CHAPTER 3.

FORMAL MODELS

JOHN WOODS

Abstract: The most highly developed account of formal models in philosophy can be found in what has come to be called *formal semantics.* In its pure form, a formal semantics is the model theory of an abstract and purely formal logistic system. The formal language L of any such system is an artificial one, carrying none of the meanings to be found in natural language. In its less pure and philosophically more adaptable form, a formal semantics is a theory of truth for a natural language modelled on how the pure theory formally represents truth in L. Once truth is defined for a formal language, it is easy to define logical truth and logical implication modelled on the pure theory's provisions for their formal representation in L. As an expository ease I'll call these adaptations "applied formal semantics."

A nearly unanimous theme that runs through Canadian approaches to argument is that formal logic is of little value, if any, in representing how best to get at the logical structure of argument in everyday life, not only about commonplace things but about anything at all that human beings argue about, including the Continuum Hypothesis or black holes. There are in the Canadian literature various instances in which "social license" of formality is contemplated and sometimes granted. Most notable perhaps is the Canadian fondness for argument-schemata. But nowhere in this literature is there any social-license consideration of consigning the

burdens of natural language argumentation to the representational devices of either pure or applied formal semantics. Not even in those cases in which systems of logic are adapted for use in fallacy theory, was any ever chosen for its model-theoretic provisions.

That alone makes a chapter on formal models in a book about Canadian argumentation theory stand out like a sore thumb, raising the question of whether it belongs there. My answer is that the present paper is no sore thumb, and that it has a perfectly proper place in a book like this. In the pages to follow, I'll try to show that even an applied formal semantics of the mother tongues in which humans advance their arguments is saturated with problems which haven't yet been laid to rest. I will suggest that, in its sweeping indifference to formal semantics, the Canadian theorists of argument have shown an intuitive reluctance which reflects great credit on them.

"The most reliable way of carrying out a proof, obviously, is to follow pure logic, a way that, disregarding the particular characteristics of objects, depends solely on those laws upon which all knowledge rests. Accordingly, we divide all truths that require justification into two kinds, those for which the proof can be carried out purely by means of logic and those for which it must be supported by the facts of experience. But that a proposition is of the first kind is surely compatible with the fact that it could nevertheless not have come to consciousness in a human mind without any activity of the senses." Frege, 1879[1]

1. THE FORMALIST PRESENCE IN INFORMAL LOGIC

To a dominant extent, the Canadian influence on theories of argument flows from their contributions to informal logic in the aftermath of Charles Hamblin's call to arms in 1970 for the restoration of the fallacies project to the research programmes of logical theory. A good early overview of informal logic's self-directed remit is provided by Ralph Johnson and Tony Blair in "Informal logic: The past five years, 1978-1983" in the *American Philosophical Quarterly*.[2] It was clear even that early on, that infor-

1. Gottlob Frege, *Begriffsschrift, a Formula Language, Modelled upon that of Arithmetic, for Pure Thought*, in van Heijenoort 1967 at pages 5-82.

2. Vol. 22 (1985), 181-196. See also *Informal Logic*, 7 (1985), 69-82, Douglas Walton, *Informal Fallacies*, Amsterdam: John Benjamins, 1987. Earlier was John Woods,

mal logic had been spurred to more than just one revival. In addition to fallacies, dialogue logic and dialectical logic received an even more productive boost, made so by the fact that there were bustling developments already in full swing in the more formal and mathematical treatments of these matters. Similar developments were taking root in logic programming and other computational approaches to reasoning and arguing. Adaptations of the modal logics of knowledge, time and action were also being made. Informal logicians who took the path of dialogue and dialectic had more fellow-travellers to talk to than those who took the fallacies path. The dialogue and dialectic path-takers had large and rapidly developing current literatures to react to and learn from.[3] The fallacy path-takers had no current literature to immerse themselves in, and were driven to the desperate expedient of consulting the leading undergraduate textbooks

"What is informal logic?" in Ralph H. Johnson and J. Anthony Blair, editors, pages 57-68, Point Reyes, CA: Edgepress, 1980, and later his "The necessity of formalism", in John Woods, *The Death of Argument: Fallacies in Agent-Based Reasoning*, pages 25-42, Dordrecht: Kluwer, 2004, and "The informal core of formal logic", pages 43-61, *The Death of Argument*. I add now a stylistic remark: Since I myself am part of the Canadian story, I shall adopt the following conventions. When I refer to me as a participator in this literature I'll adopt the third person perspective. When I refer to myself as the person writing this essay, I'll adopt the first person perspective.

3. See, for example, E. M. Barth and Erik C. W. Krabbe, *From Axiom to Dialogue: A Philosophical Study of Logics and Argumentation*, Berlin and New York: de Gruyter, 1982. In the first year of its publication, *Argumentation* published Jaakko Hintikka's "The fallacy of fallacies", 1 (1987), 211-238, in which fallacies were worked up within an interrogative logic of game-theoretic cast. In a number of places, the influence of Hintikka's foundational contributions to epistemic logic was also discernible. In Woods and Walton's *Fallacies: Selected Papers* 1972-1982, there are nineteen chapters, and no fewer than nine of them involve dialectical factors. The influence, direct or otherwise, of epistemic logic is discernible in six of them. Ralph Johnson is a bit more circumspect in his engagement of dialogical and dialectical considerations. See his *Manifest Rationality: A Pragmatic Theory of Argument*, Mahwah, NJ: Erlbaum, 2000. Even so, pragmatic and dialectical considerations including Johnson's own recognition of the dialectical tier, are evident in all the Canadian writings. See, for example, Robert C. Pinto's, *Argument, Inference and Dialectic: Collected Papers*, Dordrecht: Kluwer, 2010. In their dialectical tilt and pragmatic and contextual sensitivities, these papers are typical of Canadian practice.

for use in introductory logic classes in universities and four-year colleges. Virtually without exception, they saw in them abundant confirmation of Hamblin's own already low opinion of how, if at all, they handled fallacies. In no time at all, informal logicians would be publishing what they hoped would be better introductory texts.[4] Certainly they were no substitute for frontier scholarship and, in that regard, the newly minted fallacy theorists had little to rely on but their wits, their intuitions, and the older literatures that had been put into an undignified retirement by the overthrow of logic by mathematics, drawing upon what they took to be adaptable features of current literatures in philosophical logic. The significance of the comparatively scant references in note 1 of this essay to Canadian publications in which there is explicit reference to informal logic by title is that, by and large, Canadian informalists had their say about the nature of their subject by just getting on with the job of developing it.

It might strike us as strange that a book devoted to the Canadian influence on theories of argument arising from contributions to informal logic, should make room for a chapter on formal models. It will have been noticed in earlier chapters of this volume that the organizational, congregational and publishing centre of the Canadian movement in informal logic is the University of Windsor, inaugurated in 1979 by the First International Symposium on Informal Logic. A number of the movement's leading figures are based in Windsor. Even more are based elsewhere in the country, and several score more are "honorary Windsorites" from foreign climes. As of today, at least four or five of Windsor's locals made their reputations elsewhere, and two of its first three elders weren't always Canadian. The umbrella under which the Windsor conferences are staged is

4. Ralph H. Johnson and J. Anthony Blair, *Logical Self-Defense*, Toronto: McGraw-Hill Ryerson, 1977; John Woods and Douglas Walton, *Argument: The Logic of the Fallacies*, Toronto: McGraw-Hill Ryerson, 1982; David Hitchcock *Critical Thinking*, Toronto: Methuen, 1983; and Trudy Govier, *A Practical Study of Argument*, Belmont, CA: Wadsworth, 1985. All are still in print in newer editions.

