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Accepting a Logic, Accepting a Theory

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Abstract: This chapter responds to Saul Kripke's critique of the idea of adopting an alternative logic. It defends an anti-exceptionalist view of logic, on which coming to accept a new logic is a special case of coming to accept a new scientific theory. The approach is illustrated in detail by debates on quantified modal logic. A distinction between folk logic and scientific logic is modelled on the distinction between folk physics and scientific physics. The importance of not confusing logic with metalogic in applying this distinction is emphasized. Defeasible inferential dispositions are shown to play a major role in theory acceptance in logic and mathematics as well as in natural and social science. Like beliefs, such dispositions are malleable in response to evidence, though not simply at will. Consideration is given to the Quinean objection that accepting an alternative logic involves changing the subject rather than denying the doctrine. The objection is shown to depend on neglect of the social dimension of meaning determination, akin to the descriptivism about proper names and natural kind terms criticized by Kripke and Putnam. Normal standards of interpretation indicate that disputes between classical and non-classical logicians are genuine disagreements.

Keywords: Modal logic, intuitionistic logic, alternative logics, Kripke, Quine, Dummett, Putnam

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1. Introduction

I first encountered Saul Kripke in my first term as an undergraduate at Oxford University, studying mathematics and philosophy, when he gave the 1973 John Locke Lectures (later published as Kripke 2013). A huge audience listened intently, including most of the brightest Oxford philosophers of the time. Kripke was a charismatic figure; his head looked as though it belonged on an Assyrian monumental bull, and he gave a sense of easy intellectual power in reserve. I was struck by his combination of crystal clarity, informal rigour, apt examples, good sense, and pointed humour. He became the closest I had to a model of how to do philosophy. I used to pore over a little book of readings on reference and modality (Linsky 1971); the chapter to which I most often returned was a reprint of 'Semantical Considerations on Modal Logic' (Kripke 1963). I raced through 'Naming and Necessity' at a sitting (it was already available as Kripke 1972). Naturally, in my own book on modal logic, Kripke's work occupies a central role; he figures on sixty of its pages (Williamson 2013).

This chapter comes at logic, including modal logic, from a different angle, prompted by a different aspect of Kripke's thinking. In 'The Question of Logic' (Kripke 2020, derived from a lecture originally given in 1974, though I heard about it only much later), he argues that the common philosophical talk of 'adopting an alternative logic' to solve a problem is fundamentally confused, because without already having logic one cannot derive the consequences of a specification of the 'alternative system'.

By contrast, in the Oxford of my philosophical upbringing, the dominant philosophical logician was Michael Dummett, who supervised me for the final year of my doctoral studies (1979-80). Dummett presented intuitionistic logic, with its rejection of the law of excluded middle, as an alternative logic which one arguably *should* adopt, on grounds in the philosophy of language, and thereby reject classical logic (Dummett 1975). Under his influence, some of my Oxford contemporaries *had* to all appearances taken the plunge and adopted intuitionistic logic. They certainly thought they had. Had they done the very thing which, for Kripke, makes no sense? I was never at all tempted to join them; with my firmly realist sympathies, I found the philosophical case for intuitionistic logic thoroughly unconvincing. Nevertheless, I did not doubt my contemporaries when they claimed to have adopted intuitionistic logic.

'The Question of Logic' focusses primarily on a different case: Hilary Putnam's case for adopting 'quantum logic' to resolve problems explaining experimental results in quantum mechanics (Putnam 1968; he retracted the view for other reasons in Putnam 2005). However, Kripke briefly discusses intuitionistic logic in the paper, rejecting claims that intuitionists have adopted it as an alternative to classical logic.

I strongly agree with Kripke that the cases offered for adopting one or other formal system as an alternative logic are often very superficial, in ways closely related to his critique of adoptionism. In particular, their advocates often use classical logic in their metalogical reasoning, for which they provide little or no justification. It can easily make a difference. For example, many theorists have turned to various forms of three-valued or continuum-valued semantics to handle sorites paradoxes for vague languages. Such accounts are typically taken to imply that the law of excluded middle is invalid in the object-language. However, by using excluded middle in the metalanguage, one can prove valid some formulas of the object-language which cannot be proved valid without it. Moreover, the phenomenon of higher-order vagueness strongly suggests that many-valued logicians' vagueness-related reasons for resisting excluded middle in the object-language generalize naturally to excluded middle in the metalanguage. In these circumstances, many-valued logicians' tendency to use classical logic, with excluded middle, in the metalanguage looks problematic. Indeed, if by their own lights they cannot rely on classical logic in the metalanguage, it is quite unclear what logical principles they *are* entitled to rely on in the metalanguage for purposes of calculating which logical principles are valid in the object-language. They should rely only on those valid for a vague language, but which those are is just what they are trying to work out (Williamson 1994: 111-13, 127-30). That is reminiscent of the problems Kripke identifies for 'adopting a logic'.

Many critics of classical logic deny that their criticisms extend to its applications to mathematics: since the language of pure mathematics is precise and unparadoxical, no revision of standard mathematics is required. But that lazy view ignores applications of pure mathematics to the concrete world, as in physics, which require instantiating variables in the mathematical theorems by terms from outside the language of pure mathematics. Their original criticisms, if sound, apply to the extended language. Thus they are in no position to shirk the hard task of reconstructing as much mathematics as they can on the basis of their proposed non-classical logic (Williamson 2018).

A similar dialectic plays out if the critics of classical logic deny that their criticisms extend to its applications to their model-theoretic semantics, usually formulated in the language of mathematics, specifically set theory, extended by some vocabulary for syntax: since such a language is precise and unparadoxical, no revision of their model theory is required. But that lazy view ignores their eventual applications of the model theory to *intended* models for the language, which require instantiating variables in the model theory by terms in an extended language capable of specifying the possibly vague or paradoxical intended readings. Their original criticisms of classical logic, if sound, apply to such a language. Thus they are in no position to shirk the hard task of reconstructing as much of their model theory as they can on the basis of their proposed non-classical logic.

However, that objection is unfair to *some* proponents of alternative logics, who *do* restrict themselves to their officially endorsed principles of non-classical logic in the metalanguage too. In particular, there is a well-developed tradition of intuitionistic metalogic for intuitionistic logic, with results quite different from those of classical metalogic for intuitionistic logic (see McCarty 2008, for example). Intuitionists start from proof theory, not from model theory. They already know which logical principles they are initially willing to use, and they start reasoning on that basis in the metalanguage as well as the object-language. They seem to have succeeded in adopting intuitionistic logic, internalizing their alternative in a way proponents of many-valued logic may have failed to do.

