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2008 WINTER MEETING OF THE ASSOCIATION FOR SYMBOLIC LOGIC

The Marriott Hotel
Philadelphia, Pennsylvania
December 27–30, 2008

A meeting of the Association of Symbolic Logic was held December 27–30, 2008, at the Marriott Hotel, in conjunction with the annual meeting of the Eastern Division of the American Philosophical Association. The Program Committee consisted of William Ewald (Chair), Juliet Floyd, and Michael Hallett. The ASL hosted a reception on the evening of December 28th.

The program included two symposia and a special session with an invited talk by Saul Kripke:

Symposium on Historical Ideals of Rigor in Mathematics
Janet Folina (Macalaster College)
Douglas Jesseph (University of South Florida)
Dirk Schlimm (McGill University)

Symposium on Diagrammatic Reasoning in Mathematics
Emily Grosholz (Pennsylvania State University)
Kenneth Manders (University of Pittsburgh)
Sun-Joo Shin (Yale University)

Special Session: "The Collapse of the Hilbert Program"
Saul Kripke (CUNY-Graduate Center)

The program also included one session of contributed papers in which three talks were presented by logicians from the US and abroad.

Abstracts of the invited talks and contributed talks given (in person or by title) by members of the Association for Symbolic Logic follow.

For the Program Committee
WILLIAM EWALD

Abstract of invited Special Talk

► SAUL A. KRIPKE, *The collapse of the Hilbert program: why a system cannot prove its own 1-consistency*.
The City University of New York, Graduate Center, 365 Fifth Avenue, New York, NY 10016, USA.

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Consider a standard system S of number theory—the usual 'Peano Arithmetic' is much more than sufficient. Hilbert's program of the 1920's, had it succeeded, would have proved not only the mere consistency of S, but that every Π^0_2 statement provable in S is true. Gödel's famous paper showed that if the program is to be carried out by means formalizable in S itself, it must fail. His argument, however, used something of a *deus ex machina* as a corollary of an ingeniously constructed undecidable self-referential statement. Here we show that the program can be seen more directly to imply its own collapse, even though it convinced a generation of logicians to believe that it obviously must succeed and that only technical work remained to finish it. In fact, it contains an implicitly self-referential element in and of itself. Moreover, a minimalization argument already used in set theory shows the failure of the program.

The basic ideas of the program are two:

(1) Instead of writing $(\exists x)A(x)$, write $A(\varepsilon x Ax)$, where $(\varepsilon x Ax)$ denotes any true instance of $A(x)$, and is arbitrary if there is none. When all quantifiers have been eliminated in this way, one needs only the axiom scheme if $A(t) \supset A(\varepsilon x Ax)$, were t is any (ordinary) term. One then needs only propositional logic. Moreover, when particular values are assigned to each ε term, it is always decidable whether a given formula is true. (One could have arbitrary primitive recursive predicates as primitive; one could not bother eliminating bounded quantifiers on the inside.)

(2) By a systematic process of trial and error (somewhat anticipating Friedberg–Mucnik priorities, because of the complexities involved when some ε terms involve others), one tries to find values for the ε terms that would make all the lines of a proof true. Rather than model-theoretically interpreting the axioms, one need only interpret proofs. (If one believes, though not 'officially', that the axioms are true, it might seem obvious that in particular proofs appropriate values for the ε terms can be found.)

