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A METHODOLOGICAL CRITIQUE OF THE SEMANTIC CONCEPTION OF THEORIES

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A new PhD slated to teach a beginning undergraduate course on scientific reasoning recently asked me to recommend topics. I launched into a description of my “baby-Popper-plus-statistics” class – give them enough deductive logic to understand the Duhemian problem, do the Galileo case study, use the notion of severe test to introduce a bit of probability theory, then segue to the problem of testing statistical hypotheses.... My interlocutor was looking impatient. “But I’m a strong adherent of the Semantic Conception of theories,” he said. “I can’t teach all that stuff about trying to falsify bold conjectures.” This was not a moment for proselytizing, so I loaned him a copy of Giere’s textbook, which is based on the Semantic Conception, and sent him happily on his way. However, this episode raises an interesting question, one that takes on some urgency as the Semantic Conception of scientific theories (SC) seems well on its way to becoming the new received view: What accounts of scientific method, confirmation and explanation does the SC support?

A major motivation of the Semantic Conception philosophers was to replace the awkward syntactic account of theories proposed by the positivists, who favored a formal axiomatic system accompanied by “correspondence rules” or meaning postulates. But since Popper never gave an account of meaning and was never worried about the problem of how to interpret theoretical terms, it might seem that there should be no inherent tension between Popperian methodology and an account of science that views theories as sets of models. Because the Semantic Conception liberates the content of a theory from any particular linguistic formulation, this move might appear congenial to those in agreement with the Popperian dictum that “Words don’t matter”. Furthermore, the Semantic Conception’s emphasis on mapping structures that reside in the world also seems to mesh with Popper’s anti-essentialism.

Yet I will argue that the overall approach to scientific inquiry that accompanies the SC approach is antithetical to a Popperian account of scientific methodology, which is intended to maximize the role of criticism. Furthermore the methodological glosses that commonly accompany expositions of the Semantic Conception are either antithetical to commonly accepted norms of scientific inquiry or hopelessly ad hoc. The points I wish to make are concerned neither with the technical details of the SC nor the

more idiosyncratic aspects of Popperian methodology, such as his views on induction. Neither am I concerned for the purposes of this paper to worry about whether we should view our best scientific theories as being true, approximately true or merely empirically adequate. My intention is rather to dramatize the *methodological* differences between an approach to science that focuses on model building and one that construes scientific inquiry as a search for true or approximately true or empirically adequate generalizations.

Most of my claims are straightforward and do not hinge on the particular variant of the Semantic Conception one adopts as long as that conception views scientific theories as a class of models of relational structures, not as universal generalizations. Glymour gave this characterization of the SC: “[T]he product of science in this view is not so much knowledge of general propositions ... as an understanding of systems of models and how to embed various classes of phenomena within these models.” (Salmon, p. 122) Thus for my purposes, van Fraassen is not a target simply because his SC account includes generalizations as part of the theory. He says a theory is empirically adequate exactly when “*all* appearances are isomorphic to empirical substructures in at least one of its models”(Boyd, p. 192) Neither am I concerned here about the SC approach

to a theory such as cosmology which has only one intended application. The da Costa and French book arrived too late for me to evaluate it carefully, but it may also evade the criticisms that follow.

As these exceptions make clear, my quarrel here is less with the SC *per se* than with the methodology associated with it especially by writers who take the disunity of science to be a virtue. Ron Giere is the SC theorist who has paid most attention to methodological issues. Therefore I will generally illustrate my argument using examples from his writings, which have the additional virtues of being clear and extensive. To my mind, his is an especially interesting version of the Semantic Conception, in part because he takes seriously the goal of accounting for our best examples of scientific practice. Nevertheless, I find that it does not hang together as well as a methodology that construes theories as statements.

What does the SC say about refutation?

To remind ourselves of how the Semantic Conception represents science, let us begin with Giere's take on Halley's Comet. What Halley did was fit a "Newtonian model for two bodies in an elliptical orbit attracting one another by the force of gravity" [1991, p. 72] to Halley's observational data concerning the 1682 Comet and show that the same model fit

observations of Comets sighted at 76 year intervals before. Halley also predicted that the Comet would return in 1758 (which it did). Thus the hypothesis that the Comet of 1682 fit the Newtonian two-body model was confirmed. Giere does *not* say that Newton's theory taken as a world system or Newton's Laws were confirmed. Rather he points out that Newtonian models were applied so successfully to both terrestrial and celestial phenomena that Newtonian science became an inspiration for the Age of Enlightenment. [1991, p. 69]

Exponents of the SC all make this point quite explicit - a theory defines a set of models. When we find some of these models fit discrete portions of the world we may decide to search for more instantiations in situations that appear to be analogous in some way. But we neither confirm nor refute theories. Rather we show that one model of the theory does or does not fit some real world situation. It is the limited hypothesis that one of the models fits one aspect of the world that is confirmed or refuted.

