# On Logical Relativity

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One logic or many? I say—many. Or rather, I say there is one logic for each way of specifying the class of all possible circumstances, or models, i.e., all ways of interpreting a given language. But because there is no unique way of doing this, I say there is no unique logic except in a relative sense. Indeed, given any two competing logical theories $T_1$ and $T_2$ (in the same language) one could always consider their common core, $T$, and settle on *that* theory. So, given any language $L$, one could settle on the minimal logic $T_0$ corresponding to the common core shared by all competitors. That would be a way of resisting relativism, as long as one is willing to redraw the bounds of logic accordingly. However, such a minimal theory $T_0$ may be empty if the syntax of $L$ contains no special ingredients the interpretation of which is independent of the specification of the relevant $L$-models. And generally—I argue—this is indeed the case.

## 1. From Pluralism to Relativism

The view that I hold stems from the familiar semantic conception of logic, according to which

(1) A valid argument is one whose conclusion is true in every model in which all its premises are true.

As JC Beall and Greg Restall have recently argued,[1] this definition is by itself relativistic insofar as the notion of 'model' may be cashed out in different ways. Take models to be worlds (or world-like structures) and (1) yields some sort of classical logic. Take models to be situation-theoretic set-ups (possibly incomplete and/or inconsistent) and (1) results in some sort of relevant logic. Thus, the question "Is this argument valid?" does not admit of a unique answer because there is more than one sense in which an argument can be valid, and to

[1] In Beall and Restall [2000].

the extent that these senses are equally good one would be entitled to hold a relativistic position.

This sort of consideration could leave one unmoved. Ambiguity is no evidence for relativism if disambiguation stamps out all disagreement. In fact, Beall and Restall prefer to speak of ‘pluralism’ rather than ‘relativism’, and I think their pluralism is best interpreted as the moderate claim that there are several equally good, non-equivalent ways of *reading* (1), i.e., several equally good senses of construing the relevant notion of a model, regardless of whether these senses leave room for internal disagreement. The view that I intend to outline, and that provides evidence for a more recalcitrant brand of relativism, is stronger. I hold that there exist ways of reading (1) that do leave room for internal disagreement. They leave room for disagreement because they are compatible with different ways of characterizing that portion of the language that is responsible for the required nexus between the premises and the conclusion of a valid argument—different ways of characterizing the “logical vocabulary”. And these ways of reading (1) need not be idiosyncratic. They can be as ordinary as one likes, provided that we do away with a number of misleading traits that we are accustomed to associate with our favorite notion of a model.

Ultimately, of course, even this sort of disagreement could be construed as a form of ambiguity: it is still ambiguity on the relevant notion of a model. Suppose we disagree on whether the identity predicate should be treated as a logical constant. You say that it should be so treated, and therefore you exclude from the range of admissible models anything that doesn’t do justice to the intended interpretation of this predicate. I say that the identity predicate should not be treated as a logical constant, and therefore regard as admissible even models that reflect a different interpretation. Here ‘model’ could be construed in the ordinary fashion, assuming the language to be some familiar sort of first-order language. So our disagreement concerns the exact composition of the class of first-order models: You say it should only include certain models, I say it should not. And surely we could blame it on semantic ambiguity. We could say that we are not using the same notion of a first-order model after all. In this sense our disagreement would be just as innocuous as any other divergence that trades on ambiguity. However, this is only one way of looking at the impasse. Surely we could also insist that we possess exactly the same concept and yet we disagree on its extension: You say the extension only includes certain models and I say it includes many more. In this sense, our disagreement would not just be a sign of ambiguity. It would be genuine and irreducible, and enough to make a difference when it comes to the logic of arguments involving the identity predicate.

This sort of disagreement concerning the status of identity is familiar from logic textbooks. Is identity the only case in point? I don't think so. On the contrary, I think the same sort of disagreement may apply across the board and affect the status of any portion of the relevant vocabulary. Tarski once suggested that *every* term can in principle be treated as a logical term or as a non-logical term, as the case may be,[2] so the relativism ensuing from this view may be termed *Tarskian Relativism*. Indeed, once Tarskian relativism is admitted a different sort of logical relativism must be admitted as well, according to which different ways of specifying the semantics of a fixed logical vocabulary are also possible. You and I may agree that identity is a logical constant but you may think that it stands for a transitive relation whereas I may not.[3] Again, this amounts to a genuine disagreement concerning the range of admissible models. And, again, I see no reason to restrict the possibility of such disagreement to a few cases: given any way of drawing the boundary around the logical vocabulary, we may in principle disagree on the exact interpretation of any portion of that vocabulary. Quine famously argued against this view by stigmatizing deviance: "Change of logic, change of subject".[4] On the other hand, Carnap's "principle of tolerance" famously implied that everyone is at liberty to build his or her own logical theory, even when this means casting the ship of logic off from the *terra firma* of classical forms.[5] So we may label this view *Carnapian Relativism*. It is the relativism that comes with the claim that the meaning of the logical vocabulary is up for grabs, which is not the same as the claim that the choice of the vocabulary is itself up for grabs. My contention in the following is that both varieties of relativism, even in their most extreme forms, are defensible.

## 2. Logical and Extra-logical

Let us begin with Tarskian Relativism. What are the reasons for maintaining that the distinction between the logical and the extra-logical is up for grabs? Broadly speaking, my reasons stem from the consideration that all bits of language get their meaning fixed in the same way, namely, by choosing some class of models as the only admissible ones. One notable difference, of course, is that in one case

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2 In Tarski [1936].

3 In fact I do agree that identity is transitive. But there are philosophers who have denied it—e.g., Garrett [1985].

4 Quine [1970], Ch. 6.

5 See Carnap [1934].

(the logical terms) the relevant class of admissible models is normally thought of as constituting the class of *all* possible models, whereas in the other case the chosen models are just meant to characterize a certain way of understanding the terms in question—a preferred way among many possible others. In this sense, logic is a uniquely ambitious theory. It aims to be the theory included in every other theory; its models want to include the models of every other. Yet this notable difference—I argue—does not rest on any intrinsic peculiarity of the logical terms. One can draw the line between the logical and the extra-logical vocabulary in many ways, and depending on how one draws the line one can think of the models that fix the meaning of the logical terms as constituting the class of all models. Alternatively, one can specify the class of all possible models in many different ways, and depending on how one specifies that class one can think of the terms whose meaning is invariant across the board (in some sense to be clarified) as constituting the logical vocabulary. Logic is ambitious, but precisely for that reason the competition can be tough.

Here is how Tarski put it in his 1936 paper, "On the Concept of Logical Consequence":

> The division of all terms of the language discussed into logical and extra-logical . . . is certainly not quite arbitrary. If, for example, we were to include among the extra-logical signs the implication sign, or the universal quantifier, then our definition of the concept of consequence would lead to results which obviously contradict ordinary usage. On the other hand no objective grounds are known to me which permit us to draw a sharp boundary between the two groups of terms. It seems to be possible to include among logical terms some which are usually regarded by logicians as extra-logical without running into consequences which stand in sharp contrast to ordinary usage.[6]

As I mentioned, Tarski even went as far as saying that

> In the extreme case we could regard all terms of the language as logical. The concept of *formal* consequence would then coincide with that of *material* consequence.[7]

This last statement is actually a non-sequitur, unless treating all terms as logical is taken to imply drastic restrictions on the cardinality of the admissible models. (Ordinarily, a statement of the form 'There are exactly $m$ things' is a material consequence of a statement of the form 'There are exactly $n$ things' ($n \neq m$) if,

[6] Tarski [1936], pp. 418-419.
[7] Ibid. p. 419.

and only if, the number of objects in the domain of quantification is either different from *n* or equal to *m*.[8]) But never mind that. The relevant claim is that all (or any) terms of the language could in principle be regarded "as logical"—and I agree with that.

