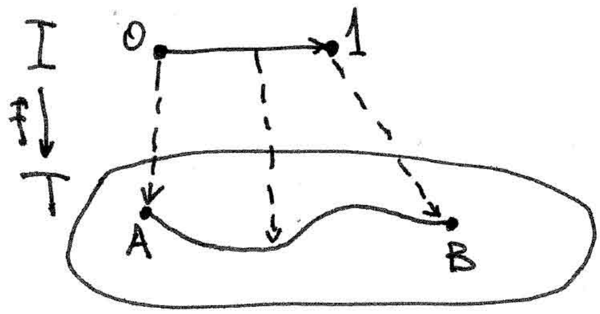

Fig. 3: Path in a topological space

The relevant concept of path is defined as a continuous map $p : I \to S$ from some distinguished *unit space* I into base space S. Space S can be thought of as a topological space; but in fact for our purposes we need only a weaker notion of *homotopy space* [218]. In applications the unit space I is usually identified with the real interval [0,1], which can be thought of as a time interval; then path p can be thought of as a continuous motion of a test point that begins at point $A = p(0)$ and ends at point $B = p(1)$ (Fig.3).

Beware that the same curve with endpoints A, B may represent different paths p, q: this happens when p, q have the same image. Such paths can be thought of as being traced by a particle which moves between the same endpoints A, B along the "same route" during the same time interval [0,1] and differ only in the character of their motion. So what in homotopy theory is called a "path" is rather a motion, which is equipped with some notion of "absolute time", that allows one to localise a given moving particle at any given moment of time $t \in [0, 1]$ at a certain point P of the base space S.

Let A, B be two different points of space S. Whether or not there is a path between points A, B depends on a topological property of S called *path-connectedness*. Space S is called *connected* when, informally, it consists of a single "chunk". Space S is called *path-connected* if any two points of the space are connected by a path. Path-connectedness implies connectedness but not vice versa. Thus by distinguishing between those pairs of points of a space that are connected by a path and those pairs of points that are not connected one gets a very rough picture of the topological properties of that space.

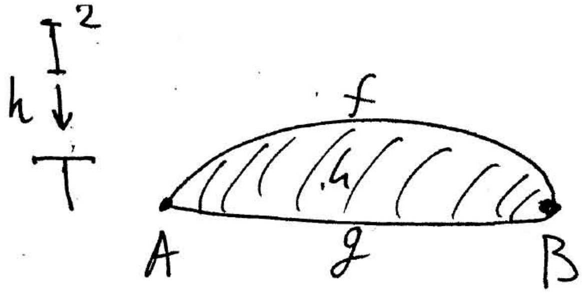

Fig. 4: path homotopy

A *homotopy* is a continuous map that transforms a path into another path with the same endpoints. Formally, a homotopy is a continuous map of form $h : I^2 \to S$. If $I = [0,1]$ then $[0,1]^2$ is the real square and $h(t,0) = p(t)$, $h(t,1) = q(t)$ where p, q are paths that share the endpoints: $h(0,r) = p(0) = q(0)$, $h(1,r) = p(1) = q(1)$, for all values of parameters t, r from [0,1]. So a homotopy can be thought of as a "path between paths" or as a "path of the second order". Correspondingly, a path can be described as a "zero-order homotopy". A homotopy can be pictured as a two-dimensional *surface* (cell) delimited by a pair of curves (Fig.4):

If there exists a homotopy between paths p, q that leaves the shared endpoints of these paths fixed then such paths are called *path-homotopic* or simply homotopic (unless there is a risk of confusion of path homotopies with more general homotopies that are not subject to the above condition). Like the term "isomorphism", the term "homotopy" is sometimes used in the sense of a map and sometimes in the sense of the relation of being homotopic. Since for our purposes it is essential to distinguish between different homotopies (as maps), we use the term "homotopy" in the sense of a map.

Whether two given paths between fixed points are homotopic or not depends on the topology of the base space. Fig.5 represents two non-homotopical paths between points O, S (as an effect of gravitational lensing produced by an imaginary hole in the spacetime). The homotopy between the paths does not exist because the space has a hole that does not allow one path to be continuously transformed into the other.

Thus the structure of homotopies between paths between points of a space provides more information about the topology of this space.

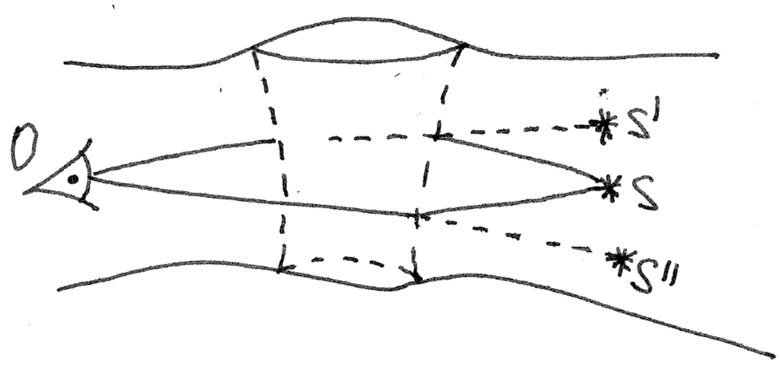

Fig. 5: non-homotopic paths

This ladder is continued by considering 2-homotopies and n-homotopies for all natural n. An n-homotopy is a continuous map of the form $h^n : I^{n+1} \to S$. Intuitively, an n-homotopy can be thought of as a continuous transformation of one $n-1$-homotopy into another. The higher homotopic structure of a space comprises more topological information about this space. For a standard introduction to homotopy theory, see [91].

C) Fundamental Groupoids

Now we shall build some categories from paths and homotopies. Consider a tentative category P_S with objects $A, B, ...$ points of space S and morphisms paths $p, q, \ldots$ between these points. We assume that for every path $p : [0,1] \to S$, $f(0) = A$ and $f(1) = B$, there exists the inverse path $g : [0,1] \to S$, $g(0) = B$ and $g(1) = A$. This assumption reflects the common idea that any motion or its record can be "played backward". (More general spaces that do not satisfy this condition are known as *directed spaces*, see **3.2.5 D** below). Thus so P_S is a groupoid.

The composition of paths may appear unproblematic: given path p from A to B and another path q from B to C, one may easily imagine a composite path qp from A to C, where B is a midpoint. However by composing $p : [0,1] \to S$ with $q : [0,1] \to S$ we get a map of the form $[0,2] \to T$, which is not a path in the sense of our definition. In order to get the composed path $r = qp$ of the same form $h : [0,1] \to T$, we should move the test point faster (Fig.6).

This intuitive idea can be realised as follows. First, we introduce a real variable $t \in [0,1]$ called in this context a *parameter*, so that for every value of t $p(t)$ represents a precise position (i.e. a point) on path p. Path q is treated

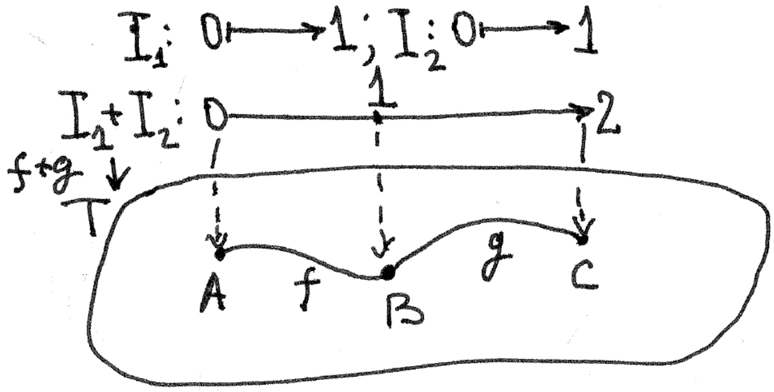

Fig. 6: composition of two paths

similarly. In order to compose p and q one needs to choose a different speed. For this purpose we introduce a new parameter $t' = \frac{t}{2}$ (so the time is halved and hence the speed gets doubled) and define the composition $r = qp$ as:

$$r(t') = \begin{cases} p(2t'), 0 \leq t' \leq \frac{1}{2} \\ q(2t'-1), \frac{1}{2} < t' \leq 1 \end{cases}$$

so t' now ranges from 0 to 1 as required. This trick is called a *reparameterisation.*

The problem is that there are many different reparameterisations, that can be used for this purpose, and which produce different results. So no particular reparameterisation gives us a *general* definition of path composition. The unit interval in $r : [0,1] \to S$ can be cut not only into two equal halves but also in any other proportion. Moreover, the reparameterisation need not be linear: the speed of motion along either of the two composed paths p, q needs not to be constant. Generally, reparameterisation amounts to choosing a particular continuous "scaling map" $s : [0,1] \to [0,2]$. Each particular choice of s brings about a new definition of path composition (Fig.7).

Suppose now that some particular map $s : [0,1] \to [0,2]$ is chosen and the composition of paths $r = qp$ is defined as above. It remains to check whether points and paths composed according this rule indeed form a category. The identity morphism of a given point A is defined as the map sending each point of [0,1] to A and it is straightforward to check that it behaves as expected. A simple check reveals a further problem, however: the composition of paths so defined is <u>not</u> associative. So P defined as above is not a category!

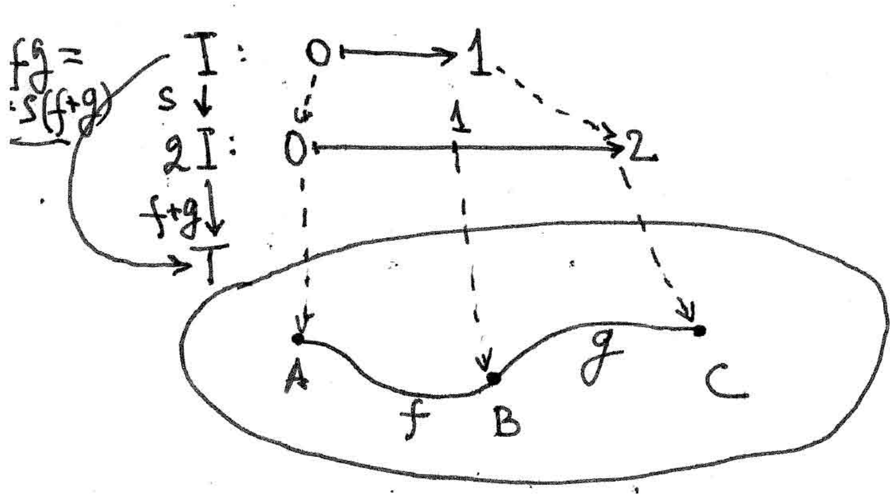

Fig. 7: reparameterization

In order to fix the associativity of path composition, it is sufficient to redefine our category P by taking its morphisms to be equivalence classes of *homotopic* paths rather than the paths themselves. Indeed, however the scale function s is chosen, $r = qp$ *up to homotopy*. Thus we get a well-defined category P_1 with objects points of S and morphisms equivalence classes of homotopic paths between these points. This category is called the *fundamental groupoid* of space S.

Notice that P_1 distinguishes between paths up to homotopy but does not distinguish between different homotopies between a fixed pair of homotopic paths. A higher-order construction that distinguishes between such homotopies (up to 2-homotopies) is called the fundamental 2-groupoid P_2 of space S with objects points, 1-morphisms paths, and 2-morphisms equivalence classes of 2-homotopic 1-homotopies. Since the composition of 1-morphisms in this case is not unique and not strictly associative, this 2-category is called *weak*.

In this way one associates with the given space S a ladder of its fundamental groupoids P_n that extends to the infinity groupoid P_ω. This latter groupoid was first described by Alexander Grothendieck in a private letter [94] dated by 19.02.1983 and then used by other authors as a basic motivating example of weak n-category [169], [172]. Since weak categories are central in Higher Category theory, in this theory categories are often assumed to be weak by default while the case of strict categories is specially distinguished.

D) Homotopy Type theory

Let us return to MLTT. Let p, q be two judgmentally different proofs of the proposition saying that two terms of a given type are equal:

$$p, q : P =_T Q$$

It may be the case that p, q, in their turn, are propositionally equal, and that there are two judgmentally different proofs p', q' of this fact:

$$p', q' : p =_{P=_T Q} q$$

This and similar multi-layer syntactic constructions in MLTT can be continued unlimitedly. Before the rise of HoTT it was not clear that this syntactic feature of the intensional MLTT can be significant from a semantic point of view. However it became the key point of the homotopical interpretation of this syntax. Under this interpretation

- types and their terms are interpreted, correspondingly, as spaces and their points;
- identity proofs of form $p, q : P =_T Q$ are interpreted as paths between points P, Q of space T;
- identity proofs of the second level of form $p', q' : p =_{P=_T Q} q$ are interpreted as homotopies between paths p, q;
- all higher identity proofs are interpreted as higher homotopies;

(which is coherent since a path $p : P =_T Q$ counts as a point of the corresponding path space $P =_T Q$, homotopies of all levels are treated similarly).

Thus the homotopical interpretation makes the complex structure of identity types in the intensional MLTT meaningful. The homotopical interpretation extends to all of MLTT including the key concept of type dependence that is interpreted as *fibration* in the sense of homotopy theory [95]. In view of the homotopy interpretation of MLTT outlined above, spaces (understood up to the homotopy equivalence [28]) are also called "homotopy types". A space that

[28]Topological spaces X, Y are called homotopy equivalent if there exists a pair of continuous maps $A \underset{g}{\overset{f}{\rightleftarrows}} B$ such that composition gf is homotopic to the identity map 1_A and composition fg is homotopic to the identity map 1_B. The relation of being homotopy equivalent should not be confused with that of being homotopic.

is homotopy equivalent to a point is called *contractible*. Space S is contractible when it has a point $p : S$ connected by a path with all other points $x : S$ in such a way that all these paths are homotopic. In what follows we refer to contractible spaces "as if they were effectively contracted" and identify such spaces with points.

As we have already seen, the ladder of fundamental n-groupoids provides more information about the corresponding base space, which, generally, increments with the rise of dimension n. Now we slightly change this point of view as follows: we identify spaces with their fundamental ω-groupoids and observe that for certain classes of spaces the rise of dimension after some n does not bring anything new. For example, when the topology of the base space is discrete, i.e., the given space is a set of disconnected points, then all its fundamental groupoids P_k beginning with P_1 are trivial. Similarly, one may think about a space with a non-trivial fundamental 1-groupoid but such that all its k-groupoids with $k > 1$ are trivial. This motivates the following recursive definition:

Definition: S is a space of h-level $n+1$ if for all its points x, y path spaces $x \ =_S \ y$ are of h-level n.

where h-level is read as as the homotopy level. By setting the h-level of point (= contractible space) equal (-2) we obtain the following stratification of spaces aka homotopy types :

- h-level (-2): single point pt;
- h-level (-1): the empty space $\emptyset$ and the point pt: truth-values aka (mere) propositions
- h-level 0: sets (discrete point spaces)
- h-level 1: flat path groupoids : no non-contractibe surfaces
- h-level 2: 2-groupoids : paths and surfaces but no non-contractible volumes
-
-
- h-level n: n-groupoids
- ...