Giere's account of the failure of the phlogiston theory makes this move explicit. After summarizing the phlogiston theory and Lavoisier's experiments with mercury, Giere concludes: "The data, therefore, provide evidence that the phlogiston model fails to represent the controlled combustion of mercury as carried out in this experiment." [1991, p. 76] So

far, so good. What the Semantic Conception does not sanction is the further claim that Lavoisier's experiment gave evidence that the phlogiston account of other chemical reactions was also thereby discredited.

Of course Lakatos and others have emphasized that we should not advocate a naïve view of refutation. Giere's discussion of the discovery of Neptune illustrates nicely the sort of tinkering with initial conditions that is characteristic of science: When a simple Newtonian model failed to fit the observed orbit of Uranus, scientists were "forced to conclude that their current models were not correct." [1991, p.90] Around 1843 Adams and Leverrier proposed a more complicated model that posited a new planet. The new model did fit the orbit and Neptune was observed in 1846. But exactly why did Adams and Leverrier keep tinkering with Newtonian models? Did they think that Newton's Laws were true of all celestial bodies or that the whole universe was a Newtonian system? Giere's explanation is ambiguously worded: "By that time, there had been so many successful predictions using Newtonian models that they were reluctant to conclude that the general theory could be wrong." [1991, p.90] Unless Giere has unconsciously lapsed into thinking of theories as making claims, he probably should replace talk of a theory "being wrong" with something like "failing to provide models that fit all situations."

Giere does not discuss the problem with Mercury's perihelion or its role in the replacement of Newtonian mechanics and it is difficult to come up with a Semantic Conception account of the motivation for this transition. Perhaps it would go something like this: Research showed that Newtonian models fit an enormous variety of physical phenomena, but, after repeated attempts, scientists failed to find a Newtonian model for the trajectory of Mercury. However they could fit that data with an Einsteinian model. For that reason (and other reasons as well) they decided to start to advocate that in principle Einsteinian models should be used everywhere, even in the situations where the old Newtonian models fitted to within the limits of experimental error. (Of course, using the more sophisticated model is not necessary for practical purposes in most cases.)

On the Statement View, Einstein proves Newton false although Newtonian models do fit most data sets pretty well. However, since scientists seek true theories, they prefer Einstein to Newton. On the Semantic Conception Einsteinian models fit more data sets more accurately than do Newtonian models. To explain the fact that scientists prefer Einstein to Newton, Semantic Conception adherents must also posit that scientists value theories whose models fit more phenomena. Appeals to the truth of the theory have been replaced by appeals to a wide domain of applicability.

The metaphysical differences between these accounts may exercise philosophers, but would it make any difference to actual scientific inquiry if the Semantic Conception were to prevail? As Giere persuasively argues in his 1988 monograph *Explaining Science*, both physics textbooks and the practice of working physicists provide numerous examples of scientists reasoning just as described by the Semantic Conception, so it might seem that there would be no practical consequences to which stance one adopts. Nevertheless, let's probe the matter further by continuing our attempts to translate the traditional maxims of good scientific inquiry into Semantic Conception talk. Although the influence of philosophizing on the practice of science may be tenuous, the impact on science education is more direct. In this arena at least, philosophical ideas do have consequences!

What does the SC say about variety of evidence?

What about the familiar exhortation of Bacon and Mill to test theories against a wide variety of evidence? From the perspective of the Statement View this advice makes good sense. If a theory makes claims about a wide variety of phenomena and we want to find out whether it is true or not, we are well advised to look for its weak spots and to test it against unfamiliar cases, especially ones that were not contemplated when the theory was

formulated. In order to capture this aspect of good scientific practice, the Semantic Conception must once again invoke the importance of wide applicability.