Now, what are the objections to this view? A lot has been said in this regard,[9] but I think the main complaints boil down to three, and none of them is compelling. Two objections can be stated and replied to easily; the third requires a detailed response, and the bulk of the sequel will be devoted to it.

The first objection comes with the intuition that a logical term is semantically invariant—that it is a logical *constant*. Take a first-order language *L* whose vocabulary includes an extra-logical constant, say, the binary predicate 'parallel to'. As an extra-logical term, this predicate is characterized by a strong semantic variability: its extension in an *L*-model can be any binary relation whatsoever, any set of ordered pairs. Indeed, all things being equal, 'parallel to' is an extra-logical predicate precisely insofar as the class of its possible extensions coincides with that of any other extra-logical binary predicate. Can we make it into a logical term just by stipulating instead that its extension be kept constant in all *L*-models? Hardly so. The only way to make the stipulation would involve drastic restrictions on the variability of a model's domain. For example, if *R* is the extension in question, we would have to rule out as inadmissible all models whose domain contains fewer elements than the field of *R*, and this is hardly a way of doing justice to the intended meaning of 'parallel to'. So, at least cardinalitywise, the interpretation of 'parallel to' must vary from model to model.

This objection, however, proves little. After all, the same consideration would affect the status of some typical logical constants. The interpretation of 'identical with', for instance, depends just as much on the domain of discourse, or on its cardinality, so strictly speaking it cannot be kept constant in all models. Semantically, what distinguishes a logical constant is not the fact that its interpretation is invariant from model to model, for sometimes its does vary. So the fact that the interpretation of 'parallel to' *must* vary can hardly be a reason for not including it into the logical vocabulary.

A second line of objection stems from the intuition that logicality requires *generality* (as opposed to semantic invariability). True—one could argue—the intended interpretation of a predicate such as 'identical with' may vary from model to model. Nonetheless it can always be identified with the identity rela-

[8] On this point see Sher [1991], p. 46f.

[9] See Gómez-Torrente [2002] for a critical overview of the literature.

tion defined on the model's domain. Its intended interpretation works fine in every domain, for all (pairs of) objects in the domain. As Quine put it, identity knows no preferences, it treats all objects impartially.[10] By contrast, the intended interpretation of a predicate such as 'parallel to' only makes limited sense in certain domains. (What is this predicate supposed to mean in a domain of entities that cannot be compared with regard to direction—say: properties?) So, again, treating 'parallel to' as a logical term would seem to involve drastic restrictions on the composition of a model's domain, and this would suffice to cash out a significant difference between this predicate and a logical predicate such as 'identical with'.

This objection, I think, is also inadequate. For if treating certain bits of language as logical constants amounts to identifying a certain class of models with the class of *all* possible models, then the sort of restriction at issue, though drastic, would have to be expected. Models in which the parallel-to relation makes no sense would simply have to be dismissed if 'parallel to' were treated as a logical constant. On pain of begging the question, it is hard to see how the necessary condition of generality can be violated, in this case as in many others. Moreover, as a sufficient condition generality is dubious, too. For there are other theories besides logic that seem to fit the bill. Formal ontology, for instance, understood in the spirit of Husserl's "pure theory of the objects as such",[11] is arguably a theory of equal generality and its primitive notions (such as 'part of' or 'depends on') would seem to apply across the board.

One could press the objection, here. One could argue that the difference between 'identical with' and 'parallel to' (or 'part of') is that the meaning of the former can be captured by a rule that does not require distinguishing the identity of objects in a given universe, whereas the meaning of the latter does require that. This, in turn, could be explained in terms of invariance under permutations: no matter how one picks a model, the extension of 'identical with' is not affected by any permutation of the universe (i.e., any one-one transformation of the universe onto itself), as all things are sure to remain self-identical no matter how one manipulates them. By contrast, a rule for 'parallel to' (or 'part of') could be so affected. Ergo, only 'identical with' is a logical term—or so one could argue.

This way of pressing the objection has a respectable pedigree. Tarski himself considered the invariance criterion in a joint article with Adolf Lindenbaum, "On the Limitations of the Means of Expression of Deductive Theo-

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10 Quine [1970], p. 62.

11 See Husserl [1900/01]. Investigation III.

ries"[12] (where it is shown that every notion definable in the simple theory of types is invariant under all permutations of any given domain) and eventually articulated it extensively in his 1966 lecture, "What are Logical Notions?".[13] The same idea was taken up by Mostowski and Lindström in their papers on generalized quantifiers[14] (where the invariance property was explicitly employed to license a genuine extension of standard first-order logic) and recently refined and defended by Gila Sher.[15] Indeed, the invariance criterion is nowadays widely accepted as an extensionally adequate criterion, i.e., as a tool for correctly identifying the traditional set of logical constants and some of its most natural extensions. (It is precisely in this sense that Tarski was interested in the criterion, in spite of the fundamentalist skepticism of his 1936 paper.) However, the criterion is ultimately inadequate—in fact, question-begging—if our concern is what distinguishes logical from extra-logical terms *in its most general form*, without reference to any particular logical theory. For what is it that would allow us to say whether the interpretation of a given term is *always* invariant under all permutations of the given domain, if not a preconceived understanding of what is logically admissible? Take again the case of predicates. The problem is not only that we could interpret 'identical with' as a relation other than identity, for that would still be compatible with the identity relation itself enjoying a special status regardless of how we choose to designate it. Nor is the problem that *many* predicates could turn out to designate the same identity relation (consider '*x* is identical with *y* iff *y* is either white or not white'). Rather, the problem is that the special status of the identity relation is itself dependent on a conception of the range of admissible models. If models with self-different objects were admitted (as someone might urge), then identity would not comply with the invariance criterion. Conversely, if all admissible models had their domain defined by a set of parallel lines (for instance), or by a set of lines no pair of which are parallel, then the parallel-to relation *would* comply with the criterion and the predicate 'parallel to' could therefore be treated as a logical constant.

From a general semantic standpoint, then, I am inclined to resist the objection. Generality is hardly a better criterion for logicality than constancy of meaning. Are there any other options? One last, important option would seem to

12 Lindenbaum and Tarski [1934/35].

13 Tarski [1986].

14 See Mostowski [1957] and Lindström [1966].

15 See especially Sher [1991] and [1999]. Actually, Sher's account includes the invariance criterion but is not exhausted by it. This allows her to meet certain formal objections that can be advanced against the criterion, such as those put forward by McCarthy [1981].

be the flat refusal of the liberal semantic standpoint that we have been presupposing. My reasons for maintaining that the distinction between the logical and the extra-logical is up for grabs stem from the thought that all bits of language get their meaning fixed in the same way, namely, by choosing some class of models as the only admissible ones. However, one could object that only the meaning of the extra-logical terms is fixed that way. Indeed, according to the usual way of spelling out a semantics for a given language (most notably inspired by Tarski's own characterization of the semantics for first-order languages), the logical constants are interpreted *outside* the system of models. Their meaning is not captured by the basic semantic interface relating a language with its models. Rather, it is imposed upon it *ab initio.* It is characterized only indirectly through the recursive definition of truth (or satisfaction). In Sher's words

> The meaning of a logical constant is not given by the definitions of particular models but is part of the same metatheoretical machinery used to define the entire network of models. . . . The meaning of logical constants is given by *rules external to the system*.[16]

