Book of Abstracts

Realism and Anti-Realism

Paradigms and research programmes in logic and the philosophy of mathematics

April 28th-29th 2025
Carl Friedrich von Weizsäcker-Zentrum
Eberhard Karls University of Tübingen, Germany

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2.1 Carolin Antos - The Model-Based Turn in Set Theory. A non-Kuhnian Revolution

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In the 1960's, set theory underwent a fundamental change: The introduction and development of new model-theoretic methods led to an explosion of results, clarified long-standing research questions and opened up new areas of research. In particular, it unified the methodological basis for the two main epistemic functions of set theory: providing a foundational theory on the one hand, and being the discipline that studies mathematical infinity on the other. In this talk, I investigate what this change signifies in terms of non-Kuhnian revolutions in the sciences.

Set theory as a foundational theory is concerned with investigating questions of consistency and provability. In its beginning this was studied in the context of axiomatization, proposing and rejecting specific axioms or axiom systems as the "right" ones for a foundation of mathematics. With the introduction of model-based methods, such as forcing or inner models, these issues could now be investigated on the semantic side by building models of set theory that vary towards the statements that hold or fail within them (such as the Continuum Hypothesis). While axioms are still discussed, in set-theoretic practice they are now considered through the lens of models that satisfy or fail them. This change of focus has lead to the development of alternative set-theoretic foundations, such as multiversism, but it also influences ongoing projects for foundations in the classical universist tradition, such as Woodin's Ultimate-L.

Set theory can also be pursued (relatively) independently from foundational questions, simply as the part of mathematics studying the continuum or, more generally, mathematical infinity. Examples of this are areas such as infinite combinatorics, cardinal characteristics or descriptive set theory. Here, model-based methods are nowadays an indispensable part of methodology, allowing the study of infinity beyond ZFC, investigate questions of absoluteness and prove statements within the standard model V (see for example (Miller, 2017)). These areas are thereby situated within the larger picture of set theory, connecting it closely with meta-mathematical questions.

Therefore model-based methods introduced a turn from syntactic to semantic considerations in set-theoretic foundations and fully revealed the dependence of facts about infinity on the meta-mathematical setting in which they are considered in. This unifies the different epistemic functions of set theory with respect to methods, clarifications of concepts and proof strategies. But does this turn constitute a revolution or paradigm change in the way described by Thomas Kuhn in (Kuhn, 1962)?

The general question if revolutions are possible in mathematics is a contested one. Michael Crowe gives a decisive answer to the negative: "Tenth law: Revolutions never occur in mathematics." (Crowe, 1975, p. 165) One way to understand the "tenth law" is that Crowe objects to the existence, in mathematics, of the phenomenon called *Kuhn-loss* (after (Post, 1971, p. 229)), i.e. a theory's loss of its predecessor's ability to offer explanations and predictions in a certain puzzle-solving domain. Post himself maintains not only that theory change can happen without Kuhn-loss, but that such loss usually does not occur, as it is a desirable feature of the new theory that it accommodates at least the successful parts of the old one. Post allows for (partial) translations between the theories and therefore also rejects the idea that incommensurability is a critical part of theory change. Understanding revolutions in such a non-Kuhnian manner is much better suited to studying revolutions in mathematics as the cumulative character of mathematical knowledge can be accommodated.

I will argue that the change set theory underwent through the introduction of model-based methods is a case of such a non-Kuhnian revolution. Adapting Post's description of theory change in physics to the setting of mathematics, I will argue that the turn in set theory fulfils the following four criteria: Firstly, it came about to deal with a fundamental *flaw* in the old theory, namely the factual incompleteness of ZFC. "Factual incompleteness" refers to not only the theoretical incompleteness all sufficiently advanced axiomatizations of mathematics, but also that this incompleteness shows itself in the undecidability of a sentence that is fundamentally of interest to mathematics, namely the Continuum Hypothesis. Secondly, the new theory *extends* the old one by picking up features or elements that appear in the old theory but do not fit it in a satisfactory manner and provides a new, more extensive framework for them. Models were already a part of set theory before the turn through Tarski's work. However, there was no fruitful way to work with them or even a uniform methods of how they could be build and studied. Set theory after the turn delivers exactly this.

While these first two criteria detail how the old and new theories differ, Post introduces two further criteria that show how they fit together. For him, continuity expresses the adherence of the new theory with general laws of the old one and other theories that might not overlap with it. In mathematics this can be spelled out by requiring that the new theory is consistent with the old and other ones. This is standardly required in mathematical practice, e.g. when extending theories of numbers like the real to the complex numbers. While such an inter-theoretic *consistency* might not always be immediately achievable, such as in the case of infinitesimals, a theory is generally only accepted in full if it is provided. The fourth criterion is called the *General Correspondence Principle*: "Roughly speaking, this is the requirement that any acceptable new theory L should account for the success of its predecessor S by 'degenerating' into that theory under those conditions under which S has been well confirmed by tests." (Post, 1971, p. 228) This is the case for both aspects of set theory: The approach via models in foundations can be condensed to talking about axioms by the correspondence between semantic and syntax. Also, the broader view on infinity in a multi-model setting can be focused on the standard model V.

I conclude that the model-based turn in set theory is a case of a non-Kuhnian revolution in mathematics in a double manner: Not only did it introduce a profound change for each of the areas connected to set theories main epistemic functions - providing a foundation and studying infinity - it also provided a unified and fruitful methodological basis for interactions between these areas. This is what Kanamori (2008, p. 374) calls "the transformation of set theory into a modern, sophisticated field of mathematics".

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2.2 David Corfield - The Fivefold Way: Category Theory, Physics, Topology, Logic and Computation

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A number of authors have observed that various subsets of the quintuple of category theory, physics, topology, logic and computation share common ground. To mention just a few moments in this five-way convergence, the logic-computation relation may be dated from at least the 17th century with Leibniz's Universal Calculus. Topological semantics for intuitionistic logic was investigated by Tarski and McKinsey from the 1930s. Plenty of work from the 1970s onward related computer science to category theory. The recent homotopy type theory, closely related to the category-theoretic concept of an infinity-topos, has been dubbed by Michael Shulman "The logic of space" [1]. Physics and category theory have been united via the concept of monoidal categories in the work of Bob Coecke and colleagues, with connections to quantum computing [2].

Some have looked to be explicit about these connections. Robert Harper jokingly named the doctrine that good ideas in computation, category theory and logic coincide, Computational Trinitarianism, three manifestations of the same notion [3]. Meanwhile John Baez and Mike Stay depict category theory's ability to represent the common core of the other four components via the analogy of the Rosetta Stone [4].

The recent work of Sati, Schreiber and coauthors [5] brings this confluence of ideas to its pinnacle of development. Here we see that linear homotopy type theory, itself the internal logic of a certain kind of infinity-category, provides a certification language for quantum computation, both in terms of hardware in the form of verification of topologically protected quantum gates, and in terms of software in the form of a programming language.

Such elaborate relations are now used as important pointers in theory development. As with the navigational idea of triangulation, constructions in one branch that have meaning in others are taken as signs of being on track. Out of this framing of a fivefold way emerges as strong a notion of realism as we might hope for. In this talk I shall explore these themes.

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[3]R. Harper, "The Holy Trinity" (2011), https://existentialtype.wordpress.com/2011/03/27/the-holy-trinity/

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2.3 Cesare Cozzo - An Epistemic Conception of Deductive Validity

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Let a *deduction* be a linguistic act of transition from premises to conclusion such that the speaker-reasoner undertakes the commitment that the premises provide conclusive rational support for the conclusion. A deduction is valid if, and only if, the premises do in fact provide a conclusive rational support. I distinguish conceptions of validity that are realistic, i.e. non-epistemic, and conceptions of validity that are epistemic. According to a realistic (i.e. non-epistemic) conception of validity, the concept DEDUCTIVELY VALID is entirely independent of epistemic notions. A widespread non-epistemic conception is the classical model-theoretic analysis according to which an inference is (logically) valid if, and only if, the conclusion is true in all structures where all the premises are true. On the other side, a conception of validity is epistemic if, and only if, the corresponding concept DEDUCTIVELY VALID presupposes an epistemic concept. Concept X presupposes concept Y if, and only if, a subject S possesses concept X only if S possesses concept Y (this relation is not symmetric and not antisymmetric). Various epistemic conceptions of deductive validity were elaborated by Dag Prawitz in many essays (e.g. Prawitz 2005, 2015, 2024, cf. Cozzo 2024). In this talk I shall present an epistemic conception of validity that is different from those elaborated by Prawitz. I shall focus on the epistemic concept of COGENT INFERENCE. A cogent (deductive) inference is an inference that compels us to accept the conclusion if we accept the premises and we aim at knowledge and truth. In Cozzo 2019 the notions of aiming at knowledge and truth, and of cogency, are explained in terms of epistemic virtues. In particular, an inference I is cogent in a context C of epistemically virtuous reasoners if, and only if, in context C inference I stands up to all the objections raised by the reasoners in C. As regards the relation between validity and cogency, one can distinguish three kinds of epistemic conceptions of validity: reductive, strong, and regulative. I propose a regulative epistemic conception of validity. The regulative conception is the following:

1. the concept DEDUCTIVELY VALID presupposes the concept COGENT DEDUCTION;

2. the concept COGENT DEDUCTION presupposes the concept DEDUCTIVELY VALID;

3. "deduction *I* is cogent" does not imply "deduction *I* is valid";

4. "deduction *I* is valid" does not imply "deduction *I* is cogent";

5. the concept DEDUCTIVELY VALID is a *regulative idea* for deductive practice.

