MODELS OF HOTT AND THE CONSTRUCTIVE VIEW OF THEORIES

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Abstract. Homotopy Type theory and its Model theory provide a novel formal semantic framework for representing scientific theories. This framework supports a constructive view of theories according to which a theory is essentially characterised by its methods. The constructive view of theories was earlier defended by Ernest Nagel and a number of other philosophers of the past but available logical means did not allow these people to build formal representational frameworks that implement this view.

1. Introduction

During the second half of the 20th century Evert Beth, Patrick Suppes, Bas van Fraassen followed by a large group of other contributors developed an approach to formal representation and formal logical analysis of scientific theories, which became known under the name of *semantic view of theories* [2] . This approach was proposed as a replacement for a different approach to the same subject developed several decades earlier by *logical positivists* including Rudolf Carnap, Carl Gustav "Peter" Hempel and Ernest Nagel. The proponents of the new *semantic view* dubbed the older approach *syntactic*. They used resources of the recently emerged Model theory and argued that a formal representation of scientific theories should first of all account for the intended semantics of these theories and leave aloof many syntactic details studied by their predecessors. Model theory helped these people

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[2]For an overview of the early history of the semantic view see [40], Part 1, Prologue.

to develop and defend a "new picture of theories" [45, p. 64] where the semantic aspect played the leading role.

As a number of authors remarked more recently, from the viewpoint of today's theories of formal semantics it appears that the proponents of the "semantic" approach at that time did not properly understand the involved relationships between syntactic and semantic aspects of formal theories, and for this reason often conceived of these relationships as a form of concurrence. As it became clear more recently the model-theoretic concepts used by the proponents of the semantic view could not be rigorously defined in an open air without taking related syntactic issues into consideration [11, 19, 4].

Nevetheless the "fight against the syntax" led by the proponents of the semantic view did not reduce to a mere rhetoric. Suppes had a strong insight according to which a general framework appropriate for formal representation of scientific theories should not involve arbitrary syntactic choices but should be *invariant* with respect to translations between different formal representations of the same theory. Suppes' epistemological emphasis on the invariant character of (non-formalized) theories in Physics and Geometry makes it evident where this insight comes from; in particular, Suppes is motivated here by the geometrical Erlangen Program [41, p. 99]). It is clear that the idea of invariant formal representational framework for scientific theories cannot be realised via disregarding syntactic issues; on the contrary, this project calls for new syntactic inventions [3]

Let us conclude this Introduction with a short statement that expresses our take on the debate between the syntactic and the semantic views of theories. Following Halvorson we take this issue to be mostly historical. In order to evaluate the contribution of the semantic

[3] Halvorson and Glymour rightly observe that the realisation of such a project requires, in particular, to develop an appropriate notion of equivalence of theories, which is still missing [10, 9]. Interestingly, the pioneering works of Bill Lawvere in categorical logic published in the 1960-ies were equally driven by the desire to avoid syntactic arbitrariness; in addition to examples of invariant theories borrowed from Geometry and Physics Lawvere used Hegel's notion of *objective logic* as a motivation for his groundbreaking research [33, 5.8].

view properly this approach should be compared with earlier attempts to use formal logical methods in science and in the philosophy of science but not with an artificially construed "syntactic view" which has been hardly ever defended by anyone. These earlier attempts did not involve and could not involve anything like formal semantics, which emerged later. When Nagel and his contemporaries considered possible applications of mathematical logic in the representation of scientific theories the formal part of their work was necessarily limited to syntactic issues. The intended semantics of formal theories these authors described informally by associating with appropriate symbols and symbolic expressions certain empirical and mathematical contents in the same traditional manner in which logicians did this since Aristotle. Suppes and other pioneers of the new "semantic" approach used a mathematical theory of semantics, viz. Tarski's set-theoretic semantics, as an additional intermediate layer of formal representation between the symbolic syntax of formal theory F and the contents of scientific theory T which F formally represents. In order to recognise the importance of this formal semantic layer one doesn't need to downplay the role of syntax. In *that* conservative sense the semantic view is clearly more advanced than its "syntactic" predecessor. However we argue in what follows that the "semantic turn" also left aside some important insights of Nagel's "syntactic" approach, which we are trying to save from the oblivion in this paper by using a new formal technique.

2. Constructive View of Theories

The *once-received* [11] aka syntactic view of theories and the more recent *semantic view* developed by Suppes and his followers were equally motivated by their contemporary developments in logic and foundations of mathematics: while logical positivists of the older generation were motivated by the rise of new symbolic logic on the edge between the 19-th and the 20th centuries, the younger generation of researchers in the 1950-ies was motivated by the rise of Model theory and formal semantics. In this paper we stick to the same pattern and base our proposal on the recently emerged Homotopy Type theory (HoTT) and the closely related research on the Univalent Foundations of mathematics. The view of theories that we arrive at is a version of the *non-statement* view according to which a theory, generally, does not reduce either to a bare class of sentences or to a class of statements ordered by the relation of logical consequence. But this proposed view, which hereafter we call *constructive*, differs from the usual semantic view in how it answers the question "What are fundamental constituents of a theory (except its sentences)? A proponent of the standard semantic view's answers "models". A proponent of the constructive view answers "methods".

As a general philosophical view on scientific theories the constructive view is not new: the fundamental role of methods in science has been stressed in the past by many philosophers from René Descartes to Ernest Nagel (see 3.1 below). That methods (including mathematical, experimental and observational methods) are abound in today's science hardly needs a special justification. The specification of relevant methods called by scientists a "methodology" (which should not be confused with the methodology of science in the sense used by philosophers) makes an important part of any piece of scientific knowledge (but not only of an open-ended scientific research). However we still lack convenient formal logical frameworks for representing methods as proper elements of scientific theories. In (4.1) we explain why standard logical and semantic means are not appropriate for the task. In (5) we show how HoTT helps to solve this problem. In the next Section 3 we take a closer look at Nagel's and Suppes work, which gives us some additional motivations for our proposed notion of constructive theory.

3. Ernest Nagel and Patrick Suppes on logic and the scientific method

3.1. **Ernest Nagel.** According to Nagel [4] the most distinctive and valuable feature of science is its method:

[4]The bibliographical source we refer to is a joint work by Ernest Nagel and Morris R. Cohen [26]. For brevity hereafter we do not repeatedly mention the second author.

> [T]he method of science is more stable and more important to men of science than any particular result achieved by its means [26, p. 395];

which he, in fact, identifies 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 [26, p. 192].

What kind of logic Nagel has here in his mind? Informally Nagel describes his core conception of logic as the "study of what constitutes a proof, that is, complete or conclusive evidence" [26, p. 5]. However there is a wide gap, as we shall see, between this general conception of logic and the construal of logic in form of symbolic calculus, which Nagel introduces in the same book.