To this line of objection I reply that the customary way of spelling out semantics is indeed significant, but also misleading. If we are not going to consider other ways of interpreting certain symbols, then of course there is no need to do otherwise. So, if the meaning of the logical terms is to be kept constant (in some relevant sense) throughout the class of all models, then of course it is convenient to pull them out of the model-theoretic machinery and not worry about them every time we specify a model. But does this have any significance apart from pragmatic convenience? Does this provide any ground for a principled distinction between logical and extra-logical terms? I think not. In principle, one could certainly proceed otherwise. Provided that one works with a semantic apparatus that is sufficiently general and unbiased to support alternative practices, one could treat any given term outside the system of models or inside it, as the case may be. This is sometimes a genuine option with regard to the identity predicate, whose meaning is sometimes fixed *not* by a recursive clause of the form

(2) '*a* is identical with *b*' is true iff *Val*(*a*) is identical with *Val*(*b*)

(where *Val* is a function assigning semantic values) but rather by a stipulation about the interpretation of the predicate itself, namely a stipulation to the effect that 'identical with' picks out the identity relation. This stipulation, as we

[16] Sher [1991], p. 49.

have seen, amounts to a constraint on the class of the admissible models. But if this option is available for the identity predicate, then it is equally available when it comes to other predicates that we might want to treat as logical constants. If we are ready to regard a semantic rule such as (2) as a sign of logicality for 'identical with', then there is no reason not to regard the analogous rule for 'parallel to',

(3) '*a* is parallel to *b*' is true iff *Val*(*a*) is parallel to *Val*(*b*),

as a sign of logicality as well. And if we do so, what prevents us from doing the same with all other predicate and relation terms?

This reply, of course, only works to the extent that it can be fully generalized. I am saying that one could treat any given term outside the system of models or inside it, as the case may be, provided that a semantic apparatus is available that is sufficiently general and unbiased to support such practices. In the case of binary predicates the familiar Tarskian apparatus seems to be fine. But it remains to be shown that the same sort of flexibility is available in every case, with regard to expressions of any syntactic category, including for example the familiar connectives and quantifiers. If only some bits of language would resist the treatment that I am advocating, then the boundary between the logical and the extra-logical would not be up for grabs and the prospects for a relativistic account of logic would be undermined. Thus, in order for the reply to be successful we have to delve deeper into the relevant "metateoretical machinery" and provide evidence to the contrary. This is why I said that the third objection calls for a detailed response, and to these details I now turn.

## 3. The Paradigm of Functional Application

If our aim is generality we cannot just confine ourselves to first-order languages, as I implicitly did so far. And of course we cannot just confine ourselves to classical Tarskian models, i.e., interpretation structures defined by a non-empty domain of discourse along with a series of individuals, subsets, and relations based on this domain. These are the customary structures used in model theory but they are not general enough for our purposes. The semantic framework that we need consider must be much broader as regards both the notion of a language and the notion of a model.

Now, I reckon that the best suggestion in this sense is still the general theory of types, or better the theory of types as filtered through the theory of categorial grammars. This is known to be utterly overgenerating from the linguist's stantpoint, but it is also a theory that covers virtually every case of logical interest. So let us take a closer look.

Simply put, the guiding idea is that a language typically involves expressions of various types, which can be classified into two sorts: individual (or primitive) types, and functional (or derived) types. Intuitively, the individual types correspond to those categories of expressions whose syntactic status is not analyzed in terms of other categories: sentences, proper names, and presumably not many others. By contrast, functional types are defined in terms of simpler types in a way that fixes the combinatorial properties of the corresponding categories: for each pair of types $t$ and $t'$, primitive or functional, a new derived type $t'/t$ can be formed, corresponding to the category of those functors that combine with expressions of type $t'$ to produce expressions of type $t$. Thus, for instance, if $S$ is the type of sentences and $N$ the type of names, then $S/S$ will be the type of connectives, $N/S$ the type of predicates, and so on. (More generally, one could consider $n$-adic types of the form $t_1 \ldots t_n/t'$ for each $n>0$, corresponding to those categories of $n$-place functors that build expressions of type $t'$ out of expressions of type $t_1,\ldots, t_n$, in that order. However, such types can be ignored without loss of generality, as they can always be represented by monadic types of the form $t_1/(t_2/(\ldots/(t_n/t')\ldots))$.[17])

Suppose, then, that we have fixed upon a sufficiently large set $T$ of types. For instance, we may take an infinite stock of individual types $S$, $N$, $\tau_0,\ldots, \tau_n,\ldots$ and close it under the slash operation /. Then we can define languages and models of variable complexity in a uniform way. On the one hand, a language's expressions can be specified by recursion on the basis of some type assignment to its symbols: for each type $t$, the corresponding category of expressions will comprise all $t$-typed symbols (if any) plus all those expressions that can be obtained by applying some structural operation (e.g., juxtaposition) to pairs of expressions of type $t'/t$ and $t'$ (respectively) for some $t'$. In other words, a language is essentially a triple consisting of (i) a sequence $s$ of symbols of various types, (ii) a structural operation $g$ for building compound expressions, and (iii) the resulting $T$-termed system of (possibly empty) categories of expressions, $E$, one category for each type $t \quad T$. Specifically, $E = (E_t\!: t \quad T)$ would be the system defined by:

(4) If $s_i$ is a symbol of type $t$, then $s_i \quad E_t$.
If $x \quad E_{t'/t}$ and $y \quad E_{t'}$, then $g(x, y) \quad E_t$.

[17] The point is due to Schönfinkel [1924] and reflects the set-theoretic isomorphism between $A^{B_1\times B_2\times\,\cdots\,B_n}$ and $(\ldots((A^{B_1})^{B_2})\ldots)^{B_n}$.

(Some refinements would be in order to rule out certain linguistically implausible structures, but we need not go into such minutiae here.[18]) On the other hand, the notion of a model can be characterized in a perfectly symmetric way. A model must act as a semantic lexicon: it must determine what kind of things may be assigned to the basic components of the given language as their semantic counterparts, and it must do so within the limits set by the relevant type distinctions. Thus, a model for a language $L=(s, g, E)$ is essentially a triple $M=(d, h, I)$ such that (i) $d$ is a sequence of typed denotations, one for each symbol in $s$; (ii) $h$ is a structural operation subject to the same type restrictions as $g$, and (iii) $I$ is a $T$-termed system of domains, one for each category of expressions in $E$. More precisely, $I = (I_t: t \in \mathsf{T})$ is a sequence of domains satisfying the obvious counterpart of (4):

(5) If $s_i$ is a symbol of type $t$, then $d_i \in I_t$
If $x \in I_{t'/t}$ and $y \in I_{t'}$, then $h(x,y) \in I_t$.

Granted, in actual cases a lot depends on the exact make-up of $d$, $h$, and $I$, but from the present perspective the virtue of this definition is precisely that it allows for the greatest flexibility. For instance, typically one would require every functorial domain $I_{t'/t}$ to be a set of functions $f: I_{t'} \rightarrow I_t$, so that $h$ could truly be identified with the corresponding operation of functional application (i.e., $h(x,y)$ would always yield $x(y)$). Models that satisfy such further requirements—call them *stratified models*—are nice because they give direct expression to the paradigm of functional application. But such requirements are nonetheless optional.

So, broadly speaking languages and models are *homomorphic* structures. A language is literally *mirrored* in its models.[19] And this means that the semantic bridge between languages and models—the notion of a valuation—is straightforward. For a model $M=(d, h, I)$ is always sure to provide all the information that is required in order to evaluate every expression of the corresponding language

[18] For example, it is understood that both $s$ and $g$ need be one-one to avoid ambiguities: combined with the requirement that $g$ be well-grounded on $s$ (i.e., that $g$ and $s$ have disjoint ranges), this will secure that each expression be uniquely defined as either a symbol or a compound of the form $g(x, y)$. Moreover, we may want to require that all functional expressions cancel to individual expressions (in the sense that $E_{t'/t} \neq \emptyset$ always implies that $E_{t'} \neq \emptyset$, hence $E_t \neq \emptyset$), or that $g$ be the operation of concatenation (so that $g(x, y)$ is always the string $xy$). For a full treatment I refer to Varzi [1999], Ch. 1.

