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Theoretical Equivalence and the Semantic View of Theories

Clark Glymour*

Halvorson argues through a series of examples and a general result due to Myers that the "semantic view" of theories has no available account of formal theoretical equivalence. De Bouvere provides criteria overlooked in Halvorson's paper that are immune to his counterexamples and to the theorem he cites. Those criteria accord with a modest version of the semantic view that rejects some of Van Fraassen's apparent claims while retaining the core of Patrick Suppes's proposal. I do not endorse any version of the semantic view of theories.

1. Introduction. Halvorson (2012) argues that the "semantic view" of theories (Suppes 1967; Van Fraassen 1980) has, and can have, no account of theoretical equivalence—indeed, it "fails miserably" to be amenable to any such account. He considers three model-theoretical criteria for theoretical equivalence—identity, equicardinality of model classes, and 1:1 mapping of models of one class to isomorphic models of the other class. He produces counterexamples to each of them and claims the three possibilities exhaust the plausible alternatives. I have no brief for the semantic view, but whatever its faults, Halvorson's objections on this score need not be among them. The reasons reveal aspects of the semantic view that some of its advocates appear to have contradicted, or at least chosen not to display, and suggest that, despite the rhetoric of its advocates, in important respects a tenable version of the semantic view is not much different from the "syntactic" view that, whatever more it may be, a theory is something that is said in a language.

Halvorson also criticizes Van Fraassen's claims that for philosophical purposes model theory has more explanatory resources than logical syntax,

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but I shall not be concerned with those comments. Halvorson's broader point is that the rhetoric of the semantic view regrettably eschews the full resources of logic and mathematics that are needed to understand the properties of theories, and I agree, but I will not elaborate. My argument is entirely with his claim that no plausible account of the structural equivalence of theories is available to the semantic view that defeats his criticisms. Such an account has been available for almost half a century.

2. Syntax and the Semantic View of Theories. Halvorson's strategy is to examine three model-theoretical mappings that might correspond to interdefinability relations of syntactical objects/sentences or formulas, and, finding that all three succumb to his counterexamples, he concludes that no account of theoretical equivalence is possible on the semantic view of theories. I will dispute Halvorson's contention that his counterexamples refute all possible accounts of theoretical equivalence compatible with the semantic view, but first two lacunae in his argument need to be addressed, gaps that might be thought to invalidate his argument (and mine) without the necessity of considering the details of his examples or offering an alternative to his three possible criteria for theoretical equivalence. I think one of the gaps can be filled, and the other is sufficiently vague that it is worth considering Halvorson's arguments without trying fully to fill the hole.

First, it can be objected that Halvorson's strategy begs the question against the semantic view. The objection goes like this: many of Halvorson's examples are model classes characterized as the models of a first-order theory in the ordinary linguistic sense of "theory," and he requires that different formulations of the same theory be "interdefinable" by a mutual interpretability relation. But according to the semantic view, a theory just *is* a class of relational structures, and therefore interdefinability relations among syntactical objects are irrelevant.

That is not among my objections to Halvorson's arguments because I think the objection is quite wrong, obviously wrong, for the following reasons: On the semantic view, to present a theory is to specify a class of relational structures. That can only be done through a description, in a language, of the class of structures.

Patrick Suppes (1967), for example, says that a theory is a set-theoretical *predicate*, and a predicate is something linguistic. Van Fraassen seems to take exception: "The impact of Suppes' innovation is lost if models are defined, as in many standard logic texts, to be partially linguistic entities, each yoked to a particular syntax. In my terminology here the models are mathematical structures, called models of a given theory only by virtue of belonging to the class defined to be the models of the theory" (1989, 366). Other writers (e.g., Lloyd 1994), perhaps influenced by Van Fraassen, suggest that model classes are directly given without the use of a language. What they and

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Van Fraassen, unlike Suppes, say cannot be taken entirely seriously because it yields a conception of theories that makes them ineffable and their characterization magical. Newton, Einstein, Schrodinger, and so on, had no way of specifying the class of relational structures they intended, except indirectly, as those structures satisfying their theoretical claims. Nor do we today.[1] The "yoke" to language may be neglected, but it remains.