By an axiomatic theory Nagel understands a Hilbert-style formal theory [5] ; he stresses the fact that that such a theory has no bearing on the material truth of its axioms and theorems. Nagel describes an axiomatic theory as a hypothetico-deductive structure, which represents objective relations of logical consequence between its sentences and only eventually may "conform" to the "world of existence". Whether or not a given axiomatic theory conforms to the world is an empirical question that cannot be answered within this given theory:

> The deduction makes no appeal whatsoever to experiment or observation, to any sensory elements. ... Whether anything in the world of existence conforms to this system requires empirical knowledge. ... That the world

[5] This part of Nagel's exposition is very outdated and, if judged by today's standard, also mostly informal. Since this historical detail has no bearing on the following arguments the reader is invited to think here about a Hilbert-style axiomatic theory in its modern form.

> does exemplify such a structure can be verified only within the limits of the errors of our experimental procedure. [26, p. 137]

Thus Nagel makes it clear that logical means, which make part of an axiomatic theory, don't help one to determine a conclusive evidence for and thus establish the truth of any scientific statement. But this, recall, is exactly what logic is supposed to do in science according to Nagel's general conception. Facing this problem Nagel goes as far as to suggest that Hilbert's conception of *proof* as a formal deduction from axioms is unwarranted and that a more reasonable terminological choice would be to talk (in context of axiomatic theories) about *deductions* without calling such deductions *proofs*. "But so habitual — says Nagel — is the usage which speaks of 'proving' theorems in pure mathematics that it would be vain to try to abolish it" [26, p. 7]. Needless to say that this linguistic compromise is very far from being innocent. Moreover the last quoted Nagel's remark that the author uses as an excuse for Hilbert's identification of proofs with deductions is hardly sound. Mathematicians indeed colloquially speak of "proving" theorems. However they typically do *not* present their proofs in form of formal logical deductions. The identification of proofs with deductions is a strong assumption that cannot be downplayed by referring to the colloquial mathematical parlance. Apparently Nagel wants here to use Hilbert's axiomatic approach without being agree with Hilbert on the nature and scope of logic and, more specifically, on what constitutes a proof. He clearly sees the gap between his general conception of logic and the available formal logical techniques and describes a possible role of these techniques in science as follows:

> The evidence for propositions which are elements in a [hypothetico-deductive]system accumulates more rapidly than that for isolated propositions. The evidence for a proposition may come from its own verifying instances, or from the verifying instances of other propositions which are connected with the first [i.e., the given proposition] in a system. It is this schematic character of scientific theories which gives such high probabilities to the various individual propositions of a science. [26, p. 395]

Thus the logical deduction supported by formal techniques available to Nagel can play at best an auxiliary role in science helping one to accumulate evidences more rapidly. By no means it can perform the fundamental methodological role, which Nagel reserves for logic in his informal treatment of this question.

3.2. **Suppes.** Unlike Nagel Suppes doesn't expose an informal conception of logic independent of symbolic and mathematical techniques. He regards the first-order logic and Tarskian Model theory as representational tools for scientific theories, which help to clarify the logical structure of these theories and their mutual relationships but not as justificatory tools nor as tools for encoding scientific methods in a more general sense. Suppes' strategy with respect to methods is to build a scene where various scientific methods can be used but he doesn't attempt to represent such methods at the formal level. See, for example, his chapter on *Representations of Probablity* [41, Ch. 5] , which provides a foundation for statistical methods in science.

In the Introduction to the same book, which summarizes Suppes' work on formal analysis of science during four decades, Suppes briefly considers, without providing references, a number of alternative approaches to formalization of scientific theories, which he qualifies as "instrumentalist" [41, p. 8 - 10] even if, as far as we can see, these approaches don't really commit one to the instrumentalist view on science in a strong epistemological sense.

According to one such proposal, "[t]he most important function of a theory [...] is not to organize or assert statements that are true or false but to furnish material principles of inference that may be used in inferring one set of facts from another." According to another proposal, "[t]heories are [...] methods of organizing evidence to decide which one of several actions to take." Suppes does not object these proposals on philosophical and epistemological grounds but rejects them for technical reasons stressing the fact that none of these proposal is supported with a developed formal technique. In what follows we show that today we are in a better position because some techniques appropriate for the task has been recently developed.

Thus we can conclude that neither the old-fashioned axiomatic representations of theories, which associates empirical contents to the (appropriate part of) symbolic syntax directly, nor the more advanced semantic approach, which uses the Tarski-style set-theoretic semantics and Model theory, have resources for representing scientific methods in a formal setting. This concerns methods of justification of the ready-made scientific knowledge as well as heuristic methods. Arguably a formal representation of ready-made scientific theories does not need to take into account heuristic methods and may safely leave them to the *context of discovery*. But the same argument does not apply to methods of justification, which make part of any scientific theory that deserves the name.

4. How to represent methods with symbolic calculi

Knowing a method amounts to knowing *how* to perform certain operations, which bring certain wanted outcome. One can distinguish between different senses in which a particular method can be known by an epistemic agent. It is possible that an agent is aware about certain instruction and can repeat it by hart but is unable to implement it in certain circumstances. It is also possible that an agent is capable to perform an operation and get the wanted outcome without being aware of a formal linguistic description of this operation. For a contingent historical reason the current epistemological discussion on knowledge-how is mainly focused on examples of this latter sort such as one's knowledge how to ride a bicycle [5]. Here, on the contrary, we focus on *explicit* knowledge-how, that is, the sort of knowledge-how, which is representable in the form of explicit rules and algorithms. In this paper we leave aside the analysis of epistemic attitudes and relations to syntactically represented methods and focus on the form of their representation.

4.1. **Why Hilbert-style axiomatic architecture and Tarski's model-theoretic semantic of logical inference are not appropriate for the task.** Syntactic derivations in suitable symbolic calculi are natural candidates for the role of symbolic representatives of various mathematical and material procedures regulated by the corresponding methods. However the familiar logical calculi (such as Classical first-order logic) and Tarskian set-theoretic semantics of these calculi don't support such an interpretation of syntactic derivations as we shall now see. Let us first fix some basic concepts for the further discussion.

A syntactic *rule* is a partial function of form $\mathcal{F}^n \to \mathcal{F}$ where $\mathcal{F}$ is a distinguished set of expressions of the given formal language L called *well-formed formulae*. In what follows we use word "formula" as a shorthand for "well-formed formula". The usual logical notation for rules is this:

$$\frac{A_1, \ldots, A_n}{B} \tag{1}$$

where the inputed formulae $A_1, \ldots, A_n$ are called *premises* and the outputed formula B is called *conclusion* [6].

A syntactic *derivation* d (aka *formal proof*) is a finite sequence of formulae $F_1, \ldots, F_k$ where each formula F_i is either an *axiom* or a *hypothesis* or is obtained from some of preceding formulae $F_1, \ldots, Fi - 1$ according to some of the available syntactic rules.

Axioms are fixed distinguished formulae, which serve as generators for syntactic derivations. It is common to identify axioms with rules with the empty set of premises (which requires a more general concept of rule than we use here). However since the conceptual difference between rules and axioms plays an important rule in our argument, we shall not use such a formal identification.

[6]This definition of syntactic rule leaves aside non-functional rules with multiple conclusions, which are also considered in some logical theories. However this limitation is not essential for the purpose of this paper.