OSSA, the Ontario Society for the Study of Argumentation, in emulation of the earlier example of ISSA, the International Society for the Study of Argumentation, established in Amsterdam as the organizational, congregational and publication centre of the pragma-dialectical approach to argument.[5] The name "ISSA" has two virtues which "OSSA" lacks. It is earlier, and it is accurate. OSSA's active membership is as far-flung as ISSA's, and there is nothing noticeably Ontarian about the logics contrived by OSSAnians.[6] A foundational work for the Canadians was published by an Englishman who in due course would become an OSSA star.[7]

Among locals and awayers alike, the Windsor approach to formal logic ranges from hostile and dismissive to the highly acquiescent. There is a theme that runs throughout that strikes me as certainly right. It is the confident belief that all the going formal logics of 1979 would have had a hopeless time in elucidating the logical structures of everyday argument and inference, including, by the way, the everyday inferences of Frege, Russell and Tarski. Human reasoning is inherently practical, but there are no people in the standard logics of deduction. Those logics were and still are the wrong keys for those locks.

A good many informal logicians think that the principal reason for this alienation is that formal logics – certainly those of the 1970s – were mainly about deductive reasoning, whereas most

5. I must confess to a disliking of the word "argumentation". There is no need for it in English. "Argument" will do all of the heavy lifting intended for "argumentation". It is a count noun and a mass term, and it honours the process-product divide. I've decided on a slight indulgence. If the reader will grant me "theories of argument", I'll grant him an occasional "argumentation theory."

6. We might note that, since the beginning, Windsor's Tony Blair has served on ISSA's executive committee. *Argumentation*'s editor-in-chief, Frans van Eemeren has had a lengthy presence on *Informal Logic*'s editorial board, and John Woods is one of *Argumentation*'s three editors and a member of the *Argumentation Library*'s editorial board.

7. Stephen Toulmin, *The Uses of Argument*, Cambridge: University of Cambridge Press, 1958

of the best of human reasoning is deductively invalid. Seen this way, formal logics simply miss most of the target set by informal logicians. This is true as far as it goes. But it too readily cedes to the formal logics of deduction their *bona fides* as accurate expositors and assessors of what a human reasoner is up to when he makes what he intends to be a truth-preserving argument, that is to say, a deductively valid one. I shall say more of this three sections hence when I briefly survey the route from Tarski to Quine to Donaldson which brought to philosophy the *formal semantics* of the mother tongues in which we humans frame our arguments. Suffice it to say for now that the formal logics of that day were encumbered by two problems. One, as we just said, is "the missed target problem" and the other is "the conceptual distortion problem." By this I mean that the more abstract our representations of a natural language concept, the greater the likelihood of making it unrecognizable in the formalizing wash. Jointly these problems produce what I'll call the *formalist crisis* for theories of real-life argument and inference. In what follows, I'll consider the crisis' prospects of relief. But first, something more should be said about the words "formal" and "model".

2. THE UNRULINESS OF "FORMAL MODEL"

The expression "formal model" is ambiguous in English, as are the two words within. They are unruly and challenging ambiguities. "Formal" ranges all the way from the correctness of one's words to our Sovereign Lady the Queen, to the suit one dons at his nuptials, to the abstractions of plane geometry. Models model clothes on the runways of Milan and in the design centres of Paris. Toy-stores sell models of World War II Spitfires, and sometimes, for good or ill, Dads are models emulated by their sons. In first-order classical logic, a model is a set-theoretic structure, and in macroeconomics models are mathematical entities of a quite different construction. In climate science, they are yet another kind of mathematical thing. Sometimes a model is a way the world couldn't possibly be, and that the good that's sometimes in it is a strictly collateral benefit. This happens when reflecting on how *this* particular aspect of the world couldn't possibly be, we are led to see how *that other* feature of the world actually is. In other cases, a theorist formalizes a concept simply by giving it a biconditional definition. In still others we give our arguments formal expression when we avoid enthymematic formulation. Sometimes a formalization of something is a pictorial representation of it or a schematic rendering. It might also be true that, in some cases, real-life arguments are pictorially advanceble.[8] For all this semantic fog, some clear lines are discernible. Simplifying slightly, formal logics of argument heavily traffic in applied mathematics. Informal logics of argument show little trace of it.[9]

For a good many of Canada's theorists of argument and reasoning the only point of contact with formal modelling is by way of what is mistakenly called the "translation" rules for mapping natural language arguments to their logical forms in a formal language L usually that of first-order classical logic. In its standard understanding, translation preserves meanings or at least approximations to them. While natural languages brim with meanings, formal "languages" have none at all. It is not possible to order a hamburger in L or simply to say what your name is.

This present view of formal languages requires historical qualification. When we turn to Frege's treatment of the sentential calculus in the *Begriffsschrift* of 1879, we see that he was serious in saying that the formulae of his "formula language of pure thought" would be both vehicles for real thought and susceptible to affirmation and denial. A horizontal stroke or *Inhaltsstrich* prefixed to a formula ϕ signifies its propositional content or the thought it expresses, as with "$-\phi$" for example. When a small

8. See, for example, Leo Groarke, "Logic, art and argumentation", *Informal Logic,* 18 (1996), 105-129, and J. A. Blair, "The possibility and actuality of visual argument", *Argumentation and Advocacy,* 33 (1996), 23-39.

9. A questionable exception, as I think is the over-modelling of inductive argument and non-demonstrative belief revision in the probability calculus.

vertical line is attached to the left side of the content stroke, a *Urteilsstrich* or judgement stroke is formed. The two combined together come to be called the assertion sign. The content stroke prefixed to ϕ signifies the judgeability of its content. When the judgement stroke is prefixed, it expresses the affirmation of the thought conveyed by ϕ. Negation works in the following way, "-┬-ϕ" signifies the negation of ϕ, and " | -┬ϕ" the denial of ϕ. There is no inkling of these strokes in what has long been the standard notation for classical sentential logic. The last part of the English subtitle of the *Begriffsschrift* is also important. Frege's formula language of pure thought would be "modelled upon that of arithmetic". It is provable in number theory that 2 + 2 = 4. Frege wanted a formal language capable of saying that same thing, but not in the workaday language of arithmetic. What Frege wanted from his formula language is the means to say that 2 + 2 = 4 without the necessity to mention or quantify over numbers. In this way, the way of logicism,[10] Frege's formal language would be purpose-built for the reduction without relevant loss of number theory to the pure logic laid out in the *Begriffsschrift.* It would be designed from the get-go to give to arithmetic a comfortable truth-preserving home.

The idea that the formal language of a logistic system is entirely devoid of propositional content arises from a somewhat later source. In Hibert's quest for a logic freed from the burdens of propositional content, truth and meaning, launched the proof theories in which this quest is fulfilled.[11] In due course, modern logic would accommodate both model theory and proof theory, and would prove important correspondences between them. But for that to happen, both parties had to agree (and did) that the

10. Frege develops the philosophical case for logicism in *The Foundations of Arithmetic*, translated by J. L. Austin, Oxford: Blackwell, 1950. First published, in German, in 1884.

11. David Hilbert, "On the foundations of logic and arithmetic", in van Heijenoort 1967 at pages 129-138. Original German text of a talk in 1904 to the Third International Congress of Mathematicians.

respective properties in correspondence were definable over a common artificial language bereft of content, and incapable of expressing thought.

I come back now to Frege, briefly. By the time Frege issued the first volume of *Basic Laws of Arithmetic* in 1903, the judgement stroke " | –" would be given a new role to play. It would now signify a function from names to truth values. Formal sentences formerly taken to be thought-expressing were now names of truth functions. Accordingly, "—ϕ" would be a name that denoted the truth-value (*Wahrheitswert*) the True (T), and since all names therein must also denote truth-values, the horizontal line must assign truth-values to them, notwithstanding their own thought-inexpressibility. In these cases, they would name truth-value the False (F).[12] This is yet another striking difference from today's standard logics in which only sentences are assigned truth-values.[13]

The point to emphasize is that the state in which classical logic has been for decades is one in which formal languages are semantically dead, prompting thereby the question of whether they are capable of semantic revival by mathematical means.