2. Accepting an interpreted modal logic

How do these issues impact on modal logic? In principle, one can try to develop a modal model theory for modal logic, using a metalanguage with modal operators semantically like those in the modal object-language to characterize classes of models for the object-language—just as the standard model theory for first-order non-modal logic uses a metalanguage with first-order quantifiers semantically like those in the object-language to characterize classes of models for the object-language. In practice, however, that more homophonic approach has made little progress; technically, it is at best desperately cumbersome (Humberstone 1996 is a brave attempt).

By contrast, the 'possible worlds' approach pioneered by Kripke (1963) has been immensely fruitful, mathematically, philosophically, and in applications to computer science and theoretical economics. The metalanguage is the language of set theory, extended by some vocabulary for syntax. It is *non-modal*: it has no modal operators. In that language, one formally defines in set-theoretic terms what a *Kripke model* is. Each such model comes equipped with its own set of what are informally conceived as (possible) worlds. Formally, one defines the truth-value of a formula of the modal object-language at a world in a Kripke model, validity over a class of Kripke models, and so on. By purely non-modal reasoning, one can study which modal formulas are valid over a given class of Kripke models. The standard metalogic of modal logic concerns such mathematical questions, which are independent of any intended interpretation of the modal operators.

Intending the modal operators in a formal language to be interpreted in terms of a given informally specified modality, one may conjecture that the formulas of the language always true on that interpretation are exactly the formulas true in every member of a given mathematically specified class of Kripke models (for more details where metaphysical modality is intended see Williamson 2013: 92-118, 130-47). Relying on that conjecture, one may guide one's reasoning about the intended modality by using non-modal reasoning to calculate which formulas are true in every model in the specified class. But the conjecture needs to be tested. In doing so, one tests the choice of models against informal modal judgments at least as much as one tests informal modal judgments against the choice of models. The chosen class may just be used to *systematize* some initial modal judgments, and to work out which other modal judgments follow from, or are consistent with, that systematization. But those services may also serve a more critical purpose, by making some of the initial modal judgments look anomalous, or revealing them to be in tension with each other.

Interpreted modal logics are in effect austere structural *theories* about the intended modality, such as metaphysical or practical possibility. Arguably, the appropriate methodology for deciding between rival theories of such a subject matter is *abductive*, like that for deciding between rival theories in other sciences (Williamson 2013: 423-9). The initial evidence comes from our pre-theoretic knowledge of the intended modality in particular cases. For example, we have pre-theoretic knowledge of practical possibility, often expressed by 'can'. We know that some people can run a mile in under four minutes; of course, they can also run a mile in over four minutes. We know that you do only what you can. We know that if you can run and cannot fly, you can run without flying. And so on. A modal logic proposed for a given modality is expected to systematize such already known truths. It must be consistent with them; the more it unifies them by bringing them under strong and simple generalizations, the better.

As in other sciences, answering theoretical questions may involve various kinds of indirect strategy. For instance, a central dispute in modal metaphysics is between *necessitism* and *contingentism*. Necessitism is expressed by the formula $\Box\forall x\Box\exists y\ x = y$, where $\Box$ expresses metaphysical necessity and the quantifiers range over absolutely everything; it says that necessarily everything is necessarily something. Contingentism is just the negation of necessitism. They are rival answers to the question 'Is there contingency in which individuals there are?' Each of them is a broadly logical claim: it is expressed without non-logical constants (counting $\Box$ as a broadly logical constant). Under reasonable assumptions, the validity of necessitism is equivalent to the validity of reasoning by interchanging $\Diamond\exists v$ and $\exists v\Diamond$, or the Barcan formula and its converse (see Barcan 1946 and Kripke 1963; such interchanges of $\Diamond$ and $\exists$ were in effect already endorsed Ibn-Sina = Avicenna, see Williamson 2013: 45). Necessitism corresponds to the class of Kripke models for quantified modal logic in which the domain of individuals is constant across worlds; contingentism corresponds to the broader class of models with no such restriction (Kripke 1963). Arguably, our pre-theoretical knowledge is insufficient to settle the issue between necessitism and contingentism. A more systematic, theoretical case is needed for one side or the other; it may take hundreds of pages to build. One's views may shift as new considerations come into view. I first encountered the issue by reading Kripke (1963). At first, I inclined to contingentism because I found the apparent common sense counterexamples to necessitism metaphysically compelling. Later reflection persuaded me that necessitism can do justice to such examples, and I came to prefer it on abductive grounds: necessitism beats contingentism in its combination of simplicity and strength. (Williamson 2013). That certainly *felt* like adopting an interpreted modal logic. But perhaps I just accepted an interpreted modal system, without adopting it.

We can see accepting an interpreted modal system as intermediate between accepting a theory in natural science and accepting a mathematical theory such as Zermelo-Fraenkel set theory with the Axiom of Choice. In interpreted modal logic, like mathematics and unlike natural science, evidence from experiment, observation, and measurement normally plays no special role in accepting a theory. On the other hand, in interpreted modal logic, like natural science and unlike mathematics, disputes between rival theories are both usual and central to the development of the field. Obviously, as those contrasts underline, the methodology of theory acceptance is far from uniform across the sciences, including both the natural sciences and mathematics. Nevertheless, interpreted modal logic does not look like an *outlier* with respect to theory acceptance.

Of course, modal logic might be an atypical branch of logic. Why might 'core' first-order non-modal logic be different?

One reason is that first-order non-modal logic seems to be needed as part of the background logic for just about any theory. Without it, how can we extract consequences from the theory's postulates? Although its apparent ubiquity does not by itself prove that one cannot change one's system of first-order non-modal logic, it might make accepting such a system somehow special. Section 4 bears on that issue.

Another reason why modal logic might be atypical is that accepting an interpreted modal system is usually a matter of assessing various potential *axioms*, not rules of inference. The background non-modal logic is normally axiomatized with at least one rule of inference (such as modus ponens), but the latter is rarely at stake in the choice of modal system: the modal principles are the issue. Although most modal systems are standardly axiomatized with the rule of necessitation (if *A* is a theorem, so is □*A*), Kripke has shown how it can be eliminated for normal modal systems, by replacing each axiom *A* with the infinite family of axioms *A*, □*A*, □□*A*, … (Kripke 1963). By contrast, rules of inference are often central to the choice of a first-order non-modal system. The focus of Kripke's account of the adoption problem is on rules of inference such as universal instantiation and modus ponens, which one must apply to extract the required consequences from the formal specification of the system to be adopted (Kripke 2020). Psychologically, a rule of inference corresponds not to a belief or even an infinite family of beliefs but to a pattern of *transitions* between beliefs (Carroll 1895, which Kripke 2020 cites; see Besson 2019 for some relevant discussion). The difference between believing and making transitions between beliefs might have repercussions for the comparison between accepting a system of logic and accepting a theory in other sciences.