Further, were the class of models so free of language that relations in two models cannot be understood to be denotata of the same predicate (or, what amounts to the same, realizations of the same property in different structures), some forms of prediction would be impossible, in particular, the use of a theory and the results of one experiment to predict what would happen in another experiment with different conditions but the same, or some of the same, measured quantities. Suppose a theory has multiple models, one of which is $\langle D_1;\ \phi,\ \chi,\ \varphi\rangle$, and there is a data model that maps three measured quantities to the three relations. To predict what would happen were the conditions of measurement changed, another model, say $\langle D_2;\ \tau,\ \delta,\ \omega\rangle$, must be chosen and the measured quantities mapped to relations of that model, but the radical semantic view has insufficient resources to specify which relations among τ, δ, ω in this second model correspond to ϕ, χ, φ, respectively. Prediction of this kind requires a set of privileged maps of relations to relations across models of a theory, which is exactly what denotation by a common predicate provides.

To be at all plausible, the semantic view must distinguish between the content of a theory (its class of models) and the means of characterizing that class—the theory expressed in some language. That specification is only possible if predicates in the language denote specific relations in each model for the theory. Otherwise the very notion of a theory—even construed as a complex predicate—specifying the class of models it is true of or satisfied by makes no sense. Advocates of the semantic view implicitly agree in giving examples implying that the models constituting the content of a theory are described by open formulas or equations, often with the qualifying conditions of the usual formulations of the theory removed. Thus, Lloyd (1994) describes population genetics models by various sets of equations, and Van Fraassen describes theories as given by dynamic laws among variables ranging in value over some "state space," so that, for example, the models of Newtonian gravitational theory are described as the solutions to the dynamical equations, eliminating force between the second law of motion and the gravitational force law, with the third law as a constraint. Equations are sentences

1. First-order theories have a Lindenbaum algebra, and Makkai and Reyes (1977) provide category-theoretical representations of first-order theories. I do not know a general way to produce descriptions of a class of models with either kind of object without beginning with a theory in the ordinary sense.

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or formulas in a language. (If the models were construed to be relational structures that satisfy claims with qualifiers or suppositions such as "If all forces are gravitational . . ." or "If all nongravitational forces cancel . . . ," the model structures would have to be more elaborate than Van Fraassen proposes, but my point would remain.)

The space of models of, say, Newtonian theory cannot be directly indicated by pointing or looking in a magic closet or through a magical looking glass. Muggle science has no Diagon Alley. Like it or not, on the semantic view, language and logical syntax are indispensable tools for the presentation of theoretical content, and that being so, there is no reason why proposed syntactical equivalence relations corresponding to model-theoretical equivalence relations should not be considered and no reason why the fact that relations in different models are co-denoted by the same expression in a language should not be taken into account. It is a familiar point that one and the same theory can be specified in different languages, and we expect some interdefinability relations between the sentences of the theory in two or more such presentations. A tenable semantic view, as I construe it, also requires that there is a model-theoretical relation that establishes a common content to different presentations of a theory in different languages. Invariance over alternative linguistic presentations is, of course, not the same as having no suppositions about relations between structures and languages; it is not the same as being entirely a-lingual. It is a weaker requirement but one that I will argue can be met. Ideally, equivalences characterized by interdefinability of languages and theories would perfectly correspond with the appropriate model-theoretical equivalences, and I will argue that that, too, can be satisfied. Halvorson is correct in assuming that what the semantic view requires is an account of theoretical equivalence in which interdefinability relations between linguistic presentations of theories are somehow accommodated by relations among models or classes of models of the theories and vice versa. It is just that Halvorson has the wrong interdefinability relations and the wrong model-theoretical relations.