- h-level ω: ω-groupoids

Notice that h-levels are not equivalence classes of spaces. The homotopical hierarchy is cumulative in the sense that all types of h-level n also qualify as types of level m for all $m > n$. For example *pt* qualifies as truth-value, as singleton set, as one-object groupoid, etc. Hereafter we qualify a space (type) as n-space (n-type), when its h-level is n but is not $n-1$. Given a n-space S_n we associate with it in a canonical way spaces S_k of all lower dimensions ($k < n$) by "forgetting the higher-order structure", i.e., by the identification of all higher-level homotopies of order $> k$ with trivial homotopies. This procedure is called (for geometric reasons) *k-truncation*. In particular the (-1)-truncation of any given space S brings point *pt* when S is not empty and brings the empty space $\emptyset$ otherwise. For more precise explanations of HoTT basics, see [95].

We would like to stress that the cumulative h-hierarchy is a formal feature of the intensional MLTT that can be described (even if not properly understood) in purely syntactic terms without referring to homotopy theory. For example saying that given type A is (-1)-type is tantamount to saying that all terms $x, y : A$ of this type are judgementally equal and that all terms of the corresponding propositional equality $x =_A y$ are judgementally equal to $refl_x$ and so on for all higher identity types. The presence of the h-hierarchy of types in MLTT is a robust mathematical feature of this theory, not an epiphenomenon of its special interpretation.

HoTT has been motivated — and its idea has in a certain sense been justified — by the model of MLTT in the category of *simplicial sets* found by Voevodsky in mid-2000s [139] and, independently, by Steven Awodey and Michael Warren, who studied links between Type theory and theory of Model categories [12][29]. HoTT does not qualify as model of MLTT in the standard ZFC-based homotopy theory (albeit it can be used as a basis for the axiomatic *Synthetic Homotopy theory* [263]) and, generally, it would be misleading to understand HoTT as a model of MLTT even in some broadened and liberal sense of being a model. HoTT in its original form presented in [95] is MLTT, possibly extended

[29]Simplicial set is a combinatorial model of topological space, which is a standard tool in modern homotopy theory. [139] is a systematic presentation of Voevodsky's results prepared by Kapulkin and Lumsdaine on the basis of Voevodsky's talks and unpublished manuscripts. A model category is a setting for axiomatic homotopy theory due to Quillen [218].

with the Univalence Axiom, and provided with a novel homotopical semantics via a Martin-Löf-style informal meaning explanation briefly sketched above. In what follows we discuss some epistemically significant features of this semantics including its intuitive aspect. Models of MLTT/HoTT are discussed in **3.2.5 C**.

E) Logical and Extra-Logical Semantics in HoTT (see [246])

The h-hierarchy of types in MLTT suggests an important modification of the original preformal semantics of this theory due to Martin-Löf, which was explained in the last Section. This modification is not a mere conservative extension. Recall that Matin-Löf proposes multiple informal meaning explanations for judgements, and suggests that they conceptually converge. HoTT in its turn provides a geometrically motivated conception of a set as 0-type and a proposition (called in [95, p. 103] a *mere* proposition) as (-1)-type. The latter is motivated by the idea that the empty type is the false proposition and the one-point type is the true proposition. This interpretation squares with Martin-Löf's interpretation of terms of propositional types as their proofs aka witnesses aka truth-makers. 0-type, aka a set M that has more than one element, can be interpreted as a set of proofs of a proposition, aka (-1)-type $\|M\|$ obtained from M via the (-1)-truncation, i.e., by the identification of these proofs with a single truth-value. Thus HoTT suggests the following revision of Martin-Löf's original semantics for MLTT: sets and propositions are not just two alternative ways of contentful thinking about MLTT-types, but canonical interpretations of MLTT-types of corresponding kinds: (-1)-types in the case of propositions and 0-types in the case of sets. The corresponding canonical interpretations for n-types with $n > 0$ are given in terms of homotopy theory as explained above.

Martin-Löf's theory of judgement, systematically exposed in his Siena 1983 lectures (published in a revised form in 1996 [188]), also needs revision in view of HoTT. Martin-Löf proposes the following twofold definition of a judgement:

> "[U]nderstood as an act of judging, a judgement is nothing but an act of knowing, and, when understood as that which is judged, it is a piece or, more solemnly, an object of knowledge." [188, p.19]

Accordingly, a judgement of the form A *is true*, where A is a proposition, is knowledge *how* to prove this proposition. Thus as Martin-Löf puts this, "the distinction between knowledge how and knowledge that [[254]] evaporates on the intuitionistic analysis of the notion of truth" [188, p.36].

Taking the h-hierarchy of types into account suggests the following modification of the above view on judgement. Let A be a higher-order type (h-level $>$ - 1) and $\|A\|$ its propositional truncation. Judgement $a : A$ along with its derivation in MLTT represents knowledge of how to build term a of that type. Term a proves or evidences proposition $\|M\|$: the truncated version of judgement $a : A$, i.e., the propositional judgement $\|a : A\|$ comprises the term $\|a\|$, which is a "trace" of a that witnesses the existence of a proof of $\|A\|$. The problematic epistemological question of whether a proof that given proposition P has a proof itself qualifies as a proof of P in this case needs to be answered positively (more on this will be said in **3.2.5 A** below). Anyway, in this setting, it makes perfect sense to distinguish between the knowledge-*that* expressed with $\|a : A\|$ and the knowledge-*how* expressed with $a : A$. So the distinction between knowing that and knowing how does not evaporate.

Assuming that any formal theory of judgement falls wholly under the scope of logic one can qualify HoTT as a logical calculus. Probably this is a reason why Michael Shoulman calls HoTT the "logic of space" [263]. However, such a conception of logic is much broader than usual, and includes homotopy theory in some form. One who wants to stick to a more narrow and more familiar conception of logic that assumes that logic deals only with judgements of the form A *is true* (where A is a proposition) may qualify the HoTT semantics as logical at the h-level (-1) and as extra-logical (namely, homotopical) at all higher h-levels. We leave aside for now the question of how to qualify the "pre-logical" h-level (-2). Apparently Michael Rathjen has this more familiar conception of logic in mind when he remarks that in MLTT

> "The interrelationship between logical inferences and mathematical constructions connects together logic and mathematics. Logic gets intertwined with mathematical objects and operations, and it appears that its role therein cannot be separated from mathematical constructions. [..] [L]ogical operators can be construed as special cases of more general mathematical operations." ([220, p. 95])

The h-hierarchy of types revealed with HoTT allows one to specify the structure of the interrelationship between logical inferences and mathematical (to wit homotopical) constructions more precisely: the same formal MLTT rules are interpreted as rules of logical inferences at the propositional (-1) h-level and as rules for building extra-logical mathematical constructions, similar to Euclids geometrical *Postulates*, at all higher h-levels. Extra-logical constructions generated with these rules are not isolated from logical operations but have a clear logical impact: each such construction, i.e., a term of higher-level type, functions as a proof of a proposition obtained via the (-1) truncation of that type. Consider for example MLTT rule according to which the judgemental identity of terms entails their propositional identity:

$$\frac{a \ \equiv_A \ a'}{refl_a : a \ =_A \ a'}$$

When A is a 0-type, i.e., a set, then $a \quad =_A \quad a'$ is a (-1)-type, i.e., a proposition. In this case the above rule reads: When elements a, a' of set A are judgementally (definitionally) equal then the proposition $a = a'$ is true. When A is a 1-type, i.e., a (flat) groupoid where different loops of the form $l_a : a \ =_A \ a'$ are distinguished and form a set (i.e., a 0-type), then the term $refl_a$ is interpreted not as a mere truth-value but as a trivial loop (distinguished from other loops of the same type) that witnesses proposition $\|A\|$ obtained from type A via the (-1)-truncation.

The (proof/evidence of) non-judgemental identity of the Morning Star (MS) and the Evening Star (ES) from Frege's *Venus* example (see **3.2.3** above) is represented in HoTT by a continuous path between the two objects that in this case can be thought of as an observed trajectory of motion of Venus [241] (Fig.8).

While at the propositional level the (propositional) identity of given terms is a relation — it either holds or does not hold — the higher identity types have a more complex structure that may involve multiple paths, multiple homotopies between the paths, etc. all the way upward. The case of the identity type of h-level $= 1$ (flat groupoid) can be illustrated by the picture of multiple paths used in

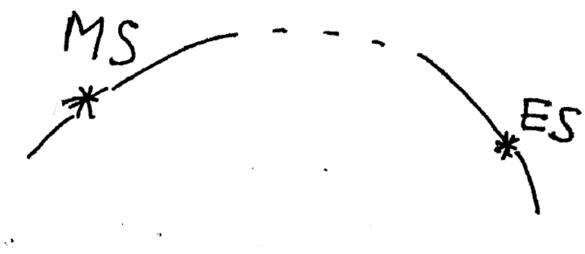

Fig. 8: the Morning Star is the Evening Star

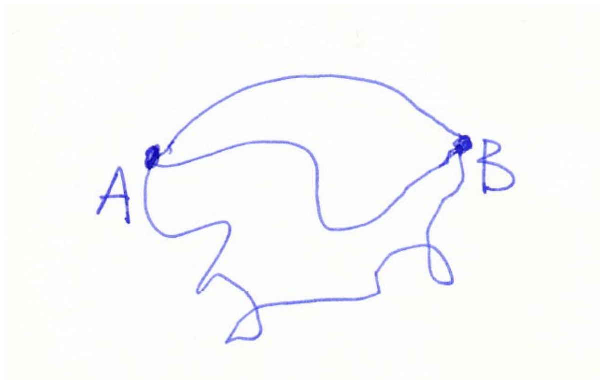

Fig. 9: quantum paths

Feynman's Path Integral formulation of Quantum Mechanics assuming that the pictured paths are not homotopic (Fig.9).

Terms of higher identity types can always be thought of as witnesses of the underlying propositional identity type (obtained with truncation). However they can be also understood as elements of the structure of the identified object in question. Suppose that the identity of MS and ES is now taken for granted, so that we are given judgemental (definitional) identity $MS \equiv ES \equiv Venus$. In that case path $p : MS = ES$ shown at Fig.9 is a non-trivial loop to be distinguished from the trivial loop $refl_{Venus}$, the existence of which is entailed by the assumption [230], [234]. In this context it makes sense to think of p and other non-trivial loops of the same type (and terms of associated higher identity types if any) as features of the same object *Venus*, rather than as auxiliary external constructions that can help one to know that the Morning Star is the Evening Star. This provides an interesting new solution of the much discussed controversy concerning the choice between the 3D and the 4D ontology for spatio-temporal objects like *Venus* [241, Section 8].

In light of the above analysis/explanation, HoTT appears as a modern version of the traditional Euclidean pattern of mathematical reasoning, where theorems are proven with geometric constructions; see **1.1.4** above. Under this analysis HoTT also squares perfectly with Lawvere's view according to which "logic is a special case of geometry" [162, p. 329] that this author develops in the context of Topos theory (**3.1.4** above): the HoTT semantics is geometric (in the broad sense of being space-related) all the way through, but a fragment of this semantics also qualifies as logical. The "merely logical", to wit propositional, (-1)-truncated fragment of HoTT is *internal* with respect to the full HoTT and its models in the sense of being a proper and integral part of this larger framework that also comprises non-propositional objects such as sets and homotopy spaces.

3.2.5 Univalent Foundations

The Univalent Foundations (UF) is a novel foundation of mathematics proposed by Vladimir Voevodsky in the 2000s, which uses HoTT and its internal logic for the formal axiomatic (in the sense of being "axiomatic" fully explained in **4.2.2** below) representation of mathematical theories. UF involves an additional principle, viz. the *Univalence Axiom* (UA), which is absent from MLTT/HoTT as presented above. Before we discus UA we highlight some other important aspects of UF.

A) Computer-Assisted Proofs and Automated Proof-Checking One of Voevodsky's major motivations for developing UF was pragmatic. As we have already stressed and explained in **2.4**, the standard set-theoretic foundations of mathematics and, even more generally, the Hilbert-style axiomatic approach in mathematics, does not help one to formally check and verify mathematical proofs beyond some trivial cases. However, the practical need for such a reliable verification becomes more and more pressing. The "neglect of epistemic considerations in logic" stressed by Sundholm as a general philosophical problem [272] in fact has a clear practical dimension. Here is just one example that belongs to Vladimir Voevodsky's personal intellectual biography.

In 1990 Mikhail Kapranov and Vladimir Voevodsky published a paper in Russian in which the authors announced a proof of an important theorem that says, roughly, that the homotopy category of homotopy spaces is equivalent to the homotopy category of weak ω-groupoids of a certain form [137, Th.2]. One year later the same result and a more detailed proof was published by the same authors in English as [138]. In 1998, Carlos Simpson published a preprint [264] in which he gave a counter-example to Kapranov&Voevodsky's alleged theorem, though he did not point to a mistake in their proof. Kapranov and Voevodsky considered this critique, but were unable to see a mistake in their arguments and suspected a mistake in Simpson's paper. The reaction of the mathematical community was described by Voevodsky in 2014 as follows:

> "By the time Simpson's paper appeared, both Kapranov and I had strong reputations. Simpson's paper created doubts in our result, which led to it being unused by other researchers, but no one came forward and challenged us on it." [300, slide 10].

This situation persisted until Fall of 2013, when Voevodsky finally discovered a mistake in his and Kapronov's 1991 proof. The mistake turned out to be incorrigible, so the community today believes that Simpson is right and the main theorem of [138] is in fact a non-theorem.

The above case, which Voevodsky describes as "outrageous", is not isolated. In his 2014 lecture Voevodsky describes another persisted and later discovered mistake in his own argument; for more examples of persisting mistakes in past and recent mathematics see [293]. Nikolai Vavilov [ib.] argues that today's mathematical practice is not unlike the mathematical practice of past centuries in this respect: consensus on the truth of mathematical theorems and on the validity of their proofs is eventually reached within a tiny group of reputed experts; a wider recognition is based on their authority. Mistakes in this process are unavoidable and regularly occur throughout the history of mathematics. However it may be argued that while mathematics develops and becomes increasingly more technical and more specialised, the problem becomes more acute, and the traditional academic procedures of verification and justification of new announced results become even less reliable. This was how Vladimir Voeovodsky conceived of this problem in the mid-2000s.

Voevodsky believed that the only viable solution to this problem would be using computer-assisted formal proof checking as a standard procedure in mainstream mathematical practice:

> "Ideally, a paper submitted to a journal should contain text for human readers integrated with references to formalised proofs of all the results. Before being sent to a referee the publisher runs all these proofs through a proof checker which verifies their validity. What remains for a referee is to check that the paper is interesting and that the formalisations of the statements correspond to their intended meaning." [299, slide 3]

Stated in this general form the idea was not new. Recall Leibniz's proposal to resolve philosophical disputes with a computation (*"calculemus!"*). When Hilbert aimed at making his formal symbolic axiomatic method into the "basic instrument of all theoretical research" ([108, p. 467]) he, apparently, had a similar motivating idea in mind. The first proof assistant designed for automated "mechanical" verification of formalised mathematical proofs with an electronic computer was Peter De Bruijn's *Automath*, presented in 1967 and further developed during the following years. It is worth noticing that the *Automath* does not implement Hilbert-style axiomatic reasoning, but uses a Gentzen-style type-theoretic approach in formal reasoning based on the Curry-Howard correspondence. The emergence of MLTT in the 1980s motivated the development of new proof-assistants that implemented fragments of this theory or otherwise used its ideas. This includes NuPrl (1986), ALF (1994), Agda, LF, Lego and Coq; see [82] and further references therein. An updated comprehensive list of presently existing proof-assistants and similar software tools is found in [70]. Agda and Coq are commonly used today for the computational implementation of UF-based mathematical proofs. A growing corpus of UF-based formal mathematics implemented in Coq (the *UniMath* project) is found in [298]. A recent overview of existing proof-assistants and presentation of the state of the art in interactive computer-assisted theorem proving — which however does not cover *UniMath* and other UF-motivated projects — is found in [10] and references therein.