But note that this addendum stands in tension with the SC account of how we apply the theory. Remember that on the Semantic Conception Newton's theory does not make universal generalizations about any domain. Rather, it defines a set of models that one can match up against bits of the world. The Semantic Conception *per se* does not tell us where to apply these models. (Newton did, of course, but these are *obiter dicta*.) The only systematic advice that the Semantic Conception provides is to look for similarities between cases where the model works and new examples. The analogies may be expressed in ordinary language and describe similarities in the physical systems themselves (if a Newtonian model fits Earth's moon, perhaps it will fit the Jupiter's moons) or they may focus on formal properties of the model (e.g., let's try a harmonic oscillator again in this new case).

The concomitant methodological advice is to extend the domain of applicability of the theory's models cautiously, first looking for new phenomena that are quite similar to the ones already modeled. Deploying models in radically new domains may eventually be necessary, however, to

ensure the auxiliary desideratum of wide applicability. Once again the role played by the central value of truth in the Statement View is carried by the secondary desideratum of wide applicability in the Semantic Conception. Yet we often find scientists going out of their way to test a theory in novel situations where there are no strong analogues to previous successes. Stock examples include Eddington's eclipse expedition and the experiments on conical refraction. Since no one had tried to model the bending of light in a gravitational field before might not the SC suggest that it might be a better use of Eddington's time and money to simply work out the Einsteinian model of Mercury's perihelion in more detail!

According to Salmon, [p. 119] when Poisson showed that Fresnel's wave theory implied that light shone on a circular disk should produce a shadow with a bright spot in the middle, he thought the result was absurd. But Arago performed the experiment and confirmed Fresnel's theory. Such scientific behavior is what would be expected on the Statement View, but it appears unmotivated from the SC perspective. If Fresnelian models work elsewhere, on the SC why should we care whether or not there is one model predicting an obscure phenomenon whereby a bright spot appears in the middle of the shadow of a circular disk? And why should we be so impressed when this wave theory model is successful?

What does the SC say about crucial experiments?

I also find the SC account of crucial experiments between theories (as opposed to cases where we are choosing between models within a particular theory) a bit contrived. Giere gives a clear summary of the inability of the Ptolemaic model to account for the phases of Venus and draws this conclusion: “The data would be impossible on the Ptolemaic model, and no other plausible models have been mentioned. So the data must be taken as positive evidence that the Copernican model provides a good fit to the actual universe.” [1991, p.68] My question is this: At the time, many astronomers considered their accounts of celestial movements to be mathematical devices that “saved the phenomena” but did not describe what the heavens were really like. In the spirit of the SC why should we not say that the Ptolemaic system was never intended to model the sources of illumination of the planets, only their relative positions? If some other model speaks to that issue, fine, but on the SC why should that fact discredit Ptolemy? Since models are only intended to map certain aspects of the world, why try to stretch them to account for phenomena outside their original scope?

On the Statement View Ptolemy and Copernicus are inconsistent, not both can be true. On the Semantic Conception, we can easily claim that the

Ptolemaic model fits the observed positions of planets perfectly well and the fact that navigators continue to use geocentric models reflects this interpretation. We can use the Ptolemaic model for some purposes, the Copernican for others, such as predicting the phases of Venus. On the SC there is no reason to attribute epistemic value to this crucial experiment. A similar gloss can be made on the import of the Eddington eclipse crucial experiment between Newton and Einstein. (I am ignoring the issue of how well Eddington's data actually confirmed either account.) If the Newtonian model does not fit the data, why shouldn't the SC adherent simply say that Newtonian models were never designed to talk about the path of light rays in intense gravitational fields and had never been applied in analogous situations? On the SC, this experiment helps us delimit the scope of application of Newtonian models, but it gives us no grounds for saying that Newtonian theory itself is false.

In *Science Without Laws* Giere recommends as a methodological rule that when two models posit conflicting accounts of some aspect of the world this is "an indication that one or both types of models fail to fit the world as they might. It is an invitation to further inquiry to find models that eliminate the conflict..." [p.83] He notes that proceeding as if the world had "a single structure" has been fruitful in the history of science. So Giere's instincts

agree with those who think the goal of science is good comprehensive theories but he has to gerrymander the SC in order to accommodate this salient feature of past science.

