APMP2010 Abstracts

in alphabetical order

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Andrew Arana (Kansas State University)
On the relationship between plane and solid geometry

Traditional geometry concerns itself with planimetric and stereometric considerations, which are at the root of the division between plane and solid geometry. To raise the issue of the relation between these two areas brings with it a host of different problems that pertain to mathematical practice, epistemology, semantics, ontology, methodology, and logic. In addition, issues of psychology and pedagogy are also important here.

In this talk (based on joint work with Paolo Mancosu) the major concern is with methodological issues of purity. I will begin with a brief discussion of some key episodes in mathematical practice that relate to the interaction between plane and solid geometry. In particular I discuss the late nineteenth-century debate on "fusionism". I then turn to a more detailed case study concerning Desargues' theorem on homological triangles, and its implications for the relationship between plane and solid geometry. This case study provides a basis for the analytic work necessary for exploring various important claims on "purity", "content" and other relevant notions.

Tom Archibald (Simon Fraser University, Canada)
Poincaré, Stability Theory, and the Prestige of Mathematics circa 1900

In the late nineteenth century, mathematics enjoyed considerable prestige, probably more than at present. In part this rested on its foundational character, its educational role, and the certainty associated with mathematical knowledge. The apparent power of mathematical analysis in the study of nature likewise contributed greatly to this prestige, though the relative roles of these different sources of disciplinary power varied locally. In this paper, we consider the importance in this connection of Henri Poincaré's efforts, dating from the mid-1880s, to provide a mathematical account of stability of planetary figures and orbits. The research program that Poincaré's researches spawned was both compelling and chimerical. One the one hand, we discuss the relevance of the work to broad contemporary debates concerning determinism and free will (and hence to the basis of the Catholic religion, confronting discussions by Boussinesq and others). On the other we look at the reception of the notion that one could determine the numbers of satellites of the planets, the configuration of the rings of Saturn, and the very structure of the solar system using Poincaré's qualitative techniques. Issues concerning values and aesthetics in mathematics circa 1900 will also be approached.

Jean Paul van Bendegem (Vrije Universiteit Brussel)
Ideal and real practices: philosophy of mathematics within mathematical practice

It is well-known that philosophical-foundational views of mathematics can be interpreted in terms of an ideal being that is capable of performing the mathematics required. The most famous example is surely Brouwer's creative subject. The question at the heart of this presentation is: how do such ideal beings relate to actual real-life mathematicians? Should there be a connection in the first place or are they altogether something entirely different? I will argue that such relations are indeed interesting and that they open the perspective of another kind of integration of theories of mathematical practice with philosophy of mathematics.

Maarten Bullynck (Université de Paris 8)
Many Sides to a Polygon. A historical line-up of mathematical routines and their analyses

The concept of "mathematical practice" has been used in the field of history and philosophy of mathematics of the last twenty years in a manner similar to the use and function of the concept of "social action" (soziales Handeln) in the elaboration of sociology in the beginning of the 20th century. "Social action" was introduced by Max Weber as a concept to study the processes of social behaviour and the constitution of their meanings. Also, most importantly, the concept helped to emphasize the fact that social action has not necessarily the same meaning for the actor as for the observer/scientist, a distinction fundamental to modern sociology. Both aspects, the focus on process rather than product and the distinction between the meaning the act has for its doers and the meaning attributed to it by later readings and appropriations, are also central to the concept of "mathematical practice".

Taking up this parallel, I want to present some fragments of historical case studies that, for their methodology, take their inspiration in Alfred Schütz's sociology. Schütz's approach can be called a phenomenologically informed take on the Weberian problem of social action and specializes in the study of structures of everyday routines. In the fragments of case studies I will present, the focus will be on the polygon as the projected issue of an everyday mathematical routine of construction and calculation as performed at various points in history, and also as an object of mathematics, outcome of the routine, that has been used to illustrate epistemological claims. In historical order, I will talk about Descartes's chiliagon, Christian Wolff's polygon, Gauss's 17-gon, and the Minsky pseudo-circle. While the process of the polygon, its construction or calculation, informs the analysis made of the outcome of the routine, it is nevertheless fundamentally distinct and different from the analysis. This comes to the fore most explicitly so when looking at the spatial and temporal structure and sequence of the mathematical routine in question, which is irrevocably reduced and cut short after the fact.

Thomas Burke (University of South Carolina)
Pragmatism and Dynamic Logic: A Sorted Affair

This project aims to show, contrary to Tarski, how a pragmatist like Charles S. Peirce might interpret a first-order language. This introduces a pragmatist perspective on the foundations of mathematics, moving set theory from its privileged position in favor of a theory of ability-based logics. As an investigation of the mathematics of practice, it bears on the practice of mathematics itself—grounding it not in the notion of sets but in the notion of systems of abilities to act in the world. I would like to outline the project and discuss some of its consequences.

Since analytic pragmatism and pragmatist perspectives on the foundations of mathematics are unfamiliar, this work emphasizes examples that effectively illustrate the fundamental nature of this perspective and at the same time demonstrate its formal power. The project utilizes a running example of an ability-based interpretation of the simple first-order blocks language used in Barwise and Etchemendy 1999. The basic idea is to introduce a level of analysis prior to constructing an extensional first-order structure for a first-order language (interpreting predicate symbols in particular with sets of things). This intermediate intensional semantics interprets first-order predicate symbols rather with classes of models for a particular ability-based dynamic language, from which extensional structures may then be devised.

Addressed to mathematical foundations, this project entails that many issues in epistemology and cognitive science (not to mention metaphysics and ethics) should be revisited. The project has no particular agenda in this regard other than to stick closely to classical pragmatist methods and ideas (Peirce, James, Dewey) to see how far one can run with them. The main aim is to show that analytic pragmatism, properly conceived, does indeed work in a rigorous way and in fact points squarely at where and how, in practical terms, mathematics connects with real-world applications.

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Jessica Carter (University of Southern Denmark)
Diagrams as Representations in Proofs from Analysis

The use and role of diagrams in mathematical reasoning have since long been a topic for discussion. On one side of this debate is the view that diagrams should play no role in proofs. Tennant, in a famous quote, claiming to describe a widely accepted view, claims that diagrams have no proper place in a proof as such. On the other side of this debate is Azzouni who wishes "to give indications for why – and in which sense – such a view is false" (Azzouni 2005, 20). To do this, he writes, it is not enough just to point to the fact that there are diagrams in mathematics books. Rather, to establish that diagrams play a role in mathematical arguments requires that these roles be explicitly formulated. That is precisely the intention of this presentation.