Sections 3-5 will argue that theory acceptance in logic is *not* exceptional, compared to theory acceptance elsewhere. Before that, some preliminary clarifications will be useful.

First, in speaking of 'theories' in logic, we will always mean *interpreted* logical theories, where each logical constant (including any modal operator) has an intended interpretation. Uninterpreted formal systems can be studied mathematically, but they are not normally candidates for acceptance or rejection.

Second, we must be careful not to confuse *logical* theories with *metalogical* theories. Even if we count all metalogical theories as logical (in a broad sense), we must not count all logical theories as metalogical. For instance, one theorem of an orthodox logical theory of identity is $\forall x\, x = x$, which just says that everything is identical with itself (for definiteness, we may read the quantifier as unrestricted). That is *not* a metalogical statement; it says no more *about* logic, formulas, proofs, or models than it says about cabbages and kings (by implication, that they are self-identical). In particular, it must not be confused with a metalogical statement such as $\vdash \forall x\, x = x$ (which says that the formula $\forall x\, x = x$ has a proof of the relevant formal kind), or $\vDash \forall x\, x = x$ (which says that the formula $\forall x\, x = x$ is true in every model of the relevant formal kind), or the less formal '$\forall x\, x = x$ is a logical truth'. Although $\forall x\, x = x$ *is* a logical truth, it does not say of itself that it is such; its meaning is compositionally derived from the meanings of its constituents; neither the universal quantifier, nor the identity sign, nor the variable 'x' imports anything distinctively metalogical into the meaning of formulas in which they occur. Correspondingly, to *think* 'Everything is identical with itself' is not to think 'It is a logical truth that everything is identical with itself', otherwise an infinite regress would threaten: to think 'It is a logical truth that everything is identical with itself' would be to think 'It is a metalogical truth that it is a logical truth that everything is identical with itself', and so on.

Similarly, although perhaps a little less obviously, to think *C* having inferred it from *A* is not to think '*C* is a logical consequence of *A'* (even if *C* is in fact a logical consequence of *A*). In general, a theory can enable one to mount good arguments without being a theory *about* which arguments are good. For example, a simple physical theory may enable one to infer 'This will not float in water' from 'This is a rock' (with the reference of 'this' held fixed). That does not show that the physical theory is about such argumentative connections; it may be just about things like water, rocks, and floating.

In what follows, logical theories and inferences based on them are assumed non-metalogical unless otherwise specified. Such logical theories are *not* theories of validity, which are metalogical (contrast Hjortland 2017). Rather they are theories of what the logical constants refer to: negation, conjunction, disjunction, conditionality, universality, existence, identity, various kinds of possibility and necessity, and so on (a more accurate but cumbersome statement would be in higher-order terms, dropping the nominalizations in favour of the logical constants themselves).

3. Anti-exceptionalism about logic

The vague hypothesis inspiring this paper is anti-exceptionalism about logic: amongst the sciences, logic is no outlier; it differs from the rest only as much as they differ from each other.

The classic statement of a version of anti-exceptionalism about logic is of course Quine's, in 'Two Dogmas of Empiricism' (Quine 1951), to which the paper (Kripke 2020) responds, turning against Quine a style of argument which Quine had deployed against Carnap's conventionalism about logic (Quine 1936). The present version of anti-exceptionalism differs from Quine's. In particular, his 1951 article contains a strong residue of empiricism; his anti-exceptionalism about logic is articulated in the final section, entitled 'Empiricism without the Dogmas'. Sensory experience is still described as the boundary condition for total science. That experience has any such unique privilege is not a truism, unless 'experience' is just another word for *learning*. On an alternative view, the evidence we have to go on, in both science and daily life, is simply whatever we happen to know, however we happen to know it (Williamson 2000: 184-208). The present version of anti-exceptionalism is not a form of empiricism about logic, since it ascribes no special privilege to experience, even if in practice most human knowledge is inseparable from sensory experience. However, distancing this version of anti-exceptionalism about logic from empiricism does not by itself immunize it against Kripke's critique, which engages with a different aspect of Quine's version.

The aim in what follows is to sketch a picture of theory acceptance on which we can and do accept theories in our logical reasoning, just as we can and do accept theories in our physical reasoning. I say 'accept' rather than 'adopt' to let the latter be reserved for something some advocates of 'alternative logics' have thought they could do in logic which Kripke has shown they cannot do. My anti-exceptionalism requires only that we can and do *accept* theories in logic; it does not require us to do whatever else it takes to *adopt* them.

In developing the analogy between logic and physics, it is useful to recall the standard distinction between *scientific physics* and *folk physics* (also known as 'naïve physics' and 'intuitive physics'; see for example Smith and Casati 1994, Baillargeon 2004, Spelke and Kinzler 2007, Gelman and Noles 2011, and Kubricht, Holyoak, and Lu 2017). Folk physics comprises the instinctive, pre-reflective forms of physical reasoning on which humans rely, independently of their education. It is more or less innate and universal to the species. For example, we typically use folk physics in automatically forming expectations about the movement of inanimate objects in our environment. By contrast, scientific physics comprises non-instinctive, reflective forms of physical reasoning which human may learn, depending on their education. It is neither innate nor universal to the species. The distinction is of course not perfectly precise, and it is complicated by various factors: for example, learnt scientific forms of physical reasoning can gradually become habitual. Through reflection, scientific physics can sometimes override and correct folk physics. The process of accepting a new theory in physics pertains to scientific rather than folk physics, though once the new theory is accepted, thinking in accordance with it may of course become habitual.

We can draw an analogous distinction between *scientific logic* and *folk logic* (naïve logic, intuitive logic). Folk logic comprises the instinctive, pre-reflective forms of logical reasoning on which humans rely, independently of their education. It is more or less innate and universal to the species. For example, we typically use folk logic in automatically making very simple deductions from what we know. By contrast, scientific logic comprises non-instinctive, reflective forms of logical reasoning which human may learn, depending on their education. It is neither innate nor universal to the species. The distinction is of course not perfectly precise, and it is complicated by various factors: for example, learnt scientific forms of logical reasoning can gradually become habitual. Through reflection, scientific logic can sometimes override and correct folk logic. The process of accepting a new theory in logic pertains to scientific rather than folk logic, though once the new theory is accepted, thinking in accordance with it may of course become habitual.