A second objection to Halvorson's argument is that he does not consider observational constraints. At least on Van Fraassen's version of the semantic view, an equivalence relation among theories ought to preserve observational equivalence: the equivalent theories should make the same observational claims. Two theories might be equivalent by the kinds of structural relations among models that Halvorson considers, or others, but nonetheless be inequivalent because they make different observational claims or because the observational fragments of their models are distinct. According to Van Fraassen (2006), what counts as observational adequacy of a theory for a body of data is that a "data model" can be "embedded" in a model of the theory. But to take but one of Halvorson's criteria for equivalence, isomorphism, if two models are isomorphic then any relational structure (i.e.,

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any "data model") that can be isomorphically embedded in one can be isomorphically embedded in the other. It would seem, therefore, that some purely formal model-theoretical equivalence relation should be necessary and sufficient for observational equivalence on the semantic view of theories. Even if some further invariance were required—for example, if substructures of all models were somehow designated as "observational" (although that, too, could only be specified in a language) and relations among model classes sufficient for theoretical equivalence had also to map "observational substructures" to isomorphic "observational substructures"—a formal equivalence relation of some kind between model classes would be at least necessary for theoretical equivalence.

3. Isomorphisms and Objections. Halvorson offers three possible criteria of equivalence—identity of model sets, equicardinality of model sets, and 1:1 correspondence of isomorphic models—of which only the last is minimally plausible: two sets of models are equivalent if and only if there is a bijection taking models to isomorphic models. Without defining "isomorphism," Halvorson gives two examples to show that his isomorphism criterion makes inequivalent theories equivalent, an example to show that it leaves equivalent theories inequivalent, and a general argument. Other examples show that identity and equicardinality proposals would make obviously inequivalent theories equivalent. Since these latter criteria are patently in disagreement with the criteria for theoretical equivalence I will describe, I will ignore them and the examples addressed to them.

Halvorson's first isomorphism example reveals a fundamental ambiguity in the notion of "isomorphism." That argument is as follows:

> Let $L(T)$ be the language with a countable infinity of 1-place predicate symbols P_1, P_2, P_3, . . . , and let T have a single axiom $\exists_{=1}x(x = x)$ (there is exactly one thing). Let $L(T')$ be the language with a countable infinity of 1-place predicate symbols Q_0, Q_1, Q_2, . . . , and let T' have axioms $\exists_{=1}x(x = x)$ as well as $Q_0x \vdash_x Q_ix$ for each $i \in \mathbb{N}$.
>
> Every model of T is isomorphic to a model of T' and vice versa. Indeed, a model of T has a domain with one object that has a countable infinity of monadic properties, and [a] model of T' also has a domain with one object that has a countable infinity of monadic properties. Therefore, T and T' are equivalent according to [the isomorphism] criterion. . . . And yet, T and T' are intuitively inequivalent. We might reason as follows: the first theory tells us nothing about the relations between the predicates, but the second theory stipulates a nontrivial relation between one of the predicates and the rest of them. Again, our intuition is backed up by the syntactic account of equivalence: the theories T and T' are not definitionally equivalent. (2012, 192–93)

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Halvorson does not define what he means by "isomorphism," and that turns out to be crucial. He claims (2012, 192 n. 5) that there is no notion of isomorphism between models of different theories, which seems flatly to contradict the argument he gives above. Since I cannot make his text consistent, I will defend the argument rather than the footnote and offer an interpretation of "isomorphism" that makes sense of the quoted text. I take it from the example that his idea is something like this: $M_1 = \langle D_1;\ Q_0 \ldots \rangle$ and $M_2 = \langle D_2;\ P_0 \ldots \rangle$, where the Qs and Ps are relations of any orders and are isomorphic if and only if there is a 1:1 map f from D_1 onto D_2 and a 1:1 map g from the Qs onto the Ps taking each n-order relation to an n-order relation, and for all nth-order Qs and all $d_1, \ldots, d_n$ in D_1, $(f(d_1), \ldots, f(d_n)) \in g(Q)$ if and only if $(d_1, \ldots, d_n) \in Q$ and (to be redundant) inversely. Halvorson might have had in mind a relation that is in some respects more general than this, for example, allowing appropriate expansions by new relations of models in various model classes. I cannot tell from his text. But in any case, the notion of isomorphism Halvorson uses in the example is purely structural—any nth-order relation can be mapped to any other nth-order relation. Thus, if M_1 is a relational structure for the P language of his example in which all P properties hold except P_0, and M_2 is a structure in which all P properties hold except P_1, Halvorson would count (indeed have to count) as an isomorphism the map that interchanges P_0 and P_1 and leaves all other P properties unchanged. That P_0 is the interpretation of the predicate P_0 and P_1 the interpretation of the predicate P_1 is ignored by the isomorphism. The relations become merely placeholders. That is a natural enough notion of isomorphism, but it is not the standard notion of model theory, and, I claim, since Halvorson is considering mappings for relational structures for definite languages, it is the standard notion that is appropriate for the semantic view and the one that the semantic view must adopt if it is to allow a theory to be presented at all.