Hypotheses are also distinguished formulae, which in derivations function like axioms. The difference between axioms and hypotheses concerns their mutual roles in the formal framework (that typically has a form of formal *theory* in which derivations are exhibited: while the set of axioms is fixed the sets of hypotheses vary from one derivation to another.

The last formula F_k in d is conveniently called a *theorem* and it is said that F_k is derived from the axioms and hypotheses, which belong to (or *are used in*) d, according to syntactic rules applied in d for obtaining other formulae. Notice that the sequence of formulae $F_1, \ldots, F_k$ does not provide the full information about the syntactic derivation as a fully determined syntactic procedure. In order to make the full information explicit each formula F_i in $F_1, \ldots, F_k$ needs to be supplied with a commentary, which specifies whether it is an axiom or a hypothesis or, by default, which rule has been used for obtaining it and with which premises.

In standard logical calculi including Classical propositional and first-order calculi well-formed formulae are supposed to express certain *statements*; this is the intended preformal semantics of these formulae. Accordingly, the syntactic rules used in these calculi such as *modus ponens* are interpreted as logical rules, which allow one, given some true statements (axioms), to deduce some other true statements (theorems). The same rules allow one to make conclusions on the basis of certain hypotheses. This general remark alone points to the fact that the syntactic rules and syntactic procedures of standard logical calculi are not apt for representing directly *extra-logical* scientific methods such as methods of conducting specific physical experiments or even mathematical extra-logical methods such as methods of calculating integrals.

The usual idea behind the project of axiomatizing science and mathematics is to reduce (in the sense, which is explained shortly) such extra-logical methods to logical methods via an appropriate choice of axioms. Consider a typical physical theory T that includes descriptions of observations and experiments, which verify empirically its theoretical statements. Think of Newton's *Principia* for a concrete historical example. An ideal formal version of this theory T_F axiomatized in Hilbert's vein allows one to deduce results of all the observations and experiments from its axioms (properly interpreted). Instead of using extra-logical observational and experimental methods specified in T one applies now logical methods specified in T_F for making the corresponding deductions. This does not mean, of course — and has been never meant by Hilbert or any other serious enthusiast of axiomatizing Physics — that logical and empirical methods perform in this situation the same epistemic role and that empirical methods are redundant in science. Logical methods in such a formal framework serve for clarifying logical relations (the precise character of which will be discussed shortly) between different theoretical statements but they cannot by themselves help one to verify these sentences. The task of verification aka proof [7] of theoretical statements is still reserved to empirical methods from T, which do not belong to T_F but may play a role in a meta-theory of T_F, which shows that T_F is empirically adequate or inadequate.

Thus T_F can be called a physical theory only in a peculiar sense of the word, which leaves central issues concerning the verification of a given theory outside this very theory. This certainly does not correspond to how the word "theory" is colloquially used by working scientists. It might be argued that even if T_F fails to represent adequately all epistemologically relevant aspects of physical theory T it still does satisfactorily represent its logical aspects. However this claim is also objectionable because it takes for granted a view of logic, which is not uncontroversial to say the least. One who shares Nagel's understanding

[7] The word "proof" used in such a context is ambiguous because it can also refer to formal deductions of theorems from axioms in T_F. It might seem to be a reasonable terminological decision, which would be more in line with the current logical parlance, to reserve term "proof" for formal deductions and talk of verifications or justifications in more general contexts including empirical ones. However following Nagel 3.1 and Prawitz [30] we believe that such a terminological habit hides the problem rather than helps to solve it. Under certain semantic conditions a syntactic derivation aka formal proof $F_1, \dots, F_k$ of formula F_k indeed may represent a proof of statement expressed by this formula, i.e., may represent a conclusive evidence in favour of this statement. In order to study and specify such semantic conditions one needs to evaluate whether or not a given interpretation of one's syntax makes syntactic derivations into proofs. The bold identification of proofs with syntactic derivations makes such a critical analysis impossible.

of the scope of logic can argue that since T_F leaves aside issues concerning the verification/proof/evidencing of T-statements it fails to capture the very logical core of T.

Does the same argument apply to theories of pure mathematics? The claim that computing an integral reduces to (or even "ultimately is") a logical deduction from axioms (say, the axioms of ZFC) and some additional hypotheses appears more plausible than a parallel claim about physical measurements; in spite of the fact that this way of computing integrals is not used in the current mathematical practice its theoretical possibility can be reasonably explained and justified by referring to standard set-theoretic foundations of the Integral Calculus. On such grounds one may argue that in the case of pure mathematics the above argument does not apply. However in fact the standard Hilbert-style axiomatic framework combined with (some version of) axioms of Set theory doesn't provide a systematic account of evidencing mathematical statements just as it doesn't provide an account of evidencing empirical statements.

Firstly, it leaves to a general philosophical discussion the question of truth of set-theoretic axioms (under their appropriate interpretation). *Secondly*, such foundations of mathematics in their usual form don't involve a satisfactory (at least from an epistemological viewpoint) account of valid logical *inference* but reduce that crucial concept to a meta-theoretical concept of logical *consequence*. (This point will be further explained shortly.) *Thirdly*, there is the problem of gap between the technical notion of (formal) proof as logical deduction from axioms of Set theory and the colloquial concept of mathematical proof used in today's mathematics.

Some people see this latter problem as merely pragmatic or "practical" and having no logical and epistemological significance. We agree that logic and epistemology are normative disciplines and for that reason references to past and present mathematical practices cannot constitute by themselves conclusive arguments in these fields. However such references may point to real logical and epistemological problems, which is, in our view, indeed happens in the given case. As far as deductions from ZFC are treated as ideal mathematical (or meta-mathematical) objects their epistemological role as evidences for certain mathematical statements remains unclear. In this form such deductions cannot be used as an effective tool for proof-checking. A theoretical possibility of set-theoretic formalization (justified with some meta-mathematical arguments) in practical contexts is used rather counter-positively. If some mathematical argument can be shown to be not formalizable in ZFC this is a reason to regard it as problematic (which, however, doesn't generally imply its dismissal since the given argument can be shown to be formalizable in some appropriate axiomatic extension of ZFC). If, on the contrary, a mathematical argument is shown to be formalizable in ZFC this doesn't imply that the corresponding informal proof is correct because its formal counterpart may possibly not qualify as a valid deduction. Unless such a formal proof is effectively exhibited it is impossible to check this. The fact that the standard foundations of mathematics do not provide for an effective proof-checking was the main motivation of Voevodsky behind his project of developing alternative foundations that he called Univalent Foundations (see 6 below).

Let us now see what Tarski style Model theory brings into this picture. *Firstly*, it provides for a precise semantics of logical deduction as follows. Formula B is called a semantic consequence of formulas $A_1, \dots, A_k$ just in case every interpretation of non-logical terms in $A_1, \dots, A_k$ that makes all A_i into true statements (i.e., every *model* of $A_1, \dots, A_k$) also makes formula B into a true statement (equiv. is also a model of B), in symbols $A_1, \dots, A_k \models B$. Then it is proved that if there exists a syntactic deduction from $A_1, \dots, A_k$ to B, in symbols $A_1, \dots, A_k \vdash B$, then $A_1, \dots, A_k \models B$ (soundness). This clarifies our point concerning the lack of proper semantic for logical inference in this semantic setting: it interprets the *existence* of syntactic deduction in terms of a meta-theoretical statement that express a logical relation between theoretical statements but it provides no precise semantics for individual deductions. We shall see in 4.3 how this problem is treated with various versions of *proof-theoretic* logical semantics.