In spite of the continuing efforts and achievements of enthusiasts, the use of computer-assisted proofs remains today quite uncommon in mainstream mathematical research. Nevertheless there is a growing corpus of mathematical results proved with computer; in many such cases non-assisted "purely human" proofs are not known and there are indications that such proofs may not exist. This is the case of the Four Colour Map theorem (4CT) proved by Kenneth Appel, Wolfgang Haken and John Koch in 1977 [6]. Appel and his co-authors used a low-level computer code written specifically for this purpose in order to check 1482 different cases (configurations) one by one, which was not feasible by hand. More recently a fully formalised version of Appel&Haken&Koch's proof was implemented with Coq [88]. Appel&Haken&Koch's proof raised a vivid philosophical discussion that we shall briefly review here. For another recent overview of this discussion that focuses on issues relates to Wittgenstein's views on mathematical proof see also [258]

The discussion was started with Thomas Tymoczko's paper [285] in which he argues that the computer-assisted proof of 4CT does not qualify as a mathematical proof in anything like the usual sense of the word because the computer part of this proof cannot be surveyed and verified in detail by a human mathematician, or even a group of human mathematicians. On this basis, Tymoczko suggests that 4CT and its existing proof represents a wholly new kind of *experimental* mathematics akin to experimental natural sciences, where the computer plays the role of experimental equipment.

Paul Teller in his response to Tymoczko [282] argues that Tymoczko misconceives of the concept of mathematical proof by confusing the epistemic notion of verification that something is a proof of a given statement with this proof itself, which under Teller's general conception of mathematical proof has no intrinsic epistemic content in it. Assuming that Appel&Haken&Koch's alleged proof of 4CT is indeed a proof, Teller argues that this proof is unusual only in how one gets epistemic access (if any) to it but that, contra Tymoczko, there is nothing unusual in the involved concept of mathematical proof itself.

Commenting on Teller's analysis in 2008, Dag Prawitz [214] approves of Teller's distinction between a proof and its verification. However, since Prawitz's conception of proof is essentially epistemic (see **3.2.2**), his analysis of Appel&Haken&Koch's proof of 4CT is very different. Contra Teller and in accordance with Tymoczko, Prawitz argues that *if* Appel&Haken&Koch's alleged proof is indeed a proof, then it comprises a crucial piece of empirical evidence provided by computer and is thus not deductive.

In their response to Tymoczko, Mic Detlefsen and Mark Luker [56] quite convincingly show that the difference between the computer-assisted proof of 4CT by Appel&Haken&Koch and traditional mathematical proofs is less dramatic than Tymoczko thinks. For traditional mathematical proofs quite often, and perhaps even typically, comprise some "blind" symbolic calculations, like one that is needed in order to compute the product $50 \times 101 = 5050$. The extent to which a given symbolic calculation is epistemically transparent or blind, is, according to Detlefsen&Luker, a matter of degree rather than a matter of principle. Using an electronic or mechanical computing device instead of or along with pen and paper does not change the nature of mathematical reasoning and mathematical proof. That the reliance on pen and paper or, alternatively, on mechanical or electronic devices is indeed a matter of social habit and social convention can be illustrated by Kenneth Appel's observation, made in 1977, soon after the release of his and his co-author's proof of 4CT, during a public presentation of this result. According to Appel

> " [The public] clearly divided into two groups: people with more than 40 years that 'could not be convinced that a proof by computer could be correct' and 'people under forty [who] could not be convinced that a proof that took 700 pages of hand calculations could be correct' " (quoted after [258, p. 291]).

O. Bradley Bassler [17] suggests distinguishing in this context between the *local* and the *global* surveyability of mathematical proofs. By the local surveyability of proof p, Bassler understands the property of p that makes it possible for a human to follow each elementary step of p. Bassler argues that local serveyability of p does not, by itself, make p epistemically transparent or surveyable in the usual intended sense because on the top of local surveyability at least a minimal *global* surveyability is required, which allows one to see that all steps of p taken together provide p with a sufficient epistemic force that warrants its conclusion on the basis of its premises. In the historical part of his paper, Bassler shows that there is an unfortunate tendency to neglect global surveability in proofs by assuming that it reduces to the local case. We can provide an additional supporting piece of evidence to this Bassler's claim by referring to Hilbert's analysis of formal mathematics. Recall from **1.2.3** that Hilbert stresses the foundational importance of “concrete signs themselves, whose shape, according to the conception we have adopted, is immediately clear and recognizable”. ([108, p. 465]); the capacity to identify and distinguish symbols clearly we called above *symbolic intuition*. Obviously, symbolic intuition provides only for local surveyability of formal symbolic proofs and has no bearing on their global surveyability. Hilbert is silent on the possible foundational impact of *global* surveyability, and apparently treats this holistic aspect of mathematical proofs as a psychological or heuristic rather than a logical and foundational issue.

When one applies the distinction between local and global surveyability in the analysis of Appel&Haken&Koch’s proof of 4CT (as Bassler does in his paper [17]) the resulting picture is more complex than the one suggested by Tymoczko [285]. The computer part of the proof is fully locally surveyable in the sense that each piece of the computer code can be checked and interpreted by a human (since it was written by a human). Arguments explaining why the computation so encoded, if performed correctly, completes the proof of the theorem, which Appel&Haken&Koch present in the form of traditional mathematical prose, provide a global survey of this proof and of this computation in particular. What this proof still lacks is rather an expected surveyability and traceability at the intermediate scale between the general understanding of what the given computation computes and the low-level computational steps expressed with the program code. We shall shortly see how this specific problem is successfully treated in UF-based approach to the computer-assisted theorem proving.

Without going further into the epistemology of computer-assisted mathematical proofs, let us briefly formulate our take on this problem. We share Prawitz’s epistemically-laden conception of proof, including mathematical proof. For this reason we consider the issue of the surveyability and transparency of proofs to be a logical and foundational issue, not merely a pragmatic or practical one. We shall use Bassler’s distinction between local and global surveyability, which is very helpful in our analysis of UF. We also share Voevodsky’s conviction that the computer, conceived of and properly used as an epistemic tool (rather than as an autonomous epistemic agent), can help one to obtain more confidence in certain mathematical results, to find errors in other alleged results, and thus to improve on current problematic situation in the cutting edge mathematical research indicated above. Below we provide more details on how the interaction between human and computer can be organised in the case of UF-based computer proofs. A detailed comparative analysis of other approaches in automated proof-checking is beyond the scope of the present work. However, to the best of our knowledge, the features of the UF-based approach, that are highlighted below and which, according to our analysis, are of crucial epistemological importance, are wholly absent from all other approaches.

B) Univalent Foundations and Mathematical Intuition[247]

The homotopical semantics of MLTT briefly described in **3.2.4** allows one to think of formal derivations in this calculus as a geometrical or, more precisely, homotopical spatial constructions. When this base calculus or its fragment is implemented in the form of programming code, the same homotopical interpretation along with the associated spatial intuition applies to the code. This spatial (homotopical) intuition makes formal symbolic derivations and the corresponding programming code humanly surveyable in a new way: on top of the *local* surveyability that allows one to control elementary steps of the process, and in addition to the high-scale *global* surveyability that provides one with a general grasp of the resulting construction, homotopical intuition provides an epistemic access to the intermediate scale (mesoscopic) level of construction, which allows one to follow and control all significant steps of the construction (reasoning), ignoring its minute details. Such an intuitive reading of the formalism bridges the usual gap between the rigorous formal representation of mathematical reasoning using logical calculi, on the one hand, and conventional representations of mathematical reasoning, which typically rely heavily on various symbolic means of expression without strict syntactic rules, on the other hand. Thus HoTT supports a representation of mathematical reasoning in general and mathematical proof in particular which is:

- fully formal in the sense that it uses a symbolic calculus with an explicit rigorous syntax;
- computer-checkable;

- supported by a spatial (homotopical) intuition that balances local and global aspects of mathematical intuition in the usual way [228].

A simple (but not trivial) example of a mathematical proof represented in this way is found in [170]. It is a proof of a basic theorem in Algebraic Topology according to which the fundamental group $\pi_1(S^1)$ of a (topological) circle is S^1 (isomorphic to) the infinite cyclic group $\mathbb{Z}$, which is canonically represented as the additive group of integers, see [99, p. 29 ff] for the standard presentation of this material[30]. The theorem and its proof have a clear intuitive meaning. Choose an arbitrary point of a given circle as the base point and attach a thread to it. Then wind the thread around the circle (in all possible ways) and try to classify the forming loops. First, distinguish between clockwise and counterclockwise loops. Second, in each of these two classes distinguish between loops of different orders where the order of a given loop is its winding number with respect to a point inside the circle, that is, the number of times one winds the thread when one obtains the given loop. By convention the counterclockwise loops have positive winding numbers and the clockwise loops have negative ones. Introduce the trivial loop with winding number 0. The loops are composable in the obvious way, so that the winding number of the resulting loop is the sum of the winding numbers of the composed loops. The set of loops with composition form a group, which is isomorphic to the additive group of integers, i.e., the infinite cyclic group.

The standard proof of the theorem involves the construction of a "winding map" $w : \mathbb{R} \to S^1$ that can be visualised in the form of a helix, and a projection that sends each point of the helix to the point of a circle below it (Fig.10).
This map is a fibration called the *universal cover* of the circle; each fiber in it is isomorphic to the integers. Leaving aside other details of the argument (which are found in [170]), let us only stress the fact that the above construction is formally reproduced with HoTT and then encoded with the programming code (Licata and Shulman encode this proof in AGDA) without losing its intuitive appeal.

Let *base* be a point of a given circle S^1 (the base point). This judgement is formally reproduced with MLTT syntax as the formula

[30]The fundamental group is a fundamental groupoid where all paths are loops that start and end at the same point of the given space, which is called the base point. The fundamental group of a given topological space does not depend on the choice of the base point.

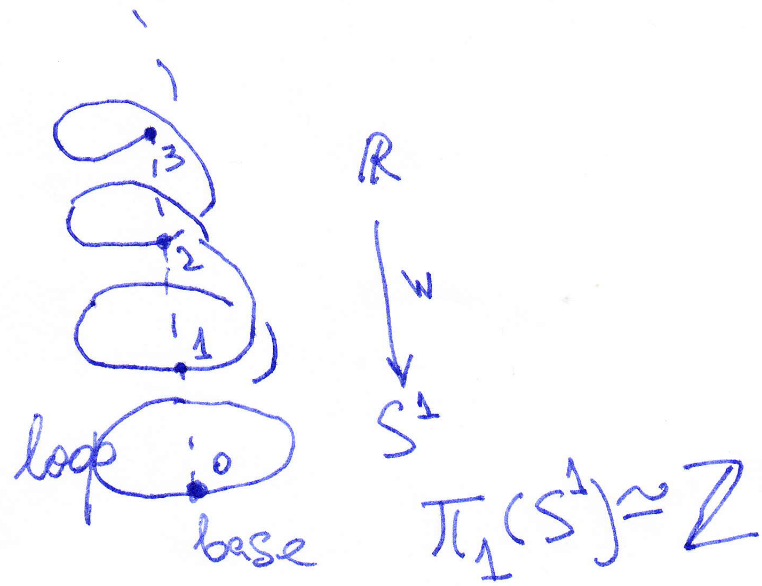

Fig. 10: winding map

$$b : S^1$$

Then loops associated with this base point are terms of the form:

$$loop : b =_{S^1} b$$

The resulting formal proof and its implementation in a programming code are interpretable in terms of such intuitive spatial (homotopical) constructions all the way through. When the program is run on a computer, the user, as usual, is in a position to grasp the general idea of the proof (see the informal explanation above) and to survey fragments of the executed computation at the microscopic level of elementary computational steps by looking into the programming code all the way down to the machine code. What is specific for the UF-based approach is that in the given case the user can also follow the mathematical argument at the crucial mesoscopic level of the proof structure. In this case a computer-assisted proof is no longer a "black box proof" where a part of the argument is hidden and replaced by non-deductive empirical evidence. In this case the computer is used only as a tool that helps one to ensure that the microscopic structure of the given proof is correct. (Arguably computers do this better than humans.) This makes a UF-based formal computer-assisted proof quite like traditional mathematical proofs (along the lines of Detlefsen&Luker's argument [56]).

The idea of Univalent Foundations is to reconstruct in the same way the whole of mathematics. Since the above theorem belongs to Algebraic Topology and even more specifically to Homotopy theory, the relevance of HoTT in this case is not surprising. It is not so clear, thus far, whether the homotopical intuition is relevant to all mathematical subjects and disciplines and can play the same role in their foundations. The *UniMath* library [298] is growing, but at this point in history we don't have elementary mathematics textbooks based on this approach, which could be compared to the early 20th century progressive elementary geometry textbooks written in the Hilbert style and adopted for school education (like [97]), or with the Bourbaki-style textbooks that appeared later in the same century. Even if the idea to use the Univalent Foundations in mathematics education, including elementary mathematics education, appears weird, we believe that some general lessons for MathEd can be learned from UF. One such lesson is that there is no sufficient reason for considering the Hilbert-style axiomatic architecture as the default theoretical ideal that school and university mathematical textbooks need to approximate.

C) Models of HoTT and the Initiality Conjecture [248]

The model theory of MLTT/HoTT is so far less developed than HoTT itself, and we do not intend to describe here the present state of the art in this area of research. In what follows we provide a brief overview of the subject and highlight certain conceptual issues, which, in our view, are of general importance.

Alfred Tarski designed his model theory back in the early 1950s [277], having in mind Hilbert-style axiomatic theories. Let us recall its basics concepts. A model of (uninterpreted) axiom A is an interpretation m of non-logical terms in A that makes it into a true sentence A^m; if such m exists A is called *satisfiable* and said to be satisfied by m. Model M of uninterpreted axiomatic theory T is an interpretation that makes all its axioms and theorems true. Since the rules of inference used in T preserve truth, it is sufficient to check that M satisfies all axioms of T to establish that it also satisfies all its theorems (soundness). The model-theoretic logical semantics proposed by Tarski and the Tarski-style model theory together form a coherent semantic framework for first-order Hilbert-style axiomatic theories. Models built in this framework are set-theoretic models by default. Bourbaki-style set-theoretic foundations of mathematics and the "semantic" approach in the formal representation of scientific theories (see **2.2** and **2.3.3**) are based, theoretically, on Tarski's pioneering works in model theory.