Notice that the Hilbert program would imply that every provable Σ^0_1 statement must be true, and hence provable, since it is of the form $A(\varepsilon x Ax)$, and a true numerical value for the ε term must be available. Provable Π^0_2 statements must also be true, and instances of the universal quantifier must have provable instantiations. So given any proof p of a Π^0_2 statement $(x)(\exists y)A(x,y)$ where $A(x,y)$ is PR (or bounded quantifier, etc.), and number m, then there is a proof p_1 of an instance $A(0^{(m)}, 0^{(n)})$.

$$(p)\ (m)\ ((\text{p proves a } \Pi^0_2 \text{ statement } \ll(x)(\exists y)A(x,y)\gg) \supset (\exists p_1)\ (p_1 \text{ proves an instance } \ll A(0^{(m)}, 0^{(n)})\gg)) \qquad (^*)$$

In fact, we can show (assuming the Σ_1 correctness of the system) that not only is this general statement unprovable, but even the instance where we assume p = m must be unprovable:

$$(p)\ ((\text{p proves a } \Pi^0_2 \text{ statement } \ll(x)(\exists y)A(x,y)\gg) \supset (\exists p_1)\ (p_1 \text{ proves an instance } \ll A(0^{(p)}, 0^{(n)})\gg)) \qquad (^{**})$$

[Since $(^*)$ is the stronger generalization where we don't insist that m = p, and is *a fortiori* unprovable. $(^*)$ is easily seen to be equivalent to the conditional 'if S is consistent, it is 1-consistent'. Hence if we show that $(^{**})$ is unprovable, we have shown that the conditional in question cannot be provable in S itself.

Now $(^*)$, and hence, $(^{**})$ is itself a Π^0_2 statement. (So in fact is the statement that values can be found for the ε terms of every proof. This is the self-referential element implicit in the program: it makes a sweeping Π^0_2 statement about all provable Π^0_2 statements.) Inspection of what it means shows that if it had a proof p, there would have to be a proof p_1 of an instance $A(0^{(p)}, 0^{(n)})$, and n would have to be itself a Gödel number of a proof of the very same instance, which is impossible if we assume that p_1 is the shortest such proof.

Analogously to the Gödel case, $(^{**})$ cannot be provable if S is 1-consistent, but each numerical instance must be. So $(^{**})$ must be undecidable if S is 2-consistent.

Note that we can prove all this about (**) directly, no knowledge of ε terms is required.

A variant argument would use a different statement than (*) claiming the truth, rather than the provability of the appropriate Σ_1 instance.

Yet another variant would argue for the unprovability of (**) by the simpler minimalization argument based on the equivalence of $(\exists w)A$ with $(\exists z)(\exists w < z)A$. I prefer the original argument as showing conceptually what it is involved, and as applicable even if we abandoned arithmetization in favor of a direct formulation of syntax.

The original argument is also somewhat analogous to the oldest argument against the proof of a sort of consistency statement; that one cannot prove in ZF that ZF has an $R(\alpha)$ model (in fact, as observed later, that it has a transitive model). Though one might think this ought to be possible, in fact it is not, since there would be no model of smallest ordinal. The present writer thought of a model theoretic approach to Gödel's theorem (to be presented elsewhere), and finally came up with the present argument as a proof-theoretic analog applicable to the Hilbert program, which purported to replace models by interpreted proofs.

Abstracts of Invited Papers for the invited panel on Diagrammatic Reasoning in Mathematics

► EMILY R. GROSHOLZ, *Combining abstract and concrete representations in mathematics: a case study in textbook exposition.*

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New mathematical methods often propose a more abstract way of representing problematic items to be used alongside more familiar and concrete ways of representing them. One well-known example is Descartes' use of the algebra of arithmetic (sporting a perspicuous new notation thanks to him, Vieta and Fermat) in order to classify and investigate geometric curves; another is Poincaré's invention and introduction of the Fundamental Group (first homotopy group) to classify 2-dimensional smooth manifolds, a central problem for the nascent domain of topology. In these applications of novel methods we typically find that the more abstract representation only applies if one of the more concrete representations also applies. Nancy Cartwright in ***The Dappled World*** makes a similar point about reasoning in physics, and uses the suggestive image of dressing up for an occasion: thus we may say here that in novel applications of method a more concrete representation must 'fit out' the abstract representation in a given context of use, a given problem-solving situation. In mathematics textbooks, we often find that diagrammatic representations do the work of more concretely indicating the item that is being talked about, while algebraic notation does the work of more abstractly analyzing and exhibiting the item's 'conditions of intelligibility' and thus the problem's 'conditions of solvability,' phrases that I borrow from Leibniz. These two kinds of representation, juxtaposed and superimposed, are set in rational relation by natural language, which explains how they are to be understood in tandem. To support my claim, I will use two late twentieth century textbook expositions of important results. One immediate consequence of my case studies is that re-writing such expositions in predicate logic will erase the irreducible and productive heterogeneity among the idioms of the two kinds of representation and natural language, and thus fail to capture important aspects of the reasoning.