Secondly Tarski's Model theory allows one to associate with a given first-order Hilbert-style axiomatic theory a canonical class of its set-theoretic models [14]. For an easy example think of usual axioms for (definition) of algebraic group and their model in form of set G

with a binary operation $\otimes$ on it, which satisfies the appropriate axioms. The canonical character of this construction allows one to define a group from the outset as a set with an additional structure, namely, with a binary operation on this set that has certain required formal properties (associativity, existence of unit and of inverse elements). This is the "semantic" semi-formal presentation used systematically by Bourbaki [1, p.30], which has been adopted by Suppes and his followers for the representation of scientific theories beyond the scope of pure mathematics via the identification of appropriate sets with physical systems such as sets of physical particles or sets of points of the physical space-time. Importantly, the same structure type (such as the group structure type) can be determined by, i.e., be a canonical model of (in the above sense) different sets of axioms. This feature is in accord with Suppes' "semantic" view according to which a scientific theory must be identified with a particular class of models rather than a particular syntactic structure.

The semantic set-theoretic presentation of mathematical theories developed by Bourbaki and a similar representation of scientific theories developed by Suppes and other enthusiasts of the *semantic view* provides a theory with a sense of objecthood: it represents the content of a given theory in terms of its objects such as algebraic groups or systems of physical particles. In this way the semantic representation supports a quasi-constructive reasoning about sets and set-theoretic structures. For example, one may think of forming the powerset $P(A)$ of given set A as a construction after the pattern of traditional geometrical constructions like the construction of a circle by its given radius. This helps one to develop within the set-theoretic mathematics a form of helpful intuition but the problem is that such set-theoretic "constructions" lack formal *rules*, which could be used for representing extra-logical mathematical and possibly scientific *methods*. When one needs to justify the "construction" of powerset in this framework one refers to the powerset *axiom* of ZFC, which is an existential statement that guarantees the existence of powerset for any given set. One may argue that in such contexts the difference between constructive rules and existential axioms is only a matter of taste or fasion. In the next Section we argue that this is not the case.

4.2. **Two axiomatic"styles".** Hilbert and Tarski after him conceive of a theory T as an ordered set of formal sentences satisfied by a class of intended models of this theory 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 non-logical symbols that belong to s. Thus this approach assumes that one distinguishes in advance 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. In other words the axiomatic method in its Hilbert-Tarski version requires that one first fixes logical calculus L and then applies it in an axiomatic presentation of some particular non-logical theory T [8]. Suppes assumes this basic scheme and says that a theory admits a "standard formalization" when L is the Classical first-order logic with identity [41, p. 24].

All existing approaches to formal representation of scientific theories use this familiar axiomatic architecture. However it is not unique. 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. ([8], p. 68)

and proposed an alternative approach to syntactic presentation of deductive systems, which involved relatively complex systems of rules and didn't use logical tautologies. In [8] Gentzen builds in this way two formal calculi known today as Natural Deduction and Sequent Calculus.

[8]We refer here to a version of axiomatic method described by Tarski in [42]. Tarski's view of this method builds on Hilbert's but does it in a particular and original way. The exact relationships between Tarski's semantic conception of the axiomatic method and Hilbert's original ideas about this method is a matter of historical study, which is out of the scope of this paper.

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

> The introductions [i.e. introduction rules] represent, as it were, the 'definitions' of the symbol concerned. ([8], p. 80)

is seen today by some authors as an origin of an alternative non-Tarskian conception of logical consequence and alternative logical semantics more generally, which has been developed in a mature form only in late 1999-ies or early 2000-ies and is known today under the name of *proof-theoretic* semantics (PTS) [37, 28].

Unlike Hilbert Gentzen never tried apply his approach beyond the pure mathematics and today the formal representation of scientific theories mostly follows in Hilbert's and Tarski's steps. However as we argue below the Gentzen style rule-based architecture of formal systems can be also applied in empirical contexts for representing various scientific methods, which include but are not limited to logical and mathematical methods.

4.3. **Constructive Logic, General Proof Theory, Meaning Explanation and Proof-Theoretic Semantics.** The conception of logic that hinges on the concept of proof rather than that of truth has a long history that can be traced back to Aristotle; here we review only relatively recent developments. In a series of publications that begins in early seventies Dag Prawitz formulates and defends his idea of *general proof theory* (GPT) as an alternative or a complement to the proof theory understood after Hilbert and Bernays as a meta-mathematical study of formal derivations [12]. Prawitz defends here the traditional conception of proof as evidence and argues that Tarski's semantic conception of logical consequence [43]does not provide, by itself, a theoretical account of proofs in mathematics and elsewhere:

> In model theory, one concentrates on questions like what sentences are logically valid and what sentences follow logically from other sentences. But one disregards questions concerning how we know that a sentence is

> logically valid or follows logically from another sentence. General proof theory would thus be an attempt to supplement model theory by studying also the evidence or the process - i.e., in other words, the proofs - by which we come to know logical validities and logical consequences. [29, p. 66]

Prawitz calls this conception of logic *intuitionistic* and, as the name suggests, sees Brouwer and Heyting as its founding fathers. Following Kolmogorov and some other important contributors to the same circle of ideas we prefer to call this approach to logic *constructive*. As we have seen in 3.1 Nagel's informal conception of logic also falls under this category. Since in this paper we discuss applications of logic in empirical sciences we would like also to point to the interpretation of proofs in constructive logic as *truth-makers* [39], which may be material objects and events either artificially produced or occurred naturally.

In 1972 Per Martin-Löf accomplished a draft (published only in 1998 as [25]) of a constructive (or intuitionistic as the author calls it) theory of types, which implements the general constructive approach outlined above. In a more mature form this system known as MLTT is presented in [21]. MLTT is a Gentzen-style typed calculus that comprises no axiom. The meaning of its syntactic rules and other syntactic elements is provided via a special semantic procedure that Martin-Löf calls the *meaning explanation*. In [22] 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 rule their meaning and simultaneously justifies them. [24]. Martin-Löf's meaning explanation is an informal version of the proof-theoretic semantics mentioned in 4.2 above [37].

In the late 2000-ies MLTT started a new life as the syntactic core of the emerging Homotopy Type theory (HoTT) [31].

5. From MLTT to HoTT

MLTT comprises four types of *judgements* (which should not be confused with propositions, see 7.3 below).

(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. Let me leave (i) and (ii) aside and provide more details on (iii) and (iv).