This will be done in the light of a case study from contemporary mathematical practice, more precisely from analysis. In the presented example diagrams were used in order to obtain certain combinatorial expressions. In the talk I will argue that, although these diagrams are removed in the final versions of the proofs, they still play an important role in these proofs. I will point to definitions in the articles that arise from certain properties of diagrams, as well as proofs that arise from proofs using diagrams. Thus diagrams in this case play a role in concept formation, and in some cases they can be used to represent (parts of) proofs. In addition, I note that the term 'visualization' is used in different ways. One way to use diagrams or pictures is as representations. This is the case with the examples given above. The second sense of 'visualization' refers to our mental pictures, helping us to see that something is the case. In this case diagrams could play the role of providing 'fruitful frameworks' triggering our imagination. One way to describe this difference is that in the second sense visualization refers to an inner, mental model, whereas in the first sense visualization denotes our "vision" (seeing) of certain external representations that consist of, for example, drawings on paper. Note that I do not claim there is a sharp distinction to be drawn here. A picture or diagram may trigger our imagination, producing a mental picture, and a mental picture may be reproducible as a concrete drawing.

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José Ferreirós (University of Sevilla)
The web of practices. On the notion of practice in history and philosophy of mathematics

The talk will try to advance towards a general characterisation of mathematical practice. To this end, we shall begin reviewing some related developments in recent history of math, and philosophy of math. On this basis, a few reasonable conditions for a characterisation of mathematical practice will be discussed, and a tentative description proposed. The talk will end with more particular considerations about recent work on practice, and desirable extensions.

David Corfield (University of Kent)
Coalgebra: the hidden face of mathematics

In this paper we give an account of the rise and development of coalgebraic thinking in mathematics and computer science as an illustration of the way mathematical frameworks may be transformed. Originating in a foundational dispute as to the correct way to characterise sets, logicians and computer scientists came to see maximizing and minimizing extremal axiomatisations as a dual pair, each necessary to represent entities of interest. In particular, many important infinitely large entities can be characterised in terms of such axiomatisations. We consider reasons for the delay in arriving at the coalgebraic framework, despite many unrecognised manifestations occurring years earlier, and discuss an apparent asymmetry in the relationship between algebra and coalgebra.

Jairo José da Silva (?)
The Applicability of Mathematics

The physicist Eugene Wigner, in a much-talked-about paper (Wigner 1960), raised the following question: can we account for the (according to him) "unreasonable" applicability of mathematics in physics? His answer was that we cannot, that this is a "miracle we do not understand, or deserve". Some time later, Mark Steiner (1989, 1998) pushed the issue a bit further. For him, on the contrary, we can explain "the unreasonable effectiveness of mathematics in the natural science", sometimes along well-known Fregean lines, but sometimes, when only formal analogies are involved, from an "anti-naturalist" perspective, by denying that nature is indifferent to man. For Steiner, nature is "user friendly"; it is, he thinks, somehow tuned to our aesthetic sensibility.

In the above-indicated works Steiner provides some examples of what he takes for fruitful heuristic uses of purely formal mathematical analogies in physics, among them Maxwell's discovery of "displacement" currents, and, consequently, electromagnetic waves. In my talk I'll argue that Steiner's account of this important scientific accomplishment does not do justice to the facts; that Maxwell's procedure was grounded on physical presuppositions (concerning, for example, dielectric properties of the ether), besides sound scientific methodology, to a much larger extent than Steiner is willing to concede.

However, it is a fact formal mathematical analogies are useful in science (and mathematics too, for that matter). But, I claim, there is a perfectly ordinary explanation for this, the main lines of which I plan to draw in my talk.

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Lorenz Demey (University of Leuven), Benedikt Löwe (University of Amsterdam) & Bernhard Schröder (University of Duisburg-Essen)

The Use of Corpus Linguistics in the Philosophy of Mathematical Practice

The philosophy of mathematical practice (PMP) claims that valuable philosophical insights can be gathered from the way actual mathematicians do mathematics. It treats mathematical practice as a specific kind of human practice. It is therefore not surprising that PMP has fruitfully interacted with many disciplines that study various aspects of (any kind of) human practice, such as sociology and psychology.

One discipline that PMP has not much interacted with so far, is linguistics. The potential fruitfulness of this interaction is based on the trivial observation that mathematical practice –again, like any other human practice– is thoroughly linguistic in nature. Mathematicians, when doing research, teaching, exposition, etc., communicate with each other via language. This language has many features which do not immediately fit into the traditional picture of 'mathematic(al reasoning/language) as logic(al reasoning/language)', but which are nevertheless philosophically interesting.

We therefore propose linking PMP with linguistics, and, in particular, corpus linguistics. This discipline studies language use via quantitative analyses of large amounts (corpora) of actually produced (written/spoken) language. In the talk, we will discuss the potential of using corpus linguistics to analyze specifically mathematical language. Most importantly, however, we aim to illustrate the usefulness of corpus-linguistics methods for PMP by presenting two case studies:

1. Mathematicians often use phrases such as 'it is easy to see that ϕ', 'it is obvious/trivial that ϕ', 'it now follows straightforwardly that ϕ' –where ϕ itself is often still a quite complicated mathematical proposition. These expressions do not contribute to the logical organization of the (proof) text, and therefore were classically deemed irrelevant. Using corpus-linguistics methods, however, we show that there is a certain systematicity in the use of these expressions, which can be explained on pragmatic/cognitive grounds (thus viewing mathematical texts as being written with a certain purpose in mind, rather than as strictly logical derivations).

2. There are many competing ways to introduce 'arbitrary' new objects into 1 mathematical discourse like 'Any/every group/all groups G...', '...for groups G', 'Let G be a group...', 'Take a group G...', etc. Using corpus-linguistic methods, we are investigating the historical evolution and distribution of these phrases. They all have identical truth conditions, but differ in many pragmatic respects, like the prominence of the newly introduced entities in the discourse and the scope of the entities with respect to others. Such means of 'foregrounding' and 'backgrounding' call the reader's attention to

a small number of 'main' entities, and renders proof texts more predictable and cognitively more accessible. The mechanisms employed here do not differ in principle from non-mathematical types.