During the decades following its birth, the model theory of the 1950s significantly developed and changed its shape. As Angus Macintyre remarked back in 2003

> "I see model theory as becoming increasingly detached from set theory, and the Tarskian notion of set-theoretic model being no longer central to model theory. In much of modern mathematics, the set-theoretic component is of minor interest, and basic notions are geometric or category-theoretic. [...] The resulting relativization and 'transfer of structure' is incomparably more flexible and powerful than anything yet known in 'set-theoretic model theory'." [174, p. 197]

Even if Macintyre focuses in this paper mostly on technical developments it is clear that these developments call for revision of the philosophically-laden conceptual foundations of Tarskian model theory. This latter task remains mostly unsolved in today's philosophical logic. For a valuable attempt to introduce the up-to-date model theory into the current philosophical discussion see the recent monograph by John Baldwin [13]. In this Section, we describe some specific features of the model theory of MLTT/HoTT and consider Voevodsky's *Initiality Conjecture*.

When we deal with modelling a theory presented in the Gentzen style rather than in the Hilbert style, which has a default proof-theoretic semantics that is supposed to be preserved in all further interpretations, the familiar Tarskian semantic framework does not apply or at least cannot be applied straightforwardly. First of all, we need a notion of modelling a rule (rather than modelling an axiom). Although such a notion is not immediately found in standard textbooks on model theory (such as [121]), it can be easily construed on this standard basis as follows. We shall say that interpretation m is a model of rule R, in symbols

$$\frac{A_1^m, \ldots, A_n^m}{B^m} \tag{12}$$

if whenever $A_1^m, \ldots, A_n^m$ are true sentences B^m is also true sentence. This scheme is used by Dimitris Tsementzis in what he calls a preformal meaning explanation of HoTT [283]. However, since not every judgement in MLTT/HoTT is of the form P *is true*, the above notion of modelling a rule does not accurately preserve the default semantics of this theory in all cases of interest. In particular, (12) under this standard interpretation is not compatible with the extra-logical proof-theoretic semantics of HoTT described above in **3.2.4** because formula A^m cannot stand simultaneously for a true sentence and for a non-propositional object. In order to fix this difficulty, we shall understand the satisfaction relation in a more general way than is usual by interpreting formulas A_i^m in (12) as correct judgements without specifying their form. The *soundness* of interpretation m in this new setting is the requirement according to which m does not destroy the inferential force of R understood in epistemic terms: the inference from assumptions $A_1^m, \ldots, A_n^m$ to conclusion B^m needs to be justified via a joint application of the default HoTT semantics and the additional semantics provided by interpretation m itself (as in the case in which a path between points MS, ES is interpreted as a trajectory of a celestial body).

In the standard Tarskian setting, by interpretation of formula one understands an interpretation of *non-logical* symbols in this formula; the distinction between logical and non-logical symbols is supposed to be set beforehand. In interpreting HoTT one deals with a very different situation, in which the distinction between logical and extra-logical elements does not apply at the syntactic level. This is why in this case one needs to check how the intended meanings of logical constants are modified with the given interpretation, and make sure that they still work as expected.

Models of MLTT include (see [58, Section 6]):

- realisability models;
- set-theoretic models (in ZFC and in constructive set theories);
- category-theoretic models of three sorts:

– locally cartesian closed categories (LCCC);

– contextual categories (and their versions known as categories with families and categories with attributes);

– hypodoctrines

Realisability models of MLTT first appear as formalised versions of Martin-Löf's informal *meaning explanation* of MLTT; for further details see [221], [124]. However, Martin-Löf insists that meaning explanation is a sui generis semantic procedure [188] which should be distinguished from modelling. Recall that when we talk about models of MLTT and HoTT, we don't consider these theories to be purely syntactic constructs devoid of meaning, but assume that they have default meanings provided by their meaning explanations, as was sketched above. Unlike meaning explanations realisability models are external mathematical structures.

Set-theoretic model(s) of MLTT, including ZFC-based models, are hardly helpful as a means of representation, but their existence is important from a foundational point of view; notice that in order to model the "big" universe of types in ZFC, one needs to assume on top of ZFC the existence of at least one inaccessible cardinal.

We have already discussed above the close relationships between Lawvere's hypodoctrines, LCCC's and MLTT: any LCCC qualifies as (an example of) a hypodictirine, and MLTT qualifies as the *internal language* of LCCC. When hypodoctrines and LCCC's are described as models of MLTT, the viewpoint is different but the mathematical content that justifies the name is the same.

All the above models of MLTT verify the Reflexion Principle (RP) (see **3.2.3**) and thus cannot serve as models of HoTT even though MLTT and HoTT (without the Univalence Axiom) coincide syntactically. As we already mentioned in **3.2.3**, the first model of MLTT that violates RP and thus opens the room for a homotopical interpretation of intensional MLTT, including its higher-order identity types, was first published in 1993 by Thomas Streicher [269]; see also [123].

Let us finally point to an open problem in the model theory of HoTT, which involves an interesting attempt to rethink the received concept of a model. This problem was central in Vladimir Voevodsky's research at least since 2010 until the end of his life and career. Voevodsky proposed a refinement of the received concept of model related to what he called the *Initiality Conjecture.* In order to explain this proposal of Voevodsky's we need some preliminaries. Voevodsky uses a version of model theory today called *Functorial* Model theory, the idea of which goes back to Lawvere's 1963 thesis [155]; for a modern exposition see [132, vol. 2, D1-4] and [203].

The idea of functorial model theory can be briefly described in the form of the following construction:

- a given theory $\mathbf{T}$ is presented as a syntactic category called the canonical syntactic model of $\mathbf{T}$;
- models of $\mathbf{T}$ are construed as functors $m : \mathbf{T} \to S$ from $\mathbf{T}$ into an appropriate background category (such as the category of sets), which preserve the relevant structure;
- models of $\mathbf{T}$ form a functor category $C_{\mathbf{T}} = S^{\mathbf{T}}$;
- in the above context theory $\mathbf{T}$ is construed, in Lawere words, as a *generic model* , i.e., as an object (or a subcategory as in [155]) of $C_{\mathbf{T}}$.

Voevodsky and his co-authors proceed according to a similar (albeit not quite identical) pattern:

- Construct a general model of a given type theory $\mathbf{T}$ (MLTT or its variant) as a category $\mathcal{C}$ with additional structures which model $\mathbf{T}$-rules. For that purpose the authors use the aforementioned notion of *contextual category* due to Cartmell [40]; in later works Voevodsky uses a modified version of this concept named by the author a *C-system.*
- Construct a particular contextual category (variant: a C-system) $\mathcal{C}(\mathbf{T})$ of a syntactic character, which is called a *term model.* Objects of $\mathcal{C}(\mathbf{T})$ are MLTT-contexts, i.e., expressions of the form

  $$[x_1 : A_1, \ldots, x_n : A_n]$$

  taken up to definitional equality and the renaming of free variables and its morphisms are substitutions (of the contexts into $\mathbf{T}$-rule schemata) also identified up to the definitional equality and the renaming of variables). More precisely, morphisms of $\mathcal{C}(T)$ are of the form

$$f : [x_1 : A_1, \ldots, x_n : A_n] \to [y_1 : B_1, \ldots, y_m : B_m]$$

where f is represented by a sequent of terms $f_1, \ldots, f_m$ such that

$x_1 : A_1, \ldots, x_n : A_n \vdash f_1 : B_1$

$\vdots$

$x_1 : A_1, \ldots, x_n : A_n \vdash f_m : B_m(f_1, \ldots, f_m)$

Thus morphisms of $\mathcal{C}(T)$ represent derivations in $\mathbf{T}$.

- Define an appropriate notion of morphism between contextual categories (C-systems) and form category $CTXT$ of such categories.
- Show that $\mathcal{C}(\mathbf{T})$ is *initial* in $CTXT$, that is, that for any object $\mathcal{C}$ of $CTXT$ there is precisely one morphism (functor) of the form $\mathcal{C}(\mathbf{T}) \to \mathcal{C}$.

The latter proposition is stated in [139] as Theorem 1.2.9 without proof; the authors refer to [269] where a special case of this theorem is proved and then mention that "the fact that it holds for other selections from among the standard rules is well-known in folklore".

Unlike some other colleagues Voevodsky considered the above *Initiality Conjecture* (IC) to be a genuine open problem. In its general form this conjecture is not a mathematical statement which waits to be proved or disproved, but a problem of building a general formal semantic framework for type theories in the form of a $CTXT$-like category that has a syntactic object with the initiality property explained above. This conjecture still stands open to the date of writing.

According to Veovodsky, a functor m of the form $\mathcal{C}(\mathbf{T}) \to \mathcal{C}$ does not qualify as a *model* of $\mathbf{T}$ unless the functor of this form is unique; otherwise it should be called a *representation* of $\mathbf{T}$. The initiality of $\mathcal{C}(\mathbf{T})$ guarantees that each representation in the given category of contexts is a model of $\mathbf{T}$.

The initiality requirement has the following epistemological meaning [31]. Think of the generic term model $\mathcal{C}(\mathbf{T})$ as a symbolically presented system of *instructions* (formal rules). This system of instructions is schematic in the sense

[31] The following epistemological argument is ours, not Voevodsky's.

that it is applicable in more than just one single context (recall that the available contexts are objects of $CTXT$). Then the initiality property of $\mathcal{C}(\mathbf{T})$ in $CTXT$ guarantees that in each particular context $\bar{\mathcal{C}}$ the general instruction $\mathcal{C}(\mathbf{T})$ is interpreted and applied unambiguously as a unique $\bar{m}$. Indeed, a useful instruction can and arguably should be schematic but it definitely should not be ambiguous.

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D) <u>Univalence</u>

Univalence is a property of Voevodsky's simplicial model of MLTT [139], which was stipulated by Voevodsky as an axiom (UA). UA, along with the syntactic rules of MLTT and their homotopical interpretation (HoTT), constitutes Voevodsky's original version of Univalent Foundations and gives it its name. The term "univalence" used in this context is due to Voevodsky and its origin is somewhat arbitrary: it comes from the expression "faithful functor" translated into Russian on some occasion as "univalent functor" [92, footnote 4].

Let A, B be types, in symbols $A, B\ TYPE$. Consider the identity type $A =_{TYPE} B$ and the type of *equivalences* $A \simeq_{TYPE} B$ where by equivalence one understands a function $f : A \to B$, which is in an appropriate sense invertible; under the homotopical interpretation such equivalences are homotopy equivalences. The rules of MLTT/HoTT allow one to construct a canonical map of the form

$$e\,(A = B) \to (A \simeq B)$$

which, to put it informally, witnesses the fact that identity is a special case of equivalence. The *Univalence Axiom* states that this map e has an inverse and thus is itself an equivalence. In other words, UA says that the type

$$(A = B) \simeq (A \simeq B)$$

is inhabited.

In order to make sense of UA it is instructive to consider first the case in which A, B are (-1)-types, i.e., propositions. In this case UA is, to first approximation, the familiar Church's *propositional extensionality* principle (PE), which says that two propositions are equal just in case they are equivalent [32] , or in standard symbols

$$(A = B) \leftrightarrow (A \leftrightarrow B)$$

PE implies that in all relevant contexts equivalent propositions count as the same and are interchangeable *salva veritate*; by analogy with the Isomorphism Equivalence Principle (IEP) and Category Equivalence Principle (CEP) considered in **2.2.2** and **3.1.2 B**) above, we shall call this latter statement the Propositional Equivalence Principle (PEP).

A necessary disclaimer is that PE is not derived in MLTT from UA via a simple restriction of the general case to the case of propositional types; the corresponding internal construction, which involves a construal of a proposition as a pair $P \equiv < A, p >$, where A is a type and p is a proof that A is a proposition, is given in [2, p.144-145]. The construction shows that propositions P, Q construed as above are equal just in case their underlying types A, B are (logically) equivalent, which is a constructive form of PE. So UA implies that the "right" notion of equivalence for propositions, which agrees with the corresponding equivalence principle, to wit with PEP, is, not surprisingly, their *logical* equivalence.

In case A, B are sets (0-types), UA says, roughly, that the type of equalities of sets is equivalent to the type of isomorphisms of sets, which implies IEP. The required internal construction in this case is more involved and its sketch is also found in [2, p.145]. Recall that in the Bourbaki-style semantic set-theoretic setting IEP is nothing but an heuristic principle. It is not a theorem of set theory, and it cannot be effectively used an additional foundational axiom for two reasons: (i) If IEP is applied to all properties of mathematical objects indiscriminately then it contradicts the axioms of set theory since not all such properties are invariant under isomorphisms; (ii) if IEP is applied only to the special class of *structural* properties, i.e., properties invariant under (structural) isomorphisms, then the contradiction is avoided but it remains unclear which ZFC-properties are structural and which are not.

[32] This principle that was first considered (but not adopted) by Alonso Church in his 1940 version of Simple Type theory and in 1950 used by Leon Henkin; see [21].

One might think that IEP used as an axiom on top of the set-theoretic axioms would provide an "implicit definition" of the concept of structural property in a similar way to that in which the ZFC axioms "define" the concept of a set. This proposal does not go through, however, because IEP, unlike the axioms of ZFC, is a second-order statement. Any class of ZFC-based structures, say, the class of groups, consists of objects that have both structural (group-theoretic in our example) properties, which are invariant under structural isomorphisms, and non-structural properties, which are not isomorphisms-invariant. So IEP used as an axiom on top of the ZFC axioms does not allow one to distinguish a class of structures such that all expressible properties of these structures would be structural by design. IEP helps one at best only to distinguish between structural (and thus relevant) and non-structural properties for a given type of structure (such as groups).

This is a serious argument supporting the claim that the Bourbaki-style set-theoretic foundations of mathematics are internally conceptually incoherent (even if formally consistent): mathematical structures construed on these foundations do not have an expected key epistemic feature, which is the invariance of all expressible properties under structural isomorphisms. Although Bourbaki never stated this argument in this form, Bourbaki's misgivings about set-theoretic foundations quoted in **2.2.2** can be anachronistically understood and justified in this way. UF solves this problem via UA: all properties of mathematical structures (whether Bourbaki-style or not) reconstructed on this foundation are structural in the intended sense (namely that of IEP); the wobble of identity and equivalence of mathematical objects, which is abundant in Bourbaki-style mathematics, is fixed and treated rigorously in UF. [33].

On this ground, some authors regard UF as a "structuralist foundation" [284]; see also [11] and [4]. We recognise that UF, unlike ZFC, supports IEP, which is a pillar of Mathematical Structuralism (MS) in any of its existing multiple versions. However, we have some reservations about the idea that UF indeed fully squares with the core of MS as it is usually presented. It is common to trace the history of MS at least back to Hilbert's 1899 *Foundations of Geometry* [115];

[33]Solving this problem was an important part of Voevodsky's motivation for developing UF [2, p.141].

Bourbaki's version of MS inherits Hilbert's concept of axiomatic method via the semantic version of this method. UF in its turn derives from MLTT and the intuitionistic/constructive trend in 20th century mathematics, which has Kantian origins (in particular, this concerns Brouwer). Unlike ZFC, MLTT is a Gentzen-styel but not a Hilbert-style formal calculus, and its intended logical semantics is proof-theoretic but not model-theoretic (see **3.2.3**) According to Martin-Löf, MLTT "resolves the Hilbert-Brouwer controversy" [190]; according to our analysis UF goes further in the same direction by revindicating the epistemic role of spatial intuition in mathematical proofs. Thus UF combines many ideas from logic, mathematics and philosophy; only a part of these ideas issue from Bourbaki-style structural mathematics and Mathematical Structuralism.