Martin-Löf offers four different informal readings of (iii) [21, p. 5]:

(1) a is an element of set A

(2) a is a proof (construction) of proposition A ("propositions-as-types")

(3) a is a method of fulfilling (realizing) the intention (expectation) A

(4) a is a method of solving the problem (doing the task) A (BHK semantics)

The author argues that these interpretations of judgement form (iii) not only share a logical form but also are closely conceptually related. The correspondence between (2) and (4) is based on the conceptual duality between problems and theorems, which dates back to Euclid [35]; among more recent sources (4) refers to Kolmogorov's *Calculus of Problems* [18] and the so-called *BHK-semantics* (after Brouwer, Heyting and Kolmogorov) of the Intuitionistic propositional calculus. (iii) is an analysis of judgement based on Husserl's Phenomenology. The correspondence between (1) and (2) at the formal level is the Curry-Howard correspondence. But in Martin-Löf's view this correspondence is not *only* formal but also contentful: 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. [21, p. 13]

Let us now turn to judgement type (iv). It says that terms a, a', both of the same type A, are equal. This equality is called *judgemental* or *definitional* and does *not* qualify as a proposition; the corresponding *propositional* equality writes as $a =_A a'$ and counts as a type on its own ($a =_A a' : TYPE$) called an *identity type.* In accordance to reading (2) of judgement form (iii) a term of identity type is understood as a proof (also called a witness or evidence) of the corresponding proposition. MLTT validates the rule according to which a judgemental equality entails the corresponding propositional equality:

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

where $refl_x$ is the canonical proof of proposition $a =_A a'$.

The *extensional* version of MLTT also validates the converse rule called *equality reflection rule*:

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

but HoTT draws on an *intensional* version of MLTT that does not use such a principle and, in addition, allows for multiple proofs of the same propositional equality (the proof-relevant conception of equality) [31, p. 52].

Let p, q be two judgmentally different proofs of 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:

$$p', q' : p =_{P=_TQ} q$$

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 $p', q' : p =_{P=_TQ} q$ 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).

In order to get an intuitive picture the non-mathematical reader is advised to think of homotopy h between paths $p, q : P =_T Q$ as a curve surface bounded by the two paths, which share their endpoints P, Q. Given two such surfaces h, i two-homotopy f between h and i is a solid bounded by these surfaces. Picturing of higher homotopies of level ¿ 2 is more difficult because it requires a geometrical intuition that extends beyond the three spatial dimensions.

Thus the homotopical interpretation makes meaningful the complex structure of identity types in the intensional MLTT. It makes this structure surveyable, as we shall now see.

Definition 1. *Space S is called* contractible *or space of h-level (-2) when there is point $p : S$ connected by a path with each point $x : A$ in such a way that all these paths are homotopic (i.e., there exists a homotopy between any two such paths).*

In what follows we refer to contractible spaces “as if they were effectively contracted” and identify such spaces with points. A more precise mathematical formulation involves the notion of *homotopy equivalence*, which provides a suitable identity criterion for spaces in Homotopy theory. In view of the homotopy interpretation of MLTT outlined above such spaces (defined up to the homotopy equivalence) are also called “homotopy types”.

Definition 2. *We say that 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.*

The two definitions gives rise to the following stratification of types/spaces in HoTT by their h-levels:

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

A general space is of h-level ω: this is just another way of saying that the ladder of paths, homotopies and higher homotopies of all levels is, generally, infinite. The above stratification classifies special cases when this ladder is finite in the sense that after certain level n spaces of all higher-order homotopies (or paths if $n < 1$) are trivial, i.e., such homotopies (paths) are no longer distinguished one from another at the same level. For example a *set* aka *discrete space* (h-level 0) is a space that either does or does not have a path between any pair of its points; different paths between two given points are not distinguished. If two points are connected by a path they are contracted into one point as explained above. At the next h-level 1 live flat path groupoids that comprise sets of points connected by paths, which are, generally, multiple and well-distinguished *up to homotopy*: homotopical paths are identified and non-homotopical paths count as different. Thus in a flat groupoid the space of paths between any two given points is a set. This inductive construction proceeds to all higher levels.

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 call a n-type a type, which is of h-level n but not of level $n - 1$.

A space of h-level l (now including the infinite case $l = \omega$) can be transformed into a space of h-level $k < l$ via its k-*truncation*, which can be described as a forced identification of all homotopies (paths) of levels higher than k. 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.

A more mathematically precise explanation of HoTT basics can be found in [31] and other special literature.

The h-stratification of types in MLTT suggests an important modification of the original preformal semantics of this theory introduced in the beginning of this Section. This modification is not a mere conservative extension. Recall that in [21, p. 5] Matin-Löf proposes multiple informal meaning explanations for judgements and suggests that they conceptually converge. In particular, Martin-Löf proposes here to think of types as sets or as propositions and suggests that in the last analysis the two notions are the same. HoTT in its turn provides a geometrically motivated conceptions of set as 0-type and proposition (called in [31, p. 103] *mere* proposition) as (-1)-type. The latter is motivated as follows. Recall that there are just two (-1)-types (up to the homotopical equivalence): the empty type $\emptyset$ and the one-point type pt. HoTT retains the idea that propositions are types and the idea that terms of such types are truth-makers (proofs, evidences, witnesses). The adjective "mere" used in [31, p. 103] expresses the fact that (-1)-type interpreted as proposition does not distinguish between its different proofs but only reflects the fact that such a proof exists (in case of pt) or the fact that the given proposition has no proof (in case of $\emptyset$). In other words, the adjective "mere" expresses the fact that mere propositions are proof-irrelevant. For the same reason (-1)-types can be also described as truth-value types, where truth is understood constructively as the existence of proof/evidence/truth-maker.

Whether or not the proof-irrelevant conception of proposition is fully satisfactory as a working conception of proposition *tout court* has been recently a matter of controversy. One may argue that in certain context people tend to think of and speak about propositions in a proof-relevant sense [9]. In our opinion this fact does not constitute a strong objection to the identification of propositions with (-1)-types because all proof-relevant aspects of propositions are taken into account at higher levels of the homotopical hierarchy as follows. General n-type S^n where $n > -1$ comprises:

- the "propositional layer" S^{-1}, which is a proposition extracted from S^n via its propositional truncation, and

[9] as some time ago argued Michael Shulman in several blogs and in a private correspondence

- the higher-order structure H present on all h-levels ¿ - 1, which is a (possibly multi-layer) structure of *proofs* of proposition S^{-1}.

This observation suggests the following possible application of HoTT in the formal representation of theory T:

- extra-logical rules of T (including rules for conducting observations and experiments) are MLTT rules applied at h-levels ¿ -1
- logical rules are MLTT rules applied to (-1)types.

This gives us a preliminary answer to the question posed in 4 How to represent scientific methods (including extra-logical methods) with symbolic calculi? (A more elaborated version of this answer will be given in 7.3 below). Although HoTT does not comprise different sets of logical and extra-logical rules it allows one to distinguish between logical and extra-logical applications of its rules as follows. *Logical* application of rules can be isolated from the extra-logical via (-1)-truncation. The application of HoTT rules to S^n comprises a logical application of these rules at h-level (- 1) *and* an extra-logical application of the same rules at all higher h-levels. Such a joint application of MLTT rules makes immediately evident the logical function of extra-logical (i.e., higher order) structures and constructions in S^n, which is truth-making (or *verification* (if one prefers this term borrowed from the philosophy of science) of proposition S^{-1}.