Liesbeth de Mol (University of Ghent)
What is the impact of the computer on mathematics? A quantitative approach

In his paper *Mechanized mathematics* published in 1966 Derrick H. Lehmer, one of the first mathematicians to use the computer within his research, identifies two schools of thought within mathematics characterized by two kinds of different activities in mathematical research. The first, he describes as being concerned with "the improvement of highways between the well-established parts of mathematics and the outposts of the realm [favoring] the extension of existing methods of proof to more general situations". The second school was described as "the establishment of new outposts [...] This school favors explorations as a means of discovery". Lehmer situates the significance of the computer for mathematics within this second school as an "instrument of our observatory, our window to the hard facts of the world of mathematics." At the end of this paper, Lehmer speculates on the possible future impact of the computer on mathematics. On the one hand, he expresses the hope that because of the more widespread use of the computer, the second school of thought would become the more dominant one in mathematics and change the nature of mathematics. On the other hand, he considers the possibility that "[s]oon disciplinary fences will be erected" between, on the one hand, computer scientists (identified with the second school of thought) and "pure" mathematicians who are frightened by the new technology and will be driven away from the concrete and practical just as artists felt relieved from the obligation to depict nature because of the invention of photography and were driven into abstraction.

Nowadays, the impact of the computer on mathematics is still very much debated. The advocates of computer-assisted mathematics make strong claims about the possible future of mathematics that echo some of the words of Lehmer. I.e., it is claimed that because of the computer, mathematics as we know it today will be replaced by a mathematics that is quasi-heuristic and fallible in nature and characterized by methods of exploration, experimentation and extensive computation. These claims have also been opposed by some who do not see any future for a computer-assisted mathematics exactly because it is considered to be too "heuristic" and fallible in nature. This debate also has its influence on the philosophy of mathematics: several papers have been devoted to the question of the epistemological and philosophical significance of the computer for mathematics.

The aim of this talk is to contribute to this debate, not by evaluating the arguments of individual mathematicians and philosophers, but by trying to evaluate in how far Lehmer's predictions have actually come true. This talk wants to tackle the question: what was and is the actual impact of the computer on the everyday mathematical "practice"? In other words, the aim of this talk is to evaluate the computer's significance for the average mathematician. This will be done through a quantitative study of this impact. The main method is to investigate the evolution within mathematical databases such as MathSciNet and ZMath database of the use of computer-related terminology within mathematical writings. The aim is not only to get a general view on the evolution of the quantitative impact of the computer on mathematics but also to investigate this evolution within specific disciplines, to detect the speed at which these evolutions occur and to check which kind of terminology is the more dominant. Such quantitative methods make it possible not only to provide a factual basis for discussions on the question whether or not and how computer-assisted mathematics is actually changing the way we are doing mathematics but also to gain an improved understanding of the evolution of the mathematical "practice" known as computer-assisted mathematics.

Johannes Hafner (North Carolina State University)
Can Proofs by Mathematical Induction be Explanatory?

A couple of attempts have been made in the literature to argue that proofs by mathematical induction are not explanatory. Mark Steiner's argument (1978) fails as was shown in Hafner & Mancosu (2005). More recently, Marc Lange (2009) put forward an argument to the effect that proofs by mathematical induction involve an explanatory circularity and thus cannot furnish explanations. It is not very hard to see that Lange's argument fails, too, provided close enough attention is paid to the actual workings of proofs by mathematical induction. (Surprisingly enough, however, the claim that mathematical induction is circular in an explanatory sense seems to gain some acceptance.)

Hence the original question still remains open, the possible explanatoriness of proofs by mathematical induction has not been ruled out yet. On the other hand, one upshot of the analysis of (the failure of) Lange's argument is that there can't be a *general, sweeping* argument for the explanatoriness of proofs by mathematical induction. Looking just at their overall – inductive – structure, does not provide enough evidence for such a claim. Rather, one has to take into account how this structure is actually "filled" in individual proofs. Only case-by-case studies may reveal that certain proofs by mathematical induction are, indeed, explanatory.

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Ansten Klev (University of Leiden)
Dedekind and the primacy of epistemology

Much of the philosophical interest around Richard Dedekind has focussed on his purported structuralism, and has thereby let ontological issues take center stage. Thus, for instance, Reck (2003) has claimed that "[Dedekind's] main mathematical and logical insights are, in his own view, *intimately linked* with metaphysical views." I wish to go against this tendency and claim that epistemological matters were much more important to Dedekind's work. This can be seen by a close reading of *Stetigkeit und irrationale Zahlen, Was sind und was sollen die Zahlen?*, and the writings on ideal theory. The motivation was roughly the same for each of these works: to provide a foundation for complete demonstrations in the relevant science (analysis, arithmetic, ideal theory); moreover, this foundation would be found in a definition.

First I will go through the passages that I take to be indicative of Dedekind's epistemic motivation. In *Stetigkeit*, the key is the story recounted in its Preface: Dedekind was unsatisfied with the demonstrations he could present to the students of his calculus classes. He reports that he set himself the task to "find a purely arithmetical and perfectly rigorous grounding for the principles of the infinitesimal analsysis"; the outcome was the definition of real numbers in terms of cuts. Thus the motivation for the work was to provide a proper foundation for analysis, and this is motivation of an epistemic kind. The opening sentence of *Was sind* speaks for itself: "Was beweisbar ist, soll in der Wissenschaft nicht ohne Beweis geglaubt werden." The message is clearly that the work at hand will provide the necessary foundations for demonstrations of what is demonstrable in arithmetic. Again this shows motivation of an epistemic sort. An objection to the claim that *Was sind* had an epistemic motivation is that its title asks for “the nature and meaning of numbers.” This objection can be met by considering what Dedekind in fact regards as the answer to the questions in the title: “the numbers are free creations of the human mind, they serve as a means to comprehend more clearly the difference of things.” I claim that this answer cannot be appropriately viewed as an answer to the ontological question of “what numbers are.” In his *Sur la Théorie des Nombres entiers algébriques* Dedekind motivates his ideal theory; while he praises Kummer’s introduction of ideal numbers, Dedekind fears that Kummer’s definition of them “may lead to hasty conclusions and incomplete demonstrations.” The suggestion must be that his own definition will provide a foundation for complete actual demonstrations. Thus in all these three works we have found an epistemic motivation. Moreover, expression of any particular metaphysical views is hard to come by in Dedekind’s works, making it clear that epistemology here has priority over ontology.