[19] At least, things are ideally so. I shall come back shortly to the possibility that such mirroring fails to yield a full homomorphism.

$L$=($s$, $g$, $E$): the denotation function $d$ assigns a value to each basic expression and the structural operation $h$ says how to compute the value of a compound expression given the values of its components. In other words, the valuation of a language $L$ on a model $M$ is the unique homomorphism between $L$ and $M$ induced by $d$, i.e., that function $Val$: $\bigcup E \rightarrow \bigcup I$ such that, in general:

(6) $Val(s_i)=d_i$.
$Val(g(x, y)) = h(Val(x), Val(y))$.

Broadly speaking, then, it is a general semantic framework of this sort that I think should be considered when it comes to assessing the claims of Section 2. And surely enough, a framework of this sort is compatible with most natural readings of (1). If every model of a given language were admitted, that language would not have any logical terms, i.e., any terms whose meaning is kept fixed in the relevant sense, and nothing guarantees that the notion of logical validity defined in (1) be non-empty. As soon as we rule out some models, though, some expressions get a fixed interpretation and logical validities begin to accrue. The question which may lead to serious disagreement (as opposed to mere ambiguity) is precisely the question of which models should be ruled out as inadmissible. (It is understood that the selection must somehow be exhaustive. The model class defining a logic should not just consist of an arbitrary bunch of widely disparate and ill-assorted models. But this is true of all good theories. In this sense, a logic is just as good a theory as any other, albeit a very important and arguably more fundamental one.)

By way of illustration, let us see how this way of describing languages and models subsumes the familiar semantics of logic textbooks, though in a much more abstract setting. Take a typical propositional language: this can be defined as a language $L$=($s$,$g$,$E$) whose symbols are either sentence variables (of type $S$) or connectives (of type $S/S$ or, more generally, $S/(S/(\ldots/(S/S)\ldots)))$. What exactly such symbols are, and how exactly they combine with one another by means of $g$ to yield compound expressions, we need not specify unless we expressly wish to do so. Rather let us say what a classical model is. It is not just any model $M$ for $L$. We must additionally require, first, that $I_S$—the domain corresponding to the category of sentences $E_S$—is a two-valued set, say the set 2={0, 1} (with 0 representing falsehood and 1 representing truth). Second, and most importantly in view of the third objection of Section 2, relative to such models we need not define the meaning of connectives *via* the recursive definition of truth. Just as connectives are characterized syntactically as symbols that combine with sentences to yield sentences, their denotations are characterized semantically as operations that combine truth-values to yield truth-values. In particular, if $L$ includes connectives for negation, '~' (of type $S/S$), conjunction, ' ' (of type $S/(S/S)$), etc., then $M$ would have to satisfy the additional requirement that the denotations of these symbols combine in a way akin to the Boolean operations of complementation, meet, etc.:

(7) If $s_i = \sim$, then $d_i(x)=1-x$ for all $x$ 2,
If $s_i =$ , then $d_i(x)(y)=x \cap y$ for all $x,y$ 2,

etc. (I am assuming for simplicity that $M$ is stratified and that numbers are sets, so that 0= and 1={ }.) It is easy to see that relative to models satisfying these specific conditions, the resulting valuation (homomorphism) would yield the usual results, i.e., the semantic conditions of classical bivalent propositional logic:

(8) $Val(\sim\varphi) = 1$ iff $Val(\varphi)=0$.
$Val(\varphi \quad \psi) = 1$ iff $Val(\varphi)=Val(\psi)=1$.

So, in particular, the notion of validity defined in (1) would yield exactly the valid arguments of classical propositional logic. From this point of view, we are just doing standard semantics. But note the level of abstraction (and the consequent degree of generality). Here not only the domain of individuals, but every domain of every category is specified by the model; not only the "extra-logical" terms but all symbols, including the connectives, are interpreted *inside the models.* And this is exactly what is needed to provide a reply to the third objection of Section 2. This is the sort of treatment that provides support to the claim that the boundary between the logical and the extra-logical is up for grabs—hence support for what I have called Tarskian Relativism—in spite of certain customary practices that suggest otherwise. We can regard any symbols of the language as logical because there is no external constraint on the interpretation of any symbol.

It should also be obvious how this picture supports relativism of the Carnapian variety, at least with reference to propositional logic. We can consider models with a different set of truth-values and corresponding conditions on the interpretation of connectives and obtain, say, Kleene's three-valued logic, or Post's, or Lukasiewicz's. In fact, by the same pattern one can give a semantic account conforming to a plurality of non-classical propositional logics: all that matters is that the desired domains of interpretation and the denotation of each connective be specified accordingly, by setting the relevant constraints on the admissible models. The general format need not change.

## 4. Extensions

We are not done, though. Truth-functional connectives are easy to handle. But can the picture be generalized? Can we deal with every bit of logic terminology in the same fashion?

I think we can. First of all, note that we can in a similar way account for the semantics of *intensional* languages, say languages with modalities. The semantic analysis of such languages is sometimes viewed as inducing a significant departure from that of purely extensional languages, for the meaning of a modal connective is taken to depend on factors that cannot possibly be captured by a standard model. Thus, a Kripke-style semantics for a modal language is conceptually more convoluted than pure Boolean semantics (though of course the underlying connection can be made to emerge). By contrast, in a framework like the one we are considering the treatment is perfectly uniform: to account for the relevant factors one only has to refer to the appropriate class of models, requiring for instance that the basic domains of interpretation associated with the primitive types be not just sets of flat, unanalysed entities, but sets of functions ranging over those entities and taking as arguments items from an appropriate set of intensional features. Thus, if $L$ is a propositional language with modalities, a suitable model for $L$ could be a model $M$ where the domain corresponding to the category of sentences is not the set 2 of truth-values, but the set $2^W$ of all functions mapping some set $W$ of "possible worlds" into 2. The interpretation of '~', '  ', and the other extensional connectives is not disturbed by this shift from truth-values to truth-valued functions, for we can require that their denotations be constant functions yielding the standard Boolean operations relative to every world in $W$. But the shift becomes relevant as we turn to the modal connectives, say the necessity connective '□'. For the intensional character of such a connective can be accounted for precisely by requiring its denotation to be a function whose value for a given argument at a given world (relative to $h$) depends on the value of the argument at different worlds—a function whose value at that world is true iff its argument is true at every world. For example, assuming for simplicity that $M$ is stratified, the relevant clauses would look like this:

(9) If $s_i = \sim$, then $d_i(x)(w) = 1 - x(w)$ for all $x$   $2^W$ and $w$   $W$
If $s_i =$   , then $d_i(x)(y)(w) = x(w) \cap y(w)$ for all $x, y$   $2^W$ and $w$   $W$
If $s_i = \Box$, then $d_i(x)(w) = \bigcap\{x(w') : w'$   $W\}$ for all $x$   $2^W$ and $w$   $W$

And these are clauses that result in a restriction of the class of admissible models. Models satisfying these clauses, we could say, determine the *logic* of '~', '  ', and '□'.