An example where scientific logic may have overridden folk logic concerns the form of inference from ‘Every F is G’ to ‘Some F is G’, standardly rejected as invalid by both modern logic and modern semantic theories of natural language. Kripke (2020) interprets the episode as one where Aristotelian proto-scientific logical theory erred, by neglecting the case of an empty subject term, and was corrected by intuitive reflection. However, that interpretation is hard to square with the fact that the consensus was achieved only in modern logic. Medieval logicians quite capable of intuitive reflection sided with Aristotle, not by neglecting the empty case but by denying the truth of the universal generalization in that case. Nor is the problem resolved by the claim that we *presuppose* the non-emptiness of the subject term, for pre-theoretically both the inference from ‘There is no F’ to ‘There is no F which is not G’ and the inference from there to ‘Every F is G’ seem good, while the original inference still seems good too. More likely, folk logic relies on quick and easy *heuristics*, efficient but less than perfectly reliable forms of reasoning. Their status as heuristics need not be transparent to the naïve user; when they lead one into inconsistency, one may have no pre-theoretic way of locating the source of the error. Similar problems arise for conditionals. Just as folk logic may erroneously regard ‘Every F is a G’ and ‘No F is a G’ as contraries, so it may erroneously regard ‘If A, C’ and ‘If A, not C’ as contraries too. A more abstract, systematic, and theoretical approach may be needed to resolve the issue (see Williamson 2020 for further development of these themes). One of Aristotle’s skills seems to have been in articulating good approximations to folk theories, in logic as in physics.

By ‘accepting’ a theory (in physics, logic, or anywhere else) I mean more than just accepting it for the sake of argument or some other limited purpose. In particular, I mean more than just accepting the theory as ‘empirically adequate’, which is in any case a rather obscure status, given the deep unclarity of the distinction between 'empirical' and 'non-empirical' knowledge (compare van Fraassen 1980 and Boghossian and Williamson 2020). I mean something like believing it, relying on it, being disposed to apply it and act on the results, much more generally. That is all-out acceptance. Of course, one can also take various attitudes to a theory of qualified or partial acceptance, to which the arguments below will generalize in suitable ways.

Why not simply speak of 'believing' a theory, rather than 'accepting' it? Recall the distinction between axioms and rules of inference. It is fine to speak of 'accepting' an axiom and of 'believing' the axiom. It is also fine to speak of 'accepting' a rule of inference, but it is *not* fine to speak of 'believing' a rule of inference; it does not seem to be the right kind of thing to be believed. Of course, one can believe a rule of inference *to be valid*, but that is a metalogical belief. By contrast, accepting a rule of inference—like accepting an axiom—is not metalogical. For example, one accepts the rule of modus ponens by relying on it in one's reasoning; in doing so, one thinks the premises and conclusion, but normally one does not think about validity: one might think about it if one started having reflective *doubts* about modus ponens. Thus 'accept' is a better word than 'believe' for the non-metalogical relation one has to both axioms and rules of inference.

Invoking the distinction between 'axioms' and 'rules of inference' may suggest that we are talking about logic, but it makes sense for theories in physics too. Of course, the axioms are not required to be self-evident; they may simply be *postulates*, not derived from anything else. The category of rules of inference seems especially apt for folk physics. For example, if you want a device which takes as input the presently seen position, velocity, and direction of an object and rapidly delivers as output its predicted position, velocity, and direction one second into the future, a rule of inference is exactly what you need. Obviously, the agent is not aware of any formula by which the prediction is made; the agent does not need the ability to articulate the rule.

Naturally, scientific physics is more articulate than folk physics. Even so, Thomas Kuhn's influential account in *The Structure of Scientific Revolutions* of what it is to accept a scientific paradigm suggests that much of it involves recognitional capacities for types of problem and ways of solving them which the scientists may be unable to articulate properly in words (Kuhn 1962). The category of (loose) rules of inference may be needed there too.

Needless to say, this appeal to Kuhnian descriptions involves no commitment to the gratuitously relativist and constructivist remarks with which he tended to betray his lack of philosophical training. Nor is it to endorse the vulgar misreading of his work as showing (or aiming to show) that science is as irrational as everything else; rather, he offered a more sophisticated account of how individual irrationality can contribute to a collectively rational process—for example, when the obstinate loyalty of scientists to a paradigm in which they are invested, despite all the anomalies they cannot explain, helps ensure that its intellectual resources are thoroughly explored.

On this account of theory acceptance, it is seriously misleading to picture it as a matter of writing a sentence or list of sentences into one's 'belief box'. Still, it is helpful to keep in mind the connection with belief. In particular, the metaphor of adoption suggests a *voluntary* process (as does the metaphor of writing in one's belief box), whereas belief is notoriously involuntary. One cannot just switch on the belief that one is ten metres tall, or switch off the belief that one is much less than ten metres tall, however much money one is offered to do so. Indirectly, one can sometimes manipulate oneself into or out of a belief, but that is not the same thing. Since acceptance is a generalization of belief, the involuntariness of belief is likely to generalize to the involuntariness of acceptance. The evolutionary pressures towards involuntariness are the same in the two cases: we act on what we believe or accept, so it is dangerous to leave such matters to individual caprice. In more epistemological terms, both beliefs and acceptances are normally products of processes whose function is to produce knowledge, whereas voluntary beliefs and acceptances would normally not be knowledge. Thus, if adoption is voluntary, the idea of adopting a logical theory will be suspect.

Admittedly, it is not clear that belief is *always* involuntary. For example, imagine a member of the jury in a murder trial. From early in the trial, there is strong evidence that the accused is guilty, enough by ordinary standards to justify a bystander in believing in his guilt, but perhaps not quite enough by legal standards to secure conviction. The juror is strongly inclined to believe him guilty. However, she has a keen sense of responsibility, and wants to hear the remaining evidence and arguments with an open mind. She may therefore decide to hold off believing that he is guilty, deliberately suspending judgment until she has heard both sides of the case in full. Alternatively, she may decide that doing so would be over-scrupulous, and just go ahead anyway and believe him guilty. In such a case, she may genuinely be deciding whether to believe that he is guilty. There is nothing obviously psychologically impossible about the case (see Ginet 2001, van Fraassen 2002, Weatherson 2008, Rott 2017, and Williamson 202X for further discussion and references).

One can construct analogues of the juror case for believing a theoretical proposition or accepting a theory, in physics or logic, where the evidence is strong but perhaps not quite strong enough. There may be just enough wiggle room for voluntariness. For *partial* acceptance, for a limited purpose, the scope for voluntariness may be quite large.