By "deductive practice" I mean the entire complex of actions consisting of making deductions, accepting them, challenging them, justifying them and retracting them. My presentation will explain the crucial claim 5 that deductive validity is a *regulative idea* for deductive practice on the basis of seven general principles on regulative ideas presented in (Cozzo, 2025).

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2.4 Reinhard Kahle - What is Semantics?

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Set-theoretic semantics is widely regarded as the standard approach to assigning meaning to formal languages. We will show how the formal notion of set-theoretic semantics relieves the mathematician of having to take a stand on the fundamental question of realism and anti-realism. The burden of this question is thus placed on set theory alone.

The historical development of the notion of semantics reveals how the realist objective that was present at the beginning could have been set aside when an additional layer of idealization was installed between the formal language and the intended realistic realm. This approach, however, was challenged by the intuitionistic endeavor to dispense with semantics as an external tool altogether.

Thus, in the second part we compare this intuitionistic ambition with the standard approach. The discussion also addresses the programme of proof-theoretic semantics which renders the meaning of logical constants without reference to truth.

2.5 Ladislav Kvasz - The Fregean Revolution in Logic. A Kuhnian Reconstruction

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In his paper *Funktion und Begriff* from 1891 Frege described the creation of his *Begriffsschrift* as "the next step forward" in the development from arithmetic (calculation with individual numbers), algebra (transition to the literal notation) and calculus (recognition of general laws about functions). If we introduce a refinement into Kuhn's theory and discriminate four kinds of scientific revolutions (idealizations, re-codings, relativizations, re-formulations see Kvasz 2014, pp. 81-82), the characterization given by Frege corresponds to re-codings. This means that he viewed the invention of the propositional logic as the creation of a new instrument of symbolic representation.

In the paper I will argue, that this interpretation is not radical enough. I will try to show that the transition from the Aristotelian syllogistic logic to the Fregean predicate calculus has all the features of an *idealization*. In a way it is thus parallel to the transition from Egyptian and Babylonian arithmetic, which was a more or less empirical and practical discipline to Greek geometry, which is a deductive and theoretical science.

An idealization can be characterized by means of three aspects of the scientific language, which I suggest calling relational synthesis, compositional synthesis and deductive synthesis. Egyptian and Babylonian arithmetic differs from Euclidean geometry in these three aspects. Thus, concerning relational synthesis, a number can be smaller, equal or greater than another one, two geometrical figures can be similar. Thus, geometrical objects enter into a richer system of relations. Concerning compositional synthesis (i.e. the question of how are complex objects composed from the single ones) a number can be complex only due to its magnitude, and large numbers are composed by simply adding a unit several times. In contrast to this, complex geometrical figures are composed from the single ones (from points, straight lines and circles) in the process of a construction. And concerning deductive synthesis, in arithmetic the answer to a problem is achieved simply by applying arithmetical rules, while in geometry we are dealing with a proof using rules of logical inference.

In this way I will introduce a distinction between theories of an arithmetical kind, which have relational, compositional and deductive synthesis similar to that of arithmetic, and theories of mathematical kind, the relational, compositional and deductive synthesis of which is similar to that of geometry. I will further argue, that Aristotelian syllogistic logic is a theory of the arithmetical kind while Fregean logic is a theory of the mathematical kind. That means that they differ with respect to the relational, compositional and deductive synthesis.

If we consider the relational synthesis, and take two concepts, say *A* and *B*, they can enter into Aristotelian logic in four different relations: every *A* is B, no *A* is *B*, some *A* is *B* and some A is not *B*. This is analogous to the three relations that can exist among two numbers. In contrast with this, in the Fregean system two concepts can enter into many different relationships that can be represented by means of the *Begriffsschrift*. Actually, as the very term *Begriffsschrift* indicates, Frege developed it in order to be able to analyse concepts (and their relations).

If we take the compositional synthesis, it is interesting to notice, that syllogistic logic does not have any compositional synthesis, i.e. it does not construct any complex propositions. Every proposition of the Aristotelian logic is a connection of one subject with a single predicate by means of the copula. We can quantify the subject, and the copula can be positive or negative, and that is all we can have. Four kinds of propositions. If we compare this by the complex and complicated pictures representing propositions in the *Begriffsschrift*, we see the difference in the compositional synthesis. And again, in a sense, the simplicity of the propositions of the Aristotelian logic resembles the simplicity of numbers in arithmetic, while the complexity of the Fregean formulas resembles the complexity of the diagrams that accompany theorems in Euclidean geometry.

And finally, if we take the deductive synthesis, we find the same difference. The derivations of the syllogisms in Aristotelian logic follows rather simple rules, which in the middle ages were called **c**onversio, **m**uta, **p**er accidens and **s**impliciter, by means of which a mode of syllogism could be reduced to a syllogism of the first figure. The first letter of the name of the syllogism showed to what first figure syllogism it was to be reduced (*Cesare* being reduced to *Celarent*, and *Bocardo* to *Barbara*). The letter immediately following the vowel indicated the method of proof. For instance, *Bocardo* contained a **c** that meant that by means of a conversio it could be reduced to *Barbara* (see Beaney 1996, p 272). Again, this almost mechanical codification of the proofs is parallel to the simple rules for counting. In contrast to this the different proofs in the Fregean system cannot be so simply reduced to a procedure, and by looking at a formula, it is not clear, how the proof should be constructed. A reader of the *Begriffsschrift* often admires the ingenuity of the arguments of Frege.

If this analysis is correct and the transition from the Aristotelian to the Fregean logic consisted really in idealization then it enables us to reconstruct this transition. We have simply look, how Frege changed the relational, compositional and deductive synthesis of the symbolic language, by means of which he constructed his logical system. Of course, one of the crucial steps was the replacement of the Aristotelian analysis of a proposition into a subject and a predicate by an analysis of the proposition into arguments and function. As the concept of function was introduced in the calculus (actually by Leibniz), this led Frege into believing that his logic is a further step in the line leading from arithmetic through algebra to calculus. Nevertheless, such an understanding of the Fregean revolution does not allow us to reconstruct the transition from the Aristotelian logic to the Fregean one. Only if we understand that the transition consisted actually in idealization, can we offer a rather natural reconstruction of this shift as a change of the relational, compositional and deductive synthesis of the language by means of which logic is formalized.

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2.6 Sara Negri - Invertibility of Logical Rules

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Gentzen's sequent calculus **LK** from 1938 has sequents of the form $\Gamma \to \Delta$, with lists of formulas on both sides of the sequent arrow. **LK** is a classical calculus, and therefore implication could be left out, with a resulting duality between & and $\vee$, and between $\forall$ and $\exists$, with $\neg$ being self-dual.

The two-premiss rules $\& R$ and $\vee L$ have shared contexts, a property which restricts the space of root-first proof search. In Gentzen's calculus there are then two rules for left conjunction and right disjunction:

$$\frac{A, \Gamma \vdash \Delta}{A \& B, \Gamma \vdash \Delta}\ \& L \qquad \frac{B, \Gamma \vdash \Delta}{A \& B, \Gamma \vdash \Delta}\ \& L \qquad \frac{\Gamma \vdash \Delta, A}{\Gamma \vdash \Delta, A \vee B}\ \vee R \qquad \frac{\Gamma \vdash \Delta, B}{\Gamma \vdash \Delta, A \vee B}\ \vee R$$

Note that none of these four rules is invertible.

The goal of invertibility of all propositional rules was fully achieved by Oiva Ketonen, who studied with Gentzen in Gottingen during 1938-39. His thesis of 1944 contains the classical invertible sequent calculus for classical propositional logic, in which root-first proof search terminates. The modification proposed by Ketonen has a single left conjunction and right disjunction rule:

$$\frac{A, B, \Gamma \vdash \Delta}{A \& B, \Gamma \vdash \Delta}\ \& L \qquad \frac{\Gamma \vdash \Delta, A, B}{\Gamma \vdash \Delta, A \vee B}\ \vee R$$

Ketonen's calculus also has invertible rules for implication. By invertibility, if a top sequent is not initial, a countermodel for it gives a countermodel for the end sequent.

In Ketonen's own words (translated from the Finnish original of 1943, cf. Ketonen 2022) what was achieved for the classical propositional calculus is the realization of Leibniz's dream for the small world of classical propositional logic:

> The proving of formulas of the propositional calculus in sequent calculus proceeds in a completely mechanical way, it does not require any invention, it is just decomposition and reassembly. This is based on the fact that the rules of inference of PC in sequent calculus represent equivalences, the upper sequents in the figures can be derived from the lower sequents, albeit in some cases with some provisos that do not materially change the outcome.

Ketonen's calculus, made known by the long review of Bernays (1945), matches the modern invertible **G3** sequent calculus, differing only in the quantificational part. To fully support root-first proof search, the principal formulas $\forall x A(x)$ and $\exists x A(x)$ in $\forall L$ and $\exists R$ must be repeated in the premises, allowing new instances if $A(t)$ fails.

Research in proof theory has led, through Ketonen, Kleene, and Dragalin, to the remarkable **G3** sequent calculi for first-order logic in which root-first proof search by just the logical rules, all of the structural ones left apart, gives a complete system.