This remark concerns not only UF in general but also UA more specifically. An important consequence of UA is *function extensionality* (FE), i.e., the property that any two functions that share their domains and are equally evaluated at all arguments are the same; for a precise formulation and proof, see [95, pp. 140-142]; recall also that FE fails in toposes, save the topos of sets. FE is a strong extensionality principle that is important from the computational point of view because it allows for effective (to wit decidable) type-checking. This consequence of UA, unlike IEP, supports a constructive view of mathematics. Many authors who attempt to analyze UA from a logical point of view, and use in this analysis a historical context, regard UA as a modern form of Leibniz' principle of the *identity of indiscernibles* [150], [2]. Leibniz' Law can be interpreted in structural (MS) terms, but it can be also interpreted in constructive terms. The same is true of UA.

Of course, if one understands the notion of being a "structuralist foundation" liberally enough, the qualification of UF as "structuralist foundations" can be justified. This title can be also misleading, however, because it highlights just one aspect of UF and shadows other aspects. In this work we avoid characteriseing UF as integrally structuralist or constructivist; instead we try to analyse the mixture of mathematical, logical and epistemological ideas behind UF, in some cases to bring in some other ideas, and draw on this basis some epistemological conclusions.

IEP does not upgrade in UF to the *Category* Equivalence Principle (CEP) fully and straightforwardly. The reason is that while the concept of a set in HoTT is simple (recall that it is a type of h-level 0), the concept of a category is not. Moving one step up along the homotopical ladder takes one to *groupoids* (categories with all morphisms isomorphisms), not to general categories. General categories need to be reconstructed in UF internally. It then turns out that only a special class of categories called *univalent categories*, which includes all categories of Bourbaki-style structures and more, supports CEP [2, p.147-149]. So even if UF can be rightly seen as the structuralist dream coming true, this result shows that UF also has room for developing different kinds of mathematics. The possibility of interesting contentful mathematics developed beyond the univalence principle cannot be ruled out either — even if today such a possibility appears as sheer speculation.

Let us finally point to two important works in progress related to UA. First is *Cubical Type theory* (CTT). This theory is based on an alternative model of MLTT/HoTT in the category of *cubical sets*, which like Voevodsky's simplicial model has the univalence property [173]. The cubical model motivates an extension of MLTT, where the univalence property is proved but not postulated in the form of an axiom [38]. Recall that UA is the only axiom of the standard UF while MLTT is presented in the Gentzen-style rule-based form. This gives to the standard UF its mixed Gentzen/Hilbert-style form and makes it not fully computable because the inverse map

$$(A = B) \leftarrow (A \simeq B)$$

in this case is simply stipulated (i.e., introduced by fiat via UA), not effectively constructed. CTT, like MLTT, is wholly rule-based, which solves the computability issue. In addition, CTT admits meaning explanations of higher inductive types (HITs) (such as the type of loops $b =_{S^1} b$ considered above in this Section), which are more detailed and precise than those available in the standard HoTT [5]. Because of these features of CTT it can be argued that this theory better serves as a formal carrier for UF/HoTT than MLTT. But since CTT is still in its nascent state, we do not use it more systematically in the present work.

The other development is *Directed Type theory* (DTT) [202]. The idea here is to design a type theory along with its (directed) homotopy interpretation, in which groupoids of the standard HoTT are replaced by general categories; for the geometrical background of this approach see [89], [90]. From the computation point of view DTT is interesting because it goes beyond the functional paradigm of programming (where MLTT/HoTT-based programming languages belong) and allows for modelling concurrent computations. From the foundation of mathematics perspective, DTT is interesting because it allows one to represent general categories in a simpler way than in the standard UF . Finally, this approach appears to be interesting and important philosophically because it overtly goes beyond the structuralist viewpoint in mathematics, by treating invertible and non-invertible maps on equal epistemological grounds [229].

4 Conclusion and Further Research

4.1 Summary

In chapter **1** it was shown that the popular view according to which Hilbert's version of a formal axiomatic method, as presented first in his *Foundations of Geometry* of 1899, is a modern improved version of Euclid's method as presented in his *Elements*, needs significant qualifications and reservations. It was also shown that Hilbert was well aware of the specific character of his axiomatic approach and at least at a late stage of his career he did not regard Euclid's method as merely an imperfect version of his own method (Section **1.3.2**). The difference between Euclid's and Hilbert's axiomatic conceptions could remain an important but still purely historical issue, which was already discussed in earlier literature. However as we have further argued in the following chapters the specific character and limitations of Hilbert's axiomatic method also have important implications for today's mathematics. Some essential features of the traditional Euclid-style mathematical reasoning —such as its rule-based constructive aka "genetic" character —survive in today's mathematics and, as we argue, cannot be ignored as mere anachronisms (**2.2.4**). Our analysis of attempts to implement the Hilbert-style axiomatic method in broad mathematical and scientific practice

(Ch. **2**) as well as an analysis of some successful axiomatic approaches, which deviate from Hilbert's line (Ch. **3**) lead us to a conclusion according to which the very concept of axiomatic theory once again, today, needs a serious revision. Our version of such a revision is presented in this chapter below (Sections **4.2** and **4.3**).

A disclaimer is here in order. As we have already stressed, Hilbert designed his axiomatic method as a part of his foundational project, in which *meta-mathematical* tasks such as (meta-mathematical) proofs of consistency and of the epistemic completeness of theories played a central role. Such a focus on meta-mathematics within the foundations is a specific feature of Hilbert's vision of FOM, which is wholly absent from more traditional FOMs including Euclid's [34]. Mainstream FOM-related mathematical research in the 20th century followed in Hilbert's steps and brought about a number of important meta-mathematical results, such as Gödel's Incompleteness Theorems and the independence of the Continuum Hypothesis. In the present study we have not discussed such results and their philosophical implications, but focused instead on more traditional functions of FOM, which (with a pinch of salt) can be called "practical": the capacity to represent mathematical and scientific contents, to provide a common language for various areas of mathematics and science, and to serve as a guide for mathematics education. Our critique of Hilbert's axiomatic method primarily concerns its efficiency and adequacy as a representational and justificatory tool for mathematical and scientific theories, not its specific role in meta-mathematical studies. This critique may have certain implications for the interpretation of meta-mathematical results as well (since the relevance of these results to broad

[34]We call a theory *epistemically complete* when it allows one to solve any well-posed problem within this very theory. In order to understand the historical origins of today's received FOM it is essential to take into account the fact that Hilbert held a strong epistemological view in favour of the epistemic completeness of mathematical theories, which remained quite stable throughout his carrier. Hilbert expressed this view publicly on many occasions, most famously in his 1900 address delivered in the Sorbonne at the International Congress of Mathematicians where he pronounced his "no ignoramibus" (in mathematics), and also in his last major public lecture delivered in 1930 in Königsberg, which Hilbert concluded with the words "We must know. We will know". Hilbert formed this epistemological view in the context of the continuing *Ignoramibusstreit* (*ignoramibus* debate) started back in 1872 by Emil du Bois-Reymond who defended the opposite view according to which certain scientific questions and problems are unsolvable in principle [192].

mathematical practice obviously depends on whether or not this practice is modelled *adequately* in meta-mathematical studies) but in the present work we do not extend our study in this direction.

In Section **2.2** we overviewed the most significant and most systematic attempt to implement the modern Hilbert-style axiomatic approach in mathematical practice, namely the continuing project aiming at the axiomatic representation of the core mathematical knowledge available to date, which is generally known after the (pseudo-) name of Nicolas Bourbaki. It is important to realise that Bourbaki's method of theory-building differs from the one conceived by Hilbert. This difference is not just a matter of practical compromise between formal rigour and the demands of working mathematicians and mathematics educators, who usually systematically bypass various tedious routines in their reasoning. Unlike Hilbert's original version, Bourbaki's version of the axiomatic method is *semantic*, which means that Bourbaki-style axiomatic theories are equipped with default set-theoretic models, which are, generally, not isomorphic. Without further ado, Bourbaki treat these different models on equal footing and consider them as theoretical objects that belong to the same mathematical theory. Each particular algebraic group construed by Bourbaki as a set with a group-theoretic structure (no matter whether one prefers to identify this structure up to isomorphism or up to strict set-theoretic identity) qualifies as a model of group-theoretic axioms. However, group theory in Bourbaki treats such specific groups and various relationships between different groups so construed, not just properties shared by all groups that can be formally deduced from the group-theoretic axioms taken in isolation from the set-theoretic axioms. If one thinks about these models in terms of Tarski-style model theory (as do Suppes and his followers) then Bourbaki's *semantic* framework appears to be closely related to the Hilbert-style axiomatic architecture and can be justly seen as its extension or improvement.

The Bourbaki-style semantic version of the axiomatic method allows one to abstract away from syntactic details, which working mathematician usually consider to be irrelevant to their object of study, and thus more effectively use this method for representational purposes. A drawback of this approach is that mathematical proofs presented in Bourbaki-style are not formally checkable in practice (beyond trivial cases). Given a valid proof so presented one is always in a position to show that the conclusion of this proof can *in principle* be logically deduced from the axioms of set theory. In practice, however, one is typically not in a position to perform such a formal deduction syntactically and check its correctness.

Mathematicians have very divided opinions about Bourbaki's edifice, some of which have been quoted above. Outside a narrow group of enthusiasts who continue today to push Bourbaki's project further forward, there are very few research mathematicians and mathematics educators who are ready to use Bourbaki's volumes in their teaching and research. Nevertheless it seems to us clear that Bourbaki's *Elements* indeed "make explicit the essential general features, ingredients, and operations" (to use Lawvere&Rosebrugh's words) of a certain style and pattern of mathematical thinking that has played a major role in 20th century mathematics, and led to its many successes and further developments.

In Section **2.3** we overviewed some past attempts to implement the Hilbert-style axiomatic architecture in physical and biological theories. In the same Section we discussed some applications of the modern axiomatic method in CS and engineering. Recall that the application of an axiomatic approach in science beyond pure mathematics was Hilbert's original intention. In the beginning of the 20th century a significant number of scientists were enthusiastic about the prospects of the then-new Hilbert-style axiomatic approach in science, and attempted to introduce the axiomatic style of theory-building in contemporary scientific practice. Such attempts were never wholly abandoned but so far they failed to make axiomatic theory-building into a standard and commonly recognised scientific practice.

In chapter **3** we described two more recent axiomatic approaches that we called "novel": one related to category-theoretic foundations of mathematics first proposed in by William Lawvere in the 1960s (Section **3.1**) and the other related to Univalent Foundations, first proposed by Vladimir Voevodsky in the mid-2000s (but essentially based on earlier works by Per Martin-Löf that date back to the 1970s and 1980s),(Section **3.2**). Both these approaches deviate from Hilbert's conception of the axiomatic method and develop novel axiomatic architectures. In case of category-theoretic FOM, this deviation is implicit rather than explicit. Lawvere's early proposals made in his 1963 Ph.D. thesis [155] did not challenge Hilbert's notion of axiomatic method in its original form, but showed an alternative to Bourbaki's semantic version of this method. What we qualify as a deviation from Hilbert's axiomatic approach in Lawvere's work concerns primarily the concept of *internal logic* (of a category) that appears first (at this point without the name) in Lawvere's seminal 1970 paper [162] in which he formulates his axioms for topos theory (nowadays standard) and claims that "logic is a special case of geometry" (p. 329). This view on the relationships between logic and geometry is strikingly different from the one developed by Hilbert in his 1899 *Foundations of Geometry* [115] and in later works. While Hilbert (and after him Tarski in [276]) conceive of logic as a fixed external framework where various mathematical and non-mathematical theories are built via appropriate choices of axioms and their interpretations, the concept of internal logic makes logic into an element of a larger theoretical construction that determines special features of the given logic "from above". In our view this insight marks a significant departure from Hilbert's way of thinking about logic and axiomatic method.

In a different and more explicit form the same idea is realised in the Univalent Foundations. Unlike category-theoretic FOM, Univalent Foundations (UF) involve a Non-Hilbertian formal architecture in an explicit form. The mathematical basis of UF is Homotopy Type theory (HoTT): an interpretation of Intuitionistic Type theory due to Per Martin-Löf (MLTT) [186] in terms of homotopy theory [95]. MLTT is a Gentzen-style but not a Hilbert-style formal system, which is a conventional way of saying that it is rule-based rather than axiom-based (see **3.2.1**). Nevertheless we take the liberty of calling MLTT *axiomatic* in a broader sense than usual having in mind that Euclid's "axioms" in thier original form are also rules rather than axioms in Hilbert's or Frege's sense of the term. Making this terminological choice we also take into account Hilbert's remarks about the narrow and the broad senses of being axiomatic (see **1.3.2**).

UF in its original version presented in [95] on top of MLTT rules uses the crucial Univalent Axiom (UA), which among other consequences provides for a rigorous justification of the Equivalence Principle that has usually been informally accepted, but could not be rigorously proved in the Bourbaki-style set-theoretic setting. So in this standard form, UF is presented in the mixed Gentzen-Hilbert-style. However, more recent work on Cubical Type theory (CTT) as an alternative carrier for UF demonstrates that UF admits a pure Gentzen-style (rule-based) formal carrier that proves UA as a theorem.

The difference between the two formal “styles” is not only technical, but implies different logical semantics and supports different philosophical conceptions of logic. When Martin-Löf designed MLTT back in the 1970s, there were two major motivations behind his project, which were mutually related. *Theoretically*, he was motivated by mathematical intuitionism and constructivism, broadly conceived, which see logic, primarily, as an epistemic (rather than an ontological or metaphysical) tool, and focus on its justificatory capacities. More *practically*, Martin-Löf was motivated by the rising computer science and the opening possibilities of computational implementations of his designed formal system. Since then, a significant progress has been made on both accounts. Fragments of MLTT have been successfully implemented in a number of proof-assistants and program languages. The epistemic conception of logic, which until the late 1990s appeared rather marginal vis-à-vis the ontologically grounded and technically equipped semantic conception developed in Tarski’s steps, has been more recently revived and technically advanced by Dag Prawitz, Peter Schröder-Heister, and other people who called their area of study *proof-theoretic semantics* (see **3.2.2**).

The unexpected discovery of a homotopical interpretation of MLTT in the mid-2000s brought about HoTT/UF and boosted MLTT-related research. We have shown that in order to combine the intended proof-theoretic semantics of MLTT with the new homotopical semantics the former needs to be appropriately adjusted. In the standard 1984 version of MLTT *Martin-Lof:1984* all types admit alternative informal interpretations either as propositions or as sets (along with some other interpretations). The concepts of *homotopical level* of type and the cumulative h-hierarchy of types, which are construed with the homotopical interpretation (but also allow for a purely syntactic description), allows one to identify (-1)-types as propositions and 0-types as sets. Along with these familiar types, the h-hierarchy comprises higher types such as groupoids, 2-groupoids and so on up the homotopical ladder. Thus the homotopical interpretation reveals in MLTT a structure that was earlier left unnoticed, and provides it with an intuitive spatial (to wit homotopical) semantics.

The resulting semantics for MLTT (the key features of which are preserved in the CTT intended semantics) is not purely logical but comprises an extra-logical part. In order to justify the above claim it is sufficient to assume after Hilbert that geometrical concepts (in the broad sense of being geometrical that extends to homotopy-theoretic concepts) are not logical.