► KENNETH MANDERS, *Geometrical diagram-inference: philosophical and logical opportunities.*

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Providing diagram-attribution criteria that account for the striking success of diagram + text based geometrical demonstration brings out a number of opportunities for non-foundational approaches in logic and philosophy of mathematics.

First, it requires a precise account of rules of a specific mathematical demonstration practice, because our current sources do not provide one. Second, the ancient practices of objection and case-proposal noted in Proclus confirm observations on reliability of diagram attribution in sensitive situations in suggesting convergence rather than apodicticity as a requirement on demonstration; as well as a conception of mathematical reasoning as consisting of opposing agentive roles. Both of these would seem to remain interesting contenders for an account of contemporary mathematical thought. Both provide novel opportunities for logical research.

Finally, the notion of inferential content of geometrical diagrams in such practices poses interesting philosophical challenges.

[1] KENNETH MANDERS, *Diagram-based geometric practice*, ***The Philosophy of Mathematical Practice*** (Paolo Mancosu, editor), Oxford University Press, Oxford, New York, 2008, pp. 65–79.

[2] ———, *The Euclidean Diagram* (1995), ***The Philosophy of Mathematical Practice*** (Paolo Mancosu, editor), Oxford University Press, Oxford, New York, 2008, pp. 80–133.

[3] GLEN MORROW, ***Proclus: A Commentary on the first Book of Euclid's Elements***, Princeton University Press, 1970, 1992.

► SUN-JOO SHIN, *Abstraction and visualization in mathematics*.
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It has long been believed that drawing diagrams in mathematics may be helpful for brainstorming while the use of diagrams has not been totally welcomed as part of a rigorous proof. The paper explores the tension between the brainstorming and formal proof steps in mathematics, mainly in terms of the role of visualization. What is the source of the tension? Is there always a trade-off between these two procedures?

Examining the abstraction process, I make a case for unique features of visualization which could resolve the apparent conflict between its heuristic value and its formal status in proofs.

Abstracts of Invited Papers for the invited panel on Historical Ideals of Rigor in Mathematics

► JANET FOLINA, *Time for rigor: Hamilton on the temporal foundations of algebra*.
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Around 1835 Sir William Rowan Hamilton and Augustus De Morgan were both working on the philosophical foundations of algebra. Their visions, however, were very different. Hamilton supported a traditional conception of mathematics as a body of truths; and he argued that intuitive time supplies such a truth foundation for algebra. In contrast, De Morgan argued that time is too limiting as a foundation for algebra, supporting instead a much more modern, formalistic, account—an account from which emerges many of the ideas central to current conceptions of mathematical rigor. This paper will sketch Hamilton's vision of time as a foundation for algebra; and it will argue for two conclusions. First, De Morgan was right that a formalistic approach is well suited to both the freedom and the level of abstraction needed in algebra. Second, intuitive time proves to be too limiting, as De Morgan argued; moreover it fails to provide a foundation—even for some basic algebraic truths—in the sense Hamilton sought.