However, even if such cases are possible for all-out acceptance, they are somewhat marginal. In most cases, whether one fully believes a proposition or accepts a theory is not a matter of will—which is not to deny that it is a matter of rationality. Thus, if adopting a theory in logic is supposed to be a matter of will, it faces an obvious problem. Likewise, if adopting a theory in *physics* is supposed to be a matter of will, it faces the same obvious problem. There is no problem for anti-exceptionalism about logic here. We will finesse this issue by not assuming that theory acceptance is a matter of will.

With these clarifications in place, the next section aims to get a little more realistic about theories and what it is to accept them, in order to identify underlying similarities between theories in logic and theories elsewhere, which the disciplinary stereotypes may conceal.

4. Theories and dispositions

The talk in previous sections of ‘axioms’ and ‘rules of inference’ suggests something much more precise and perspicuously organized than most extant theories aspire to be. At first, this point may seem to tell *against* anti-exceptionalism about logic. However, properly understood, it enables us to see deeper similarities between accepting a theory in logic and accepting a theory elsewhere.

Most theories have never been axiomatized, and may not be susceptible to axiomatization in any useful way. Many theories hardly fit the standard metalogical definition of a ‘theory’ as a set of sentences closed under logical consequence. Consider Darwin’s theory of evolution. Although attempts have been made to axiomatize it, they have no special status in biology; probably most biologists have never heard of them. When one asks what the theory says, one may be told something like ‘Species evolve by natural selection’. That sounds more like a *generic* generalization than a universal one: its truth does not entail the truth of every instance (‘Ducks lay eggs’ is a standard example). It would not be refuted by a scientist artificially creating a species and then destroying it before it had the chance to evolve. As a generic generalization, it does not deductively entail very much. Yet, by reasonable standards, it is an extremely powerful explanatory theory. Where does this power comes from? A natural suggestion is that it comes from the cognitive *dispositions* involved in fully accepting it, such as dispositions to seek, develop, and accept specific explanations by natural selection—explanations in the spirit of the theory but not entailed by it, even when it is combined with background knowledge. Nor should we expect the theory to provide a precisely articulated *rule* to tell us which such explanations to accept.

Faced with such a theory, whose propositional content is so elusive, some philosophers might describe what it offers us as ‘understanding’ rather than ‘knowledge’. However, that is a false contrast, based on too narrow a conception of knowledge. One cannot understand why or how things happen without *knowing* why or how they happen (see Sliwa 2015).

Obviously, the theory of evolution is a very rich and complex case, to which only an accomplished philosopher of biology could do proper justice. It may be helpful to consider a vastly simpler theory, which is widely accepted too, though (unlike Darwin’s) not for good reasons. The *conspiracy theory of history* says something like this: ‘Historical events result from conspiracies’. That too is a generic generalization, not a universal one: its truth does not entail the truth of every instance. Proponents of the conspiracy theory can consistently admit that *not all* historical events result from conspiracies. Yet, whenever a particular historical event comes under discussion, they will typically explain it as the result of a conspiracy. Although the generic generalization ‘Historical events result from conspiracies’ does not deductively entail very much, its explanatory power comes from the cognitive dispositions involved in fully accepting it, such as dispositions to seek, develop, and accept specific explanations by conspiracy—explanations in the spirit of the theory but not entailed by it, even when it is combined with background knowledge. Nor should we expect the theory to provide a precisely articulated *rule* to tell us which such explanations to accept.

Imagine someone who assents to the generic slogan ‘Historical events result from conspiracies’ if the question arises but, when it comes to cases, is not at all disposed to prefer explanations of particular historical events by conspiracy to other explanations of them. Their acceptance of the conspiracy theory of history seems at best half-hearted. They may just be paying it lip service. Conversely, imagine someone who dissents from the generic slogan if the question arises but, when it comes to cases, is strongly disposed to prefer explanations of particular historical events by conspiracy to other explanations of them. Their rejection of the conspiracy theory of history seems at best half-hearted. They may be paying it all but lip service. In such cases, how far one accepts the generic theory depends as much on one's dispositions when it comes to cases as on one's reaction to the generic slogan itself.

The foil usually offered to the conspiracy theory of history is not a grandiose theory of history like those of Marx and Tolstoy but rather the plain *cock-up theory of history*, to which I have a sneaking sympathy. It says something like this: 'Historical events result from cock-ups [incompetence, confusion, mistakes, misunderstandings, etc.]'. What it is to accept it can be characterized in terms exactly analogous to those just used to describe what it is to accept the conspiracy theory.

Switching between the conspiracy and cock-up theories of history may be rare, but is clearly possible, in both directions. It could take the form of a gradual change in one's disposition to prefer one type of explanation over the other when it comes to cases, with one's switch from one generic slogan to the other just an after-effect.

How different is theory acceptance in logic and mathematics, where theoretical generalizations are universal rather than generic? The difference is less stark than one might have expected.

Many working mathematicians have no special concern for foundational issues. They rely on set theory as the standard framework for mathematics but are not interested in it for its own sake. Perhaps they have briefly encountered Russell's paradox as an exotic curiosity and are vaguely aware that set theorists have ways of dealing with it, but they may never have seen the axioms of a formal set theory such as ZFC, and be incurious about what the accepted theory is. In effect, such mathematicians have a defeasible disposition to accept instances of the Naïve Comprehension principle, to help themselves to a set demarcated by a predicate when they need it for their work, while subliminally aware that doing so is not a universally valid rule, since it generates contradictions by Russell's paradox. They may reject 'funny-looking' instances of Naïve Comprehension; such instances do not crop up naturally in their work. They have no formal rule to determine whether an instance does look funny, any more than we consult a rulebook to determine whether a joke is funny. They implicitly rely on Naïve Comprehension as a generic generalization rather than a universal one. Their dispositions correspond to no standard restricted Comprehension principle, in part because they work with a much wider vocabulary than a formal language for set theory, for example when they apply mathematics to the natural or social world.

Years, arguably decades, passed between the emergence of the set-theoretic paradoxes and the solidification of a consensus amongst mathematicians that the methods of Russell or Zermelo were adequate for containing them. In that period, a similar attitude to principles of set theory was rational even for mathematicians concerned about foundational issues. For example, Whitehead expressed such an attitude to the theory of classes in applying mathematics to the material world, claiming that his applications would be robust under different resolutions of the paradoxes (Whitehead 1906: 470). For logicists, taking that attitude to mathematics is tantamount to taking it to logic.