However, this claim can be made more precise by using a criterion of logicality based on the same homotopical semantics: we interpret as *logical inferences* only those formal derivations in HoTT, which restrict to judgements of the forms $p \quad P$ and $P \equiv Q$ where P, Q are (-1)-types aka *propositions*. This criterion of logicality squares with the Fregean concept of judgement as asserted proposition and the idea that truth is characteristically a logical concept. According to this criterion, to put it in Voevodsky's words, "logic lives at the h-level (-1)". Formal derivations involving higher-order types (i.e., types of higher h-levels) are interpreted here as geometrical constructions. In this setting, Lawvere's view of logic as a "special case of geometry" admits of a formal explanation, which is more rigorous and precise than the one given by Lawvere himself in the context of topos theory. Indeed, propositional and higher-order types in HoTT are not isolated from each other, and are operated with according to the same formal rules. The canonical procedure of *propositional truncation* associates a propositional type $\|A\|$ with any given higher-order type A. In line with the original proof-theoretic semantics for MLTT, terms (points) of A are interpreted here as particular proofs or truth-makers of the proposition $\|A\|$.

The way in which logic and geometry interact in HoTT/UF appears very unusual when it is judged against the standard picture presented in Hilbert's 1899 *Foundations of Geometry* [115] and Tarski's 1941 textbook on the *Logic and Methodology of Deductive Sciences* [276]. But at the same time it squares nicely with the more traditional Euclidean pattern of mathematical reasoning, in which one proves theorems by means of geometrical constructions. In our view, this is not just a historical curiosity, but a serious reason to rethink the concept of axiomatic method as well as the role and place of logic in mathematics and science.

4.2 Constructive Axiomatic Method[35]

In this Section we provide a concise informal specification of axiomatic method, which we suggest as an improvement on and enlargement of the received conception of axiomatic method stemming from Hilbert.

4.2.1 Motivations

The proposed conception is motivated by our analysis of past and recent mathematical axiomatic practices presented above. These motivations can be classified into two groups. Motivations of the *first* group are evidence that the performance of the received Hilbert-style axiomatic method in 20th century mathematical and scientific practice has been by far less successful than Hilbert and other early proponents of this method hoped for. The same line of evidence show that the axiomatic approach in 20th century mathematics was more successful when it deviated from Hilbert's standard. Here is our short list of such evidence:

- The controversial impact of Bourbaki's long-lasting attempt to introduce a semantic version of a Hilbert-style axiomatic method into wide mathematical practice, and the commonly recognised failure of this approach in mathematics education (**2.2.3**).
- Deviations of Bourbaki's axiomatic style from Hilbert's. We qualify Bourbaki's axiomatic method as a version of Hilbert's method but believe that Bourbaki's method could not become viable in practice without significant upgrades to Hilbert's original method (**2.2.1**).
- A similar remark concerns successful axiomatic theories in 20th century mathematics, which have been construed category-theoretically: the axiomatic theory of Homological Algebra due to Eilenberg and Steenrod [61], the axiomatic homotopy theory due to Quillen [218] and the axiomatic topos theory due to Lawvere [162] . Since these axiomatic theories are formulated informally (by a logician's standards) it is difficult to judge whether they are built in the Hilbert style or not. However, we have shown that at least

[35] See our [243] and [236].

Lawvere's axiomatic style differs significantly from Hilbert's in some essential aspects (**3.1**).

- The lack of significant success in continuing attempts to use the Hilbert-style axiomatic approach to build physical and biological theories. Here it is essential to distinguish between scientific theories and their logical reconstructions prepared for philosophical purposes (**2.3**). This lack of success can be contrasted with successful applications of the traditional Euclid-style axiomatic method in science of the past, e.g., in Newton's *Principia*.

Motivations of the *second* group provides us with more specific indications about the wanted upgrades and revisions of the received axiomatic method, which aim at making this method more adequate to current mathematical and scientific practices. We distinguish here two major motivations of this sort:

- Hilbert and Bernays's 1934 remarks, in which the authors point to limits of their standard "existential" axiomatic method and discuss the possibility of a more general axiomatic approach, which combines this standard method with the more traditional Euclid-style "genetic" method (**1.3.2**). Hilbert and Bernays call this hypothetical general method *constructive*. We borrow this name from Hilbert and Bernays (understanding the risks of terminological confusion) along with the idea.

- Cassirer's emphasis of the epistemic role of objecthood in the foundations of mathematics [41]; see **1.2.2**.

- The formal *Calculus of Problems* proposed in 1932 by Andrey Nikolayevitch Kolmogorov, which involves an extra-logical semantics (assuming the standard conception of the bounds of logic, which implies that every judgement is analysed into a proposition and its truth-value) [143] (see **3.1.3** and **3.2.2**). The notion of BHK-semantics, which is widely used in the current literature in philosophical logic, does not account for this extra-logical character of Kolmogorov's semantics.

- Ideas of Vladimir Alexandrovitch Smirnov concerning the possibility of using the "genetic method" for building scientific theories, which this author developed beginning in the early 1960s [266] (see **1.3**).

- Univalent Foundations of mathematics, which involve a non-standard Gentzen-style formal architecture, that effectively combines logical inferences with geometrical (to wit homotopical) constructions (**3.2.5**).

4.2.2 Axiomatic Theories

Here we describe a general concept of axiomatic theory relevant to the *constructive* axiomatic method. We deliberately leave this general description informal assuming that it can be formally specified and implemented in many different ways. We also show here how this general notion of axiomatic theory applies to examples discussed in other parts of this work.

By a *theory* we shall understand a fragment of theoretical *knowledge* construed and represented as a system of theoretical *objects* supplied with *rules* for (human) manipulations with these objects. By an *axiomatic theory* we shall understand a theory represented with a small number of distinguished *elementary objects* and *elementary rules*, which allow one to generate new objects from given objects and formulate new rules on the basis of given rules. We shall call this generation procedure *derivation* without assuming that it is purely syntactic, but also without providing it with any special, in particular, logical semantics in this general description. In special semantic contexts, such derivations can be called *deductions*, *productions* (**1.1.4**), *constructions*, and by other names. We use the term "object" here in the most neutral and general sense, which covers not only things like points, spaces, physical particles and living organisms, but also things like symbols, formulas, propositions and judgements[36]. We assume that the objects of a given theory are reusable and stable in the sense that they do not change or disappear when used to generate new objects, and rules are schematic in the sense that they are applicable repeatedly to different sets of objects [37] . The reference to

[36]Compare the quote from [188, p.19] given in **3.2.4 E** above.

[37]We make these assumptions here only in order to avoid a further unnecessary generalisation of our concept of axiomatic theory and don't use them in the following arguments. Weakening of these assumptions can be of theoretical interest. For example, in formal systems based on Girard's *Linear logic* theoretical objects, generally,

knowledge is essential in this description because many artificial systems (e.g. the game of chess) share with axiomatic theories the same basic formal structure. The reference to human manipulations is also essential and it should be understood broadly: it covers manipulations with symbols and concepts but can also extend to manipulations with physical objects (for example, in physical experiments, see below). The epistemic function of such manipulations will be discussed shortly in **4.2.3** and **4.3**.

Now observe that Euclid's theory of geometry, given in his *Elements* (as reconstructed above in **1.1**), the axiomatic theory of Euclidean geometry due to Hilbert (in the two versions of 1899 and of 1934), MLTT, HoTT/UF, and CTT all fall under the above broad notion of axiomatic theory. This notion may appear too general to be useful, but in fact it provides an interesting and unusual perspective on the aforementioned theories and their axiomatic architectures. The table below specifies elementary objects and rules in each case:

theory	**elementary objects**	**elementary rules**
Euclid	points and equalities	Postulates and Common Notions
Hilbert 1899	geometrical axioms	logical rules
Hilbert 1934	geometrical and logical axioms	modus ponens and substitution
MLTT	atomic and base types	MLTT rules
HoTT/UF	point, nat. numbers, UA	MLTT rules
CTT/UF	point, nat. numbers	CTT rules

Let us comment on this table line by line. We do not list straight lines and circles among the elementary objects of Euclid's geometry because in the *Elements* straight lines and circles are produced from points with Postulates 1-3. Beware that in order to produce a line with Post. 1, one needs *two* different points. Equalities (such as equalities of radii in a circle) are elementary objects of propositional type. Objects of this type are operated on with Common Notions aka Axioms (recall that Euclid's Axioms are rules). A rigorous formal reconstruction

are not stable in the specified sense [85]; formal systems with learning capacities may require using rules of a more dynamic character than the standard fixed schematic rules. We leave such cases out of the scope of the present work.

of Euclid's geometry made in line of the *Elements* would require considering additional elementary objects and rules, which are not fully specified in the *Elements*, such as congruences and rules for constructing intersection points of lines and circles. So the specification of objects and rules given in the table is not meant to be complete. How these two sets of objects and rules (geometrical and propositional) form a single theory in this case was discussed above in **1.1.4**.

Hilbert's axiomatic theory of Euclidean geometry is present in the table in its two different versions: in the semi-formal version presented in Hilbert's *Foundations* of 1899 [115], [107] and in the further formalised 1934 version sketched by Hilbert and Bernays in [113], [114]. In both cases Euclidean geometry is construed as a system of *sentences* (that express certain *propositions*) some of which are fixed as axioms while others are deduced from these axioms according to logical rules, and called theorems. In the 1899 version of the theory the axioms are formulated informally and the logical rules are not specified explicitly. By contrast, the 1934 version of this theory involves a symbolic language with precise syntax, which is used to spell out the geometrical axioms and for the formal specification of the logical rules. Another specific feature of the 1934 version is the presence of *logical* axioms, i.e., tautologies, which help Hilbert to reduce the number of (logical) rules used in this theory to a minimum. In this latter case the list of rules is definite and complete.

Hilbert's axioms and theorems are *objects* —correspondingly elementary and derived —in the sense explained above. These propositional objects should not be confused with primitive and defined theoretical objects in Hilbert's sense, which under the intended interpretation of Hilbert-style axiomatic Euclidean geometry are points, straight lines, planes and other geometrical objects defined in their terms. How do these familiar geometrical objects enter into the picture? The formalised 1934 version of Hilbert's theory is more helpful for answering this question. It makes it explicit that formulas that express Hilbert's axioms and theorems are built from symbols taken from a fixed alphabet. The construction of well-formed formulas from the symbols is controlled by a special set of rules. (We did not show these details in the above table.) This is a part of the logical machinery used in this geometrical theory, which is not specific to this theory. Certain elements of symbolic constructions (formulas) built in this way are used to refer to geometrical points (this is the only type of primitives in this version of the theory). Thus the geometrical points and other geometrical objects, which are construed in terms of points and relations between points, enter into the theory by being *referred to* with special elements of theoretical sentences.

As we have explained already in **1.2.1**, by "points" one can understand in Hilbert's axiomatic geometry different things, not necessarily the usual Euclidean dots. But however this theory is interpreted, it manipulates sentences "about" points (and other geometrical objects defined in terms of points) but does not directly manipulate points and other geometrical objects. This is a key difference between Euclid's theory of geometry and Hilbert's theories of Euclidean geometry. This is also the reason why Hilbert and Bernays call their version of the axiomatic method "existential": their approach involves the assumption according to which points and other geometrical objects defined in terms of points *exist* in a ready-made form, in some ideal sense. Recall that in Hilbert's view, the formal consistency of a given axiomatic theory is a sufficient condition for claiming such an ideal existence of its objects.

MLTT is a constructive derivation system, which is essentially determined by its rules rather than by its generators, which in the above table we call *elementary objects*. MLTT allows one to declare any number of *atomic* types A_i, which are called atomic in the sense that they remain unspecified but can be used for constructing (deriving) other types, which in our proposed terms we call *complex objects*. Such a declaration has the form $A : TYPE$, where $TYPE$ is the "big" type of all types (which for simplicity we assume here to be unique [38]) and qualifies as a *judgement* (see **3.2.3**) . Given atomic type A, one can declare for it a number of its *terms* via judgements of form $a : A$. Judgemental identities (equalities) of two levels of the forms $A \equiv B$ and $a \equiv_A b$ are also available in MLTT as elementary objects for further derivations.

Base types like atomic types are declared without using earlier declared types, but unlike atomic types they are provided with special information, which allows one to think of such types and their terms as concrete theoretical entities. This information is given via specific rules that regulate derivations with these

[38] MLTT allows one to declare a hierarchy of such "big" types called in such contexts *universes*.

types. Standard examples include the empty type $\emptyset$, the unit type $\mathbf{1}$ and the natural numbers type $\mathbb{N}$. However, this list is not rigidly fixed and can be modified and extended without changing the core of MLTT.

HoTT preserves all the features of MLTT mentioned above, and allows one to think of MLTT types and their terms as (homotopy) spaces and their points. HoTT supports an additional structure, namely the cumulative homotopical hierarchy of types. In order to emphasise the importance of this structure in HoTT, we have singled out among other base types the unit type called in HoTT Point $\mathbb{P}$, and the natural numbers type $\mathbb{N}$; $\mathbb{P}$ allows for an inductive definition of the homotopical hierarchy, hence the presence of $\mathbb{N}$ in the same cell of the table. With respect to regular points (of any space), $\mathbb{P}$ can be thought of as the single "generic" point.

As soon as HoTT is used as the basis for Univalent Foundations (UF) it comprises, on top of the MLTT rules, the Univalence Axiom (UA), which is a proposition, i.e., a (-1)-type, evidenced by a truth-maker (truth-value "true") stipulated by fiat. So in this case we should count UA as an extra elementary object of the theory. From a formal point of view such an introduction of a Hilbert-style axiom into the rule-based Gentzen-style formal architecture of MLTT/HoTT may appear strange; the reason why this axiom has been introduced is explained in **3.2.5**. In Cubical Type Theory, where UA is proved as a theorem (so in this case it is a derived and no longer an *elementary* object), the rule-based character of the formal architecture is restored —at the price of modification of the MLTT-rules and some of their meaning explanations. As we have already explained, this was done primarily for a computational aka "constructive" reason rather than for an aesthetic reason: in CTT-based UF, the equivalences that verify the universal univalence property are effectively constructed rather than simply stipulated, as is done in the MLTT-based UF presented in [95].

Following Hilbert and Bernays, we call an axiomatic theory *constructive* when the rules of this theory apply both to propositional objects (propositions) and to non-propositional objects such as geometrical points and straight lines in Euclid's *Elements* or judgements of the form $a : A$ in HoTT, where A is a non-propositional type. Since we assume that theories represent knowledge and, in addition, assume that in each case at least a fragment of the represented knowledge is propositional, we hold that every theory comprises propositional objects in some form. Thus our concept of constructive axiomatic theory broadens and generalises the standard concept of Hilbert-style axiomatic theory. Hence standard axiomatic theories also fall under our concept of constructive theory even if they don't comprise a properly constructive fragment in the relevant sense of the word. This may lead to some terminological confusion but at the same time such a situation is common in mathematics and logic. For example, in everyday life the expression "set of points" refers to a group of at least two different points, but in mathematics we consider sets with one element and with no elements. Therefore we shall not further discuss this terminological point. It is more essential to keep in mind that the term "constructive" in mathematics and logic is heavily overloaded, and refers to many different properties. The concept of being constructive that we study in this work following Hilbert and Bernays's hints is in many ways related to other concepts bearing the same name. We leave a study of such connections out of the scope of the present work.