The more restricted notion of model isomorphism standard in model theory requires that isomorphic relational structures be elementarily equivalent, and so the maps to which Halvorson refers in his example are not always model isomorphisms. More exactly, let L be a first-order language and M_1 and M_2 be relational structures for L. An isomorphism between M_1 and M_2 is a 1:1 map f of the domain of M_1 onto the domain of M_2 such that for each nth-order M_1 relation R_1 and nth-order relation R_2 that are, respectively, interpretations in M_1 and M_2 of a predicate R in L, and for each $a_1, \ldots, a_n$ elements of the domain of M_1, $\langle a_1, \ldots, a_n \rangle \in R_1$ if and only if $\langle f(a_1), \ldots, f(a_n) \rangle \in R_2$. For brevity, I will sometimes refer to the notion of isomorphism I impute to Halvorson as "H-isomorphism" and the standard model-theoretical notion as "M-isomorphism."

Although Halvorson's example assumes the model classes he considers are generalized elementary—that is, the class of all models of a first-order

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theory in the language of the theory—the sense of isomorphism I impute to Halvorson's example is more general than that. It presumes no syntactic structure, no language, whereas the more restricted model-theoretical notion of isomorphism implies that each class of structures considered for equivalence is generalized elementary, and isomorphism preserves elementary equivalence. On the one side, in his notion of isomorphism Halvorson has taken very strictly untenable claims that theories are entirely language free; on the other side, he presumes that theories include sentences that must be intertranslatable for theoretical equivalence. Some of his examples turn on that misfit. Halvorson's examples are reasons that advocates of the semantic view had better want the more restricted model-theoretical notion of isomorphism and had better fess up to the linguistic requirements of their view. They can do so, I think, without surrendering the claim that the content of a theory is its model class, but Van Fraassen's phrasing, quoted above, of what a theory must be cannot be sustained.

How can M-isomorphism be used to explicate the equivalence of theories in different languages, and how can that be done purely model theoretically? It is done through the notions of common definitional extensions and common definitional expansions. A *definitional extension* of a language L by a new predicate R is a formula $R(\mathbf{x}) \leftrightarrow \Phi(\mathbf{x})$ in the language $L + R$ of L extended with R, where $\mathbf{x}$ is a vector of variables and no other variables occur free in the formula, and $\Phi(\mathbf{x})$ is a well-formed formula of L. Two theories have a common definitional extension if each has a definitional extension such that the two extended theories (now in the same language) are logically equivalent. (In some cases, where the nonlogical vocabularies of the original theories are not disjoint, one of the theories must first be reexpressed by a 1:1 substitution of novel vocabulary for old preserving order types of predicates and functions.) Common definitional extensions are mutual interpretations, but not all mutual interpretations are common definitional extensions. A *definitional expansion* by relation R of a model M of a theory T with language L_T enriches (or, in other terminology, expands) M with R to form a structure for the language L_{T+R} satisfying a definitional extension of L_T by R. A definitional expansion by R of the class of models of T expands each model of T by R forming an expanded class of models such that there is a definition of R (in terms of L_T) in L_{T+R} that is satisfied by all models in the expanded class. Another way of putting the idea is *coalescence*: two theories T, T' in disjoint languages are coalescent if and only if their model classes can each be expanded to a class of relational structures for $L_{T' \cup T}$ so that every M-isomorphism of a T model extends uniquely to an M-isomorphism of an expanded structure, and every M-isomorphism of a T' model extends uniquely to an M-isomorphism of an expanded structure, and the expanded model classes are identical. Now,

