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# Understanding Scientific Theories: An Assessment of Developments, 1969–1998

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The positivistic Received View construed scientific theories syntactically as axiomatic calculi where theoretical terms were given a partial semantic interpretation via correspondence rules connecting them to observation statements. This paper assesses what, with hindsight, seem the most important defects in the Received View; surveys the main proposed successor analyses to the Received View—various Semantic Conception versions and the Structuralist Analysis; evaluates how well they avoid those defects; examines what new problems they face and where the most promising require further development or leave unanswered questions; explores implications of recent work on models for understanding theories; and rebuts the few criticisms of the Semantic Conception that have surfaced.

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**1. Introduction.** The *Received View on Theories* was the epistemic heart of Logical Positivism. Twelve hundred persons were in the audience the night it died. It was March 26, 1969—opening night of the Illinois Symposium on the Structure of Scientific Theories. The Received View had been under sustained attack for a decade and a critical mass of main protagonists had been assembled to fight it out. Carl Hempel, a main developer of the Received View, was the opening speaker and was expected to present the Received View's latest revision. Instead he told us why he was abandoning both the Received View *and* reliance on syntactic axiomatizations (Hempel

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1974). Suddenly we knew the war had been won, and the Symposium became an energized exploration of where to go now.<sup>1</sup>

I assess where we actually went and what we learned about theories and science in the process.

**2. Why the Received View Failed.** In the 1960s, the Positivist Received View on theories was attacked on the following grounds:

- (1) its observational-theoretical distinction was untenable;
- (2) correspondence rules were a heterogeneous confusion of meaning relationships, experimental design, measurement, and causal relationships some of which are not properly parts of theories;
- (3) the notion of partial interpretation associated with more liberal correspondence rules was incoherent;
- (4) theories are not axiomatic systems;
- (5) symbolic logic is an inappropriate formalism;
- (6) theories are not linguistic entities and thus theories are individuated incorrectly.

To my mind the most significant objections, in order, are (6) and (2). (1) requires qualification since not all versions of the challenge are successful. (3) is false. (4) seems to me irrelevant. I discuss (5) in the next section.

In 1969 a variety of alternative analyses were on the table such as construing theories as answers to scientific problems, or as paradigms, or conceptual frameworks. Gradually analyses that construed theories as extra-linguistic set-theoretic structures came to dominate post-positivistic understanding.

**3. Syntactical vs. Semantical Approaches.** Objection (5) seems to me correct. But wide misunderstanding of it leads to endless silly, largely unpublished debates over what semantic approaches can do that syntactical or statement approaches intrinsically cannot.

For positivists, theories were partially interpreted axiomatic systems  $TC$  where the axioms  $T$  were the theoretical laws expressed in a theoretical vocabulary  $V_T$ ;  $C$  were correspondence rules that connected  $T$  with testable consequences formulated using a separate observational vocabulary  $V_o$ . Only  $V_o$  sentences were given a direct semantic interpretation. Note

1. Unattributed details of the Received View, its critics, and some associated historical comments are based on Suppe 1994a; other undocumented historical comments regarding the Semantic Conception are based on Suppe 1989 and 1998; personal involvement and recollection are the basis for other undocumented details.

that the Received View has a semantic component and is not wholly syntactical.

The Received View was a model of science (“explication”) that had to accommodate all genuine examples of good theories and exclude all clear examples of bad theories. Modern physics was their paradigm of good science and whenever they found incompatibilities with exemplary science they modified their analyses accordingly. In practice this meant that they adjusted their analyses to accommodate the best physics had to offer, but deployed them normatively in evaluation of the social sciences.

When the  $C$  were required to be explicit definitions, theoretical assertions were guaranteed to refer to observable reality within  $TC$ ’s scope. Once reduction sentences or interpretative systems were allowed,  $V_T$  terms could refer to observables or to nonobservables. That is, models of  $TC$  included both. A general problem was that the Löwenheim-Skolem theorem implied that  $TC$  models must include both intended and wildly unintended models. Unintended models provide potential counterexamples.

Blocking them more concerns eliminating syntactical-approach artifacts than dealing with substantive analysis. Positivistic syntactical analyses of theories, confirmation, simplicity, and explanation persistently were plagued by problems of unintended models—a problem not peculiar to positivism. For example, Kitcher’s (1989) unification explanation account has a very simple idea. But he develops it syntactically—spending most of the paper trying to block unintended consequences that are artifacts of his formalism. Most of his paper has little to do with developing his main idea.