A more accurate term for what are misdescribed as translation rules from English to L is "*mapping* rules", rules attempting to establish one-to-one correspondences between natural language expressions and their formal counterparts in L. Consider a simple example. For every logician of every stripe, validity is a property of interest, and especially valued are procedures which reliably determine its presence or absence in arbitrarily selected cases. One way in which an argument's validity in English is tested is by using the mapping rules to find its counterpart argument in L, which is said to be its "logical form". The formal language has a well-defined notion of validity instantiable by L's

12. "Truth-value" is Russell's rendering of "*Wahrheitswert*" in Appendix A of *The Principles of Mathematics* 1903

13. I will provide further citations in various places ahead.

formal arguments. The further features of formal validity are provided by L's model theory, giving to "validity" a meaning in the metalanguage of L that it certainly does not possess in English. For that reason it is helpful to use "validity" to denote what valid English arguments have and "*validity" to denote what *valid formal arguments have. Even so, formal *validity and the mapping rules conduce to a good end. For, whenever an English argument maps to a formally *valid one in the model theory of classical logic, the English argument is also *valid, or as we may also put it, *formally *valid*. The mapping rules have this interesting feature. They reflect back to English arguments the *validity of its logical form in L. This is the *backwards reflection property* with respect to *validity.

It turns out that any formally *valid argument of English is also valid, i.e. is such that its conclusion follows of necessity from its premises jointly. The English term "necessity" has no formal counterpart in L. There is nothing in this logic to which "necessity" can be mapped. From which we may conclude that, whatever "*valid" means in L, it does not mean that the conclusion of an L-argument follows of necessity from its premisses jointly. Still *validity in L implies validity in English. Although the mapping rules are a perfect test of formal *validity in English, they are only a partial test of validity in English. The reason why is that the "atomic" or simple indicative sentences of English have meanings and the atomic wffs of L have none. The atomic meanings of English enable meaning connections, some of which generate validity as in the well-worn example of the "coloured shirt argument":

1. The shirt is red
2. Therefore, the shirt is coloured.

Since the conclusion of this argument follows of necessity from its premiss, it is valid. But its form in L is

1′. *p*
2′. *q*

which is not *valid (and not valid either).[14]

This creates a nontrivial problem for the mapping rules. For the rules to have the backwards reflection property for *validity it is necessary that the set of English atomic sentences not stand to one another in any semantic or logical relation. They must be pairwise semantically and logically inert. I know of no logic textbook that develops the right filtration device for inputting the atomic sentences of English to the function described by the mapping rules in ways that avert coloured shirt problems.[15]

Everyone in the Canadian informal logic community was educated in the analytic tradition. For many of them, perhaps a hefty majority, doing philosophy analytically is simply the preferred way of doing it. I won't be able to say my piece about the place of formal models in argumentation theory without having my say about the dominant presence of formal semantics in philosophy, especially the analytic philosophy of language. And I won't be able to do that without a quick Cook's Tour of *conceptual analysis*. I'll turn to that now. Formal semantics will come right after.

3. PHILOSOPHY GOES ANALYTIC

Formal semantics has a twofold parentage. One is a crisis in the foundations of arithmetic. The other is a crisis in analytical philosophy. A proper understanding of it requires that we make some brief mention of them. The crisis in arithmetic was prompted by Frege's conviction that all of higher mathematics

14. A similar difficulty attaches to the map from sentences such as "The tabletop is oval and the tabletop is rectangular", made inconsistent by virtue of predicate-meanings. Its formal representation in L is the formally consistent wff "p ∧ q".

15. For a bit more on this, interested readers could consult Woods' *The Death of Argument*, pages 48-53, chapter 3, "The informal core of formal logic".

has secure foundations in number theory. The question was whether number theory is able to furnish its own foundations. Towards the close of the 1870s Frege came to believe that it could not. If arithmetic's foundations couldn't be found elsewhere, Frege feared that all of mathematics would topple into a rubble of confusion and mystification, causing massive collateral damage to the sciences. In 1893 Frege thought he'd found the answer he'd been seeking. The answer lay in *logicism.* In the first volume of his *Basic Laws of Arithmetic,*[16] Frege thought he had demonstrated how every true statement of arithmetic could be matched in a truth-preserving way to a formula of pure logic in which there is no reference to or quantification over numbers. The logic in question was of Frege's own invention (or if preferred, discovery) which was a second-order functional calculus of great ingenuity. If Frege's solution held, the mathematical foundations' crisis would be averted.

Frege was also implicated in the rise of analytic philosophy, with the publication of papers in the 1890s such as "Function and concept", "On concept and object" and "On sense and reference", and later in the late 19 teens "Thought" and "Negation"[17]. Another important source was G. E. Moore, with early papers in the short interval from 1899 to 1903, such as "The nature of judgement", "The refutation of idealism" and "Kant's idealism" and the classic *Principia Ethica*.[18] Moore was instrumental in con-

16. Gottlob Frege, *Basic Laws of Arithmetic: Derived Using Concept-Script*, volumes I and II, translated and edited by Philip A. Ebert and Marcus Rossberg, with Crispin Wright, Oxford: Oxford University Press, 2013. Volume I first appeared, in German, in 1893 and II in 1903.

17. "Function and concept" in Peter Geach and Max Black, editors, *Translation from the Philosophical Writings of Gottlob Frege*, pages 42-55, Oxford: Blackwell, 1970; "On concept and object", in Geach and Black, pages 181-193; "On sense and reference", in Geach and Black, pages 56-78; "Thought" in Peter Geach, editor, *Logical Investigations*, pages 1-30, Oxford: Blackwell, 1977; and "Negation", in Geach, pages 31-53.

18. "The nature of judgement", *Mind*, 7 (1899), 176-193; "The refutation of idealism", *Mind*, 12 (1903), 433-453; "Kant's idealism", *Proceedings of the Aristotelian Society*, 4 (1903), 177-124; and *Principia Ethica*, Cambridge: Cambridge University Press, 1903.

verting Russell from a McTaggartian idealism to the methods of conceptual analysis. Moore likened the elucidation of a concept of philosophical interest to the decomposition of a substance of chemical interest by chemical analysis. Concepts were either simple and unanalyzable or complex. Simple concepts were intelligible as they stood and in no need of clarification. If a complex concept were in need of clarification, it would be provided by an analysis that decomposed into its simple constitutive subconcepts. Largely independently, Russell and Frege had converged on a common understanding of how to provide a conceptual analysis of the notion of set.[19] It would be provided by Frege's axioms or basic laws, laid out in the first volume of *The Basic Laws of Arithmetic.* This convergence was furthered by two other points of agreement. Russell agreed with Frege's logicism, according to which number theory could be reduced without relevant loss to pure logic. He also agreed that set theory was an essential part of the pure logic required for this reduction.

In 1902, Russell wrote to Frege with the news that the axioms of his set theory harboured a contradiction. Frege promptly and ruefully replied eight days later.[20] This, the infamous Russell paradox, would create the crisis of analytical philosophy. Frege and Russell had agreed that Frege's axioms provided a conceptual analysis which revealed the true nature of what it is to be a set. Russell expressly asserted that, thanks to the paradox, no philosophical analysis of the concept of set was possible. Frege briefly dithered and then permanently retired from the philosophy of arithmetic. As Frege and Russell saw it, what made the paradox a crisis for conceptual analysis was not that the original axioms were mistaken – principally Basic Law V which served as a Comprehension Axiom. Frege and Russell were among the

19. For most of his working life as a philosopher of arithmetic, Frege eschewed the term "sets" in favour of "courses of values of concepts", and Russell favoured "classes".

20. Bertrand Russell, Letter to Frege, in Jean van Heijenoort, 1967, pages 124-125; and Gottlob Frege, Letter to Russell, in van Heijenoort at pages 127-128.

last descendants of the long line from Aristotle, in which an axiom was true, necessary, primary, most intelligible, and neither needful of nor susceptible to independent demonstration.[21] So understood, the axioms for sets disclosed that it lies in the very nature of sets that there aren't any. But without sets transfinite arithmetic is impossible. So Frege turned the dial to geometry, and Russell borrowed the empty name "set" (actually "class") and applied it to objects defined into theoretical existence by nominal definition.[22] Since arithmetic can't get by without something *like* sets, Russell set about making something up and placed it within the regulatory control of the mathematical theory of types. The crisis of analytic philosophy was that conceptual analyses are sometimes horrifically wrong, notwithstanding their appearance of *à priori* certainty.