We review Ketonen's discovery and the first application he gave in his thesis: A proof of completeness of classical propositional logic that gives, because of invertibility, a *perfect match between syntax and semantics:* The criterion of tautologicity is, by two trivial steps, equivalent to the criterion of having all top sequents initial.

Further applications in Ketonen contain an improvement on Gentzen's midsequent theorem, namely that if any midsequent is derivable, a *weakest one* is, and its use for underivability results in plane projective and affine geometry.

In a second part of the talk, we show how invertibility of logical rules can be obtained also for intuitionistic propositional logic (and beyond, for modal and non-classical logics), through the added syntactic structure of labelled sequent calculus. A result analogous to Ketonen's shows how this extra structure generates countermodels for any sequent that is not derivable.

We conclude by observing that whereas for classical propositional logic termination of proof search is guaranteed by invertibility of the rules, for intuitionistic logic, termination and invertibility no longer come together in a straightforward manner.

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2.7 Georg Schiemer - Early Metatheory in the Type-Theoretic Tradition

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Modern logic is essentially model-theoretic in character. Logical languages are treated as formal languages that can be interpreted in different structures. This model-theoretic approach to logic was developed as late as in the 1950s, in a series of papers by Alfred Tarski, Abraham Robinson and John Kemeny (among others). Nevertheless, model theory has an interesting prehistory, tracing back to work on logic and mathematics in the nineteenth and early twentieth century. In this talk, I want to investigate a particular episode in this prehistory of model theory, namely several attempts by Rudolf Carnap and Alfred Tarski from the late 1920s and early 1930s to formulate the metatheory for axiomatic mathematical theories within a type-theoretic framework.

The development of modern mathematical logic has often been described in the literature as follows: There have been, broadly speaking, two general traditions in 19th and early 20th century work on logic, namely the "algebraic" tradition and the "type-theoretic" or "universalist" tradition. The first includes work by figures such as Schröder, Löwenheim, and Skolem, the second tradition has been shaped by the works of figures such as Frege, Russell & Whitehead, Carnap, and Tarski. Research in the first tradition eventually led to the consolidation of first-order logic as the standard logical system. In turn, logicians of the second tradition typically worked with type-theoretic logics based on Russell & Whitehead's *Principia Mathematica*. Moreover, the development of model-theoretic semantics is rooted in this algebraic tradition and not in the type-theoretic tradition. Thus, the type-theoretic tradition is considered to be largely independent of the "birth of model theory".

This historical picture of the evolution of modern logic has often been accompanied with a deeper philosophical assumption concerning these two traditions. A central evaluative assumption—developed in work by Dreben, van Heijenoort, and Hintikka, among others—is that there are incompatible *conceptions* of logic corresponding to these two traditions. Thus, the proponents of these two logical traditions not only worked with different logical frameworks, but that these logics differed also conceptually. An important consequence of this picture is that for proponents of the universalist view—such as Russell, Carnap, as well as others working with *Principia Mathematica*-style logics—model theory and metatheory in general were principally inconceivable. Thus, it has been claimed the modern model-theoretic viewpoint in logic was not compatible with the type theoretic tradition. Specifically, it has been argued that a number of characteristic features of the model theoretic approach are simply missing in the type-theoretic approach. These include, first of all, a proper distinction between object- and metatheory in the discussion of "semantic concepts" such as truth in a model, consequence, and validity. Moreover, what is also missing is the notion of a formal language and thus the very idea of the (*re-*)*interpretability* of a language. Type-theoretic languages are usually conceived as universal frameworks and thus as "meaningful formalism", i.e. as languages with a fixed interpretation.

The main objective in this talk is show how this incompatibility claim needs to be qualified, at least concerning the work of the second generation of type-theoretic logicians. It wants to show that important advances to model-theoretic semantics were made within the type-theoretic tradition in the 1920s and 1930s. In particular, the paper will discuss and compare different attempts by Rudolf Carnap and Alfred Tarski to express the model theory of axiomatic theories *within* a type theoretic framework prior to and after the metatheoretic turn around 1930. As will be shown, the formal explications of semantic notions (such as "interpretation", "model", "satisfaction", and "truth") as well as of metatheoretic notions of completeness (like "categoricity" and "semantic completeness") in their work from the period in question differ substantially from the modern model-theoretic definitions: these concepts are not formulated in an interpreted metatheory but in a single type-theoretic system. Thus, prior to the introduction of a precise object-/metalanguage distinction in Tarski's work on truth, type theory was conceived not as a separate metatheory (or metalanguage) but as a "universal" logical system in which both an axiomatic theory and its semantic metatheory were to be expressed. Nevertheless, as will be shown, their results are clearly compatible and in fact anticipated some of the now standard model-theoretic techniques at use in modern logic.

The aim in this talk is to analyze these early contributions to model theory in the type-theoretic tradition and evaluate their place in the development of model theory. More specifically, the paper has two goals, one systematic and one historical in nature: The first goal is to show how genuinely metatheoretic concepts are formulated in their early work within a type-theoretic language. A central task here will be to specify the details how the model theoretic approach is expressed in Carnap's and Tarski's work. In particular, how is the central notion of modern model theory, namely *domain variation*, simulated in their accounts? As will be shown, there are different ways in which the model- and domain variation underlying these notions is recast within a fully interpreted logical framework. Specifically, we discuss two methods present in work by both authors, namely (i) *type relativization* and (ii) *type flexibilitation*. According to the first method, the modern notion of domain variation is given by restricting the fixed range of type theoretic quantifiers to model domains of theories expressed in that language. In the second method, model variation is given by a flexbilization of the ties between the syntax of a language and its intended type-theoretic interpretations.

Based on the discussion of these technical details and a comparison with their modern model-theoretic methods, the second, more informal goal of the talk is to analyze the philosophical implications of these early attempts to develop formal semantics *within* type theory for the general evaluation of the historical development of mathematical logic and model theory. Specifically, I will suggest a reevaluation of several standard claims in the existing literature concerning the relationship between the type-theoretic tradition in logic and the consolidation of modern model-theoretic semantics.

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2.8 Göran Sundholm - An Honest but Hitherto Neglected Account of Platonism: The Case of Heinrich Scholz

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In the past I have used the term *Ontological Descriptivism* to indicate an "absolute presupposition" - in the sense of R. G. Collingwood - in currently predominant realist attitudes within the Philosophies of Language, and of Logic, especially modal logic, when cast in the possible worlds idiom. Ontological Descriptivism, then, takes the attitude that the ontology should provide the norms for knowledge and truth, but also for meaning and meaning fullness; how matters stand in the ontology, what state of affairs obtain, should provide answers to question about truth and knowledge, about meaning and meaningfulness. "Naïve", unreflected Platonism in the Philosophy of Mathematics appears to be as good example as any of Ontological Descriptivism. However, the tendency towards favouring ontologically inclined answers is not stated *explicitly*. Such doctrines and tendencies show themselves in the unfolding of the chosen positions.

The matter brought to the surface in Frege's explanation of the universal quantifier, in GGA, Vol **1**, § 8, emended by the quantifier domain explicit:

$$(*)\ (\forall x \in D)A \stackrel{\text{def}}{=} \begin{cases} \text{The True} & A[a/x] = \text{The True whenever } a \in D \\ \text{The False} & \text{otherwise} \end{cases}$$

According to Frege a proposition is a *sense*, that is, way of indicating a truth-value. It is not clear that this stipulation does yield a way of indicating a truth-value. It proceeds by means of an undecided separation of cases. When the domain D is infinite, as it *per force* has to be in the case of mathematics, or indeterminate, we have no way of deciding which of the alternatives, *if any*, does obtain, whence the propositionality of the Fregean quantifier spelled out thusly is open to doubt. These deliberations lie at the basis of Brouwer's 1908 criticism of the ***L***aw of ***E***xcluded ***M***iddle in mathematics. ***W***e cannot effect the decision in question; so current Realists let Reality make that decision.

Realism here simply postulates that one of the two alternatives *does* obtain, whence, according to it, LEM holds also for the quantifier (*). To those with a tender conscience Bertrand Russell's words in his classical *Introduction to Mathematical Philosophy* from 1919 (that was written in prison) spring to mind:

> The method of "postulating" what we want has many advantages;
> they are the same as the advantages of theft over honest toil.

Heinrich Scholz was a classically Berlin educated Philosopher-Theologian at the very Pinnacle of the German "Mandarinate", with Adolf von Harnack as his *Doktorvater*. His first Chair came 1917, in Philosophy of Religion, at Breslau (now Wroclaw, Pl.) and in 1919 he moved to Kiel as Professor in Philosophy. There in the University Library he came across a copy of Whitehead and Russell, *Principia Mathematica*, and it changed his life. Kiel had shortly before been the seen of the Sailor's Soviets and their suppression by "bloodhound" Gustav Noske. The realist foundations presupposed to logic in *Principia Mathematica*, and also in Bolzano's *Wissenschaftslehre*, gave Scholz yet again firm ground upon which to stand in the fluid *Panta Rei* situation at Kiel. Henceforth, and from 1928 at Münster, he devoted all of his considerable energy to Mathematical Logic and the Foundations of Mathematics, which, in 1943 also became the formal title of his Professorial Chair. Scholz was a skilled organizer and expositor; he wrote the second edition of the foundations of mathematics and logic for Felix Klein's *Enzyklopädie der Mathematischen Wissenschaften*, and his pupil and co-author Gisbert Hasenjäger brought out the magisterial *Grundzüge der mathematischen Logik* posthumously in 1961.