Secondly I will consider one aspect of Dedekind’s epistemology: his “*Wissenschaftstheorie.*” Traditionally axioms are taken to be the basis of a science. I argue that Dedekind instead saw concepts and their definitions as lying at the basis of mathe- matical theories. Again this can be seen by close reading of Dedekind’s works; e.g. after giving the definition of an ideal in the 1894 *XIth Supplement*, Dedekind speaks of deriving the laws of the theory of ideals from the definition of ideal. Hilbert (as can be seen from the *Zahlbericht, Grundlagen der Geometrie*, and *Axiomatisches Denken*) followed tradition in taking axioms to be the basis of theories. Thus I claim that Hilbert and Dedekind are in disagreement at this point; I am thus in opposition to Sieg and Schlimm (2005), as well as to Ferreirós (2009), who argue that Hilbert around 1900 shared Dedekind’s views on mathematical theories.

Peter Koepke (University of Bonn)
Ordinary Mathematical Texts and First-Order Logic

Mathematical logic abstracts and idealises the statements and proofs in common mathematical practice to first-order predicate formulas and first-order formal derivations. This view has been immensely successful in many respects. It allows to investigate the methodology of mathematics by the mathematical method itself. Also it provides technical criteria for the correctness of mathematical arguments. In my talk I discuss the process of construing mathematical texts as first-order texts in some detail, with particular emphasis on the faithfulness of the translation. This involves linguistic analyses of the natural language used in mathematics. I hold that first-order logic can be taken as a formal semantics of the language of mathematics. I also refer to the Naproche system (Natural Language Proof Checking) at the University of Bonn, which automatically translates controlled (natural) mathematical texts into first-order texts and then proof-checks them (see www.naproche.net).

Abel Lassalle Casanave (Federal University of Santa Maria)
Symbolic knowledge in Hilbert’s Program

Intuitive knowledge, Leibniz tells us, is knowledge obtained by considering directly ideas that have been completely analyzed into their simplest components. *Symbolic* knowledge, he proceeds, is the one obtained by means of *characters*, i.e. *signs* that can present themselves in many different formats, written, carved, drawn, etc. Mathematical symbols, typically, but also musical or stenographic signs, the symbols of chemistry, words of natural language, geometrical figures and many other graphic signs. However, it is arithmetic and algebra that provide the paradigmatic instances of symbolic knowledge. Indeed, the manipulation of arithmetical and algebraic symbols according to rules –

i.e. the operations of a *calculus* – does not require intuitive knowledge, which is why symbolic knowledge is also often called *blind* knowledge.

The use of the concept of symbolic knowledge (*cognitio symbolica, cognitio caeca, symbolische Erkenntnis*) is widespread in the German post-Leibnizian thought. We can find it in the works of Wolff, Baumgarten, Daries, Lambert and Kant. We can also find it in other post-Kantian logicians and philosophers as Frege, Schroeder, Husserl and, as we will show, Hilbert.

Hilbertian formalism is a conception in which mathematics is conceived as a formal system without content *from the perspective* of proof theory or metamathematics. Mathematical theories, as they are developed in practice, are theories "with content (= contentual)", but certainly this content is not intuitive content, in any technical sense of the word. But this methodological formalism is supported by a conception of mathematical knowledge we find in the tradition of the Leibnizean symbolic knowledge, being intuitive knowledge in technical sense codified by finitary arithmetic. We can distinguish three kinds of symbolic knowledge, namely, that of being *succedaneum* of intuitive knowledge, *extending* intuitive knowledge and of being *formal* knowledge. The last kind is the most important kind of symbolic knowledge from a conceptual point of view.

We intend to point out that Hilbert's program is a chapter of the history of symbolic knowledge. We do not intend to make history of influences (of the Leibniz over Hilbert), but to make history of ideas: the idea of mathematical knowledge obtained through the manipulation of signs present in both of them. Thus, we show in a detailed manner how in Leibniz and Hilbert the surrogate, computational, *ecthetic* and psichotechnic functions of symbolism associated with symbolic knowledge are the grounds for mathematical knowledge.

Iris Loeb (Vrije Universiteit Amsterdam)
The Foundations of Constructive Reverse Mathematics

In the field of Constructive Reverse Mathematics (CRM; see e.g. [5, 2]) the equivalence between certain mathematical theorems and certain principles or axioms is established. This is done in a constructive way, i.e. over a constructive core, using intuitionistic logic. The term "reverse" is used because one does not only prove theorems from axioms, but also—the other way around—axioms from theorems.

Several fields lie at the origin of CRM. Firstly, of course, Brouwer's intuitionism [3], which stands at the beginning of most research in constructive mathematics. Secondly, Bishop's programme of constructive mathematics [1] from which CRM often takes the informal style and from which it often uses the theorems as its constructive core, void of Brouwer's "mysticism". Thirdly classical Reverse Mathematics [6] with which it has in common the idea of proving equivalences between axioms and theorems. These fields are reasonably well understood and hence it seems often to be thought that CRM is not in need for any additional methodological explanation of its own.

However, CRM differs substantially from all three of the above mentioned fields. For example, it differs from Brouwer's intuitionism in that it also studies equivalences between intuitionistically false statements; from Bishop's constructive mathematics in that CRM should rather be considered as part of metamathematics, than of mathematics; from classical Reverse Mathematics, besides its restriction to intuitionistic logic, in that it isn't always carried out in a formal system.

Hence the question arises whether either any of these three fields separately or the combination of them can serve as a foundation in the sense that it justifies the aims and methods of CRM. I will argue that this is not the case. A study into the methodology of Constructive Reverse Mathematics is hence called for.

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Benedikt Loewe (University of Amsterdam) & Bart Van Kerkhove (University of Brussels)
Peer review and knowledge by testimony in mathematics

The peer review process has been the topic of many studies in the medical and biological sciences, but not so in mathematics. Given that mathematicians sometimes refer to results from the literature without checking the proofs in detail, it is interesting to see how the mathematical refereeing process affects the epistemic certainty of this type of mathematical knowledge by testimony. We give a description of the mathematical refereeing process and some results of empirical studies.

Danielle Macbeth (Haverford College, PA)
Frege and the Aristotelian Model of Science

Although profoundly influential for essentially the whole of philosophy's twenty-five hundred year history, the model of a science that is outlined in Aristotle's *Posterior Analytics* has recently been abandoned on grounds that developments in mathematics and logic over the last century or so have rendered it obsolete. Nor has anything emerged to take its place. As things stand we have not even the outlines of an adequate understanding of the rationality of mathematics as a scientific practice. It seems reasonable, in light of this lacuna, to return again to Frege—who was at once one of the last great defenders of the model and a key figure in the very developments that have been taken to spell its demise—in hopes of finding a way forward. What we find when we do is that although Frege remains true to the spirit of the model, he also modifies it in very fundamental ways. So modified, I will suggest, the model continues to provide a viable and compelling image of scientific rationality by showing, in broad outline, how we achieve, and maintain, cognitive control in our mathematical investigations.