Suppes (1967) contrasted positivism’s “intrinsic formulation” syntactical approach to the “extrinsic” semantic approach wherein one directly designates an intended family of models. Although formal semantics cannot designate precisely the intended set of models, our ordinary linguistic resources suffice. In practice we *do* pick out just intended models (else there would not be a problem of unintended models), and so we can proceed directly to the specification of these intended models without recourse to syntactic axiomatization.

Increasingly positivism had become sidetracked from development of their substantive ideas by technical problems that were mere artifacts of modeling science via predicate-calculus axiomatizations. This is the correct sense of (5)’s claim symbolic logic is an inappropriate formalism.

It concerns how serviceable the formalism is to the enterprise of modeling and understanding science. It has nothing to do with whether there are things semantic approaches can do that syntactical approaches in principle can’t do. That issue is irrelevant to assessing either the Received View or semantic approaches.

Rather, the Devil is in the details.

**4. The Semantic Conception of Theories.** *The Semantic Conception* identifies theories with certain kinds of *abstract theory-structures*, such as configured state spaces, standing in *mapping relations* to phenomena. Theory structures and phenomena are referents of *linguistic theory-formulations*. The basic idea is that theory structures are identified with suitably connected families of models. Depending on mapping relationships required for theoretical adequacy, realist, quasi-realist or antirealist versions are obtained (Suppe 1998).

The Semantic Conception has been very successful. It is widely accepted with remarkably little published criticism of it—none fundamental or fatal (see Section 7). It also avoids the problems that scuttled the Received View: Correspondence rules are avoided. Observational/non-observational distinctions are not required. Formalism artifacts that plagued positivism have not occurred. It individuates theories better than positivism did (see Section 5).

Avoiding such problems influenced Semantic Conception development, but was not the main inspiration or motivation. All versions built on Birkhoff and von Neumann's (1936) seminal work on observation- and phase-spaces and von Neumann's (1955) demonstration that two radically different quantum-mechanic formalisms were describing the same theory—the same configured phase space. For Suppes and myself an important motivation was making sense of personal experimental-scientist experiences we could not reconcile with the Received View.

Semantic Conception development has taught us a lot about the mediation of theories and phenomena via observation or experiment, relations between models and theories, confirmation of theories, their ontological commitments, and semantic relations between theories, phenomena, and linguistic formulations. I briefly survey what was accomplished.

First, abandoning correspondence rules taught us much about how theories and models attach to nature. Suppes (1962) explored how theory structures were associated with phenomena in experimental circumstances. Drawing on his mathematical learning theory laboratory experience, he argued that the connection between theory and experiment is mediated via a nondeductive hierarchy of models including the model of the experiment, models of data, experimental design, and *ceteris paribus* conditions. This stimulated inquiry on epistemology of experimental design, data analysis, instrumentation, and calibration culminating in recent modeling-data work discussed in Section 6 and some of Mayo's (1996) insightful work.

Second, the Semantic Conception led to rethinking theory confirmation and experiment's role. My Semantic Conception version maintains that propounding theories consists in asserting suitable mapping relations between configured state spaces and systems within the theory's scope.

Because theories ignore all but a selected finite number of variables, the relation has to be counterfactual wherein the theory structure specifies how systems within the theory's scope *would* behave *were* they isolated from influences not showing up as theory state variables. This affects how theories are confirmed by experiment. Characteristic of experimental design is control of extraneous variables so that only theory variables are influential. The logic of confirmation turns essentially on issues of experimental control. I have developed a rigorous epistemology where the adequacy of theories under controlled circumstances licenses the generalized conclusion that the theory was counterfactually true of *all* systems within its scope. One carefully-crafted experimental instance is sufficient to establish a scientific theory noninductively (Suppe 1993, 1997, forthcoming).

Van Fraassen's Semantic Conception version (1970) produced the idea that confirmation involves establishing the empirical adequacy of theories—in work (1980) sparking realism/antirealism controversies dominating the 1980s (see below).