The Preface and Introduction to this remarkable book are dated 1956 and they form the object of my inquiry. The main text of the book is virtually impossible to learn from, since Scholz uses a fomalized metalanguage. The introductory philosophical parts, though, are superb, and offer a very interesting novel approach to the tension between Platonism and Constructivism. The theologian Scholz is very much aware of the epistemic slack that clings to Platonism. One does not *know* the result of a demonstration when it deploys non-constructive methods. His logic is based on an ontology, the same ontology that has withstood the test of mathematical practice, after the fashion of Weierstrass, and to that extent it has been "crisis proof". However, an epistemic foundation is lacking. Scholz is very much aware of this, and honest enough to admit. He concedes that there is nothing wrong with the opposite constructive position. That he still remains a Platonist is not because errors within Constructivism, but rather that it would force him to jettison so much that he considers valuable, and is reluctant to lose. In the end he finds that too high a price to contemplate. The talk will spell out some of these considerations.

2.9 Claudio Ternullo - The Continuum and the Absolute Infinite: An (Alternative) Tale of the Continuum Problem

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[Hamkins, 2024] discusses an alternative scenario for the history of the Continuum Problem based on the fact that, provably, under the Continuum Hypothesis (CH) there is a unique (up to isomorphism) smallest countably saturated real-closed field. So, the author suggests, one could legitimately imagine that, had mathematicians come up with a formal theory of the *hyperreal field* (a real-closed field itself) long before [Robinson, 1966], that is, around the time when the first axiomatisation of set theory was laid out, they might have considered CH a fundamental axiom as much as, presumably, the ZFC axioms.

In this talk, by following the spirit, but not the results of Hamkins' argumentative strategy, I will discuss a scenario for the strong failure, this time, of CH by imagining that the continuum was, instead, taken to be an *absolute infinite*, or to have cardinality equal to an *inaccessible cardinal*. The former alternative is introduced in [Conway, 1976], and extensively explored in [Ehrlich, 2012]; the latter is discussed and advocated by [Keisler, 1976]. Now, further "categoricity results" concerning, on the one hand, Conway-Ehrlich's *absolute* continuum, and, on the other, *κ-saturated* hyperreal fields, where κ is inaccessible, analogous to Hamkins' result referring to countably saturated hyperreal fields, are also available, and I argue that such categorical characterisations of "maximal" versions of the hyperreal field are more *natural*, since the additional hypotheses they require are less controversial than CH. However, the underlying conception of the continuum they presuppose is, as is clear, highly questionable.

The idea that the continuum is absolutely infinite may already be found in Peirce's works dating to the 1890s (cf. [Peirce, 2010]) that thrive on the Cantorian distinction between the *transfinite* and the *absolute infinite* (for which see, among other works, [Cantor, 1885]). However, the Peircean conception, as well as those of all the other authors mentioned above, is problematic, from a purely Cantorian point of view, insofar as it entails the existence of *infinitesimals*. But historically, the hypothesis that the power of the continuum might exceed any cardinal producible in ZFC is also stated, if casually, by [Cohen, 1966], mainly as a (philosophical) corollary of the proof of the independence of CH from ZFC, hence on grounds altogether different from those of the supporters of infinitesimals.

Hence, in light of the aforementioned categoricity results concerning maximal versions of the hyperreal field, and with a view to providing more robust philosophical backing to the hypothesis that the continuum could be taken to be absolutely infinite, the talk will also examine salient moments of the history of the Continuum Problem where such an unorthodox conception has appeared and has been philosophically advocated.

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3 Contributed talks

List of contributed speakers (in alphabetic order):

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3.1 Moritz Bodner - The Twofold Origin of Proof-Theory

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The development of Hilbert's proof-theory can be segmented into four phases:

I. Inception (1904): In an address to the mathematical congress Hilbert first sketched methods for establishing the consistency of an axiom-system without exhibiting a system of objects satisfying all the axioms[1].

   The axiom-system for which these methods are designed is an exceedingly rudimentary theory of arithmetic, formulated in a prototype of an artificial language whose rules of formation remain vague, as Hilbert's use of parentheses and variables throughout his sketch attests: both are used but neither is included in Hilbert's official list of basic expressions.

   Furthermore, Hilbert fails to clearly distinguish between (object-language) expressions and (meta-language) assertions about these expressions.

II. Refinement (–1922): In 1917/18 Hilbert began working on mathematical logic. In particular, he had began working with artificial languages whose vocabulary and formation rules were stated explicitly and exhaustively.

   When Hilbert returned to his early sketch for a proof-theory, he brought these technical innovations to bear on it, and presented his earlier ideas thus in a methodologically clarified framework but with few substantial changes (he still dealt only with rather rudimentary axiom-systems and used the same arguments to establish their consistency as in 1904).

III. Evolution (1922/23): Between Hilbert's first lecture-course devoted exclusively to proof-theory (as opposed to his lecture-courses in previous years, which were primarily dedicated to mathematical logic, with proof-theory covered, if at all, only briefly) and the appearance in print of his first publication on the matter in 1923, Hilbert made some substantial changes to his methods, but worked mainly to extend them to more comprehensive arithmetical theories. The most notable change in his approach is the inclusion of certain results, which might be described as

[1] At the previous mathematical congress in 1900 Hilbert had observed that the consistency of number theory cannot be established by exhibiting a model – since numbers furnish the domains for the models commonly used – and thus "requires a direct method" (GA III, p. 300).

belonging to "structural proof-theory": They concern not only which formulas are *in principle provable* within a given axiom-system, but also *how* formulas that are provable *are actually proved in particular deductions*; in other words, these results concern the syntactic structure of deductions, i.e. the arrangement of the formulas used as lines in the deduction.

IV. Implementation (1922–): As was Hilbert's way, he effectively left the field when he felt he had done all he could or needed to. He did not withdraw his patronage entirely from the discipline he helped create, however, but left the implementation of his ideas and methods – seeing as the main axis of development their extension to axiom-systems sufficiently strong to provide a reconstruction of classical analysis – to his students and collaborators, chiefly W. Ackermann and P. Bernays.

Throughout his career – with a crucial (but in Hilbert's mind: marginal) exception – Hilbert developed proof-theory as a *theory of provability*. Although Hilbert recognised that studying the structure of deductions was of importance also for his purposes, he never seems to have come to regard it as more than a(n indispensable) preliminary to the proper object of proof-theory, which lay for Hilbert in establishing the consistency of axiom-systems. Hilbert never seems to have regarded the syntactic structure of deductions as intrinsically interesting. By contrast, another member of the Göttinger mathematical community, Paul Hertz, began around the same time (and taking his cues from Hilbert's 1904 sketch) developing the approach to the study of deductions from which what is now called "structural proof-theory" emerged eventually.

Hertz aspired to find a criterion for ascertaining the independence of a system of axioms (and a method for isolating from any given system of sentences a minimal, independent axiom-system). In order to investigate independence by proof-theoretic means – unlike the study of consistency to which Hilbert devoted his efforts – one must pay careful attention to the syntactic structure of deduction – not only of which formulas can appear as last lines of deductions but also of *how* the last line of a deduction is arrived at – because deductions can on the whole be misleading: A valid deduction of more than one line can have as its last line an axiom of a given axiom system, and other axioms as its premisses. Such a deduction would thus, upon first glance, suggest that the axiom in its last line is not independent of the other axioms used as premisses. However, closer attention to the syntactic structure of the deduction might just as soon reveal that the axioms cited as premisses, in fact, play no real role in justifying the last line – are merely "detours", as Gentzen later tantalisingly puts it – and that the deduction at issue thus doesn't show the dependence of the axiom in its last line after all. In pursuit of his aim, to exclude such deceptions, Hertz established – by means of an argument using double induction as Gentzen would later use when establishing his Hauptsatz – different normal form results for the deductive systems he studied. On the basis of these the normal form results Hertz established that: if a sentence is deducible at all from given axioms (within Hertz' limited framework), it is deducible by means of a deduction in which no such deceptive detours are taken. This result furnished a viable criterion for dependence (at least within Hertz' specific setting).

Seeing how Hertz' structural approach to proof-theory was subsequently appropriated by Gentzen also to Hilbert's goals of providing consistency proofs, and later used also used in pursuit of philosophical projects as the basis for the cultivation of a non-referential theory of meaning, raises the question whether Hilbert's approach lends itself to such alternative uses. Simply transferring the central ideas of e.g. proof-theoretic semantics into a Hilbertian setting seems to hold little promise, since they rely mainly on the very aspect of deductions Hilbert mostly ignored: the deduction's distinctive syntactic structure.

Since Hilbert's approach thus appears unfit as a basis for some of the greatest fruits of later proof-theoretical work, which grew, rather, out of the approach to proof-theory Hertz developed independently of Hilbert, one can regard Hertz as a second founder, besides Hilbert, of (what) proof-theory (has come to be).