We remain in a similar position with respect to the logic of predicates for truth and falsity, faced with the Liar and other semantic paradoxes, despite much progress in the tradition of Tarski (1935) and Kripke (1975). There is still no consensus as to exactly what approach to follow. We use instances of disquotational principles when we need them in practice, while aware that the unrestricted application of those principles will lead to contradictions. Typically, we do so with no particular formal theory of truth in mind. Rather, we treat the disquotational principles more like generic generalizations. We have a defeasible disposition to accept their instances, which we overrule for funny-looking instances. As usual, we have no formal rule to determine whether an instance does look funny: how would such a rule handle the indefinite semantic diversity of sentences in English or any other natural language to which we want to apply disquotation?

This need for genericity in logic arises even for first-order reasoning in natural languages. Consider inferences by the rule of disjunction introduction, to a disjunction from one of its disjuncts. We have a defeasible disposition to accept inferences of that form. However, we may override our disposition to infer from ‘P’ to ‘P or Q’ when ‘P’ is fine but ‘Q’ looks nonsensical, paradoxical or otherwise funny, perhaps also when it contains a slur (read the quotation marks as Quinean corner quotes). Once again, we have no formal rule for detecting such exceptions.

Similar issues arise for the rule of universal instantiation, which plays a central role in Kripke’s argument about adoption. We have a defeasible disposition to make inferences of the form of universal instantiation, say from ‘Everything is F’ to ‘t is F’, where ‘t’ is a singular term. However, we may override that disposition when ‘t’ is funny-looking, somehow suspect. For instance, I am happy to accept ‘Everything is self-identical’, but reluctant to infer ‘Satan is self-identical’ (I do not believe in devils).

Of course, such examples are the staple diet of proponents of ‘free logic’, on which one is not free to apply universal instantiation. For them, the valid move in the vicinity is from ‘Everything is F’ and ‘Something is t’ to ‘t is F’; since I do not accept ‘Something is Satan’, no wonder I am reluctant to infer ‘Satan is self-identical’. However, free logic does not remove the need for genericity. For, in order to vindicate the default status of unrestricted universal instantiation (without which free logic would be hopelessly restrictive), free logic must assign default status to ‘Something is t’. We will override the disposition to accept ‘Something is t’ in the same cases, when ‘t’ is suspect. Clearly, we have no formal rule for detecting doubt as to whether ‘t’ refers.

All these examples indicate that accepting principles of mathematics or logic outside a formal language often involves messily defeasible dispositions not utterly unlike those involved in accepting theories elsewhere. Such dispositions are not normally immutable. Let us consider some cases of changing one’s dispositions to reason in various ways in mathematics and logic.

We start with a toy example. A child is learning elementary mathematics. On the basis of recognizing a pattern, the child becomes disposed to accept any instance of ‘n/n = 1’. On the basis of recognizing another pattern, the child also becomes disposed to accept any instance of ‘0/n = 0’. One day, the child accepts ‘0/0 = 1’ as a result of the former disposition and ‘0/0 = 0’ as a result of the latter disposition, which prompts the alarming thought ‘0 = 0/0 = 1’. On reflection, the child restricts both dispositions. At first, the restriction may take the form of retaining the original dispositions while adding a secondary disposition to inhibit them for n = 0. Later, the restriction becomes habitual: the original incorrect dispositions may disappear, being replaced by correctly restricted dispositions; the child is no longer even tempted to accept ‘0/0 = 1’ or ‘0/0 = 0’. That is a case of *correcting* one’s dispositions. But if it is possible to correct one's dispositions, it is also possible to have the *illusion* of correcting one's dispositions, while in fact replacing correct dispositions by incorrect ones.

At a more advanced stage, the child has learnt to add, subtract, multiply, and divide correctly. He can do simple algebraic manipulations on the basis of pattern recognition; for instance, from '$k + m = k + n$' he infers '$m = n$'. He can distinguish natural numbers from other numbers, also on the basis of pattern recognition. However, he has not learnt any proof system for arithmetic. Moreover, he has no disposition to reason by mathematical induction. When the teacher first explains the principle of mathematical induction to him, it does not immediately strike him as valid. He has no particular objection to it; he just does not find it compelling (he is not terribly intelligent). Nevertheless, he accepts mathematical induction on the teacher's authority, and learns to use it himself. Reasoning by mathematical induction gradually becomes habitual to him, and he feels confident in such reasoning. He has acquired a disposition to reason by mathematical induction. That is a case of gaining a disposition. But a disposition so gained may also be lost. If an eccentric new mathematics teacher tells the class that recent developments in mathematics have discredited the principle of mathematical induction, the pupil will shrug his shoulders and give up such reasoning.

Now imagine an unwise philosopher, Phil, who gets it into his head that verification is *constructive*: to verify a disjunction you must verify a disjunct, and to verify an existential generalization you must verify an instance. As he recognizes, this makes trouble for the law of excluded middle. For when we have no verification of 'S' and no verification of 'not S', we have no verification of 'S or not S'. With the courage of his convictions, Phil refuses to accept excluded middle; he starts training himself not to assume it in his reasoning, and not to assume principles from which he can derive it. He hears about intuitionistic logic, which sounds like just what he needs. He starts imitating intuitionistic reasoning. Phil's thinking is much cruder than that of philosophers like Michael Dummett, attracted by intuitionistic logic as an alternative to classical logic; unlike them, Phil has no category of indirect verification for cases when one possesses a procedure guaranteed to generate a verification of one disjunct or the other, but has not yet applied it to the case at hand. But it is still *psychologically possible* to think in Phil's crude way; anyway, even acknowledging the category of indirect verification is insufficient to validate the law of excluded middle, since one may have no procedure of the kind needed for indirectly verifying 'S or not S'. From time to time I meet self-identified intuitionists whose philosophical thinking seems little better than Phil's.

Phil does not change his dispositions at a stroke. He works on them. His dislike of excluded middle enables him to apply a kind of aversion therapy to himself. There may be an unconscious level of automatic reasoning which his self-improvement exercises cannot reach, just as theoretical physicists retain some level of instinctive proto-Aristotelian folk physics, but in both cases one can habituate oneself to another form of reflective thinking, which can override one's more primitive instincts. In some sense Phil may not *adopt* intuitionistic logic, but he surely comes to accept it.