Oran Magal (McGill University, Montreal)
Real and ideal mathematics in Hilbert's Programme

(based on joint work with Michael Hallett)

The Problem Addressed by the Paper

At the beginning of Hilbert's 1899 monograph *Grundlagen der Geometrie*, there appears, as a brief epigram, a passage quoted from Kant's First Critique:

> So fängt denn alle menschliche Erkenntnis mit Anschauungen an, geht von da zu Begriffen, und endigt mit Ideen. (*Kritik der reinen Vernunft*, A702/B730.)[1]

Hilbert does not elucidate the passage; indeed, it's never referred to in the book, though clearly, since the book is a treatise on geometry, one can assume that it's meant to refer above all to a categorisation of geometrical knowledge. It is clear that Hilbert does not intend here a direct endorsement of Kant's position on geometry. For Kant, both the axioms of geometry and the mathematical reasoning that takes place by means of them are firmly grounded in the pure intuition of space. Hilbert, on the other hand, is famous for his view that one must strongly disassociate the axioms of geometry from any intuitive grasp of the meaning of the terms in question; that one must ground geometry on axioms understood as formal (logical, relational) structures; and that part of the foundational task is then to analyse the logical relations between these axioms and the central propositions, and not to give a detailed account of their correct epistemological classification (i.e., whether they are synthetic *a priori* or otherwise).

If, by using the epigram, Hilbert does not intend to endorse Kant's classification of mathematical knowledge, is then the unelucidated reference to Kant in 1899 of any genuine explanatory significance? This paper argues that it *is* of significance, and that what it signifies is a genuinely important position towards the objects of mathematics and mathematical propositions. We argue further that the position it signifies sits well with another, later and much better known reference to Kant and ideas at the end of the famous paper *Über das Unendliche* from 1926 ([5]). Hilbert writes there:

> The role which remains to the infinite is, rather, merely that of an idea—if, in accordance with Kant's words, we understand by an idea a concept of reason that transcends all experience and through which the concrete is completed so as to form a totality—an idea, moreover, in which we may have unhesitating confidence within the framework furnished by the theory which I have sketched and advocated here. (Quoted from the English translation in van Heijenoort, p. 392.)

'Über das Unendliche' was written in the middle of the development of what came to be known as 'Hilbert's Programme' (HP); the programme had as its goal nothing short of showing the consistency of the central mathematical theories, above all arithmetic and analysis, and this paper is often taken to be the central paper outlining that programme, its philosophical linchpin. At the centre of that paper is a distinction between 'real elements' and 'ideal elements'; what Hilbert calls the 'finitary' is the core of the real, and this gives rise to an elementary, primitive arithmetic which is to be used as the framework which provides the sought-after consistency proofs. Whatever has infinitary content is part of the ideal, and in particular is beyond any direct, intuitive justification, unlike the finitary. Thus, understanding how the 'ideal' is to be interpreted must be fundamental for understanding Hilbert's approach to the foundations of mathematics.

The Solution

The notion of the 'ideal' is not new to 'Über das Unendliche'; not only does Hilbert refer there (with examples) to a mathematical tradition of ideal elements, it was a notion which Hilbert had developed in his unpublished work on geometry (see [2]). Hilbert's appeal to ideal elements in his geometrical work is roughly this. Geometry has a partly intuitive and partly empirical beginning, giving rise to what Hilbert calls a body of (defeasible) 'facts [*Tatsachen*]'. These facts are subsumed under concepts and propositions, from which axioms are distilled. Two things then standardly happen. First, the concepts themselves are rendered more abstract and less tied to their empirical origin; an example of this is the concept of congruence, originally firmly tied to the notion of rigid body movement in 3-dimensional space, but then freed from this and made dependent

[1] We note that a very similar remark is also to be found at A298/B355.

just on the *existence* of points and angles and not on movement and preservation of length. Secondly, one can then alter the axioms by first postulating the 'existence' of points, lines etc. which the axioms as originally construed do not contemplate. This, says Hilbert, is exactly what is done with the introduction of 'points at infinity' in projective geometry. The introduction of ideal points not only simplifies the statement of the axioms but also makes possible the fruitful discovery of new theorems (and provides new ways of proving old theorems); more particularly he stresses that the 'points at infinity' are not located at some vague non-place extremely far away (as their designation might suggests), but are rather just as determinate as the finite points in the Euclidean plane. With respect to these, they can be considered 'ideal'; but once the new theory is framed, there are just determinate points, none of which has any special existential status, and the distinction fades away.

Hilbert saw this as part of a familiar pattern in the development of modern mathematics: the positive numbers are extended by the addition of the negative numbers, then the rationals, etc.; the real numbers are extended to include $\sqrt{-1}$, making possible the complex numbers and the proof of the Fundamental Theorem of Algebra and the results which flow from this; and so on. In each case, the uncontroversial and established is called 'real' mathematics, and the extensions of 'real' mathematics (by points at infinity, by negative integers, by imaginary numbers) are then considered, relative to this 'real' basis, as 'ideal', although, as remarked, the distinction is subsequently dropped; moreover, what had formerly been 'ideal' in an extension can be considered as 'real' when a further extension is contemplated. Thus, we shift from a conceptual structure directly grounded in 'experience' to a conceptual structure which is a product of thought and which is only partially grounded in experience.

Hilbert's distinction between real and ideal in his later foundational work from the 1920s is analogous to these examples in the following way: Hilbert set out to deal with the doubts of sceptics like Brouwer and Weyl regarding large tracts of classical mathematics, including much of analysis and set theory, as devoid of meaning. In giving a reply to these doubts, Hilbert begins by choosing an absolutely minimal, uncontroversial and universally accepted basis, what he calls finitary arithmetic, based on a slightly more general conception of intuitively apprehensible symbols. This starting point he calls the 'real'. However, very little mathematics can be done strictly on this strictly finitary basis; it has to be extended by allowing for transfinite domains of objects and quantification over such domains. This part Hilbert calls 'ideal' mathematics.'Real' mathematics we consider unproblematically meaningful when grounded in the intuitive apprehension of the finitary; 'ideal' mathematics cannot be meaningful in this way (which falls a long way short of saying that it s not meaningful at all), and to show the permissibility of this extension, we must prove it to be safe, that is, prove for such an extension that it does not introduce inconsistency into the resulting extended system. To do this, Hilbert proposes his finitary consistency programme (proving the consistency of theories by *finitary*, i.e., 'real', means), which will not concern us directly in this paper.