Third, the Semantic Conception yielded considerable insight on semantic relations between theories, their formulations, and reality as well as associated ontological issues. Van Fraassen's starting point was the supposition that not everything referred to in a theory engenders ontological commitment. The problem is giving full semantic interpretations of theoretical language without excessive ontological commitments, thereby avoiding instrumentalism. His solution (1967) is via *semi-interpreted languages* wherein languages are interpreted as referring to "logical spaces" providing full semantic interpretations for theorizing languages. Ontological commitments are left unconstrained, being a matter of which logical-space points one wishes ontologically to commit. Such commitments are via individual mapping functions (*Loc* functions) from real-world objects to points in logical space. Semi-interpreted languages provide a general noninstrumentalistic framework for evading excessive metaphysical commitment.

Fourth, and for better or worse: The Semantic Conception generated realism/antirealism controversies. Since theories are extralinguistic entities, realism/instrumentalism disputes had to be recast. On the Semantic Conception realism debates concern the nature of the mapping relationships between theory structure and the world and ontological commitments associated with theories (Suppe 1989, 1998, forthcoming). Viewed in semi-interpreted language terms, a *realist* has *Loc* functions onto every state variable and maintains a theory is empirically true just in case theory-structure-allowed state transitions are identical to those possibly occurring in the actual world. *Antirealists* do not commit ontologically to all state variables. They only require countenancing *Loc* functions from observables and that theories be *empirically adequate*: If *W* is that portion of

reality to which one attaches *Loc* functions, the image  $M^*$  of  $W$  is among the models comprising the theory.

Although van Fraassen (1980) resorts to an observational/nonobservational distinction to motivate his analysis, it works for any vocabulary bifurcation, resulting in a general account how language can have full semantic interpretations without generating ontological commitments to every semantically utilized entity (Suppe forthcoming). He is particularly concerned that science is irreducibly modal at various junctures, but it seems excessive to commit ontologically to unreal possible worlds. Semi-interpreted languages divorce full semantic interpretation from ontological commitment. Within one's ontological commitment, empirical adequacy is equivalent to empirical truth. *Antirealism thus is just realism attenuated to the range of one's ontological commitment.*

I question whether any theories meet realistic truth or antirealistic empirical adequacy requirements since theory-scopes include systems influenced by variables not taken into account. Real-world state transitions often diverge from those specified in theory structures—making the theory literally empirically false or inadequate. *Quasi-realism* consists in ontological commitment to all variables that could be detected and the claim that empirically true theories provide counterfactual characterizations how systems *would* behave *were* they isolated from influences not taken into explicit account by the theory. One can adapt semi-interpreted languages to develop quasi-realisms with van Fraassen-like restricted ontological commitments (Suppe forthcoming).

Fifth, understanding of modality in science has advanced. Semi-interpreted languages can be extended to interpret modal operators via families of logical spaces (van Fraassen 1969); ontological commitments still are just the *Loc* operators one commits to. Van Fraassen's *Scientific Image* (1980) and *Quantum Mechanics* (1991) identify areas of science he believes are irreducibly modal, then provide semi-interpreted language solutions for how to accommodate modality without unwanted ontological commitments: *Loc* function choice remains free. Semi-interpreted languages enable avoiding Lewis-like (1973) commitment to real nonactual possible worlds.

Sixth, phase space and similar Semantic Conception state-change theory structures do not seem modal. This stimulated rethinking the role of modality in theoretical laws. Van Fraassen (1989) argued that laws of nature are not intrinsically modal. On my quasi-realistic view, theoretical laws are not modal but yield counterfactual characterizations of the world appropriately modeled using causal necessity operators. I face the challenge of giving noncircular casually-possible-worlds semantic-interpretations of causal necessity. The insight that laws of nature are not intrinsically modal combined with some deep insights of Richard Mon-

tague's (1974) regarding semantic relationships between necessity and universality, and semi-interpreted languages allows solution to this reincarnation of Goodman's circularity problem (Suppe 1997, forthcoming).

Finally, insightful Semantic-Conception examinations of theories in biology, economics, various social and health sciences, and physics have been done by Lloyd, Thompson, Hausman, McKinsey, Suppes, van Fraassen, myself, and others.<sup>2</sup>

The case has been made that the Semantic Conception has been an effective vehicle for doing significant philosophical business and did not get diverted by technical problems that were mere artifacts of our semantic formalisms.