If we charted the jolt that analytical philosophy was dealt in the months from 1902 to 1903, we could chart it on a "concept clarification line", with an intuitive concept K at the far left, and on the far right a stipulated new concept K* with the same name but not the same denotation:

K: ______________________________ K*[23]

Assuming K to be analyzable, its immediate successor on the line would be K(A), that is, K in its analyzed state. As we see from the sharp change from 1902 to 1903, what the line in its present configuration tells us is that K is uninstantiated and K* is a theorist's creation of some other concept. It would be a mistake, however, to see the clarification of a *consistent*[24] concept in such harsh binary terms. It wasn't to be a matter of "Analyse it or for-

21. Frege would later say that Basic Law V hadn't carried quite the same conviction for him as did the previous four axioms. I do not think, however, that this remained for long his considered opinion.

22. Bertrand Russell, *The Principles of Mathematics*, 2nd edition, London: Allen and Unwin, 1937. First published in 1903; pp. 27 and 114.

23. A word of caution: my concept-clarification line is not a Fregean stroke.

24. More carefully: "widely believed to be consistent and neither known nor believed to be otherwise".

get it and change the subject!" To help see why, it would repay us to take note of the impact of logical positivism on what would retain the name of analytic philosophy. If there were space for it we could have a small section entitled, "Philosophy goes scientific". We don't, so we won't. We'll make do with something a good deal briefer. The two features that one associates with scientific philosophy is its interests in using numbers to achieve a qualitative concept's clarification by making it more precise. I'll call this *conceptual explication.* The other already has a name – *rational reconstruction.* A common example of explication is the representation of the idea of degrees of likeliness by real numbers via the probability calculus in the unit interval. A celebrated example of rational reconstruction was Carnap's attempted reduction of the physical world to the phenomenal one in the *Aufbau.*[25] Putting K(E) for an explication of K and K(RR) for its rational reconstruction, we see that the conceptual clarification line, more fully realized, provides four options for K, not just two:

K: _____K(A) _____K(E) _____ K(RR) _____K*

Each option is a form of *making.* Analysis makes a concept explicit. An explication makes it *precise*. A rational reconstruction makes it *over*. A stipulation changes the subject and makes a new concept *up*. With these options available, we can easily see Russell pleading that instead of just stipulating a new concept of class in his theory of types, he was rationally reconstructing the old one. But we couldn't find for Russell unless he conceded that there was little of the true nature of the intuitive mathematical concept of class in its rational reconstruction. From this, a more general point can be made. The further we proceed from the clarification line's leftmost node rightward to its terminus, the intuitive concept becomes progressively less recognizable in its successors.

25. Rudolf Carnap, *The Logical Structure of the World: Pseudoproblems in Philosophy*, Berkeley and Los Angeles, University of California Press, 1967. First published, in German, in 1928.

From which we must also see that all forms of conceptual clarification are conceptual distortion to one degree or other, and some are a good deal more distorting than others.

4. FORMAL SEMANTICS

The name "formal semantics" was coined by Tarski in what John Burgess thinks was an act of theft. Although Burgess was joking, he was making a serious point. Tarski's tort was to take a name in common scholarly and lay usage and re-apply it without formal notice (no pun intended) to something entirely different.[26] As Tarski used the word, a semantics is the model theory of a formal logistic system. As everyone else uses the word, semantics is a theory of meaning for natural languages. "Well", some might say, "what's all the fuss about? Doesn't the model theory of first-order classical logic (say) assign something like meaning to its formal expressions, strings and sequences?" If we were to ask these sceptics where they would be inclined to place a theory of Tarskian meaning on our concept clarification line, they might tick the explication box. That would be a mistake. A good case can be made for ticking the stipulation box instead, thereby making the original concept of meaning unrecognizable in the made-up concept.

Tarski's contribution to model theory was in the slipstream of Frege's early recognition of the need for it and the important advances in the early part of the 1900s, notably by Löwenheim in 1915 and Skolem in 1919/20, in what came to be known jointly as the Löwenheim-Skolem theorem.[27] The theorem asserts that any theory in first-order logic with identity has a countable

26. John P. Burgess, "Tarski's tort", in his *Mathematics, Models and Modality: Selected Philosophical Essays*, pages 149-168, New York: Cambridge University Press, 2008.

27. Leopold Löwenheim, "On possibilities in the calculus of relatives", in van Heijenoort, pages 228-251, and Thoraf Skolem, "Logico-combinatorial investigations in the satisfiability or provability of mathematical propositions: A simplified proof of a theorem by L. Löwenheim and a generalization of the theorem", in van Heijenoort, 252-263.

model if it has a model at all. Tarski would later prove that the "upward" version of it, showing that theories with infinite models also have models of every infinite cardinality. I mention these seemingly arcane results not to dwell on them here but rather to underscore the sheer distance of Tarskian models from the structural regularities of human argumental life. But the last thing models are is distant from the commonalities of a standard good second-year *textbook* on deductive logic, with which everyone in the Canadian corps will have had to deal as an undergraduate or graduate student and later, in some cases, as a teacher of formal logic.[28] The importance of saying so lies in this. It is simply not true that Canadian informalists are unacquainted with models in this sense. The fact that they don't put them to use in their own work indicates the conviction that, so used, Tarski models neither add value nor pay for their keep. But the fact remains that there is, so far as I can see, little concurrent inclination to denounce the popularity of formal semantics in analytic philosophy, which is home turf of Canada's informal logicians.

What follows now, as briskly as I can do it, is a refresher of what everyone already knows about model theory. A logistic system L is a theory which distributes properties of interest over entities constructed in its formal language L. The language arises from a lexicon of basic expressions, including those designated as atomic formal sentences or wffs. Formation rules recursively define all the non-atomic ones. The lexicon, and formation rules are part of L's syntax. L's syntax provides an infinite array of proper names each carrying its own unique index, as well as an infinity of individual variables also uniquely indexed. Rules also provide for the binding of an individual variable by quantifiers prefixed to the same variable. The syntax's further parts recursively define sequences of formal sentences, and generate pro-

28. An excellent example is George Boolos, John P. Burgess and Richard C. Jeffrey, *Computability and Logic*, 4th edition, Cambridge: Cambridge University Press, 2002. (Fallacies aren't discussed there.)

cedures for the ascription to them or their constituent parts of properties such as theoremhood, deducibility, proof, provability, and (syntactic) equivalence and (syntactic) consistency. L's semantic or model-theoretic part provides interpretations which fix L's domain (or universe) D with respect to which truth-values are the denotations of L's syntactically rendered wffs. D is an infinite set of otherwise uncharacterized individuals, each individuated by a unique index. Functions map particular parts of L's syntax to correspondingly specific elements in or set-theoretically constructable from L's domain. L's semantical rules provide rigorous specification, for the interpretation in question, of the properties of reference, quantification and *n*ary predicate-denotation, and therefrom the further properties of satisfaction, truth, valid sentence or logical truth, valid sentence-sequence, entailment, (semantic) equivalence and (semantic) consistency.