In Hilbert's use of ideal elements, there are two very important connections to Kant's 'Ideas of Pure Reason'. The first is that the new objects, or rather the conceptual and/or propositional structures that arise from their use, do not have any direct justification in experience in the same way that the 'real' objects arguably do. This fits very well with the appeal to 'ideal elements' in both Hilbert's geometrical work and also in the later 'Über das Unendliche'; part of the argument of that paper is that there is no direct evidence of the existence of actual infinities in the world of experience (according to modern physics in Hilbert's account, there could not be), and this is the very reason why Hilbert turns to ideal elements for an account of the infinitary.[2]

The second important connection to Kant is this: for Hilbert, all the objects and/or the structures under consideration are ultimately 'things of thought', and we take it that here there is an analogy (if not a parallel) to Kant's appeal to 'pure reason'. One could

[2]Of course, Hilbert does not think of science as static and unchanging; and it might well be that in the future we will acquire empirical evidence for certain 'ideal elements'. Hilbert considers this at length in his 1924/25 lectures 'Über das Unendliche', from which the paper of the same name is largely drawn.

consider 'real' mathematics as a product of something analogous to Kant's concepts of the understanding, which are grounded in possible experience and have objective validity. To continue the analogy, 'ideal' mathematics would then be the analogue of Kant's ideas of reason: they are not grounded in possible experience and in intuition, but they do have a role to play in bringing unity and guiding, as regulative ideas, the advancement of 'real' mathematics. However, again analogously to Kant's ideas of reason, they are not to be mistaken for merely useful fictions.

The partial analogy with Kant is directly connected with the latter's notion of regulative idea. Unlike the concepts of the understanding, a regulative idea is not grounded in possible experience, but rather in reason itself. However, that does not make it mere fiction or error; it has its proper, regulative role, which is one of systematising and unifying the products of the Understanding. For Hilbert, unlike Kant, there can be many different ideal extensions, even from the same 'real' basis. So, while ideal extensions might be 'canonical' in the sense that it is 'obvious' how a given extension has to proceed, they are not necessarily 'canonical' in the sense that there is only one way to extend a theory. Thus, by taking an ideal extension, we do not necessarily end up with *one* theory of *the* real world; we have many theories, any of which might be used (at least potentially) as part of an account of some aspects of the physical world.

Another important point is that for Hilbert, the notion of the ideal is quite explicitly a *relative* notion, something which again only comes out clearly in his unpublished work; the line between 'real' and 'ideal' is not given absolutely, once and for all, but is rather relative to our choice of basis with respect to which the ideal is an extension. Thus, when 'ideal elements' are first introduced they are clearly marked as distinct from the 'real'. But once a full unifying theory is presented, and that theory is shown to be consistent, then the new elements are just as 'real' as the older elements. Indeed, Hilbert says (1899) that it would be 'idle' to ask whether they genuinely exist or not. This is similar to Kant's calling it a mistake of dogmatic metaphysics to insist on a 'yes' or 'no' answer to the questions which give rise to the Antinomies, and that we should, instead, consider the notions of infinity which give rise to the antinomies as a regulative idea rather than as concepts of the Understanding.

The relativity of Hilbert's notion is, we think, of the greatest importance for a correct understanding of Hilbert's foundational work. Various commentators have given the division drawn in 'Über das Unendliche' an instrumentalist reading, namely, that for Hilbert only 'real' mathematics has genuine mathematical content, with the rest serving merely as an expedient device, lacking any significance other than as a means for proving results about the 'real' (the finitary). However, there is a great deal of textual evidence (much of it in unpublished writings, stemming from lecture courses given in 1919, 1922/23 and 1924/25) for a non-instrumentalist reading. Here the relativity of the real/ideal distinction for Hilbert is key: ideal terms fall under the label 'ideal' insofar as they are considered extensions of a 'real' basis, but what is to be regarded as the 'real' basis may well shift. The same clearly applies to the real/ideal distinction used in the philosophical underpinning of HP. One central problem with the instrumentalist view of HP is that the 'real' here corresponds to a highly truncated part of elementary arithmetic, and is so primitive that it is hard to regard it as mathematics at all. Thus, to take this as the only genuine content of mathematics would be absurd, like taking the real purpose of our physical theories to be to give us correct predictions about the positions of needles on meters, as Hermann Weyl remarked ([7], p.88). There is a specific reason why Hilbert adopts the initial view of the finitary as 'real' in HP, namely the pursuit of the syntactic consistency programme. But to read into this a final classification of the nature of mathematics is a profound mistake, just as it would be a mistake to see points at infinity or $\sqrt{-1}$ as unreal. Once the consistency programme had been successfully concluded, as Hilbert thought it would be, the theories justified by consistency proofs would be just as real as finitary arithmetic is taken to be. Moreover, the instrumentalist interpretation is in enormous tension with Hilbert's ecumenical approach to mathematics, according to which one should proliferate methods, results and theories, not reject them; any fruitful path of mathematical inquiry ought to be pursued, rather than denounced as somehow 'merely instrumental' and its objects as unreal. This approach is marvellously illustrated in Hilbert's famous lecture on mathematical problems from 1900 ([4]), which stresses above all the uniformity of the mathematical sciences. It should be stressed that the later HP was intent on securing the full range of mathematics as it is presented in the 1900 paper, and not with presenting, defending and justifying a truncation.

Conclusion

The suggestion, then, is briefly this: One should take seriously Hilbert's allusions to Kantian 'Ideas of Reason' in the relevant papers, as well as specifically his comparison of what he calls 'ideal elements' with Kant's 'regulative ideas'. This not only enables us to understand the importance of the epigram from 1899, but it also enables us to make sense, consistent with the evolution of Hilbert's foundational thought between the 1890s and the 1920s, of the appeal to the ideal in the adumbration of HP. Lastly, the relativity of Hilbert's conception makes it clear that the reading of Hilbert as an instrumentalist and anti-realist towards mathematics does not hold up. This Kantian-inspired interpretation of Hilbert the philosopher of mathematics accords far better with Hilbert the mathematician.