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3.2 Leonardo Ceragioli - Proof-Theoretic Semantics and Anti-Exceptionalism

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Anti-exceptionalism about logic has received more and more attention in the last decades.[2] Moreover, although it is usually assumed that proof-theoretic semantics adopts an exceptionalist approach, considering logical consequence as grounded on harmony and analyticity, attempts are made to integrate these *criteria* with anti-exceptionalist ones.[3]

According to Read, harmony is sufficient for analyticity, but not for validity, which also requires truth preservation in vernacular language.[4] The focus on vernacular language is justified by its usage in informal arguments, which logic wants to codify: the relation between arguments in vernacular language and formal arguments in logic corresponds to the relation, described by Lakatos, between informal concepts and formal definitions in mathematics. Moreover, still following Lakatos, Read suggests that logical knowledge improves by dealing with "*heuristic falsifiers*" of its laws in vernacular language, just like mathematics improves by dealing with informal "*heuristic falsifiers*" to its theorems.[5] Read's main example regards the distinction between *de dicto* and *de re* modalities, which is needed to make sense of some counterexamples to modal syllogisms considered valid by Aristotle. On the other hand, Cozzo focuses more on pragmatic *criteria* like simplicity, elegance, fruitfulness, intelligibility, and agreement with pre-existing practice.[6]

In my seminar, I will argue that a proper antirealist evaluation of logical validity can be reduced neither to harmony plus truth preservation in vernacular language, nor to harmony plus purely pragmatic *criteria*, nor to a combination of the two. I will consider three examples taken from Dummett to support this claim:

- First, I will focus on Dummett's famous exchange with Putnam about quantum logic to argue that truth preservation is not a sufficient complementation of harmony. Indeed, we cannot evaluate whether the distribution of conjunction over disjunction (the law dropped in quantum logic)

[2](Lakatos, 1976), (Williamson, 2000), (Priest, 2016) and (Hjortland, 2017).
[3]See (Williamson, 2000) for an anti-exceptionalist criticism of proof-theoretic semantics.
[4](Read, 2015)
[5](Lakatos, 1978) and (Read, 2019).
[6](Cozzo, 2002), (Cozzo, 2019), (Cozzo, 2024).

preserves truth just by looking at vernacular language: we need a new theory of meaning to make sense of quantum logic. To consider truth preservation as assessable independently of any conception of meaning is to "seek to have a revolution and minimize it too", by not considering the consequence of change of meaning on such preservation.[7]

- Then, I will consider Dummett's analysis of the modal axiom *B* to show that even a purely pragmatic complementation of harmony is insufficient. Indeed, we have (allegedly) harmonious systems for all traditional modal logics, and contemporary research is finding more and more such systems.[8] Moreover, pragmatic *criteria* support *at most* a pluralist attitude toward modal logics. However, Dummett applies meaning-theoretic considerations to defend (against (Kripke, 1980)) the possibility that fictional objects could have existed, and then uses this possibility to present a counterexample to modal axiom *B*: truth is not preserved, when this axiom is exemplified with the sentence "unicorns do not belong to order *Artiodactyla*".[9] Hence, truth preservation is needed for logical validity, but it has to be evaluated inside a theory of meaning.[10]

- As a last example, I will deal with Dummett's criticism of multiple conclusions to show that even a combination of pragmatic *criteria* with truth preservation is still insufficient: multiple conclusions are a powerful tool, and do not lead to obvious violations of truth preservation when a pre-theoretic evaluation of vernacular language is considered. Moreover, classical logic can easily be shown to be harmonious using multiple conclusions. However, there are considerations about the circularity of meaning that make them unacceptable, which are reducible neither to truth preservation nor to pragmatic *criteria*.[11]

From all these cases, it emerges that truth preservation can be *at most* evaluated *after* a meaning-theoretic investigation of this notion: pre-theoretic truth preservation is a *chimera* for all interesting laws of logic. Moreover, pragmatic considerations constitute only circumstantial evidence of logical validity.

In conclusion, Dummett's justification of logic consists essentially of two components:

- truth preservation in vernacular language *plus* pragmatic *criteria*;

[7] (Putnam, 1975) and (Dummett, 1976).

[8] (Poggiolesi, 2010), (Negri and von Plato, 2011), (Read, 2008).

[9] (Dummett, 1996).

[10] The relevance of a theory of meaning in enabling this counterexample seems to be overlooked in (Williamson, 2017).

[11] (Dummett, 1991) and (Steinberger, 2011).

- meaning-theoretic *criteria* (of which harmony is only a part).

Regarding the first component, truth preservation assures that our logical system adheres to vernacular language enough to understand the point of the logical rules, while pragmatic *criteria* exclude rare or uninteresting counterexamples to laws of logic.[12] The second component assures that the logical system is supported by an acceptable theory of meaning: *e.g.* it does not presuppose a realist conception of meaning, meaning is communicable, and its rules are harmonious.

These two components of logical justification cannot be addressed independently. In particular, truth preservation cannot always be evaluated regardless of a theory of meaning, truth being a key ingredient of such a theory. Logical laws in particular always require a deep investigation into meaning, because their everyday exemplifications are trivially true, while their alleged counterexamples always require philosophical investigation. For example, they can be grounded on vagueness, inaccessible truths, paradoxes, and counterfactual conditionals. This point should not be controversial, even for an anti-exceptionalist logician focusing on vernacular language. Indeed, that a theory of meaning (or at least a theory of language) is needed to decide the validity of logical laws was already clear to Quine, who rejected modal logic not because of a violation of truth preservation or pragmatic principles, but because of its philosophical unclarity. Admittedly, we should not ask Dummett to be more anti-exceptionalist than Quine.

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[12] See (Quine, 1986) and (Russell, 2018) for opposite approaches toward such counterexamples: the first excludes tensed verbs and empty universes for pragmatic reasons, while the second argues for logical nihilism from quite controversial counterexamples.

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3.3 Michele Contente - From the Decidability of the Proof-Relation to the Decidability of Type-Checking

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The problem of decidability of type-checking can be stated as follows: given a context Γ, a term p and a type A, can we decide whether the judgement $\Gamma \vdash p : A$ is derivable?
Naturally, the answer depends on the specific type system being considered and subtle syntactical details of the formalization are often crucial.
Possibly, the issue becomes even more complex with dependently typed systems such as various versions of Martin-Löf Type Theory. In presence of the *conversion rule*[13], the decidability of type-checking depends on the decidability of judgemental equality. Therefore, the type-checking algorithm needs to incorporate an algorithm to decide equality[14]. Finally, as proven in [Salvesen 1988], the decidability of type-checking is also sensitive to whether the syntax is fully annotated or not.
Under the assumption of the propositions-as-types doctrine, *proof-checking* essentially reduces to type-checking. The decidability of the latter ensures that we can mechanically check whether an alleged proof is really a proof. For this reason, the decidability of type-checking is usually regarded as a fundamental component in formalizing mathematics within *proof-assistants* and verifying the correctness of formalized proofs. This perspective can be traced back to the pioneering work of De Bruijn [De Bruijn 1970]. This constitutes the main motivation for including the decidability of type-checking among the desiderata that a type theory should satisfy.
In [Martin-Löf 1975], the decidability of type-checking is derived as a corollary of strong normalization and the decidability of conversion for an intensional version of type theory. In particular, Martin-Löf interprets this result as evidence that his system of type theory satisfies the *adequacy condition* proposed by Kreisel in [Kreisel 1970] for a theory of constructions meant to provide the intended interpretation of intuitionistic logical constants. This condition amounted to the requirement that it should be decidable whether a construc-

[13] The conversion rule is the rule saying that from $a : A$ and $A \equiv B$ (where "$\equiv$" stands for judgemental equality), then $a : B$ follows.

[14] This also explains why type-checking is undecidable in *extensional* type theory when the reflection rule for equality is introduced [Martin-Löf 1984].

tion p is a proof of a given proposition A: we recognize a proof when we see one.

Kreisel introduced his theory of constructions in [Kreisel 1962]. The theory was explicitly designed to satisfy the adequacy condition. Foundationally, the main achievement would have been distinguishing the intended interpretation of intuitionistic logical constants from other constructively meaningful ones (e.g. Kleene realizability), which do not satisfy the adequacy condition. The theory was based on two primitive entities: *constructions* and *notions*. A notion is a *decidable* property of a construction. An untyped universe is assumed and the proof-relation "c is a proof of A" is taken to be decidable. It is from this assumption that basically follows the need for second-clauses in the interpretation of implication and universal quantification as well as the possibility of indefinitely iterating the proof-relation. It turned out that the naive formulation of the theory was inconsistent and, moreover, it was unclear whether the proposed amendments could fulfill the theory's original purposes. In particular, maintaining the following three requirements simultaneously seems to be problematic: (i) untyped universe, (ii) decidability of the proof-relation and (iii) proof-relation intended as a proposition (see [Beeson 1985])[15]. Furthermore, it is clear that Martin-Löf, in [Martin-Löf 1975], had in mind a completely different interpretation of the adequacy condition.

A penetrating analysis of Kreisel's proposal is presented in [Sundholm 1983]. Sundholm points to a specific conceptual confusion in Kreisel's analysis of the notion of proof. First, treating the proof-relation as a proposition introduces a regress problem in the meaning explanations, which essentially stems from the possibility of indefinitely iterating the proof-relation. Second, and more fundamentally, Kreisel neglects the distinction between two senses of construction: *construction as process* and *construction as object*[16]. A construction-object is the result of a construction-process, but the two notions must not be conflated. Accordingly, we must carefully distinguish between *propositions* and *judgements*, as emphasized by Martin-Löf in various places. Hence, the distinction between the proof of a judgement and the proof of a proposition becomes compelling. Once such a clarification is made, then a different reading of the adequacy condition is available, which aligns with Martin-Löf's remark about the decidability of type-checking mentioned above.