In the metalogic of classical first-order logic, further results are also provable. If L's predicates are monadic, validity is a decidable property. Monadic or not, there also exists between L's syntactic and model theoretic properties a one-to-one correspondence by which ϕ is a theorem of L's syntax iff it is a logical truth of its semantics. Close by is the equivalence of syntactic deducibility and semantic entailment. Logics having this property are said to be complete with respect to their semantics. Logics in which the correspondence is not only one-one but also onto are sound with respect to their syntax. While everyone concedes that the atomic wffs (well-formed formulas) of a formal "language" are entirely meaningless, it is often (and mistakenly) said that the logical particles of such a language – e.g. the connectives of the sentential calculus – have the meanings conferred on them by the system's formation rules for molecular wffs, whereby truth conditions are imposed on sentences in which particules occur. This is not true. What the formation rules assign are truth-values, of which in first-order logic there are only two, T and F. Every wff is assigned one or the other and never both. T and F are undefined abstract *objects* denoted by wffs. They are not linguistic objects, and so the question of their having meaning doesn't arise. In natural languages such as English, truth and falsity are *properties* ascribed to linguistic objects by the predicates "is true" and "is false", neither of which occurs even in the model theory of classical logic. It is said, however, that "ϕ denotes T" *formally models* natural language sentences of the form "'S' is true" by way of a formal representability relation R. But there is in the Canadian literature, and everywhere else in the argumentation community virtually no work on how R is structured so as to deliver the desired result. Consider again the difficulties discussed in section 2 posed by the mapping rules from English to counterparts in L.

The term "truth-value", as we now see, is a tort. So are *all* the following, the very terms that make up the working vocabulary of Tarski's semantics: "*vocabulary", "*sentence", "*name", "*predicate", "*argument", "*proof", "*theorem", "*syntax", "*truth", "*valid (sentence)" "*valid (argument)", "*entailment", "*semantics", and on and on.[29] None of these expressions bears any recognizable resemblance to what those terms actually mean in pre-tort reality. The qualification "formal" no more makes a formal sentence a kind of sentence than the qualification "fools'" makes fools' gold a kind of gold. Here are two further examples to consider. In the semantics of L a formal sentence is true in an interpretation I iff it has a model in I, iff every countably infinite sequence of elements in I's domain of discourse D satisfies ϕ in I. ϕ is satisfied by a countably infinite such sequence S iff the following conditions are met: If ϕ is an atomic wff of the sentential calculus, it is satisfied by S iff ϕ denotes T in I. if ϕ is an atomic wff of the predicate calculus with *n*-ary predicate ψ, S satisfies ϕ in I iff for each denota of its singular terms stand to one another in a way that structures them as *n*-tuples of the class of *n*-tuples denoted by the predicate ψ. If ϕ is a wff in the form ⌜~ψ⌝ for arbi-

29. Also, recall our discussion of *validity in section 2.

trary wff ψ, S satisfies ϕ iff it doesn't satisfy ψ. If ϕ is a wff in the form $\ulcorner \psi \vee \psi' \urcorner$, S satisfies ϕ iff it satisfies ψ or satisfies ψ' or both. If ϕ is a wff in the form $\ulcorner \forall x_k\ (\psi) \urcorner$, with x_k is the variable whose index is *k* in I, S satisfies ψ iff every countably infinite sequence of elements in D differing from S at most in its *k*th element satisfies ψ.

The second example is more quickly dealt with. From its very foundation, logic has had an abiding interest in entailment. When considered as a property of pairs of English sentences A and B the still dominant view of what "entails" means has it that A entails B iff it is logically impossible for A to be true and B false (or anyhow not true). It is utterly routine for teachers of logic and others who should also know better to paraphrase this as "A entails B iff it is logically impossible for A to be T and B to be F (or anyhow not T). This is false. As we've already seen, T is an undefined object of the formal semantics of L, thus making "A is T" ill-formed in English and L alike, vitiating thereby the lazy paraphrase of the dominant definition of "entails" in English. Here is how it goes in L: ϕ entails ψ iff there is no interpretation in which ϕ has a model but ψ doesn't. More specifically, there is no interpretation in which every countably infinite sequence of its D-elements satisfies ϕ yet does not satisfy ψ. No one with any sense and without an axe to grind would say that in these formal notions of truth in I and entailment there is a recognizable presence of the truth and entailment in natural language.

Paragraphs ago I surmised that if an analytic philosopher of the present day were asked to place Tarski's concepts of truth in an interpretation and of entailment in all of them, he would hover over the point at which the line moves from analysis to explication. But as is now apparent that would be more hopeful than accurate. The right place over which to hover is the terminus, the place at which the ever-torting Tarski just made these things up while retaining the original names.

"What in the world would motivate Tarski to have gone so far?", people will ask. The answer lies in the Liar paradox which, as Tarski saw it, did to theories of truth in natural languages what the Russell paradox did to sets. Tarski blew very hot and only very slightly cold about the fix that the concept truth was in. In hot moments, he echoed Frege and Russell in thinking that it lies in the very nature of truth in natural language that no sentence of natural language is true – in other words, that the truth predicate has a null extension. In more reflective moments he thought, as did Russell about sets, that natural languages simply couldn't get along without a consistent predicate for truth operating in something like the way Russell thought the predicate "set" had had to be made to work. At this juncture, it is convenient to mark two sides of Tarski's intellectual personality. Considered purely as a model theorist, Tarski thinks that natural language is a dead duck. But as author of "The concept of truth in formalized languages", he turned his sights to truth's rehabilitation in natural speech.[30]

The post-1902 Russell wanted a new concept that would serve the purposes for which the logical paradox had disabled the intuitive concept of set. So he made one up. Tarski, the model theory pioneer, wanted a concept that would serve the same purposes in L from which the semantic paradox had disabled the intuitive concept of truth. He wanted to rehabilitate the logicist claim that for every true proposition of arithmetic there exists a truth-preserving relation to its unique counterpart in the *theorems of pure logic.[31] So he made up a new concept of truth, and got

30. Alfred Tarski, "The concept of truth in formalized languages", in *Logic, Semantics and Metamathematics: Papers from 1923-1938*, translated by J. H. Woodger, 2nd revised edition, with an editor's introduction and analytical index by John Corcoran, pages 152-278, Indianapolis, IN: Hacket, 1983. First published in Polish in 1933.

31. A gentle reminder. In first-order logic, the word "theorem" is a tort. The theorems of L bear no recognizable resemblance to what "theorem" means in English – a statement shown to be true by way of valid proof. "Proof" here also occurs with its ordinary meaning.

on with the logicist programme. The new concept with the old name is the one we've just finished tarrying over. The question that now presses is whether this make-up of truth can preserve the original intent of logicism. The answer is that it cannot. When it was originally proposed that every true statement of arithmetic is provable in pure logic without the need to refer to or quantify over numbers, "true" carried its intuitive mathematical meaning. I don't think that Frege and Russell were fully seized of the alienations effected by the new logic's defections from everyday mathematical speech. By the 1930s, Tarski appears to have cottoned on to the alienation from semantic reality effected by pure logic's model theory.

After 1931 Tarski will have been aware of an extraordinary technical feat pulled off by Gödel in his famous incompleteness paper.[32] Gödel's proof depends on a device of his own origination called Gödel-numbering, for arithmatizing syntax in a formal representation FA of Peano arithmetic, PA. In particular, Gödel showed that the primitive recursive functions of PA are formally representable in FA. Without that subproof, the incompleteness proof fails. The formal representability relation that mapped FA's primitive recursive functions to PA's met two essential conditions. One was that the map was isomorphic. The other was that its representations of the properties of PA's primitive recursive functions caused no telling misrepresentation of how these functions actually work in PA. The representation relation had two essential virtues. It was *tight* and *straight.*

Let's come back to our concept-clarification line, with particular reference to how the intuitive concept of truth fares in Tarski's model theory. On the face of it, and rightly, it fares very badly. But upon reflection, there is something that might be done to repair the damage. We could postulate a relation of formal

32. Kurt Gödel, "On formally undecidable propositions of Principia Mathematica and related systems I", in van Heijenoort 1967 at pages 592-616. First published, in German, in 1931.

representability mapping Tarski truth language truths, suitably adjusted to handle the havoc imposed on intuitive truth by the Liar. Call this relation R. The question that now arises is obvious: Is R both provably tight and straight? The answer is that it is not.[33]

Even so, in his 1935 paper Tarski assigned himself two tasks. One would be the reformulation of the model theory of standard first-order logic to spare its own truth predicate from the ravages of paradox. The second was to turn his sights on natural language truth-predicate which would yield to Tarski's formal representability ambitions. Thus the title of this classic paper is correct with respect to the first objective and wholly misses the mark with respect to the second.[34]

In his formalized language, Tarski handled the formalized truth predicate in the way that post-paradox set theorists handled the new concept of set. In each case infinite hierarchies were called into play. In the case of truth, sentences of the language were sorted into levels. At level one, no attributions of truth are allowed. At level two, truth-ascriptions can be made of the sentences at level one and only they. The levels pile up into the transfinite, directing truth-ascription at each level so as to keep the Liar at bay. Nowhere in the hierarchy could a sentence be found that ascribed falsity to itself. No sentence on any level would be allowed to ascribe falsity to itself. Given that a formal language is

33. More details are available in Woods, "Does changing the subject from A to B really provide an enlarged understanding of A?", *Logic Journal of the IGPL*, 24 (2016), 456-480.