**5. Structuralist Analyses and Theory Dynamics.** Reason (6) for rejecting the Received View was that theories were improperly individuated since experimental procedures were incorporated into correspondence rules. The Semantic Conception is inadequate if it cannot properly individuate theories. Theories undergo development. This has implications for theory-individuation. In present forms the Semantic Conception essentially treats theory development as progression of successive theories. If wrong, Semantic-Conception revisions making theories dynamic will be required.

Dynamics of theory development have been a central concern of *the Structuralist Approach*. Like the Semantic Conception it analyzes theories set-theoretically as comprised of a theory structure and intended applications, but is neo-positivistic in spirit and reliance on a relativized theoretical/nontheoretical term distinction. It began with Sneed's (1971) application of Suppes's (1957) set-theoretic techniques to the problem of theoretical terms. Sneed noticed that when applying classical particle mechanics, determination of mass and force function values invariably must utilize classical particle mechanics itself. He claimed this the characteristic mark of theoretical functions or terms—which threatens to make predictive testing of theory viciously circular unless contributions to theory empirical content are restricted.

Sneed's solution asserts empirical content of a theory varies from application to application, so the theory portion used to calculate theory functions are not part of the empirical content of *that* application of the theory. Yet theories have a unified content that cannot be accounted for by a conjunction of statements. It follows that theories cannot be individuated on the basis of mere theory structure.

Sneed develops an empirical-content semantic analysis wherein theories are individuated on the basis of a theoretical core and an intended-applications core. Analysis of dynamics and individuation of theories is

2. See references and discussion in Suppe 1989.

driven by his formulation of the problem of theoretical terms which depends crucially on supposing theory testing requires prediction and an independent determination whether the prediction is correct. If, as a growing number of philosophers and historians maintain, prediction is not central to testing or confirmation (e.g., Brush 1989, 1990), Sneed's analysis is unlikely to succeed.

Despite reservations over the Structuralist program, the idea of individuating dynamically-evolving theories via a structural core or partial models is promising. Da Costa and French (1990) presented a partial-models Semantic Conception analysis not tied to neo-positivistic ideas. But they did not apply it to theory-individuation issues.

As with the Received View, I see theory-individuation issues as make-or-break for any account of theories including my own Semantic Conception. It is an area that deserves more work.

**6. Models and Theories.** On both Semantic and Structuralist analyses, theories are representing families of metamathematical models which can be interpretations of formalisms. This is the literal sense in which, *pace* Wade Savage (1999), they *are* appropriately termed “semantic” in the fundamental Tarski sense. To my mind, the most exciting recent developments concern the relations between models and theories.

At the hands of positivists like Campbell, Black, and early Hesse, models were analogical, metaphor-like devices playing important heuristic roles but could not be vehicles of scientific knowledge.<sup>3</sup> That is incompatible with Semantic and Structuralist positions: If theories are vehicles of scientific knowledge, then so too must models be. From early on, Suppes and I have explored relationships between theories and models in science (Suppes 1962; Suppe 1967, 1989).

I opened *Structure of Scientific Theories* asserting that the “most central or important” problem in philosophy of science is “the nature and structure of theories . . . . For theories are the vehicle of scientific knowledge and one way or another become involved in most aspects of the scientific enterprise” (Suppe 1974a). Don't believe it for a moment! Today much of science is atheoretical, as it was then. For example, theory development is incidental to most of today's chemistry. The business of most experimental and observational science is modeling data—increasingly so as science has become computationally intensive. Today, models are the main vehicle of scientific knowledge.

It is here I see some decisive advantages of semantic approaches over positivistic ones. Campbell, Hesse, and a few others made valiant attempts

3. See Suppe 1994a, 95–102, for discussion and references.

to bring models and theories together. But their analogical heuristic view of models was too impoverished.

The last two decades there has been growing concern to understand models. I think a lot of it has been wrong-headed, more driven by philosophical agendas than understanding scientific modeling. Most of this decade I have been trying to understand models. I want to sketch insights that have resulted and what I think their implications are for understanding theories.