► DOUGLAS JESSEPH, *An excess of scruple may hinder the art of discovery: Leibniz on rigorous demonstration and the fictionality of the infinitesimal.*
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I investigate Leibniz's response to criticisms of his infinitesimal calculus, and particularly his claim that, when properly understood, the calculus does not presuppose the existence of true infinitesimals. Instead, Leibniz held the infinitesimal is a "useful fiction." This fictionalism is difficult to reconcile with traditional criteria of rigorous demonstration, and I consider two approaches that Leibniz pursued in his efforts to show that the calculus does not offend against legitimate methodological scruples. The first of these is an attempt to show that infinitesimal reasoning can always be replaced with exhaustion proofs in the style of Archimedes. The second seeks to justify the use of fictional infinitesimals by appeal to what Leibniz termed "ordinary algebra." Although neither approach yields the kind of generality Leibniz sought, an examination of both approaches helps to illuminate his conception of rigor and his approach to the foundations of the calculus.

► DIRK SCHLIMM, *Empirico-deductivism: Moritz Pasch's philosophy of mathematics.*
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In his *Lectures on Newer Geometry* (1882) Moritz Pasch (1843–1930) gave the first rigorous axiomatization of projective geometry, in which he also clearly formulated the view that deductions must be independent from the meanings of the non-logical terms involved. In addition, Pasch also presented in these lectures the main tenets of his philosophy of mathematics, which underlies all his foundational works and which he continued to elaborate on throughout the rest of his life. This philosophy is quite unique in combining an empiricist epistemology with a deductivist methodology of mathematics; his conception of axiomatic systems is rooted in the material tradition, which goes back to Euclid, but it also contains crucial aspects of modern formal axiomatics, which were taken up and developed further by Hilbert. This talk presents Pasch's philosophy of mathematics and is intended as a contribution towards a better understanding of the radical transition mathematics underwent at the turn of the twentieth century.

Abstracts of contributed papers

► KOHEI KISHIDA, *Neighborhood-sheaf semantics for first-order modal logic.*
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This talk shows how Kripke-sheaf semantics and topological-sheaf semantics for first-order modal logic can be unified, by proper subsumption, in terms of "sheaves" over well-structured neighborhood frames. The notion of local homeomorphism over a topological space naturally extends to that of local isomorphism over a neighborhood frame whose neighborhood systems are quasifilters (i.e., closed under supersets and binary intersections), so that the category of such structures provides a semantics for first-order modal logic. This notion of local isomorphism, when restricted to the one over Kripke frames as a subclass of neighborhood frames, coincides with the notion of Kripke sheaf in a way that the semantics offered in this talk properly subsumes not only topological-sheaf semantics but also Kripke-sheaf semantics. The simple union of first-order logic and modal logic containing schemes M ($\varphi \vdash \psi$ / $\Box\varphi \vdash \Box\psi$) and C ($\Box\varphi \wedge \Box\psi \vdash \Box(\varphi \wedge \psi)$)—but not necessarily N ($\vdash \varphi$ / $\vdash \Box\varphi$)—is sound and complete with respect to this neighborhood-sheaf semantics, and correspondence results are also available.

[1] H. ARLÓ-COSTA AND E. PACUIT, *First-order classical modal logic*, ***Studia Logica***, vol. 84 (2006), no. 2, pp. 171–210.

[2] S. AWODEY AND K. KISHIDA, *Topology and modality: The topological interpretation of first-order modal logic*, ***Review of Symbolic Logic***, vol. 1 (2008), no. 2, pp. 146–166.

[3] D. GABBAY, V. SHEHTMAN, AND D. SKVORTSOV, ***Quantification in nonclassical logic***, pre-final draft, October 2007.

[4] S. GHILARDI, *Presheaf semantics and independence results for some non classical first order logics*, ***Archive for Mathematical Logic***, vol. 29 (1989), no. 2, pp. 125–136.

[5] R. GOLDBLATT, ***Topoi: The categorial analysis of logic***, North-Holland, Amsterdam, 1979.