The criterion of logicality that we use here is based on the h-hierarchy and the identification of (-1)-types with propositions. It is an interesting project to evaluate it against other criteria of logicality discussed in the philosophical literature. This, however, is out of the scope of this paper. For our present purpose it is sufficient to explain how set-based and higher order construction in HoTT work as truth-makers of propositions. The crucial aspect of the shift from Martin-Löf's original preformal semantics for MLTT to the new preformal semantics based on the h-hierarchy (which hereafter we shall call h-semantics for short) can be described in the jargon of Computer Science as the shift from the propositions-as-types paradigm to the propositions-as-*some*-types paradigm. Further implications of this shift are discussed in 7.3 below.

6. Univalent Foundations

Univalent Foundations (UF) use a version of HoTT-based framework as a new purported foundation of pure mathematics. The *univalence property* of type-theoretic universes plays a central role in this project. We shall not define and discuss this property in this paper but only point to its relevance to the choice between the two axiomatic styles mentioned above. The 2013 standard version of UF uses the *Univalence Axiom* (Axiom 2.10.3 in [31]), which is in fact the only axiom added on the top of *rules* of MLTT. This added axiom makes the axiomatic architecture of UF mixed in the sense that it comprises both rules and axioms. However a more recent work by Th. Coquand and his collaborators on Cubical Type theory [3] provides a "constructive explanation for UA", i.e., a formal deduction of the univalence property in a Gentzen style rule-based formal system other than MLTT. At the present stage of development [10] such a constructive explanation of univalence is not yet achieved in all its desired generality but we take a liberty to bypass this technical issue in the present discussion and consider the Gentzen style constructive axiomatic architecture as a proper feature of UF.

Our proposed *constructive view* of theories motivated and informally described in Section 2 above applies the same axiomatic architecture for representing theories beyond the pure mathematics. A straightforward approach to representing scientific theories with HoTT consists in an identification of certain HoTT structures with structures coming from theories of Physics and other scientific disciplines. A dummy example of this sort is presented and discussed in [34] where a fragment of HoTT is used for proving the identity of Morning Star and the Evening Star; the observed continuous trajectory of this celestial body is used here as an evidence (proof) of the identity statement. Urs Schreiber formalized in

[10]as presented by Thierry Coquand in his talk given at the EUTypes meeting, Nijmegen, Netherlands, 22-24 January 2018

UF a significant body of up-to-date mathematical physics and made a new original contributions in this area using UF-based mathematics [36]. Even if none of this qualifies as a full-blood (representation of) physical theory such attempts show that the Gentzen-style axiomatic architecture and, more specifically, UF as a paradigm mathematical theory that uses this architecture can be used in science and support a constructive computer-based representation of scientific theories.

7. Modelling Rules

The main lesson of the debate between the "syntactic view" and the "semantic view", as one can see this matter after the critical analysis briefly reviewed in the above Introduction, is that the formal semantic aspect of one's representational framework is at least as much important as the syntactic one. The *formal* semantic of HoTT — which should not be confused with the informal notion of meaning explanation introduced in 4.3 above or with the suggestive description of types as spaces used in [31] and other introductions to HoTT — by the present date is largely a work in progress. The following discussion concerns only the Model theory of HoTT rather than its formal semantics in a more general sense and it does not intend to cover all relevant developments.

Since one leaves the familiar territory of first-order theories and their set-theoretic semantics the very notions of theory and model become problematic and require certain conceptual revisions, which are not necessarily innocent from a philosophical viewpoint as we shall now see. By a *theory* we shall understand here a set of formal rules as described in 1 from 4.1 together with the set of all derivations supported by these rules. What is a *model* of a given theory presented as just described?

Alfred Tarski designed his Model theory back in early 1950-ies having in mind Hilbert-style axiomatic theories. Let us repeat its basic definitions. A model of (uninterpreted) axiom A is an interpretation m of non-logical terms in A that makes it into a true statement 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. When rules of inference used in T preserve truth (soundness) it is sufficient to check that M satisfies all axioms of T for claiming that it also satisfies all its theorems.

Since we deal with modelling a theory presented in Gentzen style rather than in Hilbert style 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 [13]) it can be straightforwardly construed on this standard basis as follows. We shall say that interpretation m is a model of rule R (of general form 1), in symbols

$$\frac{A_1^m, \ldots, A_n^m}{B^m} \tag{2}$$

when the following holds: whenever $A_1^m, \ldots, A_n^m$ are true statements B^m is also true statement. Arguably this notion of modelling a rule is implicit in Tarski's Model theory, so it can be used for modelling HoTT without revisions (albeit with some add-ons, see below). There are however several problems with this approach, which we list below and suggest some tentative solutions.

7.1. **Logical Inference versus Semantic Consequence Relation.** In the standard setting considered by Tarski (Hilbert-style axiomatic theories and their set-theoretic semantics) the above notion of modelling a rule is redundant because the syntactic rules used in this setting such as *modus ponens* preserve truth under *all* interpretations of their premises and conclusions. Moreover, one can argue in Prawitz's line (see 4.3) that in the standard setting 2 does not provide a semantic for rule R properly but only specifies an instance of relation of logical consequence (conclusion B^m follows logically from assumptions $A_1^m, \ldots, A_n^m$). This is not a proper semantics for rule R, so the argument goes, because it associates with R a meta-proposition that expresses the fact that certain relation holds but doesn't tell one *how* one comes to knowing that fact.

In the passage quoted in 4.3 Prawitz makes an emphasis on the epistemological aspect of the above argument. However a form of this argument remains valid even if one doesn't accept the idea that the proper semantics of formal rules should be necessarily epistemic. To make this argument work it is sufficient to assume that the proper semantics of rules should involve the concept of "how", which may be also used in non-epistemic semantic contexts such as computational models of some natural processes. So one is in a position to argue that the above propositional semantics of rules is unsatisfactory without referring to the epistemic role of these rules.

In the quoted paper Prawitz:1974 Prawitz puts forward a project of developing General Proof theory, which is supposed to complement the standard Model theory and support of a conceptual revision of Hilbert-style Proof theory. Martin-Löf's notion of meaning explanation (4.3) also provides a semantics for syntactic rules independently of Model theory. In our view, keeping the formal semantics for rules apart from Model theory can hardly be a tenable strategy today. From a mathematical point of view meaning explanations give rise to *realizability models* of Type theory [32], [15] and the conceptual distinction between the two is not so easy to formulate and in any event it is not stably kept in the current literature [11]. This is why it makes more sense, in our view, to revise some conceptual foundations of Model theory in the light of new developments rather than leave the conceptual core of this theory untouched and develop a formal semantics for HoTT independently.

7.2. **Logical and Non-Logical Symbols.** As we have already mentioned above in 4.2 in the standard setting by interpretation of formula one understands interpretations of non-logical symbols in this formula; the distinction between logical and non-logical symbols is supposed to be set beforehand. Interpreting HoTT one deals with a very different situation, where such a distinction between logical and extra-logical elements does not apply at the

[11] For example Tsementzis [44] prefers to call preformal meaning explanation what otherwise can be called a semi-formal model of HoTT, moreover that the author interprets rules of MLTT in Tarskian style according to our scheme 2 (above in the main text).

syntactic level. At the semantical level the question of logicality (Which fragment of HoTT belongs to logic proper?) remains sound but does not have an immediate answer. Certain concepts of HoTT such as the basic concept of type qualify as logical in a broad sense but also receive an informal interpretation in terms Homotopy theory, which is normally not thought of as a theory of pure logic.