I think at this point it should be clear how the issue of Tarskian Relativism becomes emergent. *To define a logic we need not work out a specific semantic apparatus.* We need not work out the logic before the semantics. All we need to do is to provide a category for the symbols that we want to study (eventually along with a suitable structural operation) and then specify which, among the indefinitely many structures that give a homomorphic interpretation of the language, are to count as "admissible" models. Clearly, this paves the way to Tarskian Relativism (though we could also speak of Leśniewskian Relativism, or perhaps Ajdukiewiczian Relativism, since the theory of categorial grammars that licenses this line of reasoning goes back to the work of Ajdukiewicz and Leśniewski[20]). And it paves the way to Carnapian Relativism, too. For, as we have seen, there is more than one way of selecting the models that fix the meaning of a given logical term, and each way will deliver a different logical theory.

It is still worth stressing that this perspective is supported only to the extent that the same account can be extended to a significant plurality of logics: not only propositional logics or kindred systems whose algebraic structure is easily exploited, but also systems of greater complexity. In this regard, the crucial point is that the entire framework is based on a strong principle of "functional application": For every model of any language, the value of the result of applying a functor $x$ to an argument $y$ is always the result of applying the value of $x$ to the value of $y$. We saw that there is no other bridge between a language and its models except this simple exploitation of their structural homomorphism, and it is in this sense that no logic is imposed on the semantics *from the outside*. However, there may still be room for skepticism. The claim that no logic is imposed from the outside is unproblematic if we consider such functors as connectives or predicates. These are intrinsically applicative operators, and they lend themselves naturally to the kind of modeling illustrated above. In this regard, the standard practice of specifying the meaning of connectives through the recursive definition of truth is really just a different way of doing the same thing. But can this be generalized to all other operators as well? Is functional application combined with some type assignment all we need to set up the space of all possible interpretation structures?

One need not look far to see linguistic structures that seem to run afoul of the functor/argument scheme. A familiar example is provided by languages with variable-binding operators such as the quantifiers (either standard or generalized). Ajdukiewicz himself concluded his seminal paper on "Syntactic Connexion"

[20] See Leśniewski [1929] and Ajdukiewicz [1935].

with some remarks to the effect that such operators are not (and cannot be treated as) genuine functors, and consequently that languages involving them require at least an additional "circumflex" operation (essentially a form of λ-abstraction). In fact, he went as far as conjecturing that this could be the only necessary departure from the paradigm of a pure categorial grammar:

> Should . . . it be decided to smuggle the circumflex operator in, we would permit ourselves the suggestion that this subterfuge might well pay, for it is possible that all other operators . . . might be replaced by the circumflex operator and by corresponding functors.[21]

This is indeed an interesting anticipation of the ideas behind Church's λ-calculus;[22] but of course the advantage of restricting all operators to one kind does not diminish the theoretical importance of the departure from the pure functor/argument paradigm. More generally, starting from the Sixties various authors have argued that pure categorial grammars are essentially equivalent to context-free phrase-structure grammars, hence subject to the same severe limitations.[23] Others have argued that there is a strong connection between the principles of λ-abstraction and those transformation-like rules that seem so necessary to bring out the relations between different levels of linguistic analysis, e.g., between deep logical structure and surface realizations. For instance, Cresswell conjectured that all "semantically significant" transformational derivations can be seen as sequences of λ-conversions.[24] Also Montague grammars are typically seen in this light.[25] As a result, the question of whether a simple abstract model-theoretic apparatus like the one outlined above meets the requirement of generality is commonly accorded a negative answer. In particular, λ-equipped languages are seen as a necessary extension of pure categorial languages. And since such languages are commonly given a mixed Tarskian-categorial semantics (in the sense that the intended meaning of the λ-operator is fixed during a recursive definition of the value of an expression rather than specified directly by the models, in analogy to the way quantifiers are dealt with in a standard Tarskian definition of truth for first-order languages), it would seem that *some* logic must explicitly be imposed on the semantic machinery from the outside, unless we confine our-

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[21] Ajdukiewicz [1935], p. 231.

[22] Church [1941].

[23] Compare Bar-Hillel et al. [1960].

[24] Compare Cresswell [1977], pp. 266-67.

[25] See Montague [1970]. Still another example is Henkin's [1975] formulation of the (simple) theory of types, which embodies abstraction and equality as the sole primitive notions.

selves to very simple and expressively poor languages. Thus Tarskian Relativism would fall prey of the third objection of Section 2 after all, in spite of its apparent success in a number of special cases.

I reply that this is a hasty conclusion.[26] Syntactically, there is no real difficulty in squeezing variable-binders into the functor/argument scheme. For instance, a quantifier can be treated as a symbol of type $N/(S/S)$, i.e., as a “mixed” functor taking names and sentences into sentences. Even better, we can simply treat it as a kind of “structured” connective of type $S/S$, consisting of a quantifier-marker (e.g.,‘ ’) together with a corresponding bound variable. This is not uncommon even in standard logic textbooks.[27] We would then have, for instance, a universal quantifier ‘ $x$’, a universal quantifier ‘ $y$’, and so on, one for each variable: symbols are atomic relative to the syntactic operation $g$, but may still be internally structured. Let us follow this second alternative. Formally this means that an elementary language is simply a language $L = (s, g, E)$ with symbols of type $t/(t/(\ldots/(t/t')\ldots))$ for $t,t'$ $\{S, N\}$—i.e., sentence and name symbols (of type $S$ and $N$ respectively), connectives (of type $S/(S/(\ldots/(S/S)\ldots)))$, predicates (of type $N/(N/(\ldots/(N/S)\ldots)))$, and so on. Where Q is any quantifier-marker, e.g., the usual sign for universal quantification , we may then assume that the name symbols of $L$ include a denumerable subset $V$ so that the string Q$v$ is a monadic connective for each $v$ $V$, to be thought of as the Q-quantifier binding $v$.

So the syntax is straightforward. The difficulty is semantic, and it is conceptually tied to the above-mentioned fact that such symbols cannot be regarded as logical terms simply by keeping their denotation constant from model to model, for their intended meaning *depends* on the models’ make up. In particular, it is obvious that quantifiers cannot be reduced to operations on truth-values, like ordinary truth-functional connectives. However we need not do that. Truth-values are the extensions of sentences, if we like; but quantifiers introduce an *intensional* element—they make the value of a sentence depend on factors other than just the truth-values of its component parts. And we just saw that this type of dependence can easily be captured within a categorial framework. With a modal connective the intensional shift is from truth-values to truth-valued functions defined on possible worlds. With a quantifier the shift is due to a different combination of factors, namely the various values that can be assigned to the corresponding bound variables. But the shift is conceptually analogous. We may

[26] The facts and arguments that follow are articulated in greater detail elsewhere. See especially Varzi [1993] and [1995].

[27] See e.g. Enderton [1972].

accordingly define a model for a language with quantifiers simply by requiring that the domains of interpretation consist of functions defined on the set of such value-assignments. More precisely, where $U$ is any non-empty set, we obtain a model $M=(d,h,I)$ for a first-order language $L=(s,g,E)$ by setting $I_S=2^{U^V}$ and $I_N=U^{U^V}$. Then it is easy to spell out the rest of the semantics. If $M$ is stratified, for example, the interpretation conditions of classical logic are as follows:

(10) If $s_i \quad V$, then $d_i$ is constant, i.e., $d_i(a)=d_i(b)$ for all $a,b \quad U^V$.
If $s_i \quad V$, then $d_i$ is $i$-variable, i.e., $d_i(a)=a(s_i)$ for all $a \quad U^V$.
If $s_i = \sim$, then $d_i(x)(a)=1-x(a)$ for all $x \quad I_S$ and $a \quad U^V$.
If $s_i = \quad$, then $d_i(x)(y)(a)= x(a)\cap y(a)$ for all $x,y \quad I_S$ and $a \quad U^V$.
If $s_i = \quad v$, then $d_i(x)(a)=\bigcap\{x(a[^v_u]): u \quad U\}$ for all $x \quad I_S$ and $a \quad U^V$.