Of course, merely losing some of one's past beliefs need not involve disagreeing with one's past self. Since every theorem of intuitionistic propositional logic is also a theorem of classical propositional logic, and classical propositional logic is consistent, no formula α is a theorem of one logic while $\neg\alpha$ is a theorem of the other. Indeed, the negation $\neg(\alpha \vee \neg\alpha)$ of any instance of excluded middle is as inconsistent intuitionistically as it is classically. Thus the intuitionist cannot consistently deny any instance of excluded middle. For plain propositional logic, any disagreement is at another level, either metalogical—the classical logician says that $\alpha \vee \neg\alpha$ is a logical truth while the intuitionistic logician says that it is not—or epistemic—the classical logician says that $\alpha \vee \neg\alpha$ is known while the intuitionistic logician says that it is not. However, once quantifiers are added to the object-language, disagreement can arise at that level too. For intuitionistic mathematics has theorems of the form $\neg\forall x\ (Fx \vee \neg Fx)$, while of course the classical mathematician asserts $\forall x\ (Fx \vee \neg Fx)$. The disagreement becomes even starker once quantification into sentence position is permitted in the object-language. The classical logician asserts the general law of excluded middle as a universal generalization, $\forall P\ (P \vee \neg P)$, while the intuitionistic logician asserts the negation of the law, $\neg\forall P\ (P \vee \neg P)$. Thus the disagreement can be made explicit in a suitably expressive object-language, without ascent to a metalanguage.

More generally, *differences* between logics with the same vocabulary can be expressed as explicit metalogical *disagreements*, and often manifest non-metalogical disagreements expressible in a suitably extended metalanguage.

Excluded middle is not exceptional in its contentiousness. Elsewhere, I have argued—with particular attention to modus ponens and the rules for conjunction—that any principle of logic can be rejected by someone who understands the relevant logical words by normal standards (Williamson 2007, Boghossian and Williamson 2020). Given its role in the adoption problem, the rule of universal instantiation is of special interest here. It was briefly discussed above; as noted, self-identified free logicians reject it in its usual form. In the reverse direction, one might wonder how someone who did not already implicitly accept universal instantiation could ever come to accept it. But a free logician who becomes disillusioned with all the extra complications of free logic might decide that they are not worth the trouble, and revert to the standard logic of quantification. That might be quite easy: she still remembers how she used to reason, in the good old days before she became a free logician, so she can *imitate* her earlier self. In general, the human capacity for imitation provides a mechanism for taking on different ways of reasoning with no need to derive them from verbal instructions.

By imitation, one can come to accept new patterns of reasoning and reject old patterns. Similarly, one can also come to *simulate* accepting new patterns of reasoning and rejecting old ones, perhaps by actually accepting the new and rejecting the old in very limited contexts. Thus intuitionistic logicians can simulate classical reasoning and classical logicians can simulate intuitionistic reasoning. Each side imagines how things look, how far they fall into shape, from the other's perspective. Such simulations matter, because they enable us to make informed choices between different ways of reasoning: changing sides is not just taking a leap into the dark. By normal human standards, it is a rational process. In that respect, it is comparable to the kind of process by which scientists sometimes decide to switch from an old theory to a new one in a scientific revolution. In neither case should we expect all-purpose formal rules to tell us which way to go. Those who demand much more for theory change to be rational may find that the history of science fails to meet their requirements.

Of course, in the case of intuitionistic logic, I regard switching to it from classical logic as a disastrous mistake. But that does not mean that I cannot imagine how a sane person can regard it as seeing the light. In other cases—such as necessitist higher-order modal logic—I do regard the switch as more like seeing the light.

5. Denying the doctrine and changing the subject

So far, ‘alternative logics’ have been treated as genuine rivals to classical logic. Famously, Quine rejected that assumption in his later work. He suggested that would-be alternative logicians can always be more charitably reinterpreted as changing the subject rather than denying the doctrine (Quine 1970: 82-3; Morton 1973 is a detailed critique of his argument). For we need not assume that they mean what we mean by logical words such as ‘not’, ‘if’, ‘and’, ‘or’, ‘is’, ‘every’, and ‘some’ in a natural language, or their analogues in a formal language.

The ‘logical pluralism’ of Beall and Restall (2006) also involves a charge of equivocation, but it is much more limited in scope than Quine’s, and in particular focusses on metalogical vocabulary (such as ‘valid’) rather than logical vocabulary in the object-language itself.

Quine’s charge of equivocation gains some plausibility from ‘alternative’ logicians’ tendency to provide highly non-homophonic semantic theories, to explain why their systems accept some principles and reject others. If those semantic theories characterize meanings for the logical words different from the meanings characterized by the corresponding homophonic semantics, which seem to be the usual meanings, then they characterize *un*usual meanings for the logical words. Kripke discusses the equivocation hypothesis for both intuitionistic and quantum logic, briefly but sympathetically (Kripke 2020).

However, other aspects of ‘alternative’ logicians’ practice tell the other way. They typically accuse classical logicians of *errors* and *fallacies*. For instance, ideological intuitionists accuse classical logicians of illicitly relying on the law of excluded middle—that is, for the classical logicians’ own use of ‘or’ and ‘not’. Similarly, ideological quantum logicians accuse classical logicians of illicitly relying on the distributive law—that is, for the classical logicians’ own use of ‘or’ and ‘and’. If such ‘alternative’ logicians were simply proposing new and more convenient or more perspicuous meanings for old words, at least as used in some theoretical contexts, such accusations would be obviously unwarranted. Of course, the equivocation hypothesis could be extended, to imply that the ‘alternative’ logicians do not mean what *we* mean by words such as ‘error’ and ‘fallacy’, but applying that line to all their apparent protestations of disagreement and their extensive supporting arguments would soon suggest that they are not speaking English at all (or whatever natural language the conversation appears to be in), but some other language which we do not understand. That hypothesis blocks communication and fails to explain their linguistic behaviour in any specific way. By normal standards, we can make far more sense of them by interpreting them more or less homophonically, perhaps with a few exceptions in special theoretical contexts. Of course, their speech and writing, like almost everyone else’s, is riddled with logical words; ‘not’, ‘if’, ‘and’, ‘or’, ‘is’, ‘every’, and ‘some’ are amongst the most commonly used words of English. Like others who have talked and corresponded with Michael Dummett, Hilary Putnam, Graham Priest, and other ‘alternative’ logicians, I have automatically interpreted their use of logical words in English homophonically, and never found reason to do otherwise. That was so even when the topic was ‘alternative logic’. In all my experience, ‘alternative’ logicians use the logical words of natural language with their ordinary shared meanings, and refusing to interpret them so would be deeply uncharitable (see also Morton 1973).