Models are the heart of scientific experimentation, observation, instrumentation, and experimental design. Data typically are insufficient or of the wrong sort, and modeling is required to interpret them in scientifically meaningful ways. Such modeling adds assumptions in lieu of data to get interpretable structure. Assumptions often are unsubstantiated, soft, or known false. Modeling and experimental design introduce artifacts into data that one can attempt to remove thorough experimental control. Often measuring contaminating influences and correcting them is superior. A variety of modeling techniques determine which aspects of models of data are real effects or artifacts. Neither recourse to unsubstantiated nor false assumptions prevents models from being vehicles of scientific knowledge so long as claims are restricted to real-effect aspects. Suitably deployed, simulation models are probe-instruments yielding data about the real world comparable to traditional experimentation and instrumentation.<sup>4</sup>

There is continuity between these findings and earlier work of Suppes (1962) and myself (1967, 1989) on roles of models in experimentation and mediating theory and phenomena. What I want to suggest here is that the basic case is models, not theories. Thus I repudiate my younger self.

The understanding gained by focusing on models, their epistemic roles, and how they serve as vehicles of scientific knowledge, especially when incorporating false or unsubstantiated assumptions, challenges much conventional philosophical wisdom. I believe it cuts to the core. But by construing theories in terms of families of models, semantic analyses—and they alone—have real potential for parlaying such new philosophical wisdom into enhanced understanding of theories, their natures, and their roles in science.

**7. Philosophical Confusions.** Much development of the Semantic Conception has focused on detailed understanding of the relationships between scientific theories, scientific models, and metamathematical models. The enterprise begins with Patrick Suppes' 1961 classic where he argued "the concept of model in the sense of Tarski may be used without distort-

4. These extremely telegraphed comments regarding modeling are developed at length in Norton and Suppe 2000 and works cited therein.

tion and as a fundamental concept” in scientific and mathematical disciplines and that “the meaning of the concept of model is the same in mathematics and the empirical sciences. The difference to be found in these disciplines is to be found in their use of the concept” (p. 165). Suppes’ claim is that the Tarski concept of a model is a common *formal* framework for analysis of various uses of models in science and mathematics. He does not claim there are no significant nonformal differences in scientific vs. mathematical models. Nor does he claim the formal properties common to all such models exhaust all interesting or relevant properties. Indeed the point of finding such common structure for analysis is precisely to highlight different uses, deployments, and epistemological differences that *do* distinguish science from mathematics.

Earlier sections of this paper have tried to show how Suppes, van Fraassen, myself, and others associated with development of the Semantic Conception have tried to exploit the power of Tarski formal semantics to understand the roles theories and models play in science. The analyses we have presented are detailed, frequently careful, and generally nuanced and cautious. There is a lot more detail and substance to the Semantic Conception than the slogan that

(7) theories are collections of models.

Yet what few criticisms of the Semantic Conception have been advanced either attack that simplistic slogan, mount straw-person attacks, such as bogus claims the Semantic Conception erases the distinction between theories and models, or complain that it does not solve all philosophical problems regarding science including those outside its scope. More often than not, such attacks are keyed to a caricature of the Semantic Conception that “is not a version held by any particular semantic theorist” (Downes 1992, 142).

Such attacks ignore the substance and nuance of the Semantic Conception and trade on stabbing caricatures such as that “theories are mere collections of models.” Or argue over abstract claims of superiority of “semantic” vs. “syntactic” *views* that ignore *all* the detail and substance of either Received View, Structuralist, or Semantic Conception analyses. Those who refuse to join the details of the specific analyses are tilting at windmills.

If it be a battle of sloganistic digests of nuanced analyses, so be it. My nuanced sloganistic analysis of the common core of Semantic Conception analyses is:

(8) Scientific theories are causally-possible collections of state-transition models of data for which there is a representation theorem.

Now I unpack that slogan: The “are” is not identity, but rather the “is” of inclusion. *Models of data* are structures into which data are embedded that add additional mathematical structure. *State-transition models of data* take simultaneous actual or possible measures of variables and embed those measures into n-tuples of simultaneous values (“states”) which then change over time and provide a record of either actual or ideal changes in these variable-measures over time. The incorporation of those actual or ideal measures into n-tuples that are time-sequenced adds a lot of structure to the measures, and thus qualifies for being a model of the data in the sense of Norton and Suppe 2000. I maintain that such models of data constitute the fundamental sense of empirical or scientific model—and are far removed from the heuristic or analogical models philosophers have focused on (see Norton and Suppe 2000).