► FRED SEYMOUR MICHAEL, *Between the true and the untrue*.
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The law of the excluded middle or excluded third, as usually understood, asserts that any proposition must be either true or false; there is no middle ground or third value. This principle, which goes back to Aristotle, has been called into question over the centuries; but not until the twentieth century have full scale logics been developed in which it does not hold. Brouwer rejected it in a short paper published in 1908 and the intuitionistic logic derived from his principles is probably the best known example of a logic in which the law of the excluded middle fails, although the claim that there is no third value is inconsistent with any logic with more than one or more values between true and false. But while there are many logics that allow a middle ground between true and false, can one reject the assertion that any proposition must either be true or untrue? Is there a middle ground between truth and untruth? The question almost seems frivolous. It would seem that it could hardly be disputed that every proposition is true or it isn't, and also that no proposition both is true and isn't seems equally indisputable. How could such weak forms of the law of the excluded middle (LEM) and the law of non-contradiction (LNC) be false? Still it can't do any harm to investigate this. If weak LEM and LNC are true, then it must be possible to generate from a given set of propositions, some true and some not, two exclusive and exhaustive sets, one containing all true propositions relative to that set, its truth set, and the other containing all its untrue propositions, not all of which need be false, its untruth set.

An inductive definition of the truth set relative to some propositions, K, requires a clause fixing the boundary of the set, but assuming the boundary is properly fixed, that clause is a true proposition which however is not in the truth set, so the truth set is not exhaustive and it is easily shown then that no truth set is. If its boundary is included in the truth set, then what of the boundary of the untruth set? Since the two sets are exhaustive, according to weak LEM, the two boundaries must coincide, that is, must belong to both sets. In that case, weak LNC fails. The implications of this appear to be far reaching.

► ALADDIN M. YAQUB, *Duhem, Bayes, and irrationality*.
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Bayesianism is a theory of probabilistic reasoning that attempts to capture the logic of confirming and disconfirming hypotheses. I first argue that Bayesianism reveals striking parallels between structures universally held as paradigms of rational belief systems and structures that are typically considered clear examples of irrational belief systems. After making such observations, I explain that the crucial difference between these two types of belief system is to be located outside the systems, in the dynamics, i.e., attitudes, by which such systems are revised or maintained. Thus I conclude that irrationality (and hence rationality) is primarily a property of attitudes, and derivatively of persons (who exhibit such attitudes) and of beliefs (that are maintained by such attitudes). On the one hand, this tells us that the Bayesian method can reveal some deep facts about the nature of rationality and irrationality; on the other hand, it shows that Bayesianism is inadequate as a theory of rationality, since some of these facts cannot be incorporated within the theory itself. I also explore further implications of the Bayesian analysis discussed in the first part. The Bayesian analysis on the basis of which those conclusions are made arises from a careful examination of the Duhem problem, which is the problem of determining the disconfirmational impact on the plausibility of hypotheses that are collectively responsible for a false observational consequence.

Abstracts of papers presented by title

► KENNETH BARBER AND JOHN CORCORAN, *Agent and premise implication*.
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This lecture discusses two of the many senses of the transitive verb 'implies'. Giuseppe Peano stated the axiom of mathematical induction before 1900. In the *premise* sense, [the "premise"] Peano's induction axiom *implies* [the proposition] "Every number other than zero is a successor". In the *agent* sense, [the agent] Peano *implied* [the proposition] that every number other than zero is a successor. In the premise sense, a proposition *implies* exactly the propositions that follow logically from it. In the agent sense, a person *implies* exactly the propositions that follow logically from propositions the person stated: coherency requires that the subject be a person. In the agent sense, it is *coherent* (true or false) to say that a given person implied a given proposition; but it is *incoherent* (neither true nor false) to say that one given proposition implied a second given proposition. However, in the premise sense, it is coherent to say that one given proposition implies a second given proposition; but it is incoherent to say that a given person implies a given proposition. In the agent sense, 'implies' is an *action verb* that can be used in temporal tenses but cannot be used in "tenseless" or non-temporal tenses. In the premise sense, 'implies' is a *relation verb* that cannot be used in any tense except the tenseless present. By performing a speech act stating a proposition "premise-implying" a given proposition a person "agent-implies" the given proposition. The basic ideas were discovered in the 1960s by the first author, the late Kenneth Barber, and were developed collaboratively over the years in numerous conversations. This lecture was written by the second author after the sad and untimely death of the first, whose memory it honors.