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two theories in disjoint nonlogical vocabularies can be said to be formally equivalent if and only if they have a common definitional extension or if their model classes have a common definitional expansion or are coalescent. It turns out that these alternative criteria for equivalence are equivalent. The formalization and proof are due to De Bouvere (1965). With some technical qualifications, there is a further equivalence: two theories have a common definitional extension if and only if they have isomorphic Lindenbaum algebras. And so we have, as advertised, an account of the formal aspects of theoretical equivalence that perfectly matches interdefinability with model-theoretical relations and can be accommodated by a tenable version of the semantic view of theories.

Back to Halvorson's example. Consider two models, M_1 and M_2, one in which P_1 and only P_1 does not hold of the unique individual in the M_1 domain and another from his example in which P_2 and only P_2 does not hold of the unique individual in the M_2 domain, and also consider a third model, M_3, in which Q_0 and only Q_0 does not hold of the unique individual in M_3. Each of the first two models is H-isomorphic to the third, mapping Q_0 to P_1 and to P_2, respectively. But since compositions of H-isomorphisms are H-isomorphisms, the map taking P_1 to Q_0 composed with the map taking Q_0 to P_2 should be an H-isomorphism of M_1 and M_2. But M_1 and M_2 are not even elementarily equivalent. Halvorson's argument is thus invalid for M-isomorphism.

Since T has no nonlogical consequences, by Beth's theorem neither will any definitional extension of T, so no definition of Q predicates in terms of P predicates can entail the nonlogical truths $Q_0(x) \rightarrow Q_i(x)$. So T' cannot be a consequence of a definitional extension of T. So the theories have no common definitional extension, and their model spaces are, by De Bouvere's theorem, not coalescent.

Halvorson's second example is the case of theories with no finite models that are categorical in every infinite cardinality.

> For this example, we recall that there is a pair of first-order theories T and T', each of which is κ categorical for all infinite κ, but they are not definitionally equivalent to each other. By categoricity, for each cardinal κ, both T and T' have a unique model (up to isomorphism) with domain of size κ. Thus, there is an invertible mapping that pairs the size-κ model of T with the size-κ model of T'. . . . For each cardinal number κ, T has a unique model of cardinality κ and T' has a unique model of cardinality κ. Therefore, for each model m of T there is a model m' of T' and a bijection (isomorphism of sets) $i : m \rightarrow m'$. Thus, every model of T is isomorphic to a model of T' . . . even though they fail to be definitionally equivalent. (2012, 193)

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Halvorson's conclusion fails with M-isomorphism. It is possible for two theories categorical in power to fail to have coalescent model classes or common definitional extensions or expansions. For example, relations of a countable model of theory 2 imposed by expansion on a countable model of theory 1 may not be preserved by the (uncountable) infinity of automorphisms of the model of theory 1 (see Ehrenfeucht 1966).

To show that the H-isomorphism condition is too strong, treating equivalent theories as inequivalent, Halvorson gives the following example:

> *Example: Boolean Algebras*. Let $\mathcal{B}$ be the class of complete atomic Boolean algebras (CABAs); that is, an element B of $\mathcal{B}$ is a Boolean algebra such that each subset $S \subseteq B$ has a least upper bound $\vee(S)$ and such that each element $b \in B$ is a join $b = \vee b_i$, where the b_i are atoms in B. Now let $\mathcal{S}$ be the class of sets. . . .
>
> What does the semantic view say about the relation between the theories $\mathcal{B}$ and $\mathcal{S}$? . . . An arbitrary set S cannot be equipped with operations that make it a Boolean algebra; for example, there is no Boolean algebra whose underlying set has cardinality 3. Thus, there are structures in the class $\mathcal{S}$ that are not isomorphic to any structure in the class $\mathcal{B}$. . . . I claim, however, that the "theory of sets" is equivalent to the "theory of complete atomic Boolean algebras." Indeed, to each set S, we can associate a CABA, namely, its power set $F(S)$ with the operations of union, intersection, and complement. Furthermore, the set $G(F(S))$ of atoms of $F(S)$ is naturally isomorphic (as a set) to S. In the opposite direction, to each CABA B we can assign a set, namely, the set $G(B)$ of its atoms, and it follows that B is isomorphic (as a Boolean algebra) to $F(G(B))$. To summarize, there is a pair of mappings $F : \mathcal{S} \to \mathcal{B}$ and $G : \mathcal{B} \to \mathcal{S}$ that are inverse to each other, up to isomorphism. (2012, 194–95)

It is difficult to know what to make of this argument because it changes the game. By Stone's famous theorem, a set-theoretical object (a field of sets) can be defined from a Boolean algebra, but only by using set-theoretical operations that are not Boolean operations, and for atomic algebras that object will be (or be isomorphic to) an object (a power set) that can be defined in a model of set theory, as Halvorson says. In the example, he has not expanded the Boolean algebras by using only Boolean relations—he has used set-theoretical relations in addition. In a model of set theory, he has considered a set-theoretical object (a power set) and considered only isomorphism between that object and an object constructed via set theory—the power set of the set of atoms—from the Boolean algebra. He has shown only that there is an embedding of a set-theoretical expansion of the Boolean model in a model of set theory. No mutual translation of all set-theoretical statements into the language of Boolean algebra is on offer, let alone a

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common definitional extension. Halvorson's conclusion, that set theory and the theory of complete atomic Boolean algebra are the same theory, does not seem to follow at all.

The example does suggest that definability questions might be considered in the context of higher-order logics that have some set-theoretical expressivity without explicit set-theoretical axioms. Perhaps Halvorson's argument could be reformulated in that way, but it does not seem promising. On the one hand, Beth's theorem holds for second-order logic with Henkin's semantics. De Bouvere's theorem is a consequence of Beth's theorem for first-order logic, and one expects that a similar result holds for second-order Henkin logic, although I know of no proof. On the other hand, if the logic is unformalizable second-order logic, models can only be distinguished up to isomorphism by the cardinalities of their domains.

Halverson's general argument is as follows:

> Proposition. A definitional equivalence of theories does not necessarily entail that these theories have isomorphic models. In particular, there are first-order theories T and T' and a definitional equivalence $F : T \to T'$. Furthermore, for any definitional equivalence $F : T \to T'$, there is a model m' of T' such that the cardinality of m' is not equal to the cardinality of $F^*(m')$. [F^* is the contravariant model map induced by F from models of T' to models of T.]
>
> *Proof*. Let T be the empty theory formulated in a language with a single binary predicate R. Let T' be the empty theory formulated in a language with a single ternary predicate S. Myers (1997) proves that there is a definitional equivalence consisting of maps $F : T \to T'$ and $G : T' \to T$.
>
> Now we prove that there is no definitional equivalence $F : T \to T'$ such that $\mathrm{Card}(n) = \mathrm{Card}(F^*(n))$ for all models n of T'. For this, we only need the simple fact that definitional equivalences are *conservative* with respect to isomorphisms between models; that is, if $F^*(n) \equiv F^*(n')$, then $n \equiv n'$. Now let A be the set of isomorphism classes of models n of T' such that $\mathrm{Card}(n) = 2$. Let B be the set of isomorphism classes of models m of T such that $\mathrm{Card}(m) = 2$. Clearly B is a finite set that is larger than A. By conservativeness, $F^*(B)$ is larger than A; hence, there is an $n \in B$ such that $F^*(n) \notin A$. But then $\mathrm{Card}(n) = 2$ and $\mathrm{Card}(F^*(n)) \neq 2$. QED (2012, 195–96)