This has the following effect on the interpretation of syntactic rules. In the standard setting these rules are supposed to serve as logical rules and work equally for all legitimate interpretations of formulas. So in this setting the syntactic rules admit a default interpretation in terms of logical consequence relation. The situation is very different in HoTT and akin theories where no fixed distinction between logical and non-logical symbols is specified at the syntactic level. In that case the semantics of rules is a proper part of Model theory *of HoTT* — rather than a general issue that belongs to foundations of Model theory as such and is not specific to any particular theory, which one may wish to model.

7.3. **Extra-Logical Rules.** In scheme 2 assumptions $A_1^m, \dots, A_n^m$ and conclusion B^m are statements. Recall that in MLTT these formulas are interpreted as *judgements* (5). This interpretation combines with scheme 2 only at the price of ignoring certain conceptual subtleties. Frege famously describes an act judgement as "the acknowledgement of the truth of a thought" [7, p. 355-356] or an "advance from a thought to a truth-value" [6, p. 159] understanding by a thought (roughly) what we call today a proposition. Frege warns us, continuing the last quoted passage, that the above "cannot be a definition. Judgement is something quite peculiar and incomparable" (ib.). Having this Frege's warning in mind one can nevertheless think of judgements as meta-theoretical propositions and, in particular, qualify formulas $A_1^m, \dots, A_n^m, B^m$ in 2 both as judgements (internally) and meta-propositions belonging to Model theory (i.e., externally). This provides a bridge between Tarski's and Martin-Löf's semantic frameworks and allows one to interpret rules of MLTT via scheme 2. It should be stressed that this bridge has a formal rather than conceptual character.

A more difficult problem arises when one intends to use in the Model theory the internal preformal semantics of HoTT based on the homotopical hierarchy of types (h-semantics). Recall that h-semantics qualifies as propositions only types of certain h-level, namely, only (-1)-types — while Martin-Löf's early intended semantics for MLTT admits thinking of *each* type as a proposition (see 5 above). Accordingly, h-semantics hardly supports the interpretation of all formulas as judgements in Frege's sense of the word. Consider a special instance of (1) where all formulas are judgements of basic form (iii)(see 5 above):

$$\frac{a_1 : A_1, \ldots, a_n : A_n}{b : B} \tag{3}$$

If $A_1, \ldots, A_n, B$ are (-1)-types then the name of judgement used for these expressions of form $a : A$ can be understood in Frege's sense: here A stands for a proposition and a stands for its proof (witness, truth-maker). But when A is n-type A^n with $n > -1$ h-semantics suggests to read the same expression $a : A$ as a mere *declaration* of token a of certain higher, i.e., *extra-logical* type A. This extra-logical object a does perform a logical function: it guaranties that (-1)-type A^{-1} obtained from A^n via the propositional truncation is non-empty and thus gives rise to valid judgement $a^* : A^{-1}$. So there is an indirect sense in which token a of type A also functions as a truth-maker even if A is not a proposition. This fact, however, hardly justifies using the name of judgement (in anything like its usual sense) for all expressions of form $a : A$ along with their h-semantics. In case of the other three basic forms of "judgements" the situation is similar. A possible name for such basic syntactic constructions, which we borrow from Computer Science and programming practice, is "declaration". We suggest to reserve the name of judgement only for those declarations, which involve only (-1)-types.

When $A_1, \ldots, A_n, B$ are higher types the above scheme 3 represents an extra-logical rule that justifies "making up" token b from given tokens $a_1, \ldots, a_n$. Once again the characterisation of this rule as extra-logical should be understood with a pinch of salt. The

(-1)-truncated version of this rule is logical. However this truncated version represents only a simplified and impoverished version of the original rule. So we have got here a more precise answer to the question of how to represent extra-logical scientific methods with formal calculi (4). Generally, assumptions in MLTT/HoTT-rules have a more complex form than in 3. We leave it for a further research to study these complex forms from the same viewpoint.

Declarations of form $a : A$ (and of other forms) can be rendered as meta-propositions in any event (including the case when type A is not propositional) [44]; in English such a meta-proposition can be expressed by stating that a *is* a token of type A. Then scheme 2 can be used as usual. The obtained meta-theoretical propositional translation can be useful, in particular, for foundational purposes if one wants (as did Voevodsky) to certify HoTT and study its model-theoretic properties on an established foundational basis such as ZF. However this way of developing Model theory of HoTT is evidently not appropriate for our present purpose, which is the purpose of formal representation of extra-logical methods in scientific theories.

An available alternative consists of interpreting formal rules of form 2 in terms of extra-logical mathematical constructions (generally, not purely symbolic) which input and output certain non-propositional contents. In the special case 3 such assumptions are tokens of mathematical objects of certain types and the conclusion is a new token of certain new type. For a suggestive traditional example think of Euclid's geometrical rules that he calls *postulates*: the first of these rules justifies a construction of straight segment by its given endpoints. The first-order statement "For any pair of distinct points there exists a straight segment having these points as its endpoints" used by Hilbert in his axiomatic reconstruction of Euclidean geometry is a meta-theoretical reconstruction of Euclid's extra-logical rule rather than just a logically innocent linguistic paraphrase [35]).

When an appropriate construction c that instantiates given formal rule R is exhibited, rule R is justified in the sense that it provably has a model, which is c itself. Since such a notion of modelling a rule does not involve a satisfaction relation defined in terms of truth-conditions, it marks a more significant departure from the conceptual foundations of Model theory laid down by Tarski back in 1950-ies. Whether or not this conceptual difference implies a significant technical difference remains unclear to the author of this paper. The Model theory of HoTT developed by Voevodsky, which we overview in the next Section, is intended to be formalizable both in ZFC (via a meta-propositional translation described above) and in UF. The latter option amounts to using HoTT/UF as its proper meta-theory; in this case preformal h-semantics turns into a form model-theoretic semantics. In this way h-semantics becomes formal.

8. Initiality: Theories as Generic Models

HoTT as presented in the HoTT Book [31] uses the syntax of MLTT extended with the Univalence Axiom; the semantic "homotopic" aspect of this theory, a central fragment of which has been briefly described above (5), remains here wholly informal: the language of the Book deliberately and systematically confuses logical and geometrical terminology referring to types as "spaces", to terms as "points" and so on. This is what we call the preformal h-semantics throughout this paper.

A mathematical justification of such terminological liberty, which proves very useful in providing one's reasoning in and of MLTT with a helpful geometrical intuition, lies in the fact that "all of the constructions and axioms considered in this book have a model in the category of Kan complexes, due to Voevodsky" [31, p. 11], [17] [12].