This presentation has two main objectives. The first is essentially of a historical nature: clarifying the philosophical motivations behind Kreisel's introduction of the adequacy condition, which might appear somewhat obscure today. Apparently, the main source of inspiration for the adequacy condition was the

[15]See [Dean & Kurokawa 2016] for a detailed analysis of the problem.

[16]In [Scott 1970] a similar distinction is proposed.

analogy with the provability predicate as formalized in arithmetic (see [Sundholm 1983] and [Troelstra 1969]). As already observed in [Sundholm 1983], there are several passages in Wittgenstein's *Remarks on the Foundations of Mathematics* that could have influenced Kreisel, although he denied any direct influence. However, it is worth noting that Kreisel's review of Wittgenstein's *Remarks* [Kreisel 1958] insists on several passages discussing the notion of proof and even the issue of proof recognition is mentioned (p.143, *ibid*.).
Gödel emphasized the contrast between the formal notion of proof, which is a finitary notion, and the abstract notion of proof in [Gödel 1953/59]. A proof is an abstract object if understood in its original "contensive"[17] meaning. In this sense, a proof is "a sequence of thoughts convincing a sound mind" (p.341, *ibid.*). Gödel's notes for Zilsel's seminar [Gödel 1938] are also relevant. There Gödel discussed Heyting's proof explanations for intuitionistic logic and alternative approaches, mainly focusing on the problem of "impredicativity" in the proof explanation. Gödel insisted on the need for a logic-free theory and listed the decidability of primitive notions among the conditions of constructivity.
The second aim is to reassess the debate about the decidability of proofs within Martin-Löf's philosophical framework, showing that it allows for a more satisfactory development of those ideas. Fortunately, a comprehensive presentation of Martin-Löf's views is now available [Martin-Löf 1993] and will serve as our main reference. In particular, we will explore the relationship between the decidability of type-checking and the *analyticity* of judgements in type theory [Martin-Löf 1994]. There, a judgement is defined as analytic if it is evident solely by virtue of the meaning of the expressions occurring in it. Furthermore, Martin-Löf claims that every synthetic judgement is grounded on an analytic judgement and all the judgements of type theory of the form $a : A$ and $a = b : A$ are analytic. In particular, the logic of analytic judgements is decidable and complete, as a consequence of the decidability of type-checking. In [Bentzen 2024] this analysis is thoroughly criticized. The crucial objection is that hypothetical judgements are synthetic. Since there are categorical judgements of the above form whose meaning explanations involve a hypothetical judgement, not all of them can be considered analytic. Furthermore, Bentzen questions the relevance of type-checking decidability in relation to analyticity. We aim to assess these objections and show that Martin-Löf's views about analyticity can ultimately be maintained.

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3.4 Ludovica Conti - Anti-Realist Abstraction?

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In this talk, I explore a deflationist reading of the abstractionist theories in the philosophy of mathematics and discuss the legitimate suspicion that it leads to an anti-realist turn of abstractionism and reveals a unforeseen affinity of the latter with the eliminative structuralism.

Abstractionist theories in the philosophy of mathematics are systems composed of a logical theory augmented by an abstraction principle (AP) of the form $\forall X \forall Y(@X = @Y \leftrightarrow E(X,Y))$[18], originally proposed in order to pursue the project of a Logicist ([8]) or Neologicist ([13]) foundation of arithmetic, namely to support the thesis that we have an a priori knowledge of a Platonist realm of mathematical (abstract) objects. As is well known, such an ambitious agenda requires a commitment to further meta-theoretical assumptions, in particular to an *at face value* reading of the abstractionist vocabulary, a canonical meta-semantics and a so-called "genuine semantics". On closer inspection, these unjustified assumptions beg the question of a Neologicist foundation of arithmetic and turn out to be deeply involved in some unsolved abstractionist problems, such as the so-called Caesar problem and, more generally, the evaluation of mixed identity statements ([3]).

By abandoning one or more of the three metatheoretical assumptions, we obtain various non-Platonist revisions of the abstractionist projects. Such projects are interesting precisely because they originate in a less demanding framework and suggest an unexplored approach to abstraction in which the derivational results are saved and the epistemological goals may even be improved. In particular, without an *at face value* reading, the neologistic Syntactic Priority thesis fails, and various versions of so-called Austere Abstractionism appear ([7]). These projects present a form of abstractionism without Fregean semantics and metaphysics, e.g. Dummett's Intolerant Reductionism, and explicitly endanger the Platonist scenario. Analogously, by abandoning genuine semantics, we get other abstractionist projects without Fregean metaphysics, such as possible forms of fictional abstractionism. On the contrary, the abandonment of canonical metasemantics leads to a less obvious scenario. By canonical meta-semantics

[18] The symbol @ is a term-forming operator. In particular, Neologicism relies on Hume's Principle (HP): $\forall X \forall Y(\sharp X = \sharp Y \leftrightarrow X \approx Y)$, that states the cardinal number of a concept X is identical to the cardinal number of a concept Y if and only if X and Y are equinumerous

we mean a set of assumptions concerning the full referentiality of language, the existence of an intended model, and the assumption of a standard interpretation function. In the non-canonical projects, which I will call "deflationist" (cf. [2]), the semantics is definitely non-Fregean, but the metaphysical scenario is far from clear.

More precisely, the deflationist perspective arises from accepting the semantic indeterminacy exhibited by the abstractionist vocabulary. In virtue of this indeterminacy, abstract terms – as implicit *definienda* of the abstraction principles – deserve to be considered as a special kind of the theoretical terms of science: in both the cases, the theory imposes some constraints on the meaning of the defined terms, but is unable to select a unique interpretation. Such an indeterminacy has recently been spelled out – both in the abstractionist debate and in the debate on the logic of science – in terms of arbitrary reference ([4], [9], [1], [10]). This proposal is in line with Carnap's insight on the "indirect" and "partial" meaning of the theoretical vocabulary of science ([6]) and will be formalised by means of a choice-functional semantics. This approach will allow us to successfully evaluate mixed identity statements, at least on the countable models, and will shed new light on other relevant issues of these programs, such as the logicality of the abstractionist vocabulary.

The deflationist reading, and in particular the arbitrary semantics, seem to undermine the Platonist thesis of the abstractionist projects ([14], [4]), by showing that abstract terms haven't their alleged objectual reference and remain devices for indexing (second-order) equivalence classes. According to the arbitrary semantics, abstract denotations are characterised only by their roles and their relational properties, thus satisfying the so-called Structuralist thesis ([12]). Such a non-Platonist turn of arbitrary abstraction shows clear analogies with Relativist structuralism ([11]), thus suggesting an eliminative spirit of this new path of abstractionism. Relativist structuralism is characterised by a notion of reference that is relative to an arbitrary choice of a model of the theory, thus exhibiting a meta-semantics very similar to our deflationist proposal.

In the last part of the paper, I will explore the relationship between arbitrariness and structuralism in order to answer the opening challenge about the possible structuralist and anti-realist turn of deflationist abstractionism. In light of the semantics mentioned above, the informal notion of arbitrariness has been spelled out in terms of choice functions. To spell out the notion of structurality, on the basis of the debate, I consider a necessary condition of structurality to be invariance under permutation. The relation between arbitrariness and structurality can then be reformulated by asking whether, given the choice-functional interpretation presented above, the abstractionist vocabulary is invariant under permutation.

A partial positive result has already been provided ([4]), by proving the invariance under permutation of the cardinal operator implicitly defined by an arbitrary interpretation of Hume's principle. We can prove also a more general result, by showing that a wide range of abstraction operators, including those implicitly defined by consistent weakenings of Frege's Basic Law V, are invariant under permutation. We note, incidentally, that these results are compatible with two opposite readings, respectively in terms of logicality or – as we pursue – structurality. However, in order to fully support the structuralist thesis, we should also prove that the singular terms obtained by applying the abstraction operator to variables are invariant under permutation. On the contrary, we will prove the negation of such a result. Arbitrary abstract terms, unlike terms for places in a structure, are not invariant under permutation and then prevent the structuralist thesis in its full generality.

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3.5 Matteo de Ceglie, Simon Schmitt - Hierarchies of Theories, Gödel's Programme, and Set-Theoretic Pluralism

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Since Gödel proved his celebrated incompleteness theorems, the phenomenon of independence has become central in set theory, logic, and the foundations of mathematics. As the case of the Continuum Hypothesis (CH) exemplifies, these issues extend beyond consistency statements and *Gödelian trickery* (e.g., theories like $\mathsf{ZFC} + \neg\mathsf{Con}(\mathsf{ZFC})$) to natural set-theoretic questions with significant mathematical content. The decades following the seminal work of [Gödel 1938] and [Cohen 1964] have produced a vast amount of independence results, accompanied by a variety of techniques for constructing models and axiomatisations of set theory. This wealth of results carries the philosophical challenge of how to integrate the often incompatible constructions (such as $\mathsf{ZFC} + \mathsf{CH}$ and $\mathsf{ZFC} + \neg\mathsf{CH}$ together with their respective models) into a coherent philosophical picture. One way of meeting this challenge is Gödel's Programme, which was originally introduced in [Gödel 1964], and which aims to extend ZFC with axioms strong enough to settle independent statements such as CH. However, the crux of the matter is to find the *right* axioms to add to ZFC.