(In the last clause, $a[^v_u]$ is the function that is exactly like $a$ except that its value at $v$ is $u$[28].)

Of course, if we have both quantifiers and modalities, we need both possible worlds and value-assignments. The generalization is obvious. Moreover, the same treatment can be applied to provide an account of any type of variable binding operator, including the λ-operator. Variable-binders are intensional operators, and intensional operators admit of a natural (albeit perhaps not obvious) treatment within the functor/argument scheme.[29] The details are in the appendix. So here is my conclusion. In spite of the appearances, and in spite of Ajdukiewicz's own misgivings, the basic machinery outlined above does allow us to treat the semantics of every bit of language in the same fashion, as something to be handled *within* the system of models rather than via rules *outside* it. And in this sense the third objection of Section 2 is fully discarded.

## 5. Generalizations

This concludes the technical point, which together with the our discussion of Section 2 should establish the claim that the distinction between logical and ex-

[28] It is understood that the values of functional application should not depend on value-assignments unless the arguments do, i.e., one should have $x(y)(a_i)=x(y)(a_j)$ whenever $x(a_i)=x(a_j)$ and $y(a_i)=y(a_j)$. Also, such values should behave coherently, so that $x_i(y)(a)=x_j(y)(a)$ if $x_i(a)=x_j(a)$, and $x(y_i)(a)=x(y_j)(a)$ if $y_i(a)=y_j(a)$.

[29] To my knowledge, the intensional character of variable-binders was first pointed out in Lewis [1970], though the 1986 Postscript marks a change of view recommending to treat variable-binding *outside* the categorial framework, in the spirit of Cresswell [1973].

tra-logical terms is ultimately ungrounded, hence the claims leading to what I have called Tarskian Relativism. It also establishes the claim leading to Carnapian Relativism. For as we have seen, once the first sort of relativism is accepted, the second follows. Much of this should come as no surprise if one is already familiar with other ways of dealing with these matters in a uniform way. Algebraic models, for instance, provide an analogous way of looking at things "from above", as it were, before deciding which logic to choose. And the generalization of Boolean algebras to cylindric algebras is somewhat similar to the generalization outlined above when it comes to treating quantifiers and other variable-binders by means of pure functorial models. The approach that I have chosen is but one available option. (This is again a sign of the pluralism implicit in (1).)

One question could still be asked at this point: Is this general semantic outlook general enough to support a *fully* relativist position with regard to logic? Note that the argument given so far is not an argument to establish relativism on independent grounds. The direction of the argument is from semantics to logic, and much therefore depends on how one sees (1) to begin with—a starting point that I have not even questioned here. All the same, this general outlook immediately loads opposite views with a threat of inconsistency or circularity. In *The Concept of Logical Consequence*,[30] for instance, John Etchemendy has argued against the adequacy of a semantic account of logical properties on the grounds that such an account converts logical issues into substantive matters. For example, on a semantic account, a finitist would have to rule out models with infinite universes. Thus a finitist would be committed to the existence of some *n* such that the sentence "there exist fewer than *n* objects" is a logical truth, whereas a non-finitist would not. But a finitist and a non-finitist may well disagree on the philosophy of mathematics while perfectly agreeing on logic. *Ergo* the semantic account is inadequate.

There is no doubt that this conclusion is opposite to the one defended here. Yet the argument can be resisted. In fact, from the present perspective the argument appears to beg the question. For to assume that the discrepancy between the finitist and the non-finitist is not logical is to assume what is being contested. As Manuel García-Carpintero has observed:

> The finitist must disagree with our semantics. And it is far from clear that *this* is not a logical disagreement. When defenders of finitism actually provide an alternative semantics for quantifiers, it does involve logical disagreement.[31]

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[30] Etchemendy [1990].
[31] García-Carpintero [1993], p. 121.

The general semantic outlook offered above does not provide a definite proof of this view. It does, however, supply the background and the formal machinery required to support it.

So much for my bias towards a semantic account. This is enough to justify a form of semantic "conventionalism" according to which the demarcation of logic is ultimately a matter of conventions, and this in turns is enough to justify both forms of Tarskian and Carnapian Relativism. However there is also a stronger form of semantic conventionalism, and consequently of logical relativism, that we can now formulate. According to this stronger form, not only are the logical *symbols* of a language on a par with the other symbols; not only are the *theses* of a logical theory on a par with the theses of any other theory; according to this form of relativism no *principle* whatsoever escapes this account, not even the principles of the metalanguage. In other words: no specific logical fact would be fulfilled by the class of all the models admitted by a given language, which means that the relativism stance would be completely free from metalogical interferences. Does our general semantic perspective support this form of relativism, too? Is semantics really logic-free, or does it still involve hidden logical assumptions?

Here I have essentially two remarks to offer. First, there is no doubt that we can still *define* semantic properties in such a way as to make them invariant across models. In fact, although the notion of validity defined in (1) depends heavily on how one draws the boundary between logical and extra-logical terms, it doesn't depend exclusively on that. There are arguments that turn out to be valid even if they do not contain any logical terms whatsoever. An obvious case in point is an argument whose conclusion is included among the premises. More generally, consider the following extension of (1), which allows for arguments with multiple conclusions:

(11) A set of sentences $\Gamma$ entails a set of sentences $\Sigma$ (i.e., the argument from $\Gamma$ to $\Sigma$ is valid) if and only if some member of $\Sigma$ is true in every model in which all members of $\Gamma$ are true.

Then it is easy to see that the so-called classical "structural" rules of classical logic correspond without exception to valid argument forms ('$\models$' for 'entails'):

| | | |
|---|---|---|
| (12) | $\Sigma \models \Sigma$ | (Reflexivity) |
| | $\Gamma \quad \{\varphi\} \models \{\varphi\} \quad \Sigma$ | (Reiteration) |
| | If $\Sigma \models \Gamma$ and $\Gamma \models \Delta$ then $\Sigma \models \Delta$ | (Transitivity) |
| | If $\Gamma \models \Sigma$ then $\Gamma \quad \{\varphi\} \models \Sigma$ and $\Gamma \models \{\varphi\} \quad \Sigma$ | (Thinning) |
| | If $\Gamma \quad \{\varphi\} \models \Sigma$ and $\Gamma \models \{\varphi\} \quad \Sigma$ then $\Gamma \models \Sigma$ | (Cut) |

These argument forms are independent of the particular language at issue and they are valid irrespective of any particular stipulation concerning the class of admissible models: their validity just follows from (11). Obviously, one could revise (11) so as to get different results, but that is not the point. The point is that it is *possible* to define notions some properties of which hold regardless of which class of models we consider, including the class of all models (for a given language).

Now, this speaks against a fully relativist position. After all, *some* logic does show up in the metalanguage. On the other hand, one can easily explain this and similar facts in terms of metalinguistic conventions. The reason why the argument forms in (12) hold for any choice of models is that these argument forms reflect certain facts that we are presupposing in the very definition of logical validity. On a different interpretation of, say, the universal quantifier 'all', or of the notion of 'set' used in the definition (metalanguage), the picture might look quite different. This does not by itself justify a fully relativist position. However, it suggests that the position can be coherently maintained provided only that a relativism of the Tarskian or Carnapian sort is reiterated at each level of the metalinguistic hierarchy. (Again, I don't mean this to be just an issue of ambiguity. I mean to say that there may be genuine disagreement on the extension of such notions.)