There is a connection here with Kripke’s compelling critique of descriptivist views of proper names and natural kind terms (Kripke 1972, 1980). Descriptivists tend to recycle individual differences in the beliefs associated with a given proper name or natural kind term as individual differences in its meaning, with its reference determined by descriptive fit. Similarly, the equivocation hypothesis tends to recycle individual differences in the beliefs and accepted patterns of reasoning associated with a given logical word as individual differences in its meaning, with its reference determined by descriptive fit. The equivocation hypothesis is in effect a sort of descriptivism about logical words. As Kripke and Putnam showed, descriptivism radically underestimates the social determinants of meaning. Although the implicit intention to preserve reference can fail in extreme cases, it is the standard default. Normally, we use proper names and natural kind terms as terms of a publically available language, with the references determined for them in that language. The same holds for logical terms. The equivocation hypothesis radically underestimates the social determinants of meaning (see also Williamson 2007: 121-30).

The debate over Quine’s claim of equivocation in logic is also reminiscent of the debate over Kuhn and Feyerabend’s claims of equivocation in natural science, which raged in the philosophy of science during the same period (Kuhn 1962 and Feyerabend 1970, 1975, 1978). According to Kuhn and Feyerabend, scientific revolutions involve meaning variance: old theoretical terms acquire new meanings after the revolution from the new theory, thereby making the old and new theories somehow incommensurable. In both logic and natural science, the alleged meaning variance came from differences between the theoretical systems in which the term figured, and so applied to individuals derivatively from their theoretical allegiances. Underlying Kuhn and Feyerabend’s claims of meaning variance were descriptivist assumptions about meaning: reference is determined by descriptive fit. Pushback came increasingly from anti-descriptivist arguments about meaning and reference (as in Putnam 1973). Obviously, the creation myth of a single primordial specimen fixing the reference of a natural kind term must be taken with a pinch of salt, but that does not undermine the model of reference fixing by a fallible capacity to identify (some) specimens of the kind combined with relevant similarities and differences in their possibly unknown underlying natural properties. Such a model already undermines Kuhn and Feyerabend’s descriptivist assumptions, and allows a term to refer to the same kind when used in the new theory as when used in the old theory.

Of course, the two debates did not run in perfect parallel. For example, opponents of meaning variance in the philosophy of science often invoked causal links to referents, but obviously not in the philosophy of logic. Conversely, proponents of meaning variance in the philosophy of logic often regard peaceful coexistence as a possibility for the two theories, but not in the philosophy of science. Nevertheless, in both cases, descriptivist approaches had difficulty making sense of deep theoretical disagreement, where each side is genuinely denying what the other asserts. (Newton-Smith 1981 gives more detail on the debate over incommensurability in science.)

Do the objections to the equivocation hypothesis for logical words in a natural language extend to logical symbols in a formal language? An 'alternative' logician who presents a non-standard semantic model theory for an artificial language seems to be *stipulating* meanings for its logical symbols. That is quite different from using them as terms in a pre-existing language, with the meanings in it they already have. Might the symbol $\vee$ be sometimes used like that in quantum logic or intuitionistic logic, with a stipulated meaning subtly different from the meaning it usually has in logic (disjunction)? Several reservations need to be kept in mind.

First, we cannot assume that the new logical constants can peacefully co-exist with the old ones in a single language. For example, consider a language with two 'negation' operators, $\neg_C$, which obeys the standard rules of inference for classical negation, and $\neg_I$, which obeys the standard rules of inference for intuitionistic 'negation'; both are freely applicable to any sentence of the language. Then one can show that the two operators are logically equivalent, so $\neg_I$ *also* obeys the standard rules of inference for *classical* negation; in other words, intuitionistic negation collapses into classical negation (Popper 1948, Harris 1982; for discussion see Williamson 1987/88 and Schechter 2011).

Second, we should not take for granted that a non-standard semantic framework is even capable of defining genuine meanings for logical symbols.

Here is an example. Many intuitionists use a proof-conditional semantic framework in which the meaning of a sentence is the condition for something to be a proof of it, and the meanings of sub-sentential expressions are crafted to make the overall semantic theory compositional. Within such a framework, one can define a sentential constant $\perp$ by stipulating that nothing is to count as a proof of $\perp$. Since $\perp$ is a sentence with a well-defined meaning by the standards of such a semantic framework, we might expect to be able to add it to English. Of course, we will have to adjust it to the phonetics and syntax of English: we can do that simply by writing it as 'It bottoms', with the stipulation that it means whatever $\perp$ means. Since the symbol $\perp$ is usually called 'the *falsum*' or 'absurdity', we might expect 'It bottoms' to entail a contradiction, say 'It is raining and not raining'. But that is unwarranted. That nothing counts as a proof of $\perp$ does not imply that $\perp$ is *false*. Perhaps there are unprovable truths. Ideological intuitionists will presumably deny that there are, but our present concern is not ideological intuitionism, just the capacity of the proof-conditional semantic framework to define genuine meanings. That framework seems to have left the meaning of $\perp$ radically vague, to the point where its English equivalent is hardly usable. That may not be total meaninglessness, but it is pretty bad.

Here is another case. Consider a logician who wants to use the standard two-valued truth-tables to define connectives, but with an alternative pair of semantic values: 'T' is reinterpreted as 'contingently true' rather than 'true' and 'F' as 'not contingently true' (false or necessarily true) rather than 'false'. In particular, the reinterpreted table for the truth-functional conditional is used to define a new connective *. For any formula α, the formula $\alpha * \alpha$ always takes the value T, which now means that it is contingently true in every case. But since it is true in every case, it should be *necessarily* true, not contingently true. We could elaborate the example by setting it within a possible worlds framework; $\alpha * \alpha$ would then come out as contingently true at every world. Thus slightly reinterpreting a familiar semantic framework can turn it into garbage.

We should be wary, such examples suggest, of assuming that alternative semantic frameworks define new meanings for old logical symbols, rather than no meanings at all.

The third reservation is this. Even if 'alternative logicians' succeed in using an alternative semantic framework to stipulate new meanings for the logical symbols of a formal language, they often make clear that they intend those symbols to model the standard logical words of a natural language, and to show what the logical properties of the latter *really* are, and how they differ from the properties attributed to them by classical logicians. Typically, their arguments' aim is not merely to show that the logical words of natural language do not in fact have the properties classical logicians attribute to them; it is to show that no words *could* have those properties. Thus the stipulative aspect of such logicians' activity goes with a full-blooded rejection of classical logic.

Obviously, much of the published technical work on 'alternative logics' is carried out within an overarching classical framework and is not in disagreement with classical logic. But, in much of the more philosophical work by self-identified 'alternative logicians', the disagreement is genuine and unequivocal. That is just as anti-exceptionalism about logic predicts. Theoretical disagreements are to be expected in any fundamental science.

There is no sound transcendental argument for classical logic. That does not matter. The immanent case for classical logic is good enough.

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