34. There is little work on the model theories of formalized languages by Canadian informal logicians. A notable and artful exception is the translation of Tarski's follow-up paper of 1936 by Magda Stroika and David Hitchcock's translation of "The concept of following logically", *History and Philosophy of Logic*, 23 (2002), 155-196. Polish and German originals first published in 1936. The more common title in English is "On the concept of logical consequence", a translation of the original German title of 1936, "Über den Begriff der logischen Folgerung". The Stroika and Hitchcock translation is more faithful to the German. A Polish friend tells me that the same holds for the Polish title.

a made-up thing with a name that's not its own, there is no real shock in making room in its lexicon for an infinite number of inequivalent truth predicates. Let's call this theory Tarski's theory of *truth.

Having fixed *truth for formal languages, Tarski now turns to natural languages and their home predicates for truth. What Tarski wanted was a theory of truth in natural language that would be modelled on his theory of *truth. This could be accomplished in one or another of two ways. He could infinitely enlarge a natural language's number of truth *predicates* in the way that he'd done in his theory of *truth, or he could retain a single truth predicate and assign it infinitely many meanings in any given natural language. Either way, predications of truth could be subject to ascription constraints by predicate-rank or the particular meaning which the univocal predicate had at that level. English would be spared the chaos of paradox.

Whatever we may think of Tarski's theory of *truth, there is nothing to be said for his theory of truth, beyond that it has all the virtue of theft over honest toil, as Russell said of another thing.[35] Tarski's theory of truth in English is false on empirical grounds. It so greatly distorts the truth about truth as to make it virtually unrecognizable in Tarski's approach. Even had Tarski established a tight relation of formal representability that hooked up the theory of *truth with the theory of truth, it could not have been a straight one. That leaves the theory of truth hovering midway between the terminus of the conceptual clarification line and its rational reconstruction node. In 1944, Tarski published a somewhat more accessible account of his treatment of truth.[36] In no time at all, the formal semantics bug bit hard, and an

35. Actually the axiom of reducibility.

36. Alfred Tarski, "The semantic conception of truth", *Philosophy and Phenomenological Research*, 4 (1944), 341-375. Here, too, we have a misleading title, in which truth is a natural-language property and "semantic" means "model-theoretic". Tarski's most accessible account, and also the shortest, is "Truth and Proof", *Scientific American*, 220 (1969), 63-77.

ambitious literature in the philosophy of language flowed forth, attesting throughout to the determination of analytic philosophers to get to the bottom of truth and meaning in natural language, with methods pioneered by Tarski.

If the modern history of the philosophy of language in English-speaking communities is our guide, the habit of calling logic's model theoretic provisions for its formulas a truth conditional semantics for them[37] now spreads to English itself in a suitably adjusted retrofitting. With it comes the quite striking allied assumption that the meaning of an English sentence is uniquely determined by its truth conditions, that is, its honest-to-goodness no-sneer-quotes *truth* conditions. We can plot the rise of this surprisingly captive idea from Tarski's provisions for artificial languages to Suppes' application of them to the philosophy of science the so-called semantic theory of scientific theories and to Davidson's appropriation of them for the languages of mankind.[38]

In "Truth and meaning", Davidson writes as follows:

> "Much of what is called for [in a Tarski-style theory of truth] is to mechanize as far as possible what we now do by art when we put ordinary English into one or other [regimented] canonical notation. The point is not that canonical notation is better than the rough original idiom, but rather that if we know what the canonical notation is canonical for, we have as good a theory for the idiom as for its kept companion."[39]

37. More accurately, a T-conditional semantics.

38. Patrick Suppes, *Studies in the Methodology and Foundations of Science: Selected Papers from 1951-1969*, Dordrecht: Reidel, 1969, and *Representation and Invariance of Scientific Structures*, Stanford: CSLI Publications, 2002. See also Frederick Suppe, *The Semantic View of Theories and Scientific Realism*, Urbana and Chicago: University of Illinois Press, 1989. Donald Davidson, "Theories of meaning and learnable languages", reprinted in *Inquiries into Truth and Interpretation* at pages 3-15. Oxford: Clarendon Press, 1984, "Semantics for natural languages", reprinted in the same collection at pages 55-64.

39. Donald Davidson, "Truth and meaning", reprinted in *Inquiries into Truth and Interpretation* at pages 93-108. Emphasis in the original.

It is worth noting how closely what Davidson is saying here resembles what teachers of logic often say to disgruntled students smart enough to see that the mapping rules that take (certain classes) of natural language arguments to their logical forms in L are defective. The teacher will admit the difficulty and encourage the student to apply the rules with intuitive discretion. This, by the way, is not bad advice. It is easier for us to avoid sentences that meaning-imply others or are at odds with them also by virtue of meaning, than to produce well-made theories of these properties. Still, it's an embarrassing situation for the mapping rules. As normally stated, they are insufficient to deliver the backwards reflection property for *validity in the absence of a principled theory of making-entailment and meaning-inconsistency, neither of which can be modelled in a logic that provides for entailment and inconsistency by logical form.

Davidson's is an empirical theory. No empirical theory of any note or durability is wholly free of non-empirical elements. But some theories are a good deal more empirical than others. Some are only glancingly empirical. Mathematical physics is less empirical than theoretical physics and it, in turn, less so than population genetics. Davidson's theory of truth is empirical in roughly the way that theoretical physics is, namely, not all that much so. It is a theory embodying high-octane minglings of the empirical and the theoretically distortive. Davidson is fully aware that there is too much in natural English – indexicals for instance or action sentences – to be captured by a finitely axiomatized theory of truth in formalized languages with Tarskian biconditionals mapping chunks of English to L. Convention T is the problem. It is a fundamental constraint in Tarski's theory, providing that "Snow is white" is true just in case snow is white. But if, for example, we wanted to include sentences with indexicals for time and place, Convention T would deny them admission. It is not simply the case that "It is now cold here" is true just in case it is now cold here. Accordingly, Davidson constructs a two-step approach to natural language meaning. In much simplified terms, step one will draw from Tarski what works for a fragment of context-independent English, and step two will develop a way of mapping one-to-one some of the contextually sensitive ones that Tarski can't handle to regimented sentences of English which are thought to repair those omissions.

We won't understand Davidson unless we understand that, in canonical notation, the logical particles of L are neologisms that enter the lexicon of beefed-up English with a presumptive precedence over their counterparts in unenriched English. For example, "∀" now joins the lexical ranks of "all" and "every", but it enters with stipulative intent and provides ready occasion to summon up Burgess's warning. What "all" and "every" used to mean in unenriched English, they now mean what "∀" means in L. Similarly, the theory of truth that is good for canonical English is the theory of *truth for L. Then "⊨" enters the lexicon of the metalinguistic regions of spoken English as another neologism, displacing the native's "logically true" and, in two-place contexts, the native's "entails".[40] So there is something not quite to like in this rather dismissive passage of Davidson's. In light of the difficulties currently in view the canonical notation intervention carries nontrivial risk of a stipulationist high-jacking of precisely that ordinary idiom which Davidson assures us is no less good than the good of its canonical notation. I admit to thinking, however, that Quine's manic extensionalism seriously distorts Tarski's message, and that Quine's influence on him places Davidson himself at two removes from Tarski.