The second remark is more critical and relates to the question of semantic generality. As I see it, there is no doubt that a full-blown relativism calls for further generalizations of the basic "metatheoretical machinery". A framework like the one outlined here embeds the requirement that every model be homomorphic to the corresponding language. This—as we saw—allows a uniform syntactic and semantic analysis. But it also reflects the assumption (typical of a Tarskian semantics) that a model must be made of well-defined, sharp-cut entities, neatly linked to one another and to the language's expressions in a univocal way. One could find this to be a serious limitation in the scope of a semantic theory. There is no *a priori* semantic reason to rule out the possibility that (our model-theoretic representation of) what we talk about may involve "gaps" and/or "gluts" of various sorts. As a matter of fact, even if we assume that the purpose of a language's expressions is to always pick out a definite semantic value, there is no a priori reason to suppose that the underlying conditions will be always *completely* fulfilled. Ordinary language sentences typically involve expressions whose intended reference is only partially defined, or vaguely defined, or not defined at all, and we may want to allow for such phenomena even in a formally reconstructed language. Conversely, even if we assume that every expression is meant to have a unique congruous semantic value, there is in fact no guarantee that the underlying conditions can be always *consistently* fulfilled. We all know, for instance, that a sentence may turn out to be self-referential in unfavourable circumstances, leading to such troubles as the liar paradox. For these reasons, a more general semantic framework, where models with interpretational gaps and/or gluts are admitted, is arguably desirable. In any case, such a generalization appears to be a necessary prerequisite from the perspective of a full-blown relativistic position, for the exclusion of incompleteness and/or inconsistency is surely a way of restricting the range of admissible models.

Without going into too many details, let me say that this question has both a positive and a negative answer. The positive answer is that the semantic framework outlined above can be generalized rather easily to cover such deviant cases. One can allow for models in which certain categories of expressions fail to be instantiated, or in which some symbols may lack a unique denotation (i.e., fail to denote or have more than one denotation), or in which the result of applying the structural operation may not always yield a definite semantic value (i.e., may be indeterminate or overdeterminate for certain arguments). Formally all of this involves allowing a model's basic components $d$ and $h$ to be partial relations rather than total functions, and this will introduce some complexities. Since there exists no homomorphism between a language and an incomplete model, and since there can be more than one homomorphism if the model is inconsistent, the semantic bridge between a language and its models is no longer a straightforward nusiness. Nevertheless it can be defined, and it can be defined without renouncing to the conceptual uniformity of the initial framework. This is the positive answer.

The negative answer is that this can be done, not in one, but in several non-equivalent ways. For instance, personally I favor a supervaluationary approach.[32] Roughly, this says that the value of an expression on an incomplete and/or inconsistent model $M$ is a function of the values that the expression takes on the complete and consistent "sharpenings" of $M$. Since the sharpenings are models that are homomorphic to the language, we can just apply there the straightforward algorithm in (6) and then compute the function that gives the valuation for $M$. The problem is that there are *many* candidate functions that could do the job, and depending on which we choose we obtain different semantics.[33] Moreover, other approaches are possible, too. For instance, there exist

[32] Along the lines of Varzi [1999]

[33] I have examined some possible accounts in Varzi [1997], [2000].

generalizations of Montague semantics in which the link between a language and its incomplete models is given by a sort of "paramorphic" valuation function that approximates, in some intuitive way, the behavior of the missing homomorphism.[34] The account can readily be imported from the original Montagovian framework into a purely categorial framework like the one considered here, and it can easily be extended to cover inconsistent models as well. But it is a different account from the supervaluationary one, and it yields considerably different semantics. Evidently this is in contrast with the radically relativist view: an abundance of generalizations is just as bad as a total lack, for it leaves open the question of how to account for the resulting variety of metalogical theories.

The same applies to other generalizations that could be considered, and which at this point I shall only mention. For instance, can we relax the type restrictions on the behavior of the structural relations? Can we generalize the notion of a model by admitting self-applicative domains? Can we allow for dynamic models, i.e., models where the value of an expression can change depending on whether we evaluate it before or after other expressions? All of these are questions that seem to introduce serious complications in the account favored here. The account requires a logic-free semantics, but the bounds of semantics don't seem to be arbitrary.

So here my conclusions will be cautious. Perhaps a radical model-theoretic relativism is really a hybrid, belonging to that category of philosophical positions that can only be consistently maintained *up to a certain point*. At the same time, one could regard the request for a logic-free semantics as a plea for a general semantic framework—a framework wherein each of a variety of semantic policies can be accommodated—and in this sense a radical relativism would be perfectly consistent: the same criteria would just apply to a semantic theory as they apply to a logical theory. In other words, a radically relativist position could be regarded as a form of Tarskian Relativism concerning semantics itself rather than logic, or, if you prefer, as a form of meta-relativism. One could then reiterate the account to accommodate stronger and stronger forms of relativism, corresponding to higher and higher levels of analysis. This move into the territory of metalanguage might appear suspicious and is surely debatable. Nonetheless it seems inescapable. My suspicion is that it might actually prove decisive, at least for a proper assessment of logical relativism from the semantic standpoint considered here.

[34] See e.g. Muskens [1995].

**Appendix** [35]

Every mode of variable binding can be reduced to functional abstraction. So in the end (and in general terms) the question examined in Section 4 is whether abstraction can be interpreted as a form of application, using models whose domains of interpretation depend on a suitable package of intensional features and value-assignments.

Some forms of abstraction are immediately captured by the treatment illustrated in the main text. For instance, we can enrich an elementary language $L$ with an abstractor $\lambda v$ for each variable $v \in V$, to be treated as a functor of type $S/(N/S)$. The ordinary interpretation of this functor is reflected in the reading "is something $v$ such that". And it is easy to verify that within the proposed framework, this reading translates into the following direct condition on the admissible models for $L$:

(13) If $s_i = \lambda v$, then $d_i(x)(y)(a) = x(a[^{v}_{y(a)}])$ for all $x \in I_S$, $y \in I_N$ and $a \in U^V$.

At least, this is the appropriate condition on the assumption that all relevant models are stratified in the sense explained above, i.e., such that every functorial domain $I_{t'/t}$ is a set of functions $f: I_{t'} \rightarrow I_t$ and $h(x,y)$ always coincides with $x(y)$.

In the general case, where we have abstractors acting on variables of any type in expressions of any type, the account is not so straightforward. In fact it is clear that we cannot go very far if we stick to stratified models, for the presence of functor variables prevents us from defining adequate intensional models where each functorial domain is a set of functions of the right sort. However, we need not do that. We only need consider models whose domains are built *upon* sets of functions — and that can be done in the appropriate way to obtain the desired result. This is a rather natural generalization, familiar from intensional logics and Montague grammars. Here are the details.

To allow for generalized abstractors, we consider a full categorial language $L$ comprising a non-empty set $S_t$ of symbols for all $t \in T$. Each $S_t$ includes a subset $V_t$ of variables so that the string $\lambda vt'$ is a symbol of type $t'/(t/t')$ for all $t' \in T$ and all $v \in V_t$. Now let $\{U_t : t \in T\}$ be a system of sets so that $U_S = 2$ and $U_{t'/t} = U_t^{U_{t'}}$ for all $t,t' \in T$ and define $A$ to be the Cartesian product $\prod\{U_t^{V_t} : t \in T\}$. To obtain an adequate model M we simply require that $I_t = U_t^A$ for all $t \in T$. We can then make sure that each $\lambda vt'$ be interpreted as a $v$-binding abstractor by requiring $M$ to also satisfy the following general condition:

[35] This appendix draws on Varzi [1993], §4.

