we know that truth is a property. If 'true' has a role only in generalization and in disquotation, then a definition may not be to the point. Consider the case of pronouns: 'he' is used for generalizing, but how might we go about defining it? Other approaches would seem more promising in explanations of how such linguistic devices work.

Then there is David's positive thesis. He argues that the correspondence theory fares better than the disquotational theory with respect to the challenges he identifies. Perhaps, but not on David's version of the correspondence theory. The two clauses in (R), p. 31, provide the definition (my emphasis): (i) x is a true sentence $=_{\mathrm{Df}}$ x is a sentence, and there is a state of affairs y such that x represents y and y *obtains*; (ii) x is a false sentence $=_{\mathrm{Df}}$ x is a sentence, and there is a state of affairs y such that x represents y and y does *not obtain*.

This definition is unfortunately worthless: for without an explanation of 'obtains' it must be supposed circular. Some will say that states of affairs that obtain are facts, and facts are those states of affairs that are designated by true sentences. Others may argue that 'obtains' is used to provide expressibility similar to that which many deflationists claim 'true' provides: the job of 'obtains' is to turn a term or noun phrase into something with propositional form—a useful maneuver when generalizing and in other places.

David seems to suggest further that unless disquotationalists give up their rejection of a substantive correspondence theory, they have no account of language. This argument is tied to David's assumption that a definition of truth is needed. But it is only correspondence theorists who think a theory of language must be incorporated in an account of truth. As Horwich points out, deflationists can separate the issues. And consider Quine who has three issues "separated": the logic and role of 'true' (generalizing and disquotational roles), language (stimulus meaning and holism), and truth (Quine's possible "sectarian" position on "what is true" e.g. on answers to questions like, 'Do electrons have mass?'). David also has the option of separating off the language part for further study; for on his preferred kind of representational account of language, a sentence (e.g. 'snow is red') represents the same possible state of affairs (snow's being red) whether it happens to be true or false. DOROTHY GROVER

Modality, morality, and belief, Essays in honor of Ruth Barcan Marcus, edited by Walter Sinnott-Armstrong, Diana Raffman, and Nicholas Asher, Cambridge University Press, Cambridge, New York, and Oakleigh, Victoria, 1995, xiii + 270 pp.—therein:

TERENCE PARSONS. *Ruth Barcan Marcus and the Barcan formula.* Pp. 3–11.

ROBERT STALNAKER. *The interaction of modality with quantification and identity.* Pp. 12–28.

MAXWELL J. CRESSWELL. *S1 is not so simple.* Pp. 29–40.

DAVID KAPLAN. *A problem in possible-world semantics.* Pp. 41–52.

CHARLES PARSONS. *Structuralism and the concept of set.* Pp. 74–92.

The volume containing the papers under review is a festshrift for Ruth Barcan Marcus, whose ground-breaking work on quantified modal logic appeared in this JOURNAL in 1946 and 1947. The volume contains fifteen newly published essays, all by prominent philosophers. The essays are divided (loosely) into three sections —*Modality*, *Morality*, and *Belief*—corresponding to three areas in which Marcus has made her mark. Overall, the quality of the essays is quite high, and the volume aptly attests to the breadth and abiding influence of Marcus's work. This review covers five of the essays most relevant to logic.

In the lore of possible-world semantics, there is a foundational puzzle much discussed both in and out of print, and generally attributed to David Kaplan circa the mid-seventies. In *A problem in possible-world semantics*, we have (at last!) David Kaplan's own presentation and discussion of the puzzle. The paper is valuable both for its suggestive discussion of the relation between possible-world semantics and logic, and because Kaplan's version of the puzzle, it turns out, is more general (and so potentially more troubling) than the versions recounted and replied to in print.

The puzzle is this. Suppose we add to ordinary quantified modal logic propositional variables (and quantifiers), and a non-logical constant 'Q' to be interpreted as an intensional sentential operator (in effect, a property of propositions). Then, the following formula, it seems, is logically consistent, and should be satisfied by some model: (A) $\forall p \Diamond \forall q(Qq \leftrightarrow q = p)$. (*Perhaps*, Kaplan suggests, (A) is true with 'it is queried that' substituted for 'Q'. Then (A) says: for any proposition, possibly, only that proposition is queried.) But (A) is unsatisfiable, given standard assumptions about models (and propositions) in possible-world semantics. The problem, roughly, is that there are (at least) as many propositions as there are *classes* of possible worlds; so, by Cantor's argument, there cannot be, for each proposition p, a possible world at which p, and only p, has Q. The problem is inherent to the framework of possible-worlds semantics: no matter the nature or number of possible worlds, some intuitive possibilities, it seems, are left out.