40. Some readers might think that I've taken this point too far. Why would we be so hard on "⊨"? Why couldn't it simply be a notational variant of "entails" or, as the case may be "logically true"? The reason why is that the model theoretic property denoted by "⊨" is not at all the property denoted by "entails" or "logically true".

5. HOW CRITICAL IS THE FORMALIST CRISIS?

In the early 1970s virtually any philosopher working in the informal logic sector of Canadian approaches to the theory of argument would have known about formal semantics and wouldn't have been much alarmed by it, provided it was put to uses for which it was best suited. It was also true that when these researchers talked about the suitability of formal methods and formal models for real-life argument, they need not have been thinking (and often weren't) of formal semantics in the model-theoretic sense. Even so, the prevailing mood was, and still is, more *anti* formal methods than *pro*. Of course, there was a minority who thought that formal measures could be productive in ways that took proper notice of the variabilities in what real-life argument aims for and the manner it is affected by context in the formal logics of deduction, not because of coloured-shirt problems and the problems posed by formal representability presumptions, but rather for the straightforward reason that most good argument and most good reasoning is invalid. (Thus, the missed target problem.) There are several reasons for these dissatisfactions. One, as we have seen, is that formal systems can't represent meaning connections in natural languages upon which good inferences often crucially depend. Another that we haven't mentioned yet is that formal systems tend to conflate conditions on implication with rules of inference, an equation that doesn't hold true in natural language.[41] A third reservation was the indifference of formal systems to the crucial impact of context and agency on the success or failure of real-life argument. In due course, there arose the idea that there was nothing wrong with these logics in relation to what they

41. The classical paper is Gilbert Harman, "Induction: A discussion of the relevance of the theory of knowledge to the theory of induction", in Marshall Swain, editor, *Induction, Acceptance and Rational Belief*, Dordrecht: Reidel, 1970.

were designed for,[42] and nothing intrinsically misbegotten about the idea that they can profitably elucidate their own respective subject matters.

Why not, then, consider adapting existing logics, or building new ones, with a view to capturing in suitably formalized ways the peculiarities that matter for the realities of human argument-making on the ground? Early examples were the efforts by Woods and Walton to model composition and division arguments in Tyler Burge's formal theory of aggregation,[43] and to do the same for *petitio principia* in formal systems of epistemic logic in conjunction with those of formal dialectic.[44] The notion of formal dialectic was itself an attempt to broaden the formal modellability of human argument, in the way that ancient logic dealt with contentious argument.[45] Indeed the whole sweep of the Woods-Walton Approach was one that adapted various pre-existing logical formalisms to the varying characteristics of real-life argument, especially those that give rise to fallacies in what had become to be known as fallacies in the traditional sense. In that sense, a fallacy ticks the following boxes: It is an error of reasoning; it is committed with a frequency exceeding the reasoning-error norm without regard to sex or gender distinctions, ethnicity, (adult) age, or nationality; it is an inviting and attractive error that disguises its wrongfulness; and its rate of post-diagnostic recidivism is extremely high; in other words the error

42. Notably their varied and sometimes rivalrous contributions to the foundations of mathematics.

43. "Composition and division", *Studia Logica*, 36 (1979) 381-406. Reprinted as chapter 8 in *Fallacies: Selected Papers*. Tyler Burge, "A theory of aggregates", *Noûs*, 11 (1977), 97-118.

44. "Arresting circles in formal dialogues", *Journal of Philosophical Logic*, 7 (1978), 73-90. Reprinted as chapter 10 in *Fallacies: Selected Papers.*

45. See for example, Aristotle's foundational contribution in *On Sophistical Refutations*, in Jonathan Barnes, editor, *The Complete Works of Aristotle: The Revised English Translation*, two volumes, Princeton: Princeton University Press, 1984; I, 278-314.

is incorrigible.[46] One thing that soon became apparent to post-Hamblin researchers is how different in kind the fallacies on the traditional lists tended to be. Why, for example, would we think that there is a common structural core shared by the *ad baculum* fallacy and the fallacy of hasty generalization? Whereupon was born the logical *pluralism* which underlay the Woods-Walton Approach.[47] In more recent times, there have been aggressive attempts to re-engineer approaches to real-life argument in formal systems of increasingly sophisticated mathematical complexity, which have attracted little Canadian participation and

46. Not every fallacy theorist accepted the traditional concept of fallacy. See, for example, Gerald Massey, "Are there any good arguments that bad arguments are bad?" *Philosophy in Context*, 4 (1975), 61-77; "In defense of asymmetry", *Philosophy in Context*, 6 (1975), 44-45, supplementary volume; and "The fallacy behind fallacies", *Midwest Studies in Philosophy*, 6 (1981), 489-500. See also Hintikka, "The fallacy of fallacies" 1984. Much later came John Woods' "Lightening up on the ad hominem", *Informal Logic*, 27 (2007), 101-134; "The concept of fallacy is empty: A resource-bound approach to error", in Lorenzo Magnani and Li Ping, editors, *Model Based Reasoning in Science, Technology and Medicine*, pages 69-90, Berlin and Amsterdam: Springer, 2007; and "Begging the question is not a fallacy", in Cédric Dégrement, Laurent Keiff and Helge Rükert, editors, *Dialogues, Logics and Other Strange Things: Essays in Honour of Shahid Rahman*, pages 149-178, London: College Publications, 2008 (with Dov Gabbay). In *Errors of Reasoning*, Woods generalizes these findings, arguing that the traditional list of fallacies fails to instantiate the traditional conception of them. In the interest of historical accuracy, I should point out that some of these dissenters dissent from different doctrines. Massey dissents from the idea that a fallacy is an argument or inference that disguises its invalidity. Hintikka rejects the view that fallacies are errors of inference. Woods accepts the traditional conception of fallacy and rejects the traditional list.

47. The Amsterdam School's van Eemeren and Grootendorst are leading critics of W & W's pluralism in fallacy theory. Writing in 1992, they say: "The systematic exploration of advanced logical systems in order to analyse fallacies is characteristic of Woods and Walton's approach, [according to which] every fallacy needs, so to speak, its own logic. For practical purposes this approach is not very realistic... One only gets fragmentary descriptions of the various fallacies... Ideally one unified theory that is capable of dealing with all the different phenomena is to be preferred." (Frans H. van Eemeren and Rob Grootendorst, *Argumentation, Communication and Fallacies: A Pragma-Dialectical Perspective*, Hillsdale, NJ: Erlbaum, 1992; p. 103.)

only slight and equivocal attention.[48] I have it on good authority that Woods is drawn to the construction of heavy equipment technologies by the fun of making them up. When well-wrought, he sees them as works of intellectual high art. Woods harbours for the BGW attack-and-defend networks no conscientious aspirations for the conceptual clarification of the concept of adversarial argument in real life. He doesn't, however, slight the as-yet unfound good that sometimes lies in formal models that distort their original targets beyond recognition, when they lead to a better understanding of things not-yet heard of. Recall here Bohr's and Planck's utter distortion of the Newtonian concept of light in a way that helped turn physics in a direction that would greatly enlarge our understanding of the natural world, as if by chance. Not by chance, Woods thinks, but by Bohr's and Planck's amazing nose for powerful new ideas.

Although the Woods-Walton Approach is still recognized as something of foundational significance, it had actually run its course by the mid-1980s after a scant decade or slightly more of dominant play, especially in fallacy theory. In looking back now, I think that it can be said with some assurance that the good that Woods and Walton saw in modelling real-life argument and inference formally arose from the efficiencies of *simplified exemplication* and, even more so of *finite expressibility*. It is a lesson easily learned from a first course on the sentential calculus that, while there are infinitely many wffs in its formal language L, they are finitely expressible or representable as follows:

48. Howard Barringer, Dov M. Gabbay and John Woods, "Temporal dynamics of support and attack networks: From argumentation to zoology", in Dieter Hutler and Werner Stephan, editors, *Mechanizing Mathematical Reasoning*, Berlin: Springer-Verlag, 2005; "Network modalities", in G. Gross and K. U. Schulz, editors, *Linguistics, Computer Science and Language Processing*, London: College Publications, 2008; and "Modal argumentation networks", *Argumentation and Computation*, 2-3 (2012), 203-227. Also notable is the turning of some argumentation theorists to AI. See here Douglas Walton, *Witness Testimony Evidence: Argumentation, Artificial Intelligence and Law*, New York: Cambridge University Press, 2008.