A closer look at [17] shows that the concept of model used by the authors is quite far from being standard. In particular, the authors of this paper use techniques of *Functorial*

[12] [17] is a systematic presentation of Voevodsky's results prepared by Kapulkin and Lumsdain on the basis of Voevodsky's talks and unpublished manuscripts. The Book [31] refers to the first 2012 version of this paper. In what follows we refer to its latest upgraded version that dates to 2016.

Model theory (FMT), which stems from the groundbreaking Ph.D. thesis of W.F. Lawvere accomplished in 1963 [20] [13].

The idea of Functorial Model theory can be described in form of the following construction:

- a given theory $\mathbf{T}$ is presented as a category of syntactic nature (a 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 as a *generic model* , i.e., as an object (or a subcategory as in [20]) of $C_{\mathbf{T}}$.

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

- Construct a general model of 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 notion of *contextual category* due to Cartmell [2]; 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 syntactic character, which is called *term model*. Objects of $\mathcal{C}(\mathbf{T})$ are MLTT-contexts, i.e., expressions of form

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

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

[13] For a modern presentation of FMT see [16, vol. 2, D1-4] and [27].

up to the definitional equality and the renaming of variables). More precisely, morphisms of $\mathcal{C}(T)$ are of 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 form $\mathcal{C}(\mathbf{T}) \to \mathcal{C}$.

The latter proposition is stated in [17] as Theorem 1.2.9 without proof; the authors refer to [38] where a special case of this theorem is proved and mention that "the fact that it holds for other selections from among the standard rules is well-known in folklore".

The authors state that the initiality property of $\mathcal{C}(\mathbf{T})$ justifies the qualification of $\mathcal{C}$ (a general contextual category / C-system) as a model of $\mathbf{T}$ (Definition 1.2.10 in [17]). Since the initiality condition does not belong to the conceptual background of the standard Model theory this statement calls for explanation. We offer here such an explanation in terms of our proposed *constructive* view of theories (2 above). Think of generic term model $\mathcal{C}(\mathbf{T})$ as a special presentation of theory $\mathbf{T}$ in form of *instruction*, i.e., a system of rules (presented symbolically). This instruction is a schematic syntactic construction; it is schematic in the sense that it is applicable to more than one context. Available contexts are objects of $CTXT$. The initiality property of $\mathcal{C}(\mathbf{T})$ in $CTXT$ guarantees that in each particular context $\mathcal{C}$ instruction$\mathcal{C}(\mathbf{T})$ is interpreted and applied unambiguously. A useful instruction can be schematic but it cannot be ambiguous.

The initiality property of contextual categories and other relevant mathematical structures was in the focus of Vladimir Voevodsky's research during the last several years of his life and work. He considered it as a genuine open mathematical problem and dubbed it the Initiality Conjecture. 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, which includes a syntactic object with the initiality property as explained above. This conjecture still stands open to the date of writing the present paper.

9. Conclusion

We have seen that the epistemological argument between partisans of the so-called Syntactic View and the Semantic View of scientific theories, which took place in the 20th century, was at a great extent determined by basic features of formal logical tools available at that time (1, 3.2). We have also seen that Ernest Nagel, using the traditional philosophical prose, expressed epistemological views on science and on the role of logic in science, which could not be supported by formal logical means available to him in a ready-made form (3.1). This was a price payed by these people for their decision to develop epistemology and philosophy of science on a formal logical basis. This price may be or be not worth paying but one should keep in mind that formal logical tools in their turn are developed with certain intended purposes and with some ideas about the nature of logic and its epistemic role. Even if formal logic is more tightly connected to mathematics than to empirical sciences, important logical ideas may come from sciences. If a system of logic represented with a symbolic calculus does not support one's epistemological view on science this is a reason to revise both one's epistemological ideas *and* one's logical ideas and logical techniques. So the task of developing formal epistemology and formal representational schemes for science requires thinking about foundations of logic and mathematics rather than a mere application of some ready-made logical and mathematical tools.

In this paper we defend a *constructive* view of scientific theories according to which *methods* (including empirical, mathematical and eventually logical methods) are essential constituents of scientific theories, which should not be left out as a part of "context of discovery" or in any other way (2). This epistemological view is certainly not original and dates back at least to René Descartes and Francis Backon. The brief word description of this view given in this paper is evidently very imprecise and allows for various specifications, many of which may be not compatible. However our aim in this paper is to support this general view with a new formal technique rather than elaborate on it using only philosophical prose. We call this general epistemological view "constructive" referring to an essential feature of formal framework, viz. Homotopy Type theory, that we use for supporting this view; the relevant sense of being constructive we have explained in some length elsewhere [35]. In order to avoid a possible confusion let us mention here that our proposed constructive view of theories does not imply a form of epistemological constructivism that treats objects of science as mental or social constructions; in fact our constructivism is compatible with a version of scientific realism.

HoTT has emerged as a unintended homotopical interpretation of Constructive Type theory due to Martin-Löf (MLTT); MLTT in its turn has been developed as a mathematical implementation of constructive notion of logic, which focuses on proofs and inferences rather than on the relation of logical consequence between statements, and which is concerned primarily with epistemic rather than ontological issues (4.3). A significant part of this intended preformal logical semantics (albeit not all of this semantics) of MLTT is transferred to HoTT (5); the semantic features of HoTT inherited from MLTT support a constructive logical reasoning in HoTT. However HoTT also brings about a new extra-logical semantics, which is an essential ingredient of our proposed concept of constructive theory. Just as in the case of standard set-theoretic semantics of first-order theories the extra-logical semantics of HoTT is tightly related to the logical semantics: h-hierarchy of types and the notion of propositional truncation (5) provides a clear sense in which extra-logical terms have a logical impact (7.3). However the relationships between logical and extra-logical elements are organised here differently; a major difference is that h-semantics allows for making sense of the notion of extra-logical *rule* (or more precisely, an extra-logical application of formal rule), which is not available in the standard setting. This is a reason why we believe that a HoTT-based framework can be a better representational tool for science than the standard first-order framework.

Model theory of HoTT does not exist yet in a stable form. In this paper we discussed conceptual issues that arise when one attempts to use with HoTT and akin theories the standard notion of satisfaction due to Tarski (7). Then we reviewed a work in progress started by Voevodsky and continued by his collaborators where the category-theoretic concept of initiality (8) plays an important role. The purpose of this analysis is to provide a conceptual foundation for prospective applications of HoTT and akin theories in the representation of scientific theories along with the Model theory of such theories. Taking into account the Model theory of HoTT is an important upgrade of preformal h-semantics to its more advanced formal version.

Admittedly, a possibility of using HoTT or a similar theory as a standard representational tool in science at this point is a philosophical speculation rather than a concrete technical proposal. Nevertheless there are reasons to believe that this new approach can be more successful than its set-theoretic ancestor. Unlike the standard set-theoretic foundations of mathematics Univalent Foundations are effective and "practical" in the sense that they allow for an effective practical formalization of non-trivial mathematical proofs and checking these proofs with the computer. An effective UF formalization of the mathematical apparatus of today's Physics and other mathematically-laden sciences paths a way to a similar effective formalization and useful digital representation of scientific theories. The step from foundations of mathematics to representation of scientific theories is by no means trivial. This paper attempts to provide conceptual preliminaries for it.

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