If 'Q' expresses a propositional or sentential attitude, however, there is a plausible argument for rejecting (A). Not all propositions are eligible to be the content of thought or of speech; for example, a proposition that is wildly infinitarily disjunctive could not be the only proposition thought or uttered because it could not be thought or uttered at all. (See David Lewis, ***On the plurality of worlds***, Blackwell, 1986, §2.3, who defends this by invoking broadly functionalist definitions of the attitudes.)

But Kaplan does not rest his case on any particular interpretation of 'Q'. He thinks there are independent grounds for holding that (A) is logically consistent, arguing something like this. Logic should be neutral with respect to metaphysics; otherwise only one side of a metaphysical dispute could be consistently expressed. Such neutrality requires that (A) be consistent. For if (A) is contradictory, then properties of propositions cannot vary independently of the first-order properties of things; there is supervenience (of a sort) of the "intensional" on the "extensional." Logic, then, decides a substantive claim of supervenience, and is not metaphysically neutral.

This argument, I think, can be resisted, and the consistency of (A) denied. (The offhand inclination to think (A) is consistent, I suspect, comes from taking the propositional quantifiers to range over a restricted class of propositions.) The defender of the possible-world framework should not, and need not, allow Kaplan to drive a wedge between logic and metaphysics (on its broadest construal). The metaphysical framework of possible worlds *is* the framework of logic, in terms of which all logical notions are ultimately to be characterized. But, beware. This defense may require a more expansive conception of "world" than is usual: for any pair of logically non-equivalent propositions (including "non-contingent" propositions!) there must be a "world" at which one, but not the other, is true. On this conception, supervenience relations that fall out of the possible-world framework are relations of logical entailment, and holding that (A) is contradictory does not sacrifice the neutrality of logic.

Kaplan suggests, in closing, a different reply. "I have tried to show that naive possible-world semantics leads to a kind of paradox just as naive set theory does, and by means of a similar argument. I also suspect that the ultimate lesson is somewhat the same, namely that the fundamental entities must be arranged in a never completed hierarchy and cannot be taken to be given all at once" (p. 47). Is this a path to the vindication of (A)? Not really. It would allow restricted versions of (A) to be consistent, with propositional quantifiers restricted to some level of some hierarchy. But, one way or another, (A) with quantifiers wide open has got to go.

In *Ruth Barcan Marcus and the Barcan formula*, Terence Parsons mounts an unusual defense of the Barcan formula, '$\forall x \Box S \rightarrow \Box \forall x S$', and its converse, '$\Box \forall x S \rightarrow \forall x \Box S$', where '$\Box$' is unrestricted, alethic modality. He does not claim that these formulas are logically valid—indeed, they are not valid on the standard (Kripkean) semantics according to which domains are allowed to vary from world to world, and quantifiers are "actualist," ranging only over the domain of the world under evaluation. Parsons claims instead (treating the formulas as schemata) that "every instance ... is true." His case turns on how instances are to be evaluated for truth and falsity, and on what to count as an admissible instance.

First, Parsons takes up the standard putative counterexamples to the converse Barcan formula, according to which 'S' is 'x exists' or 'x is (identical with) something.' His method is to evaluate the truth or falsity of these instances, not by way of a formal semantics, but informally, by making use of ordinary English intuitions. He claims that (on their most natural readings?) these putative counterexamples fail: when 'S' is 'x exists,' the antecedent is false; when 'S' is 'x is something,' the consequent is true. Perhaps. But, even if we grant that counterexamples must be evaluated in ordinary English, I do not think that Parsons has made his case. For an instance to be a counterexample in English, it suffices for there to be *at least one* reading accessible to English speakers on which the instance is false; it matters not whether the reading is the most natural. Both the above substitutions for 'S', it seems to me, meet this criterion.