4.2.3 The Method

In order to describe the method of theory-building which corresponds to the notion of axiomatic theory sketched above, we shall point to relevant extensions of the received axiomatic method and explain their epistemic functions and advantages [243]. As we have already remarked, non-propositional theoretical objects like geometrical points are involved in Hilbert-style axiomatic theories in a roundabout way via propositional objects, namely, as referents of certain terms in formulas that express sentences "about" these non-propositional objects. According to Jaakko Hintikka's understanding of Hilbert-style axiomatic method [39]

> "What is crucial in the axiomatic method [...] is that an overview on the axiomatized theory is to capture all and only the relevant structures as so many models of the axioms." [118, p. 72]

[39] We take Hintikka's view on axiomatic method to be an accurate interpretation of Hilbert's view. However this historical point plays no role either in Hintikka's argument or in our following argument.

Where do these relevant structures (and hence models) come from? Hintikka gives the following answer:

> "The class of structures that the axioms are calculated to capture can be either given by intuition, freely chosen or else introduced by experience" (ib., p. 83)

As Hintikka emphasises in the same paper, by mathematical intuition he means not an intellectual analogue of sense-perception but an "active thought-experiment by envisaging different kinds of structures and by seeing how they can be manipulated in imagination" (ib., p. 78).

We agree with Hintikka that the "active thought-experiment" plays a key role in mathematics just as real experimentation plays a key role in physics and other natural sciences. However, we disagree with Hintikka's view according to which thought-experimentation with mathematical structures can proceed only informally outside axiomatic theories, motivating one's choice of axioms. As far as Hilbert-style axiomatic theories are concerned, Hintikka is right. In the Euclidean geometry presented axiomatically in Hilbert-style, there is no room for the thought-experiments referred to by Hintikka. But the case of Euclid's geometrical theory presented in his *Elements* is different. The "Euclidean geometrical intuition" is not just an uncontrolled activity of our imagination related to our experiences in physical space and time but a constructive activity controlled by formal rules that are (at least partly) explicitly formulated in Euclid's *Elements* as geometrical Postulates. This control should not be understood only in negative terms as a formal restriction on what can be eligibly imagined within the given theory. The complexity and richness of geometrical constructions by ruler and compass, which remain surveyable by humans, far exceeds the complexity of spatial constructions produced in bare imagination (whatever this might mean). At least when we talk about mathematically relevant spatial constructions and leave aside artistic practices, this claim is obvious.

Thus in Euclid's *Elements* thought-experimentation with mathematical objects is not only a preparatory step that helps one to design a theory but a proper part of Euclid's theory itself. It can be argued that this example is outdated and irrelevant to today's mathematical practice because the"Euclidean paradise" was irreversibly lost after the rise of non-Euclidean geometries and modern abstract mathematics. It is plausible that Hilbert himself assessed the situation in this way in the late 1890s when he designed his version of the axiomatic method. Some proponents of this method did this later very explicitly [76], [199]. However, the new historical perspective from the early 21st century imposes some essential corrections of this view on mathematics and its history. The Euclidean pattern of constructive mathematical reasoning remains quite robust in the mathematics of the 20th century and in today's mathematics (see (см. **2.2.4**)). One does not need to refer to Euclid to notice that the Univalent Foundations also supports a form of active thought-experimentation with spatial imaginary objects, which is similarly controlled by exact formal rules. The claim that this pattern of mathematical thinking is outdated is definitely not justified.

Hintikka describes the axiomatic method as a way to control and stabilise intuitive constructions obtained by thought-experimentation or real physical experimentation. Up to this point we are wholly with Hintikka [40]. However unlike Hintikka, we don't think that the received Hilbert-style axiomatic method does this job properly. Insofar as one thinks of theories as systems of sentences, the received axiomatic method appears adequate: it shows how to organise such a system in a convenient way by taking some of these sentences as axioms and deriving other sentences as theorems via truth-preserving logical inferences. However when one assumes, after Hintikka and the proponents of the *semantic view*, that *models* of mathematical and scientific theories are epistemically significant, the received axiomatic method no longer appears to be adequate and sufficient. We don't deny that there is a sense in which a deductively organized system of sentences can control and stabilise semantic structures that make these sentences true. But we claim that such propositional control is insufficient both practically and theoretically.

On the *practical* side, the continuing long-term experience of building mathematical theories *à la* Bourbaki makes it evident that the idea of organising mathematical theories into Hilbert-style axiomatic theories plays only a general normative role in this approach. As we have stressed above, the "real" Bourbaki-

[40] The case of physical experimentation is discussed below in **4.3**.

style proofs are not formal logical deductions from Bourbaki's axiomatic set theory, even if all these proofs are *theoretically* representable in this form (see **2.2.1**). The formal represent*ability* "in principle" of Bourbaki's semi-formal proofs allows one to think of these proofs as somewhat imperfect realisations of the corresponding ideal Hilbert-style formal proofs obtainable via the full formalisation. We call this target fully formalised proof *ideal* because apart from some specially chosen toy cases, such full formalisation is a theoretical possibility (justified by informal mathematical reasoning), but the possibility of building a"real" syntactic object, which can be effectively surveyed by a human, is not a practical one [41]. Recall that Hilbert called such syntactic objects "real" mathematical objects and qualified as "ideal" all other mathematical objects (**1.2.3**). The experience of using the Hilbert-style axiomatic method in 20th century mathematics turns this picture of Hilbert's upside down.

In this situation the only epistemic role of such formal proofs is to provide an insurance that proofs and theories presented in Bourbaki semantic style can be judged by the same logical criteria as their corresponding formal counterparts. Since these formal counterparts are unfeasible, the available criteria are very unspecific. One can apply to a given semi-formal proofs all known general meta-theorems obtained for formal proofs. But one cannot in such a situation effectively formally check a semi-formal proof. One can argue (counterpositively) that if a given semi-formal proof is <u>not</u> translatable into a formal proof in the given foundational setting then it is not valid. So formal translatability may count as a necessary but not sufficient criterion of validity. In practice, however, even this weak criterion is usually understood flexibly: an interesting mathematical proof that is not formalisable, say, in ZFC, can be validated by showing that it is formalisable in ZFC strengthened with a number of inaccessible cardinals. Notice also that judgements to the effect that certain classes of informal proofs are formalisable are always made on the basis of informal logical and mathematical arguments. To repeat, we don't deny that such arguments can provide relevant

[41] Whether electronic computers can help in this situation remains a research topic but the existing experience with computer-assisted mathematical proofs suggests rather a negative answer because formal theories built in Hilbert's style are less apt for computational implementations than Gentzen-style formal theories like HoTT/UF; see **3.2.5 A-B**.

and interesting information about informal and semi-formal theories and proofs, but want to stress that these arguments cannot give answers to many important specific questions such as whether or not a given mathematical proof is valid.

Thus the Hilbert-style formal axiomatic architecture *grounds* in a certain sense, rather than controls and stabilises, model-based Bourbaki-style mathematical reasoning. This reasoning is controlled by and stabilised into patterns, which are practically learned with examples and used in mathematical practice without being formally described and explained in the textbooks. As we have shown above such practical patterns of set-theoretic reasoning preserve a visible trace of the traditional pattern of geometrical reasoning that dates back to Euclid.

As far as category theory (CT) is seen as a Hilbert-style axiomatic theory in the vein of Lawvere's early proposals (see **3.1.2**) the situation in category-theoretical mathematics is similar. The fact that CT can be grounded in this way, bypassing set theory, which was discovered by Lawvere in 1960s, is an important achievement [42] that allows those mathematicians who use CT as a base language in their teaching and research to feel more secure, without being concerned about the standard set-theoretic foundations. However CT-based mathematical reasoning used in practice, like Bourbaki-style set-theoretic reasoning, is not formal but model-based, and proceeds in a constructive mode: one builds and studies various category-theoretic constructions, e.g. "constructs" functor category $[A, B]$ from given categories A, B, etc. Such practical patterns of category-theoretic reasoning, which include proofs by *diagram chasing*, are at least as remote from Hilbert-style formal axiomatic reasoning as patterns of Bourbaki-style set-theoretic reasoning, or are perhaps even more remote. It is important to stress here that this concerns not only heuristic methods that allow one to make useful conjectures, but also proof methods that are used in textbooks and research papers. So the idea that the Hilbert-style formal axiomatic representation of mathematical reasoning is fully responsible for the justification of ready-made results, while model-based

[42] Recall, however, that this way of grounding CT has been objected to by Feferman, who argues that the foundations of CT involve a primitive concept of class or collection, which in Feferman's view is not properly formalised in Lawvere's setting [67]. The issue is not technical but conceptual and comes down to different interpretations of the Hilbert-style axiomatic method (**1.3.1**).

reasoning wholly belongs to the "context of discovery" of these results, simply does not stand against current mathematical practice —no matter whether we are talking about category-theoretic mathematics or about the more old-fashioned set-theoretic mathematics.

The fact that model-based mathematical reasoning proceeds without explicit formal rules makes formal proof verification impossible and makes learning modern mathematics more difficult. Nevertheless, some mathematicians may prize this informal style of reasoning and proof as an expression of intellectual freedom in mathematics. In our view, one does not face here a difficult choice between security and freedom. Some researchers rightly describe Hilbert's axiomatic approach as "axiomatic freedom", pointing to Hilbert's approval of choosing axioms for mathematical theories freely unless they lead to a logical contradiction. Noticeably, Hilbert did not allow for a similar liberal treatment of logical rules, which, recall, in his view were the only type of formal rules appropriate in axiomatic theories. When we insist that formal rules for model-based reasoning are appropriate and even necessary, we don't mean to restrict Hilbert's axiomatic freedom. On the contrary, we aim at extending this axiomatic freedom into a new dimension which has to do with rules rather than axioms (in Hilbert's sense). Existing experience of studying Gentzen-style formal system shows that "playing with rules" can be just as productive and fruitful as Hilbert-style "playing with axioms". A possible research strategy here is to explore the possibilities of building mathematical theories on the basis of various alternative logical calculi [292]. However, we do not restrict our proposed approach to rules that admit of a logical semantics. As the examples of HoTT/UF and CTT make it clear, relevant Gentzen-style formal systems can also admit an extra-logical semantics. As we have already stressed, the case of Gentzen-style systems with extra-logical semantics is the main motivation behind our proposed concept of a *constructive* axiomatic method.

On the *theoretical* side, we submit that the popular view according to which formal derivations in Hilbert-style axiomatic theories provide the best possible justification of mathematical results (notwithstanding the fact that such formal proofs are not available in a palpable material form) is not justified. Following Prawitz, we hold that the concept of proof is essentially epistemic, so a conception of proof which is not epistemically accessible for some reason is, in our view, inconsistent (see **3.2.2**). When partial epistemic access to a formal proof is provided via some additional means —as happens when the claim of the existence of formal proof is supported with the usual informal mathematical arguments —then the quality of this access is a factor that determines and limits the epistemic force of the formal proof. In other words, a formal proof and the means of epistemic access to this formal proof should be regarded as elements of the same mathematical proof. When epistemic access to formal proofs is weak (as it really is in current mathematical practice), there is no reason to see the formal proofs as epistemically optimal, ignoring the question of how these proofs are accessed or can be accessed by epistemic agents, i.e., by humans.

Independently of the above, we reject the epistemological view according to which *all* valuable knowledge provided by mathematical and scientific theories is propositional, aka knowledge-*that*, while procedural knowledge-*how* can be at best auxiliary and for that reason should not be seen as a proper fragment of ready-made theories. Euclid's geometrical *problems* have an independent epistemic value that does not reduce to the possible use of previously solved problems in proofs of his *theorems*. Indeed, the knowledge *how* to perform some desired construction C can be used as evidence for or a truth maker of the statement of certain theorems. But one's knowledge of how to perform C can also be of independent epistemic value in applications, as in the case of one's knowledge of how to divide a straight line into two equal parts by ruler and compass. As we have stressed in Euclid's *Elements* problem-solving involves using previously proven theorems just as much as theorem-proving involves using some previously solved problems (**1.1.4**). Our analysis shows that today's mathematics is similar in this respect. Knowledge *how* to build models and *how* to prove theorems is at least as epistemically valuable as the proved theoretical statements. This is why such bits of procedural knowledge should count as full-fledged (albeit not independent) fragments of ready-made theories, rather than as auxiliary elements that fully belong to the *context of discovery* of these theories. Procedural knowledge plays a key role, namely, in the *context of justification* of mathematical theories, because any theoretical justification has a procedural character. An important point that marks a difference between our proposed constructive approach and the received

Hilbert-style axiomatic approach is that such theoretical procedures do not reduce to logical procedures (even when such procedures have some logical impact). In constructive axiomatic theories the procedural knowledge is represented as systems of formal rules, which as usual are presented syntactically, provided with a default semantics (in HoTT this is the homotopical semantics), and only then interpreted in models. As we have already explained, such a formal architecture allows for (re)constructing the models from their simple elements.

Thus we can see that the "propositional control" on model-building provided by the Hilbert-style axiomatic method is insufficient and needs to be complemented with a more direct constructive control with formal rules applied to theoretical objects themselves rather than to sentences that tell us something about these objects. This is exactly what the *constructive* axiomatic method provides. Trying to describe this method in a few words in the vein of Hintikka [118], we can summarise the above as follows. Having in view an informally described theoretical structure one needs to distinguish not only its essential properties in the form of propositional axioms but also its simple non-propositional elements (such as points) from which the target structure can be reconstructed genetically, i.e., built according to appropriate formal rules (which must also be established). The question of how the propositional layer of the theory will relate to its non-propositional "objectual" layers hardly has a universal answer. HoTT/UF provides us with a powerful scheme and technique for such an arrangement, but since we are talking now about the constructive axiomatic method in its full generality, we should not assume that this particular arrangement is unique.

4.3 The Constructive View of Theories[43]

In the above discussion on the constructive axiomatic method, we referred only to mathematical theories. Such a focus on pure mathematics has been unavoidable in this discussion because known applications of the axiomatic method beyond mathematics are sparse. Hilbert's proposal to apply the axiomatic method in physics and other natural sciences led to many interesting attempts

[43]See [245] and [248].

to realise this project, but so far such attempts have had no significant impact on today's mainstream science and current scientific practices. We believe that this state of affairs is caused by the fact that the standard Hilbert-style axiomatic architecture is inadequate to scientific theories, and argue in what follows that the constructive axiomatic architecture described in the present work is more adequate.

The following arguments are preliminary and serve us as a motivation for further research, rather than as a justification of ready-made results. These arguments and considerations belong, primarily, to the philosophy of science, but we believe that in the longer term they may also have practical significance. Today's science massively and systematically applies computer technologies for the representation, storage and analysis of scientific *data*. The storage, dissemination and revision of scientific *knowledge* is presently less affected by the digital information technologies, so traditional forms of knowledge representation and communication such as conference talks, research papers and science textbooks so far largely remain in use even though the implementation of these forms already involves modern information technologies (as in online conferences, electronic publishing, etc.) However, one may expect that in a foreseeable future computer-based knowledge representation technologies (KR) will be more widely used in scientific practice, including science educational practices. In order to design a KR system capable of representing scientific knowledge one must rely on some reasonable assumptions about the logical structure of this type of knowledge. This is where the old philosophical question about the logical structure of scientific knowledge and scientific theories becomes important practically. As we argue elsewhere the existing KR technologies are not quite apt to perform this task, but can be improved by using the logical approach outlined above in the present work [146], [145].