1. p_1 is an atomic wff.
2. If p_n is an atomic wff, so is p_n+1.
3. Nothing else is an atomic wff.
4. If ϕ is an atomic wff, it is a wff.
5. If ϕ is a wff, so is $\ulcorner{\sim}\phi\urcorner$.
6. If ϕ and ψ are wffs, so are
   $\ulcorner\phi \wedge \psi\urcorner$
   $\ulcorner\phi \vee \psi\urcorner$
   $\ulcorner\phi \supset \psi\urcorner$
   $\ulcorner\phi \equiv \psi\urcorner$
7. Nothing else is a wff.

Another thing we can say with even greater assurance is that in the early 1970s Woods and Walton certainly had *not* intended to say their piece about fallacies in the manner in which Tarski had tried (and failed) to say his piece about truth in natural language.

In reaction to Charles Hamblin's challenge to restore fallacy theory to its proper home in logical theory, Canadian contributions to the logics of argument, have been numerous, varied, and in a number of respects highly influential, as witness the work of Walton and his colleagues on *argumentation schemes*.[49] Walton's emphasis on argumentation schemes for elucidating the striking type-complexity of human argument has considerably shaped the study of argument internationally. It also reflects a difference of opinion about what makes a system formal. For most of its long history, logic had been formal in Aristotle's sense, in which real arguments would be represented by sequences of natural language sentences whose general terms have been replaced by *schematic letters*. From Frege onwards, formalization would be

49. Walton, Christopher Reed and Fabrizio Macagno, *Argumentation Schemes*, New York: Cambridge University Press, 2008; and Walton, *Methods of Argumentation*, New York: Cambridge University Press, 2013.

provided by semantically barren artificial "languages" in which quantification serves to bind *free variables*.[50] There is a world of difference between a schematic letter and a variable. Variables are bindable by quantifiers. Schematic letters are not. Consider the schema "All A are B" together with its proposed counterpart in L, "$\forall x, A(x) \supset B(x)$)". The latter is a fully expressed formal sentence of L or in a suitably regimental canonical notation. The former is not itself a sentence of English. It is a schematic rendering of numberlessly many sentences got by uniformly substituting general terms of English for the schematic letters "A" and "B". The expression "For all A, B, (All A are B)" is in several respects not well-formed in English or L. In looking back, one might think that the early days Canadians with an eye on formal modelling favoured the formalization via variables approach, but more recently have returned to the fold of argumentation schemes. This, I think, is a misconception. Here is why.

In the years closely following Hamblin, perhaps Canada's most internationally recognized contribution to the theory of argument lay in fallacy theory. If it were distinctive of the Woods-Walton Approach to call into service pre-existing logical formalisms or readily adaptable ones, this wouldn't be the case for the others. One thing is clear in retrospect. Whatever Woods and Walton thought they were doing in the 1970s and early eighties, it was *not* what Woods decidedly did try to do in 1974 with his *Logic of Fiction: A Philosophical Sounding of Deviant Logic*.[51] In that book, Woods wanted a systematic theory of reference, truth and inference for literary discourse, using a formal semantics defined over a formalized language for modal logic, adapted to the needs of a fictionality operator. This was not what he and Walton were up to in their fallacies work. What they were doing

50. Gottlob Frege, *Begriffsschrift*, Halle: Louis-Nebert, 1879. Also in van Heijenoort 1967.

51. The Hague and Paris, Mouton. Second edition, with a Foreword by Nicholas Griffin, volume 23 of Studies in Logic, London: College Publications, 2009.

together falls a long way short of a formalist crisis.[52] In the first place, they were using pre-existing theories as *examples* of how points of interest to fallacy theorists might be worked up. For example, W & W modelled their approach to the *petitio principii* in the way that certain game-theoretical dialogue logicians handled attack-and-defend arguments. Moreover, in all cases in which logical symbolism was employed, the intention was simplification, and the means of attaining it was schematic. Even in those cases in which W & W borrowed from pre-existing theories that had been formalized to a degree that would support a formal semantics, they would not be a material feature of their borrowings. From which we may safely conclude that, for all the occasional anxieties of their critics, the W & W Approach was never at risk for the formalist crisis. It came nowhere close to having missed the target problem and it ran no risk of making its target concepts unrecognizable by virtue of their formal misrepresentations. Mind you, that is far from a wholesale absolution for the errors and shortcomings that remain.

6. WHITHER?

The Canadian brand was never as well-defined and organizationally and doctrinally sustained as the Amsterdam brand. Brands, as we know, come and go, and these two have flourished for decades now. It remains to be seen how well they hold up in the years and decades ahead. Judged from where we are now on the Canadian scene, there are clear signs of where the country's research efforts are likely to be directed. One of them is logical structure of argument and reasoning in legal contexts.[53]

52. I now think that what Woods was doing with fiction in 1974 was the real formalist crisis. For more, see his *Truth in Fiction: Rethinking its Logic*, forthcoming in the *Synthese* Library.

53. In addition to Walton's contributions already noted, see Woods, *Is Legal Reasoning Irrational? An Introduction to the Epistemology of Law*, Volume 2 of Law and Society, London: College Publications, 2015.

Another signals a renewed alliance with cognitive, experimental and social psychology, neurobiology and the other empirical branches of cognitive science. In one of its streams, we see an effort to do for logic what Quine and others have done for epistemology, namely to give it the naturalized form which has been intermittently in play *in logic* since Bacon, Mill, Husserl, Dewey, and later Toulmin, notwithstanding the intense efforts of Frege and others to make all of logic dance to the tune of mathematics.[54] Also of note are the already mentioned efforts to build alliances with computer science and AI, in a way perhaps of exposing how the mathematics of software engineering might leaven the insights of those whose purpose is the elucidation of human argument on the ground. Also of growing importance is the exposure of human argument-making to the plethora of work already under the belt of theories of defeasible, default and nonmonotonic consequence. Whether any of this outreach will lead to new Canadian brands remains to be seen. Ray Reiter's paper on the logic of default reasoning, was published when he was a member of UBC's mathematics department prior to his departure for the University of Toronto.[55] Although a foundational contribution by a Canadian, no one thinks of default logics as carrying a Canadian brand.[56] In the theory of argument

54. For recent Canadian work in this vein, see Woods, *Errors of Reasoning: Naturalizing the Logic of Inference*, 2013/2014. For important work from OSSA honorary Windsorites, see Maurice Finocchiaro, *Arguments About Arguments: Systematic, Critical and Historical Essays in Logical Theory*, New York: Cambridge University Press, 2005; James B. Freeman, *Acceptable Premises*, New York: Cambridge University Press, 2005; Finocchiaro, *Meta-argumentation: An Approach to Logic and Argumentation Theory*, volume 42 of Studies in Logic, London: College Publications, 2013; and Fabio Paglieri, editor, *The Psychology of Argument: Cognitive Approaches to Argumentation and Persuasion*, volume 59 of Studies in Logic, London: College Publications, 2016.

55. Raymond Reiter, "A logic for default reasoning" *Artificial Intelligence*, 12 (1980), 81-132.

56. See here J. Anthony Blair and Ralph H. Johnson, editors, *Conductive Argument: An Overlooked Type of Defeasible Reasoning*, volume 33 of Studies in Logic, London: College Publications, 2011. Although the editors are Canadian, the chief promoter of the conductive cause, Carl Wellman, is not.

the Canadian brand is, like all brands, a fleeting thing. I foresee no successor to that Canadian throne holding sway for the next forty-seven years.

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