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Ken Manders (University of Pittsburgh)
Agentive aspects in mathematical thought

The logical and ontological turn in philosophy of mathematics tends to eliminate the agentive from the philosophical image of mathematical thought. We survey some ways in which agentive roles seem necessary to an adequate philosophical treatment.

Tyler Marghetis & Ben Sheredos (University of California),

Warranting Inference: Lessons from Cauchy and Cognitive Science

Cognition may play a role in discovery and understanding, but what of warranting inference and justifying proof? On the basis of empirical work in cognitive science and a cognitive-historical case-study of 19th century Analysis, we argue that embodied conceptual systems play a functional role in expert inferential practice — in particular, that these embodied conceptual systems constrain and elaborate the mathematician's *warranted* inferences.

We begin by considering one relation between conceptualization and proof. To account for the gap between formal proofs and the actual proofs found in practice, Azzouni (2004) proposed the *derivation-indicator* view of proof, according to which informal proofs point to formal derivations (or "inference packages") (2005). Research in cognitive linguistics — supported by evidence from neuroimaging (Saygin et al 2010) to gesture studies (Núñez & Sweetser 2006) — has identified shared, rich, and stable inferential systems or construals. Lakoff and Núñez (2001) argued that even mathematics depends on the sophisticated deployment of multiple embodied *construals*, which supply semantics and inferential structure. We propose and clarify the hypothesis that a proof's indicated derivations are the shared conceptual systems documented by cognitive linguistics — in short, proofs indicate *construals*.

A proof's evoked construals, crucially, can play a functional role in determining its validity. We illustrate this using analytic tools from cognitive linguistics ("CL") to conduct a cognitive-historical case study of a dispute in 19th century Analysis. In 1821 and again in 1853, Cauchy presented a proof of a statement widely regarded as false: The limit of a convergent series of continuous functions is itself a continuous function (Lakatos 1978). Lakatos has noted that Cauchy's proof is valid if the continuum is, in a sense, dynamic. Indeed, Cauchy shows evidence of a dynamic construal of continuity – what in CL is known as a *fictive motion* construal. Fictive motion is a phenomenon of language and thought where static or imaginary entities are construed dynamically, grounded in our embodied understanding of motion (e.g. "The fence runs along the house") (Talmy 2000). Research in CL and gesture studies indicated that fictive motion plays an active role in mathematical practice and pedagogy (Núñez & Lakoff 1998; Marghetis & Núñez 2010). We argue that fictive motion's inferential structure is sufficiently elaborate to validate Cauchy's inferences. Rooting Lakatos's philosophical analysis in cognitive science, we show that Cauchy's fictive motion construal played a functional role in expanding his repertoire of warranted inferences, while obscuring the formal warrant of others.

We conclude by suggesting that a descriptive cognitive historiography can speak to normative issues in the philosophy of mathematics. Our case study suggests that a naturalized, cognitive notion of warranted inference may track actual mathematical practice – better, in fact, than formal, syntactic notions of inference and validity. We further suggest that shared construals may be a cognitive factor which contributes to mathematics' historical stability and sense of certainty. Both theses expand the explanatory scope of descriptive, psychologistic accounts of mathematics.

Amirouche Moktefi (Universités de Nancy et Strasbourg) & Fabien Schang (Université de Nancy)

Propositions as equations: On the role of notation practices in logic

The practice-based philosophy of science, as we are know it at least since Kuhn, made a thorough use of historical investigations for philosophical purposes. Of particular interest were studies that show how controversies occur in scientific communities and how consensus eventually emerges among its members. Similarly, the growing practice-based philosophy of mathematics would get great benefits from revisiting the old and rich literature produced by historians of mathematics since more than a century. This is especially true when it comes to studying practices themselves and other non-foundational issues that were considered by the "traditional" philosophy of mathematics as extra-mathematical.

In this paper, we will question the neutrality of notation practices in logic. We will especially discuss the genesis and fate of the equational notation introduced in logic by George Boole in the mid-nineteenth century, and used by his immediate followers William S. Jevons and John Venn. In this approach, propositions are represented as equations, as one finds them in algebra, where symbols represent operations. For instance, one might represent the intersection of classes x and y as: "x.y", and then represent the proposition "No x is y" by the equation "x.y = 0". Thus, solving logical problems requires solving systems of equations. In spite of its simplicity, this notation has quickly been criticised and other notations were invented. Logicians such as Charles S. Peirce and Hugh MacColl for instance used implicational notations, while others like Oscar H. Mitchell and Lewis Carroll used subscripts.

By the end of the nineteenth century, several logical notations were available "on the market". In this paper, we will see how and why these symbolisms were invented, how they competed to survive, and how ultimately most of them disappeared, with the establishment of the "modern" logical notation. More importantly, we want to determine the conceptual, social and aesthetic considerations that guided the invention and conception of these symbolisms. Finally, we would like to examine whether the choice and use of a particular notation might have a crucial impact on the "content" of the logical system that was developed.

John Mumma (Stanford University)

Understanding Euclid's diagrammatic proofs in terms of Leitgeb's semantic/intuitive distinction

In [Leitgeb, 2009], Hannes Leitgeb proposes a cognitive theory of mathematical provability based on the premise (termed 'Goedel's insight') that 'the semantic and intuitive components of mathematically proofs are epistemically interdependent.' The aim of this paper is to explore how recent work on Euclid's diagrammatic proofs ([Avigad et al, 2010], [Mumma, 2010]) can be thought to support and advance such a theory. The work provides formal models whereby Euclid's results in the *Elements* are justified by both sentential and diagrammatic information. The distinction between these two kinds of information in the *Elements* would seem to link naturally to Leitgeb's general distinction between the semantic and intuitive components of a proof. Accordingly, what the formal models isolate as sentential in Euclid's proofs would fall on the semantic side of Leitgeb's distinction, while what they isolate as diagrammatic would fall on the intuitive side. I examine how such an interpretation can be developed, with a focus on the issues arising in connection with the diagram/intuition correspondence.