This brought the problem of axiom selection and axiom justification to the forefront of the foundations of mathematics: which axioms are good enough to be considered axiom candidates, and how to compare them and choose the right extension of ZFC? Several different methodologies have been proposed. The best possible justification, according to [Gödel 1947], would be *intrinsic* justification: the axiom has to be necessarily accepted given our intuition of the concept of set. Its nature is intuitively true. Most of the axioms of set theory, Gödel claims, fall into this category. For example, the Axiom of Extensionality or Pairing are both very intuitive and obviously motivated by the iterative conception. On the other hand, the intuitive truth of Martin's Maximum seems less certain, and we have more difficulties in justifying it by appealing to the concept of set. [Gödel 1947] argues that, in the cases where intrinsic justification is not enough, we need to fall back to *extrinsic* justification: the new axiom must have very appealing mathematical consequences. The main linchpin of the extrinsic justification is that the new axioms have to be not only with deep mathematical consequences, but also justified by some *external evidence*. Penelope Maddy, in a series of papers and books (see [Maddy 1988a], [Maddy 1988b], [Maddy 1997], [Maddy 2011]), has spelled out in more detail what extrinsic justification is with her Maximize principle: adding a new axiom to ZFC should make the resulting theory as powerful as possible, in the sense that it should *maximize* the range of available isomorphism types. Further refinements of the notion by [Löwe 2001], [Löwe 2003], and [Incurvati, Löwe 2016] have introduced a new notion of interpretability power. To all these notions we must also add the possibility of ordering all the theories by their consistency strength.

What all these methods have in common is that they allow us to arrange the axioms (and consequently the various set theories) in a hierarchical way. With consistency strength this is easily seen: if $\mathsf{ZFC} + \mathsf{A}$ proves the consistency of $\mathsf{ZFC} + \mathsf{B}$, but not the other way around, we say that $\mathsf{ZFC} + \mathsf{A}$ has a higher consistency strength. The same can be done with all the different methodologies briefly discussed above (having some care in defining them in mathematical terms, but this is possible). Consequently, we can boil down the goal of Gödel's Programme as follows: find an axiom A such that $\mathsf{ZFC} + \mathsf{A}$ settles the independent questions we are interested in (e.g. CH) *and* it is *maximal* according to one (or more than one) of the justification methods.

In this paper, we argue that such a program is destined to fail. In particular, each justification method gives rise to a sufficiently different hierarchy of theories. For example, consider the case of AD and AC. [Mycielski, Steinhaus, Swierczkowski 1971] proved that they are mutually inconsistent, so our choice is between $\mathsf{ZF} + \mathsf{AD}$ and ZFC. If we try to compare AD and AC by consistency strength, we notice that AD gives us the existence of a measurable cardinal (see [Lévy, Solovay 1967]), while AC gives us no large cardinals. However, AC gives us the classic axiomatization ZFC, and it is so fundamental for mathematical practice that hardly anybody prefers to work in $\mathsf{ZF} + \mathsf{AD}$.[19] Consequently, AC is preferable from a naturalist perspective, even if AD is stronger in consistency strength.

For another, more subtle example, consider the debate around the axiom $\mathsf{V} = \mathsf{UltL}$ and forcing axioms like Martin's Maximum (MM). From the perspective of consistency strength, MM sits very high in the hierarchy, between supercompact cardinals and Woodin's cardinal (but the precise consistency strength is still not known, see [Foreman, Magidor, Shelah 1988] and [Shelah 1987]). On the other hand, it is conjectured that $\mathsf{V} = \mathsf{UltL}$ has an even higher consistency strength, at the level of I2 or I3 (see [McCallum 2018], still unpublished). So in terms of consistency strength, we should prefer $\mathsf{V} = \mathsf{UltL}$ over MM. However, from the perspective of Maximize, the situation is different: as [Schatz 2019] shows, $\mathsf{V} = \mathsf{UltL}$ is *restrictive* over MM, thus implying that, from the naturalist

[19] The arguments usually employed in favour of determinacy are for the axiom $\mathsf{AD}^{L[\mathbb{R}]}$, that it is compatible with AC. See for example [Koellner 2009].

perspective, MM is preferable over V = UltL.

Moreover, in each hierarchy we can find theories that are equivalent, and thus sit at the same level (for example, we have several equiconsistent theories, see [Koellner 2009]). The only solution would be to apply more than one method, but in that case the *order* in which the methods are applied matters, and we still get several different hierarchies, with different maximal, incompatible theories on top.

We claim that this a particularly difficult problem for the *universist*, who believes that there is only one set-theoretic universe, instantiated by a single set theory. The only solution for them would be to argue that one justification method is better than the other, thus pointing at only one hierarchy, but no progress has been done so far (see for example [Barton, Ternullo, Venturi 2020]). On the other hand, the pluralist has an easier solution: embrace all the different methods, and just include all the maximal theories (or even more than that) to the set-theoretic multiverse.

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3.6 Marcel Ertel - The Epistemological Status of Transfinite Induction in Reductive Proof Theory

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The debate about the epistemological status of transfinite induction and recursion in the context of Hilbertian reductive proof theory after Gödel's theorems and Gentzen's ordinal analysis of first-order arithmetic Z is treated from a historical and a systematic perspective.

In the first part of the talk, the historical context is set by briefly sketching (*i*) the epistemological assumptions of the Hilbert-Bernays programme; (*ii*) the modifications to the program made necessary by Gödel's theorems; and (*iii*) Gentzen's ordinal analysis as a contribution to the modified programme. One can state the main reductions achieved by Gentzen's analysis of Z in the following way (for the mathematical background, see Arai 1):

$$I\Sigma_1 + DS(\varepsilon_0) \vdash \Sigma_1\text{-RFN}(Z), \tag{3.1}$$

and

$$I\Sigma_1 + TI(\varepsilon_0) \vdash \Sigma_n\text{-RFN}(Z) \quad \text{for each } n < \omega. \tag{3.2}$$

Here DS(ε_0) is the Π_2-statement that there is no strictly monotonically decreasing, primitive recursive sequence of ordinals $< \varepsilon_0$; TI(ε_0) is the schema of transfinite induction up to ε_0 for arithmetic formulas; and Σ_n-RFN(Z) is the uniform Σ_n-reflection schema for Z, stating that any Z-provable Σ_n-sentence is true (note that Con(Z) follows from Σ_1-reflection). Since the theory $I\Sigma_1$ is considered safe, the provability of these reflection schemata constitute a reduction of Z to the corresponding principles of transfinite recursion, resp. induction. The epistemological significance of Gentzen's analysis thus comes down to the status of these principles.

In the second part, we consider selected contributions to the debate about this status by Gentzen, Gödel, Bernays, as well as Tait and Pohlers, and present our own view.

Gentzen 3 sketched a proof of the principle of transfinite induction up to ε_0 and gave a philosophical argument for its constructive nature. We present a modern rendering of this proof, and show that Gentzen's argument is not convincing: his proof implicitly uses an impredicative notion of "accessibility", or equivalently, the property "being an ordinal", in the induction formula, which undermines his claim of its constructive – let alone finitist – nature. This partially vindicates Gödel's analysis 5 of the situation at the time. Gentzen removed this blemish in 4, but provided no further philosophical discussion.

In the second edition of 2, Bernays provided another proof of induction up to ε_0, and a very elegant, seemingly more finite-combinatorial argument, that he claims constitutes a proof of α-recursion for $\alpha < \varepsilon_0$. The former is essentially the standard proof today, and amounts to a variation on Gentzen's 4. For the latter, we again present a modern rendering, and show that its core is the reduction of *un*nested recursion along ω^α to *nested* recursion along α. Following Tait 7, we argue that this puts into doubt Bernays' interpretation of this argument as a (quasi)finitist proof of α-recursion for $\alpha < \varepsilon_0$ (by climbing to ε_0 by iterating the move $\alpha \mapsto \omega^\alpha$, starting from $\alpha = 0$). We show that going from nested α-recursion to unnested ω^α-recursion corresponds to going from α-induction on Π_2-predicates to ω^α-induction on elementary predicates: one "buys" the reduction of ω^α to α by increasing the quantifier-complexity of the induction formula. In a sense, this mirrors the impredicativity in Gentzen's original proof, brought down to a more constructive level. Our interpretation — which can actually be brought into agreement with Bernays' own philosophical views — is that while this proof breaks the restrictions of finitism *sensu stricto*, it is both constructively unproblematic *and* provides a real epistemological gain in the analysis of classical first-order number theory, by shedding light on the combinatorial nature of the well-ordering and the computational complexity measured by ε_0.

We explain this by answering a critique by Pohlers 6, who claims that doubts about the consistency of Z — and *a fortiori* about Σ_1-RFN(Z) — cannot be removed by Gentzen's analysis combined with some proof of transfinite induction up to ε_0. Pohlers' reason is that when one formalizes any such proof, it will invariably involve induction on predicates of unbounded arithmetic complexity; thereby, one implicitly presupposes, and even transgresses, the full strength of first-order arithmetic, which is what was put into doubt in the first place. Hence, one has failed to achieve any epistemological gain. Against this, we argue that Bernays' second proof (and also Gentzen's 4 and Bernays' first proof, suitably presented) does provide such a gain, because it clarifies the combinatorial nature of the well-ordering in question, and reduces arbitrary non-constructive arithmetic induction to a much better understood, computationally explicit, and uniform kind of induction. As a side remark, we also show that Pohlers' own account of reductive proof theory requires exactly such a clarification.