► JOHN CORCORAN, *Ambiguity: lexical and structural*.
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An expression with more than one normal sense or meaning is *ambiguous*. To demonstrate ambiguity it is sufficient to indicate two senses. In the broad sense of 'animal', every human is an animal; in the narrow sense, no human is an animal. In the nonexclusive sense of 'or', every human is human or human; in the exclusive sense, not even one human is human or human. A human that is human or human, in the exclusive sense, is not both human and human, thus not human. An ambiguous non-compound expression is *lexically ambiguous* in the narrow sense. A compound expression containing a lexically ambiguous constituent is *lexically ambiguous* in the broad sense. Consider 'every animal is not a human'. Reading 'not' with broad scope and 'animal' in the broad sense, it expresses the true negative proposition "not every animal is a human". Reading 'not' with narrow scope and 'animal' again in the broad sense, it expresses the false universal proposition "every animal is a non-human"—which is logically equivalent to "no animal is a human". A compound expression is *structurally* *ambiguous* in the narrow sense if none of its non-compound constituents are ambiguous but it expresses senses having different *logical structures* or *forms*. The broad sense drops the restriction to non-ambiguous constituents. The last example is structurally and lexically ambiguous in the broad senses. Lexical ambiguity of constituents is sometimes essential to structural ambiguity of compounds. Consider 'every number but one is positive'. Reading 'one' *denotationally* can yield a false proposition implying that one is not positive. Reading 'one' *quantificationally* can yield a true proposition implying that there is only one number that is not positive: taking 'number' to refer to non-negative integers.

► ALBERT A. MULLIN, *Remarks on recursive number-theory (the Riemann Hypothesis and Euler's diophantine conjectures), I.*
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This note investigates connections between the Ordinary Riemann Hypothesis (ORH) and the logical problem of the (recursive) solvability of a specific and simple diophantine conjecture of Euler [***Opera Omnia***, Series 1, vol. 4, pp. 120–124]. Elsewhere, early in *loc. cit.*, Euler also gives several other analogous diophantine conjectures, which he traces to Fermat. I propose the following variation of Euler's diophantine conjecture as, yet another, problem of intractable difficulty conneceted to the ORH.

PROPOSITION. *If* $n > 0$ *then any integer of the form* $(8n + 3)$ *is the sum of an odd perfect power* (*possibly a non-square*) *and an even square-free with precisely the same number of prime factors as that power, possibly in many essentially different ways. E.g.*, $291 = 13.13 + 2.61 = 1.1.1 + 2.5.29 = 3.3.3.3 + 2.3.5.7$.

Although Euler was not able to settle his diophantine Conjecture (and, presumably the Proposition above), several well-known people worked on his Conjecture; e.g., D. Hilbert, G. Polya and D. H. Lehmer, among others. None was able to settle it. Of course, Euler did not work on ORH, but Guass (briefly), Riemann, Hilbert, E. Laudau, Polya (again!), J. F. Nash, and P. J. Cohen, among others, did work on ORH. None was able to settle it. I pose the following *Metaconjecture*. The validity of ORH is reducible to the recursive solvability/validity of Euler's diophantine conjecture or the recursive solvability/validity of the diophantine Proposition posed above. Further, this is the sense in which I stated that the proof of the ORH does not depend on complex function theory (anymore than the proof of the Prime Number Theorem depends on complex function theory); see the last two sentences of my review of two books on Paul Erdős [***American Mathematical Monthly***, vol. 106, no. 7 (1999), 694–696].