It is correct that the two isomorphism classes Halvorson considers (the difference between H-isomorphism and M-isomorphism does not matter here) have different cardinalities. The example also shows that the two sets of models—all the models of a binary relation and all the models of a ternary relation—trivially are not coalescent since the cardinalities are different for isomorphism classes of models of the respective empty theories

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whose domains are equicardinal. There is no pair of definitional extensions of ternary predicate $T(x, y, z)$ from formulas in terms of binary predicate $B(u, w)$ (where u, w may be any of x, y, z) and of $B(x, y)$ from formulas in terms of $T(u, w, v)$ (where u, w, v may be either of x, y) that, added to the respective empty theories, results in logically equivalent theories. If there were, then since both the binary and the ternary theories are empty, every first-order theory with only a ternary primitive predicate would have a common definitional extension with a theory with only a binary primitive predicate, but Robinson (1959) shows that elementary first-order Euclidean and hyperbolic geometries, expressible with a ternary primitive, cannot be expressed equivalently with a single binary primitive.

Myers (1997) uses a notion of interdefinability, "isomorphic interpretability," that is distinct from having a common definitional extension or coalescent model sets, but he includes these as a special case and, as Halvorson argues, it is also distinct from H-isomorphic bijections of the model classes. The empty theory of the binary relation and the empty theory of the ternary relation are interpretively isomorphic in that every binary structure can be paired with a ternary structure (for simplicity, on the same domain) and, conversely, in such a way that the collection of such structures is the elementary class of an axiomatizable theory and such that every (M) isomorphism of the binary reduct of a model in the class determines an isomorphism of the ternary reduct and conversely. There are maps from the binary language to the ternary language, and conversely, but as noted they do not determine a common definitional extension. Interpretive isomorphism guarantees that many important properties of theories are shared, but Myers remarks that some metamathematical properties one might think necessary for equivalence are not preserved by interpretive isomorphism, for example, having a one-element model, being logically equivalent to a theory whose axioms are equations, having a universal axiomatization (every substructure of a model of the theory is a model of the theory), and having closure with respect to unions of chains of models. (I do not know of proofs that all of these properties are shared by theories with a common definitional extension, but I conjecture they are.)

Halvorson asserts that theoretical equivalence is a collective, or global, property of the set of models of the theories, and that seems correct. Both coalescence and interpretive isomorphism are global in the sense that they require quantification over all models of each theory and more than the existence of a 1:1 map taking models to H-isomorphs. Interpretive isomorphism offers an account of theoretical equivalence that the semantic view of theories could conceivably adopt: given that the semantic view must allow syntactic structures as the means of presentation of scientific theories, I suppose it can allow the existence of syntactic structures (e.g., the axiomatizability required for interpretive isomorphism) as part of the means of

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defining or establishing theoretical equivalence. But then again, perhaps those advocates should have misgivings both because, as Myers proves, the empty theory of the binary relation is interpretively isomorphic to the empty theory of any *n*-ary relation, *n* greater than 2, and because, if as I have suggested the syntax of their model descriptions is in terms of equations, interpretive isomorphism does not preserve the property that a theory can be expressed in equational form.

4. Conclusion. Halvorson's fundamental viewpoint is that the restrictions on mathematical analysis that advocates of the semantic view wish to impose would deny to investigators the full resources of logic and mathematics that are needed to understand the properties of theories, and I agree. For example, the differences between common definitional extensions and interpretive isomorphisms seem worth exploring, and there may be other interesting alternative candidates for formal theoretical equivalence. Halvorson concludes with a hopeful reference to recent work in category theory for the promise of a deeper account of theoretical equivalence and perhaps other relations of methodological interest. I cannot, and would not, refute a hope, and I leave its development to him with good wishes but with this caveat: on the one hand, his arguments thus far do not show that a tenable version of the semantic view of theories has no available account of theoretical equivalence; on the other hand, the account of theoretical equivalence that best fits a tenable semantic view provides no grounds for insisting on a model-theoretical conception of scientific theories utterly free of the fetters of language—a view unsustainable on its face, the moment one considers how the space of models of a theory can possibly be indicated or used to make predictions.

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