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3.7 Richard Lawrence - Ideal Forms, Real Applications, Missing Content. Hankel's Domain Extension Proof

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Hermann Hankel was an important figure in nineteenth century German mathematics. He was an early advocate of mathematical *formalism*, which he regarded as a kind of idealism. For Hankel, "formal" mathematics is "independent of the determinate content, of the substance of the objects" in any particular domain (Hankel, 1867, 1), and the signs of formal mathematics "receive their formal meaning only through our laying down the rules according to which they can be operated with" (Hankel, 1867, 70).

Similar claims are made often in the history of formalism, and are usually regarded as expressing a kind of anti-realism about mathematics. For the formalist, mathematics is a mere game of manipulating signs according to rules which we are free to set down arbitrarily; there are no meanings "behind" the signs apart from the rules, no sense in which mathematical claims are true or false apart from our determination of the rules, no mathematical objects which such claims are *about*. I would urge, though, that Hankel's formalism should not be read in this way. Despite how similar his view sounds to modern anti-realism, Hankel primarily regarded his formalism as freeing mathematics from the shackles of intuition. For him, formalism was a powerful new tool for developing the foundations of analysis independently of any potential applications in geometric intuition or the physical world.

Hankel's understanding of "formal" mathematics comes from Kant and from Hermann Grassmann. He makes a fundamental distinction between the formal and the "actual" (*actuell*), which he applies both at the level of mathematical science as a whole, and at the level of the individual things these sciences are about. So "formal mathematics" contrasts with "actual mathematics", and within formal mathematics, we have formal numbers and formal operations. Within actual mathematics, by contrast, we have actual numbers (real magnitudes) and actual operations.

To explain this distinction, Hankel appeals to Kant's distinction between intuitions and concepts. What's actual is what can be given to us in intuition. The actual numbers "find their representation in the theory of real (*wirklichen*) magnitudes and their combination" (Hankel, 1867, 7). By contrast, what's formal is conceptual, rather than intuitive. "Purely formal mathematics" is a "completely new science" whose numbers "are not capable of any construction in intuition" (Hankel, 1867, 12, 7). Following Kant, Hankel assumes that concepts not presented in intuition are "empty of content (*inhaltsleer*)" (Hankel, 1867, 47); formal mathematics is in this sense "empty".

This should not lead us to conclude, though, that Hankel thought formal mathematics was "empty" in the sense that it was meaningless or useless. Instead, he regarded formal mathematics as a powerful new tool precisely *because* it was freed of the Kantian obligation to construct concepts in intuition. To see this, we need to turn to Hankel's second major influence: Hermann Grassmann.

Grassmann had developed a new, highly abstract branch of algebra, which he first presented in his *Ausdehnungslehre* of 1844. In order to set out this algebra, Grassmann first had to make conceptual space for it within contemporary mathematics. Grassmann thus based his presentation on a strong distinction between pure and applied mathematics. Grassmann held that *pure* mathematics consists of a "theory of forms (*Formenlehre*)", where a form, or "thought-form (*Denkform*)", is something which comes to be through thought alone, without reference to anything independently "real" (Grassmann 1844/1878 XXII). Accordingly, Grassmann carefully separated each chapter of his work into two parts: a pure theoretical part, and a part dealing with applications of the theory. For Grassmann, pure mathematics is formal primarily in the sense that it can be developed prior to, and independent of, all possible applications.

Hankel, who adopts both Grassmann's terminology and his style of presentation, sees formal mathematics similarly. In formal mathematics, we can define systems of formal objects and formal operations. Such a system, "because it is set up without consideration of any subordinate actual relations, remains an empty system, without any interpretation of its results and without application" (Hankel, 1867, 11). Because it is prior to its applications, formal mathematics proves results of a general and abstract nature.

I will present Hankel's proof of one such result: given an axiomatic characterization of a domain with two complementary binary operations, one total and the other not, the domain can be completed to make the second operation total. For example, given addition and subtraction on the natural numbers, one can make subtraction total by introducing the negative numbers. But Hankel's result is more general than, and not limited to, this application.

The proof proceeds in three phases. In the first phase, confining his attention to the original domain, Hankel derives a certain equation purely algebraically from some axioms. The axioms contain general laws about the two operations (e.g., that they are functional, and injective at both argument places) which can be expressed in pure second-order logic. The derived equation thus holds in any domain where these general laws hold. In the second phase, Hankel makes an intriguing move: he adjoins new elements to the domain, one for each case where the non-total operation is not defined on the original domain, viewing the derived equation now as a definition which relates these new elements to the old. In the third phase, Hankel proves that by doing so, we have "completed" the domain: both operations are now total, and no further expansions are necessary.

This very general result belongs in what we would today call the model theory of algebraic fields. As far as I have been able to determine, it is original to Hankel. The proof illustrates why Hankel thought formal mathematics was a powerful new foundation for analysis: by abstracting from any assumptions about the particular nature of the objects concerned (and in particular, from the assumption that such objects must be presented in intuition), we obtain a conclusion general enough to be applied in many different ways across number theory, complex analysis, geometry, and physics.

But Hankel's Kantian assumptions leave him in a verbal predicament: although he can *do* formal mathematics, he has no clear way of expressing what its general results *say*, or why they represent important scientific truths. In his proclamations about "signs" and "empty" systems of objects, Hankel is best read as groping around in the dark for a way to express the highly abstract character of these results—not as denying the existence of mathematical objects, or offering a deflationist view of mathematical truth.

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3.8 Sebastian G. W. Speitel - Arithmetic between Realism and Anti-Realism

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The goal of a theory of arithmetic is to provide a comprehensive picture of the natural number structure. Gödel's incompleteness theorems establish that no such theory can simultaneously be fully descriptively adequate (i.e., complete) and computationally feasible (i.e., recursive). Any choice of a suitable formalization of arithmetical reasoning will thus invariably have to violate one of these desiderata. The way in which this choice is made in the adoption of a theory of arithmetic is informed by the underlying conception of mathematics. Taking a realist stance motivates the recovery of as much descriptive content as possible, deprioritizing epistemic concerns, whereas an anti-realist stance will, in the extreme case, opt for simply explaining away (alleged) descriptive inadequacies.

Despite these radically different stances and takes on what matters in the formalization of arithmetic realists and anti-realists alike share an interest in the determinacy of arithmetical language. For realists this is essential to managing the underdetermination resulting from the incongruous relationship between (linguistic) representation and the (mathematical) objects thus represented, and necessary to ensure referential determinacy without recourse to mysterious epistemological capacities. Anti-realists, on the other hand, seek, in the very least, to secure unproblematic applicability of arithmetical concepts in the sciences. Even stronger, they may stipulate that determinacy is a pre-condition for having successfully specified a concept and introduced it coherently into discourse, rendering determinacy vital for successful definition.

Yet, preferred implementation of this "common goal" of achieving determinacy differs significantly between realists and anti-realists: whereas the categoricity of theories of arithmetic, their ability to uniquely "pin down" the intended natural number structure, has been a central objective of traditional realist accounts, anti-realists have frequently forgone the model-theoretic characterization of determinacy altogether and instead opted for alternative frameworks and alternative ways to capture this desideratum.

To complicate the general situation, several recent developments of realist and anti-realist positions in the philosophy of mathematics seem to eschew this established trend, leading to disagreements within realist and anti-realist positions themselves. Here we find realist positions abandoning the goal of categoricity - together with the attendant model-theoretic apparatus, - and limiting themselves to guaranteeing intersubjective agreement on the basis of which they claim successful articulation of a realist position (Button & Walsh, 2018; Button, 2022). On the other hand, several conventionalist accounts of the determinacy of mathematical language maintain the importance of a categorical characterization of the natural number structure (Warren, 2020; Murzi & Topey, 2021).

This renewed interest in the notion of mathematical determinacy, together with the shifting of determinacy standards across the realist-anti-realist spectrum, provides an opportunity to reconsider traditional renderings of realist and anti-realist approaches, methodologies and desiderata in (the philosophy of) mathematics. In this talk, I want to compare three extant and popular formalizations of a theory of arithmetic with respect to how they fare with respect to realist and anti-realist standards in achieving arithmetical determinacy, and a particular emphasis on the ways in which they implement core commitments of the respective stances. The three formalizations I want to focus on are:

1. first-order Peano-arithmetic with an ω-rule;
2. (full) second-order Peano-arithmetic; and
3. first-order Peano-arithmetic with generalized quantifiers.

The different ways in which these formalizations implement and capture the determinacy of arithmetic sheds light on the interaction of the philosophical basis for adopting a given formalization, the philosophical interpretation of the underlying formalisms' capacities, and the acceptability of its mathematical presuppositions in the articulation of an overall realist or anti-realist position. This reveals, on the one hand, an interesting shift in the philosophical interpretation of properties deemed important in the context of a theory of arithmetic and, on the other, a growing variety of realist and anti-realist takes on arithmetical determinacy with partially overlapping methods despite fundamentally different commitments.

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