In (**2.3.3**) above we briefly described the "revolution in Stanford" that gave rise to the *semantic view* of scientific theories. Following Halvorson [98] we consider the debate between the *semantic view* and the *syntactic view* on scientific theories to be mostly a historical issue. Since Patrick Suppes and other pioneers of the new semantic approach used Tarski's formal set-theoretic semantics, which was not available to researchers of the older generation who pursued the "syntactic" approach, it is fair to say that the semantic approach was more advanced at this point in history. However, we also noticed that the semantic turn left behind some interesting ideas associated with the "syntactic" approach. This concerns, in particular, the conception of logic as a tool for determining the best available evidence for a given scientific assertion, which was developed by Morris Cohen and Ernest Nagel in the 1930s. [46] (**3.2.2**). This conception of logic, which emphasised its justificatory role, was in fact at odds with the Hilbert-style formal axiomatic approach that the authors attempted to apply for their purposes. In fact, no formal logical technique supporting the justificatory conception of logic existed at that time. Since the emergence of MLTT/HoTT, this situation has changed. The constructive axiomatic architecture of theories described in this work in its logical part is also motivated by the justificatory conception of logic.

Recall that the semantic view of theories is also known under the name of the *non-statement* view. Our proposed *constructive* view of theories also qualifies as a non-statement view. Along with the proponents of the standard semantic view, we reject the notion according to which a scientific theory is a deductively organised system of sentences (that express certain statements). However we provide a different answer to the question 'What are the fundamental constituents of a theory (besides its sentences)?". Proponents of the standard semantic view answer "models", having in mind Tarski-style model theory, on the one hand, and various informal concepts of being a model (e.g., model of a physical phenomenon or a chemical process) that are used in many scientific disciplines [275, p. 17-20], [253], on the other hand. We share the view that models are fundamental constituents of scientific theories. But in order to stress our divergence from the standard semantic view we give to the same question a different answer: *methods*.

The view according to which methods belong to the core of theories rather than serving as external auxiliary tools is not new, and dates back at least to René Descartes. However, until recently this view of theories had not been developed in and supported by a formal logical technique. In fact, much remains to be done in order to implement such a technique in practice. Nevertheless we are already at this point in a position to sketch a formal structure of theories, in which methods are essential rather than auxiliary elements.

We accept what proponents of the standard semantic view say about the epistemic functions of models in scientific theories. But then we ask the more technical question of how these models are determined and controlled. Since we are talking now about formal representational frameworks applied across different theories and different scientific disciplines we expect to receive a general answer to this question. We are pointed to Tarski-style model theory. We now notice that in Tarski's setting, models are essentially determined by the formal *sentences* that they model, or more precisely, by their role as truth-makers of these sentences. So a closer examination reveals that under the *non-statement* (aka semantic) view of theories proposed by Suppes and others, models, which form the bulk of scientific theories, are still determined and controlled by theoretical *statements* (axioms and theories) and nothing else. The fact that the same theory (identified with a class of models) can be so determined by more than one axiom system, which was stressed by Suppes, is important, but it does not solve all relevant problems of this formal representational framework.

In order to show this, we may reiterate our above argument according to which purely propositional (sentential) control over models is not sufficient and needs to be reinforced by a more direct control via application of constructive rules and operations that apply to non-propositional objects. Earlier we formulated these arguments having in mind mathematical theories (**4.2.3**). We now put forward similar arguments relevant to scientific theories, in which case material experiments (along with thought-experiments) and empirical measurements and observations play a major epistemic role. A scientific *experiment* is an artificially designed and produced model situation M, in which one checks whether or not everything occurs as tested theory T predicts [44]. More precisely, we have here a theoretical model M_T that implies the prediction, and its experimental implementation M_E. The two models are compared via measurements. In case the experimental results (i.e., the measurement outcomes) satisfy the prediction the experiment becomes supporting evidence for T; otherwise it serves as falsifying evidence.

The logic of evidence-based reasoning relevant to existing scientific practices is presently a vivid area of inter-disciplinary study [281] that we cannot

[44]It goes without saying that this is a very simplified picture of scientific experiment, which however is sufficient for our present purpose.

cover systematically here. So we shall point only to one specific aspect of the above setting, which concerns the formal architecture of theory T and the structure of M_T and M_E. Since we are talking now about designing, setting-up and performing scientific experiments, it is clear that models M_T, M_E need to be *constructed*, i.e., built according to certain rule-based procedures, and not only described propositionally in terms of their desired properties and relations — even if such propositional descriptions may play a role in both cases, particularly at the design stage. This remark concerns theoretical model M_T as well as the experimental model M_E, since the former is supposed to serve as a theoretical prototype of the latter. Both these constructions need to be reproducible: this is where the schematic rule-based character of these constructive procedures is essential. Such well-determined constructive procedures (both theoretical and experimental) or, more precisely, *recipes* of procedures applicable repeatedly in various contexts to various data, are known in science under the name of *methods*. Methods can be specified linguistically and in some cases symbolically in a syntactic schematic form as *algorithms*. This allows one to apply a given method repeatedly and to think of it as an abstract entity. Such specifications are commonly called *descriptions* (of methods), but their logical form is in fact different: they *prescribe* what to do (to perform certain actions) rather than describe what is the case. Prescriptions, unlike descriptions, do not express propositions and bear no truth-values. It is a trivial remark that any prescription can be linguistically rendered as a propositional description (of a *method* stipulated as an abstract entity). This simple linguistic and logical trick is universal and unspecific; it doesn't tell us anything new about the modal nature of methods and algorithms. However, in certain contexts it can blur the modal difference between descriptions and prescriptions, and thus conceal this modal nature.

The case of *thought-experiments*, which has been also touched upon above, has special relevance to the present discussion because it intermediates in a sense between theoretical and experimental models. Without making stronger claims that would require more accurate verification against the history of science and current scientific practice, we say only that a thought-experimental model M_{TE} based on theoretical model M_T can give one a good idea of how to design, produce and interpret a physical experiment that tests theory T, i.e., obtain what we call here the experimental model M_E. Examples of thought-experiments first conceived of theoretically and later realised as real laboratory experiments are abundant in the past and in today's physics. For a recent example see [204], where the authors report on a successful experiment that realises the famous Schrödinger's Cat thought-experiment first proposed back in 1935.

The above argument is also applicable to theoretically-laden empirical *observations*, which have the same formal structure as experiments. Think about the first successful observation of gravitational waves made in September 2015 at the LIGO detector [1]. It involved an underlying theory, viz. General Relativity, and tested its theoretical prediction made by Albert Einstein back in 1916 [130]. This observation equally involved a lot of specific theoretical and technical design that had the same character as the experimental design in any area of science. Characteristically and understandably, the observation of gravitational waves made with the LIGO detector is commonly called the "LIGO experiment". Unlike the case of a typical experiment, one could not schedule in advance any particular date and time when the LIGO equipment will bring the desired empirical evidence (either in support or against the tested theory), because it depended on events that were out of human control. Among the events that allowed for the successful observation of gravitational waves with LIGO in September 2015 was the merger of two black holes of about 30 solar masses each, which occurred more than one billion years earlier (by the cosmological time) in a remote part of the Universe, and produced the observed wave packet. The lack of control over observed objects and events is a distinctive feature of observations that distinguishes them from experiments. This feature of observations does not, however, affect the general formal structure that observations share with experiments. We focus now only on this formal structure.

Returning now to the issue of the formal representation of scientific theories, we remark that the only kind of methods that the Hilbert-style axiomatic architecture can represent are *logical*, and even more specifically, *deductive* methods. By the formal representation of a (logical) deductive method we mean here a syntactic scheme that represents a derivation or a class of derivations in a Hilbert-style axiomatic theory. Given the fact that formal logical methods play no significant role in science, as it has been practiced at least since Galileo's times (albeit in some earlier historical patterns of doing science, which are colloquially called *scholastic*, such methods were applied systematically), it is not surprising that this axiomatic architecture is not apt for representing current scientific theory adequately. As we have already stressed, by making such claims we do not mean to dismiss all normative epistemological arguments in favour of a logical approach in science simply by pointing to the fact that today's science does not fit into the relevant epistemic norms. Our project, instead, is to reconsider the role and the very conception of logic in the context of today's mathematics and science, and describe new available logical techniques that could be more successfully used in science as we know it.

The Bourbaki-Suppes *semantic* version of the axiomatic method allows one to disregard syntactic details —and along with syntactic details also disregard most logical details —and focus on models and structures. As in the case of Bourbaki mathematics, in science this move allows one to use the epistemic norms enforced by the Hilbert-style formal axiomatic method not directly, but in a somewhat transcendental manner: one works with a semi-formal theory that does not fulfil these norms explicitly, and provides this theory with additional arguments showing that the given theory has a fully formalised version (or many such formalised versions), which does fulfil the norms. It should be noted that however controversial Bourbaki's project may be, it obviously had a significant impact on 20th century mathematical practice; in particular, it helped to formulate alternative approaches in the "practical" FOM, including the category-theoretic foundations and the Univalent Foundations. The axiomatic representation of scientific theories in Suppes-style, in its turn, to date plays no role in scientific practice beyond the logically-oriented *philosophy* of science. If the "transcendental" application of epistemic norms just explained can be described as a practical compromise then it is fair to say that this particular compromise has been so far (partly) successful in pure mathematics, but not in science.

The problem as we see it is that the Suppes-style semantic method of building axiomatic theories in fact offers very little on top of its so-called "syntactic" predecessor. This method justifies the neglect of inessential logical details, which is a part of common scientific practice anyway, and (quite rightly in our view) focuses on models rather than theoretical statements. But it does not propose any new formal technique for working with these models and applies in its stead the semi-formal Bourbaki-style syntax. The idea of using set theory in the role of a default source of models, which works out in pure mathematics to a certain degree, apparently wholly fails in existent science, notwithstanding the aforementioned Suppes' argument according to which a model of every scientific theory can be always rebuilt in a set-theoretic form [275, p. 17-20]. At least the continuing efforts of using the Suppes-style representational framework in science don't provide us so far with any compelling evidence to the contrary.

In this contextь our proposed notion of *constructive* axiomatic theory appears as a more adequate candidate framework for the formal representation of scientific theories. Recall that the difference between the received Hilbert-style axiomatic theories and constructive theories in our sense is that the latter, generally, involve sets of rules applicable to non-propositional objects. Whether or not such rules qualify as logical is an interesting and deep question, which we can however leave aside for now. For present purposes It suffies to remark that one who wants to call these rules logical needs to broaden her conception of logic beyond its usual scope [266]. As a matter of terminological convention we stick here to a narrower and more standard conception of being logical that qualifies operations with non-propositional objects (such as geometrical operations made with ruler and compass) as *extra*-logical (save when such operations are purely syntactic, as in the case of building words and formulas from a given alphabet of symbols).

The key idea here is that formal derivations in a rule-based (aka Gentzen-style) constructive axiomatic theory can be used for the formal representation of extra-logical operations, and hence of scientific (extra-logical) methods. This concerns primarily *theoretical* methods associated with what we have called above theoretical models of a given theory such as M_T. An appropriate concept of model, which extends Tarski's notion of a model of a formal theory, was discussed in **3.2.5 C** in the context of Homotopy Type theory (HoTT). Recall that is such contexts one needs to distinguish between models of a given theory, on the one hand, and its default semantics, on the other hand. In HoTT, the default semantics is homotopical. (A similar distinction is made in the standard Tarskian setting, where the default semantics is logical.) We assume that every constructive axiomatic theory has a default semantics. When elementary rules and elementary objects of constructive theory T are interpreted in model M_T, formal derivations in T based on these formal rules and these formal objects are interpreted as constructions in M_T. This allows for building such contentful constructions genetically from simple elements (which interpret formal elementary objects of the corresponding theory) according to fixed formal rules. Insofar as theoretical model M_T is construed in this way, it can support a thought-experiment M_{TE} designed on the same formal basis (i.e., with the same constructive rules and generators, but described in a way that makes them appear less abstract), which in its turn can serve as an instrument for real experimental design that is eventually implemented in experiment M_E, as shown in the diagram below:

$$T \xrightarrow{m} M_T \xrightarrow{t} M_{TE} \xrightarrow{e} M_E$$

where the first arrow represents modelling, the second represents the design of a thought-experiment, and the third represents the experimental design. As the above diagram suggests, the language of T remains in this case interpretable in experiments designed for testing this theory. It goes without saying that the order of steps shown in the above diagram does not, generally, reflect the chronological order in which a theoretical and experimental scientific research proceeds. As usual, we assume here that the formal representation becomes relevant only when a given theory is already sufficiently mature and that it serves, primarily, to give this theory a stable, reproducible and well-grounded form.

Let us now discuss from a more general viewpoint the question of whether or not theoretical methods qualify as proper elements of the corresponding theories. In our view, this question should be answered positively. Classical Mechanics as presented in Newton's *Principia* allows one to build a mathematical model of the trajectory of a moving canon ball and of the Moon's orbit. Einstein's General Relativity allows one to model a black hole and model the effect of merging of black holes, which was experimentally observed with the LIGO detector in 2015. Even if, in scientific practice, such models are not built using formal logical methods, the constructive character of these models is reflected in the fact that they are built with special mathematical procedures rather than somehow obtained in a ready-made form, so that it remains only to check that they satisfy all relevant axioms and theorems. The history of science and current scientific practice provide no justification for the idea according to which general methods of building such models are auxiliary theoretical means of a sort, which are developed on the basis of corresponding theories without belonging to the core contents of these theories. In our view, such a way of distinguishing between theoretical contents and theoretical methods is an artefact of inadequate logical reconstructions of scientific theories, which are in need of revision. Within our proposed approach we don't make this distinction in the same way. We count the procedural knowledge expressed in the form of theoretical methods as an essential element of scientific theories along with the propositional knowledge that is expressed in the form of theoretical statements. This procedural knowledge includes, but is not exhausted by, knowledge of logical rules and procedures. Both logical and extra-logical theoretical methods belong to the core content of any scientific theory.

The view according to which any scientific method has a heuristic character and thus belongs to the *context of discovery* of a corresponding theory, rather than to this theory itself, is moreover not justified, in our view. The theoretical and experimental methods discussed above serve for the testing and *justification* of ready-made scientific theories. The issue of the heuristic value of methods is important and deserves discussion, which however is wholly out of the scope of the present work.

Our concluding remark concerns the issue of the relationships between logical and extra-logical methods in scientific theories. Recall Rudolf Carnap's much discussed idea according to which any scientific theory is analysable into a set of *protocol sentences* that express propositions about sense-data obtained in observations made by the naked eye, on the one hand, and a deductively organized system of universal theoretical sentences, on the other hand [286]. Today, such a logical reconstruction of scientific theories is commonly viewed as erroneous. A crucial argument against this approach amounts to pointing to the fact that all scientific observations and experiments are *theory-laden*, so that Carnap's idea of a theory-neutral observation by the naked eye is adequate neither to historical nor to current scientific practice [22]. Our proposed approach to the formal reconstruction of scientific theories allows us to describe the concept of theory-ladenness of scientific observations and experiments in a rigorous form and analyse its formal structure; such a formal analysis has been sketched above. This analysis makes it clear that in scientific theories, logical methods are relevant only when they constitute an integral part of a wider class of extra-logical *constructive* methods.

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