(14) If $s_i = \lambda v t'$, then $h(h(d_i, x), y)(a) = x(a[^{t}_{a_t[^{v}_{y(a)}]}])$ for all $x \in I_{t'}$, all $y \in I_{\tau(v)}$ and all $a \in A$

where $\tau(v)$ is the type of $v$. Along with the obvious conditions on the interpretation of constant and variable symbols, it can be verified that this clause conforms to the usual principles of the classical λ-calculus.[36]

## References


Ajdukiewicz, K., 1935, 'Die syntaktische Konnexität', *Studia Philosophica* 1: 1–27; Eng. trans. by H. Weber: 'Syntactic Connexion', in S. McCall (ed.), *Polish Logic 1920–1939*, Oxford: Clarendon Press, 1967, pp. 207–231.

Bar-Hillel, Y., Gaifman C., Shamir E., 1960, 'On Categorial and Phrase-Structure Grammars', *The Bulletin of the Research Council of Israel* 3: 1–16.

Beall, J. C., and Restall, G., 2000, 'Logical Pluralism', *Australasian Journal of Philosophy* 78: 475–493.

Carnap, R., 1934, *Logische Syntax der Sprache*, Vienna, Springer-Verlag; expanded Eng. trans. as *The Logical Syntax of Language*, London: Routledge and Kegan Paul, 1937.

Church, A., 1941, *The Calculi of Lambda Conversion*, Princeton: Princeton University Press.

Cresswell, M. J., 1973, *Logics and Languages*, London: Methuen.

Cresswell, M. J., 1977, 'Categorial Languages', *Studia Logica* 36: 257–269.

Enderton, H. B., 1972, *A Mathematical Introduction to Logic*, Orlando (FL): Academic Press.

Etchemendy, J., 1990, *The Concept of Logicl Consequence*, Cambridge: Harvard University Press.

García-Carpintero, M., 1993, 'The Grounds for the Model-Theoretic Account of the Logical Properties', *Notre Dame Journal of Formal Logic* 34: 107–131.

Garrett, B. J., 1985, 'Noonan, "Best Candidate" Theories, and the Ship of Theseus', *Analysis* 45:, 12–15.

Gómez-Torrente, M., 2002, 'The Problem of Logical Constants', *Bulletin of Symbolic Logic* 8: 1–37.

Henkin, L., 1975, 'Identity as a Logical Primitive', *Philosophia* 5: 31–45.

Husserl, E., 1900/01, *Logische Untersuchungen. Zweiter Band. Untersuchungen zur Phänomenologie und Theorie der Erkenntnis*, Halle: Niemeyer (2nd ed. 1913; Eng. trans. by J. N. Findlay, *Logical Investigations, Volume Two*, London: Routledge & Kegan Paul, 1970).

Lewis, D. K., 1970, 'General Semantics', *Synthese* 22: 18–67.

Lewis, D. K., 1986, 'Postscript to "General Semantics"', in *Philosophical Papers. Volume 2*, Oxford: Oxford University Press, pp. 230–232.


[36] An old ancestor of this paper was first presented at the Symposium on *Meaning* held in Karlovy Vary, in the Czech Republic (September 9, 1993), and appears with the title "Model-Theoretic Conventionalism" in the proceedings of the symposium (James Hill and Petr Koťátko, eds.) Later versions have been presented at a *Logic Colloquium* in the Department of Philosophy of SUNY Buffalo (March 5, 1998) and at the section on "Logical Pluralism" held at the *Australasian Association of Philosophy Conference* (Hobart, Australia, July 4, 2001). I am thankful to all audiences for useful comments and discussion.

Leśniewski, S., 1929, 'Grundzüge eines neuen Systems der Grundlagen der Mathematik', *Fundamenta Mathematicae* 14: 1–81; Eng. trans. by M. P. O'Neil: 'Fundamentals of a New System of the Foundations of Mathematics', in S. Leśniewski, *Collected Works* (ed. by S. J. Surma, J. T. Srzednicki, D. I. Barnett, and V. F. Rickey), Dordrecht: Kluwer Academic Publishers, 1992, Vol. 1, pp. 129–173.

Lindenbaum, A., and Tarski, A., 1934/35, 'Über die Beschränktheit der Ausdrucksmittel deduktiver Theorien', *Ergebnisse eines mathematischen Kolloquiums* 7: 15–22; Eng. trans. by J. H. Woodger: 'On the Limitations of the Means of Expression of Deductive Theories', in A. Tarski, *Logics, Semantics, Metamathematics*, *Papers from 1923 to 1938*, Oxford: Clarendon Press, 1956 (2nd edition ed. by J. Corcoran, Indianapolis: Hackett, 1983), pp. 384–392.

Lindström, P., 1966, 'First Order Predicate Logic with Generalized Quantifiers', *Theoria* 32: 186–195.

McCarthy, T., 1981, 'The Idea of a Logical Constant', *Journal of Philosophy* 78: 499–523.

Montague, R., 1970, 'Universal Grammar', *Theoria* 36: 373–398.

Mostowski, A., 1957, 'On a Generalization of Quantifiers', *Fundamenta Mathematicae* 44: 12–36.

Muskens, R., 1995, *Meaning and Partiality*, Stanford (CA): CSLI Publications.

Quine, W. V. O., 1970, *Philosophy of Logic*, Englewood Cliffs (NJ): Prentice-Hall.

Sher, G., 1991, *The Bounds of Logic. A Generalized Viewpoint*, Cambridge (MA): MIT Press/Bradford Books.

Sher, G., 1999, 'Is Logic a Theory of the Obvious?', in A. C. Varzi (ed.), *The Scope of Logic*, Stanford (CA): CSLI Publications, pp. 207–238.

Schönfinkel, M., 1924, 'Über die Bausteine der mathematischen Logik', *Mathematische Annalen* 92: 305–316; Eng. trans. by S. Bauer-Mengelberg: 'On the Building Blocks of Mathematical Logic', in J. van Heijenoort (ed.), *From Frege to Gödel: A Sourcebook in Mathematical Logic, 1879–1931*, Cambridge (MA): Harvard University Press, 1967, pp. 355–366.

Tarski, A., 1936, 'O pojciu wynikania logiczneg', *Przegląd Filozoficzny* 39: 58–68; Eng. trans. by J. H. Woodger: 'On the Concept of Logical Consequence', in A. Tarski, *Logics, Semantics, Metamathematics*, *Papers from 1923 to 1938*, Oxford: Clarendon Press, 1956 (2nd edition ed. by J. Corcoran, Indianapolis: Hackett, 1983), pp. 409–420.

Tarski, A., 1986, 'What Are Logical Notions?', text of a 1966 lecture ed. by J. Corcoran, *History and Philosophy of Logic* 7: 143–154.

Varzi, A. C., 1993, 'Do We Need Functional Abstraction?', in J. Czermak (ed.), *Philosophy of Mathematics. Proceedings of the 15th International Wittgenstein Symposium*, Part 1, Vienna: Hölder-Pichler-Tempsky, pp. 407–415.

Varzi, A. C., 1995, 'Variable-Binders as Functors', in J. Woleński and V. F. Sinesi (eds.), *The Heritage of Kazimierz Ajdukiewicz*, Amsterdam and Atlanta (GA): Rodopi, pp. 303–319.

Varzi, A. C., 1997, 'Inconsistency Without Contradiction', *Notre Dame Journal of Formal Logic* 38: 621–639.

Varzi, A. C., 1999, *An Essay in Universal Semantics*, Dordrecht: Kluwer Academic Publishers.

Varzi, A. C., 2000, 'Supervaluationism and Paraconsistency', in D. Batens, C. Mortensen, G. Priest, and J.-P. Van Bendegem (eds.), *Frontiers in Paraconsistent Logic*, Baldock: Research Studies Press, pp. 279–297.