*Causally-possible* collections of state-transition models of data consist in all and only those models of data which represent causally possible state-transition phenomena. Which collections of models represent such phenomena will vary depending whether one maintains a realist, antirealist, or quasi-realist version of the Semantic Conception.

*Representation theorems* are formal mathematical results. Roughly they mean that for some family of models, there is a subfamily such that each member of the family is isomorphic to some member of the subfamily (Suppes 1957, 263). Representation theorems do not imply that all models are isomorphic to each other. Intuitively, if there is a representation theorem for a family of models, it means there is enough structural coherence that we can use a finite variety of models to represent a far richer variety of empirical possibilities.

The representation theorem condition is a necessary, not sufficient condition. Not all collections of models for which there is a representation theorem will qualify as scientific theory structures.

It follows from (8) that the Semantic Conception is not committed to any of the following claims which critics have attempted to saddle it with:

- (9) Theories are mere collections of models
- (10) Any legitimate scientific model must qualify as a component model for a theory.

[Regarding (9), not all collections of models have representation theorems; regarding (10), not all scientific models qualify as actual or causally-possible state-transition models of data.]

Downes (1992) criticizes the Semantic Conception on the grounds that there are “qualitative” scientific models that do not meet the formal requirements of Tarski semantics and that the relations between empirical systems and the models of the semantic conception cannot be reduced to isomorphisms. Regarding the former, the models Downes trots out fail to

be state-transition models, hence are irrelevant to the Semantic Conception. Regarding the lack of isomorphism, it is a corollary to discussion in Section 4 that isomorphism at most is relevant to realist and antirealist versions of the Semantic Conception, hence constitute no general objection to the Semantic Conception and are redundant of some objections to those weaker versions.

Downes, at least, attempts to engage the Semantic Conception, appreciates its power and achievements, and wants to improve on it. Other recent objections are less impressive. Cartwright and colleagues (1996) charge that the Semantic Conception totally obliterates the distinction between models and theories. Such charge has cogency only if one supposes that the Semantic Conception claims that it encompasses, explains, and accommodates *all* scientific models. Such construal is incompatible with (8) and its glosses. *If* Nancy Cartwright has a legitimate target re obliterating the distinction between models and theories, it is none of the main developers of the Semantic Conception. Nor is the Semantic Conception a culprit. She has attempted to saddle the Semantic Conception with a fabrication of her own which she then attacks.

An article by Margaret Morrison (forthcoming), furthers the attack on the Semantic Conception on grounds that it undercuts the autonomy of models, noting that models often presage theories. Of course. Many models are developed independent of theories. Models often are advanced where theory is insignificant (much of experimental chemistry is like this). So what? Only via some supposition like (10) do these valid observations purport to challenge the Semantic Conception. And since nothing intrinsic to the Semantic Conception commits to anything like (10), such attacks are specious.

More generally, the Semantic Conception has limited goals: A semantic/logical analysis of common properties of an identifiable scientific artifact that is central to, but not exclusive of science—a product that has close and interesting structural relations to other significant scientific artifacts such as models, data, experimental design, instrumentation, and the like. No adequate analysis of theories will preempt full analyses of these other scientific artifacts.

Theories that the Semantic Conception successfully analyzes are important and central to, but not exhaustive of science. Those who seek to impugn it by charging that it fails to account for all of science, or even all of modeling in science, only attack their own fabrications. When the details are fully accounted for, the Semantic Conception is a remarkable philosophical success. It does well what it purports to do, and makes no stronger claims for success.

**8. Conclusions.** I have argued that focusing on inherent advantages of

syntactic vs. semantic approaches is fruitless. The Devil is in the details. And the details are that semantic approaches have showed impressive philosophical advances with little distraction by artifacts of infelicitous formalizations.

At the same time I do admit there is a sense in which most current development of the Semantic Conception was completed in the 1970s and that subsequently it primarily has served as a tool or basis for other philosophical examinations. I think this is because we had impoverished understanding of models and didn't know where to go next. Yet it spawned sustained look at models, which we are coming to understand (see Norton and Suppe 2000). Ultimately the deep connections between models and semantic analyses of theories promises to yield escalating understanding of theories, models, and how science *really* works.

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