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Madeline Muntersbjorn (University of Toledo) *Abstract Objects and Artificial Selection*

This essay explores Henri Poincaré's suggestion that mathematics is neither created nor discovered. Rather, abstract objects are cultivated in a process not unlike the artificial selection of domesticated kinds. On this view, both the history and the psychology of mathematics promise to shed light on the nature of mathematics as a distinct discipline. Famously, Frege insisted upon a gap between mathematical truths, which are universal and necessary, and the contingent means by which people come to know them. In the *Grundlagen* he writes, "What is called the history of concepts is really a history either of our knowledge of concepts or of the meanings of words." Frege presumes that, "If everything were in continual flux and nothing remained fixed and eternal, then knowledge of the world would cease to be possible and everything would be thrown into confusion." For Frege this conditional is a necessary truth in all possible worlds. Since knowledge of the world is possible, it follows that some things must remain fixed and eternal. In *La valeur de la science*, Poincaré concurs that knowledge is only possible because the world exhibits regularities, natural kinds and recurring facts. If nothing were to exist except unique individuals, "there would be no science; perhaps thought and even life would be impossible, since evolution could not there develop the preservational instincts." Poincaré sees order in the natural world as good fortune we are liable to take for granted, not as an *a priori* necessity. Natural kinds are neither fixed nor eternal even as enduring laws of heredity and selection make natural kinds possible. As products of evolution, our inherited abilities to interpret the world reflect the structure of the world. For these reasons, Poincaré does not distinguish as sharply as Frege does between what mathematics is and what mathematicians do. After contrasting Poincaré and Frege further, I consider arguments from Darwin's *Origin of Species* as well as Ernst Mayr's articulation of the "biological" species concept. A better understanding of these landmark developments in the realism-nominalism debate in biology has much to contribute to an account of mathematical kinds that reflects past practice as well as our cognitive endowments. On Poincaré's model of discovery, "wild" mathematical relations are domesticated through innovations in formal languages and the postulation of possible objects. This essay concludes by interpreting Poincaré's conjectures about the "aesthetic intuition of the subliminal ego" in more precise terms, namely, action schemas and genetic algorithms.

Gianluigi Oliveri (University of Palermo) *Object, Structure, and Form*

The main task of this paper is to develop the view of mathematics as a science of structures I have called, borrowing the label from Putnam, 'realism with the human face'.

According to this view, mathematics is a science of patterns (structures), where patterns are neither objects nor properties of objects, but aspects (or aspects of aspects, etc.) of concrete objects which dawn on us when we represent objects (or aspects of . . .) within a given system (of representation).

Mathematical patterns, therefore, are real, because they ultimately depend on concrete objects, but are neither objects nor properties of objects, because they are dependent, both metaphysically and epistemically, on systems of representation.

The work I intend to do in the present article consists of a discus- sion of some issues which have become the focus of critical attention. Such issues are well expressed by the following questions: am I right in asserting that mathematical patterns are neither objects nor properties of objects? What is the difference, if any, between mathematical patterns and other mind-dependent entities such as the Cleveland Symphony Orchestra? Can mathematical patterns be always assimilated to relations? Can what I call 'form of representation' be assimilated to structure? Can the standpoint I take on mathematics, which regards it as a science of patterns, be correctly described as Aristotelian? What is the relation between seeing-as in mathematics, and what E. Grosholz calls 'productive ambiguity'?

Christopher Pincock (Purdue University)
Galois' Proof of the Unsolvability of the Quintic: A Case Study in Inference to the Best Explanation in Pure Mathematics

Despite its relative novelty, philosophical approaches to mathematical practice have already drawn attention to the central role of explanation in mathematics along with debates about the proper setting for a given problem or domain (Mancosu 2008b, Tappenden 2008). In this paper I will consider a case which brings together these two topics in what I believe is a fruitful way. I consider Galois' 1831 group-theoretic proof that there is no general solution in radicals for the quintic (fifth-degree equations). This proof showed that the proper setting for questions about solvability and unsolvability was group theory and this group-theoretic approach had revolutionary consequences for nineteenth century mathematics. At the same time, Galois' proof has been said to be more explanatory than an earlier proof of the same theorem by Abel from 1824. As Stewart has recently put it, "not only does it [Galois' idea] prove that the general quintic has no radical solutions, it also explains why the general quadratic, cubic and quartic do have radical solutions and tells us roughly what they look like" (Stewart 2007, p. 116). Pesic seems to agree as he says "What is new in Galois is a turn toward abstraction in an essentially modern way, leading to a complete understanding of solvability, which Abel lacked" (Pesic 2003, pp. 108-109).

This case, then, brings together issues of explanation and proper setting. I argue that this is not an accident. In fact, explanatory power is used as evidence for the conclusion that a problem or domain has been placed in its proper setting. In some cases, these explanatory considerations can give a mathematician a good reason to extend and expand their mathematical commitments in a new direction. This makes it possible to articulate and defend a limited form of inference to the best explanation for pure mathematics. When certain conditions are met, our mathematical knowledge can be genuinely increased so that we come to know about new mathematical entities or new features of already accepted entities. I close by considering the implications of this position for the epistemology of pure mathematics more generally and the need it creates for more investigations of similar cases from the history of mathematics.

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Dirk Schlimm (McGill University, Montreal) *Some historical considerations on mathematical reasoning: Beyond the "foundational/maverick divide*

Whether mathematical proofs should be conceived as approximating some formal ideal or whether they necessarily need to involve some form of intuition has been a question that divided the "maverick" philosophers of mathematics from the traditional 20th century mainstream. In this talk I will present the debate on this matter between the nineteenth century mathematicians Moritz Pasch and Felix Klein, and discuss its apparent resolution.

Micah Tillman (The Catholic University of America) *Do Numerals Mean Anything?: The Practice of 'Mechanical' Calculation in Husserl's* Philosophy of Arithmetic

In his first book, *Philosophy of Arithmetic*, Edmund Husserl develops a theory of numbers, number signs, and calculation, using Franz Brentano's distinction between "authentic and inauthentic (or 'symbolic') presentations." Most calculation, Husserl claims, is "mechanical"; it is an activity with signs rather than numbers. Dallas Willard, however, has argued that the distinction between authentic and inauthentic (or "symbolic") presentations is inadequate for describing "mechanical calculation." Numerals, when used "mechanically," do not present anything, he claims. Against Willard, this essay argues that number signs may not *individually* present anything, but – when joined into arithmetical expressions and algebraic equations – they symbolically present other number signs. In Husserl's "mechanical calculation," in other words, numerals function like meaningless letters that can be combined into meaningful "words" (i.e., into *names*). The practice of "mechanical calculation," this paper argues, is that of replacing mathematical "names" (arithmetical expressions and algebraic equations) with the "letters" (numerals) they name.