Next, Parsons takes up the standard putative counterexample to the Barcan formula, according to which 'S' is 'x is (identical with something) actual,' where 'actual' is interpreted rigidly: it picks out things in the domain of the actual world, even when it occurs embedded in a modal context. The resulting instance, 'if everything necessarily is (identical with something) actual, then necessarily everything is (identical with something) actual,' Parsons allows, is false, with 'actual' rigidly interpreted. But the counterexample is inadmissible, Parsons holds, because 'actual' is "a logically special" predicate that "transcend[s] the framework within which the formulas were originally proposed" (p. 7). Is Parsons here requiring that counterexamples be couched entirely within the logical vocabulary of quantified modal logic? If the question is the *truth* of (instances of) the Barcan formula, it is hard to see how that restriction could be justified. Parsons concludes, "If the only way to conclusively refute the Barcan formula is to expand the notation of modal logic . . . , then maybe there is something deeply true about what is under attack" (p. 11). On the contrary, that counterexamples cannot be expressed within some restricted vocabulary would seem to provide a rather superficial defense. A deeper (though problematic) defense—as Marcus herself has urged—would go by way of the "actualist" thesis that the domain of every world is (somehow) included within the actual domain.

In *The interaction of modality with quantification and identity*, Robert Stalnaker takes a conceptual scalpel to quantified modal logic, illuminating how it relates to the propositional modal logic and extensional predicate logic that it jointly generalizes. First, he formulates the two base logics so that, when generalized, no axioms or rules of inference need to be taken back. To this end, he develops a *free* predicate logic (thus allowing for empty domains and non-denoting singular terms), and he restricts the substitutivity of identicals to *predications* (which, for Stalnaker, may contain complex predicates formed with an abstraction operator).

The centerpiece of Stalnaker's paper, I think, is the development of a counterpart semantics that, because it is neutral with respect to how modality interacts with quantification and identity, validates just those theorems that follow from the axioms and rules of the two base logics, and nothing more. In particular, the qualified converse Barcan formula, '$\Box\forall x\varphi \rightarrow \forall x\Box(Ex \rightarrow \varphi)$' (where '$E$' is the predicate of existence), and the necessity of distinctness, '$\forall x\forall y(x \neq y \rightarrow \Box x \neq y)$', which are both valid in standard quantified modal logic, are shown to be invalid on Stalnaker's semantics. What allows the semantics to be neutral is this: unlike standard counterpart semantics (of David Lewis), whether an individual satisfies a predicate at a world always depends on the counterpart of the individual at the world, even for a complex modal predicate, so that the interpretation of variables is unaffected by the presence of modal operators. For example, on Stalnaker's semantics, whether an actual individual a satisfies '$\hat{x}\Box\Box Px$' depends only on what a's counterparts satisfy at appropriate worlds; on standard counterpart semantics, it depends on what a's counterparts' *counterparts* satisfy. Stalnaker (wisely) does not claim that neutral counterpart semantics gives correct truth conditions for ordinary modal sentences. Rather, it is a valuable logical and conceptual tool for disentangling modality and quantification.

I conclude with brief summaries of two further articles that may be of interest to readers of this JOURNAL. In *S1 is not so simple*, Maxwell Cresswell presents a new completeness proof for the non-normal modal logic S1 using canonical models. Cresswell's semantics for S1 combines an ordinary relational semantics applied at "normal" worlds, and a neighborhood semantics applied at "non-normal" worlds. Various authors have claimed that a pure relational semantics can be developed for S1, and for logics in the vicinity of S1; in particular, some have claimed that the modal operator L can be interpreted to mean "it is impossible that" at "non-normal" worlds. Cresswell examines these claims, and finds them wanting. S1, he maintains, still lacks a plausible interpretation, and remains primarily of historical interest.

"The structuralist view of mathematical objects," writes Charles Parsons, "holds that reference to mathematical objects is always in the context of some structure, and that the objects involved have no more to them than can be expressed in terms of the basic relations of the structure" (p. 74). In *Structuralism and the concept of set*, Parsons defends the viability of structuralism about sets against the following apparent threat. Mathematicians and philosophers, in providing "explanations" of the concept of set and "intuitive" justifications of the axioms of set theory, typically call upon various "ontological" features—such as the ontological priority of members to their sets—that go beyond the purely structural features posited by the theory. In an interesting and wide-ranging discussion, Parsons surveys various explanations and justifications provided by mathematicians and philosophers, concluding that ontological features do not play an essential role in the development of ZF, and need not be taken to be part of the literal truth about sets. PHILLIP BRICKER

Probability and conditionals, Belief revision and rational decision, edited by Ellery Eells and Brian Skyrms, Cambridge studies in probability, induction, and decision theory, Cambridge University Press, Cambridge, New York, and Oakleigh, Victoria, 1994, viii + 207 pp.—therein:

PATRICK SUPPES. *Some questions about Adam's conditionals*. Pp. 5–11.

BRIAN SKYRMS. *Adams conditionals*. Pp. 13–26.

ROBERT STALNAKER. *Letter to Brian Skyrms*. Pp. 27–29.