We seem to end up with this result. In order for the Semantic Conception of theories to provide a normative analysis of the famous episodes in the history of physical science which philosophers of science use to illustrate basic methodological maxims, it must rely heavily on imperatives that have a pragmatic, but not an epistemic status. To explain scientists' rejection of phlogiston or Ptolemy, or the fact that they consider Einstein better than Newton, the SC must advise students to prefer theories with the most workable models. This is not bad advice, but it rather sounds like Microsoft exhorting us to buy newer versions of Word Processing programs – even though they are memory hogs and run slower, they do have additional features that might come in handy! Unlike the Statement View, the SC account does not distinguish the use of theories as instruments and the use of theories as global descriptive claims.

What does the SC say about severe testing?

One of the most basic maxims of scientific method is nicely illustrated by Bacon's account of those who used the votive offerings of rescued sailors

as evidence of the powers of the gods. As Bacon put it in his *New Organon*, “But where are the offerings of those who were drowned at sea? Such is the way of all superstition.” Running around collecting positive instances of a generalization or finding places where a template fits the world is only half of the story – we also need to actively look for situations where the theory fails. Thus Popperian and Bayesian accounts place special emphasis on conducting tests in situations where the evidence is unlikely to be positive unless the theory under test is true - neither background knowledge nor plausible competing theories make the positive outcome probable.

Giere adopts the language of severe testing but not surprisingly he restricts it to the testing of singular hypotheses. Thus when discussing what he calls “marginal science” he points out that Freud’s model of Little Hans makes no predictions that go beyond our common-sense accounts of the child’s behavior (1991, p.103). In the Hally’s Comet case, by contrast, one can argue that it is highly unlikely that the Comet of 1682 would return in 1758 unless the proposed Newtonian model was correct (1991, p.74).

Once again the SC account works *locally* – it picks out a crucial difference between the model of Little Hans and the model of Halley’s Comet. What it fails to provide are global evaluations of Freudian and Newtonian theory. Does the weakness of the Freudian account of Little Hans

give us reason to be dubious about future applications of that theory? Does Halley's impressive success give us reason to expect other Newtonian models will work? In particular, the SC cannot discourage the sort of cherry picking of favorable cases so vividly described by Bacon; instead, as we will see in the next section, it actually encourages us to stick close to favorable cases.

What does the SC say about prediction?

Any account of testing in science will talk about deriving predictions from a theory and comparing them to the results of experiment. So when Giere speaks of predicting the return of Halley's Comet, it is natural for someone accustomed to thinking of theories as if-then statements to miss the novelties of the SC account. Perhaps this toy example will highlight some of these distinctive features. Suppose I want to model the shape of my handkerchief as a square. I might proceed as follows. First I determine that my hanky has four sides. Based on my hypothesis that it is a square, I now predict that the sides are of equal length. If this is confirmed I go on to predict that the four corners are congruent. If either prediction fails, my hypothesis is refuted. But all this talk of prediction is really somewhat odd. By definition a square has four equal sides. My hanky either falls under the

definition or it doesn't. It makes no sense to take some of the defining characteristics as initial conditions and others as "predictions".

Giere's account of how scientists made predictions about the return of Halley's Comet should be read as exactly parallel to the example of my piece-meal efforts to determine whether my hanky fits the model of a square. Certainly scientists performed a mathematical derivation, the conclusion of which was "The Comet will return in 1758." But the proposition that was tested is "Halley's Comet is a Newtonian two-body system." On the SC there can never be a "Duhemian" dilemma where one is wondering whether a failed prediction should be blamed on the theory or on the initial conditions. As Rosenberg emphasizes, on the Semantic Conception theories are true by definition. [pp.96ff] So on the SC it is the choice of model which must always be faulted. Yet in the history of science there have been many cases in which scientists treated prediction failures as dilemmas and argued about which premise to fault. A standard example is the failure to observe stellar parallax at the time of Galileo. Did this finding refute Copernicus or simply mean that the distance to the stars was much greater than had been estimated?

Prediction also occurs in science when we apply a successful theory to new domains. On the statement account of theories, the scope of application

is an integral part of the theory. The kinetic theory of gases should apply to all gases; Mendeleev's periodic law of the elements was intended to apply to all elements; etc. On the Semantic Conception, however, theories *per se* simply supply models. Theories do not specify the domain of application. The SC account of extending a theory to new phenomena, therefore, depends crucially on notions such as similarity, resemblance, and analogy. If a theory fits a chloride, then we might try it out next on a bromide, *not* because the theory claims to describe all halogen salts or all chemical compounds, but simply because background knowledge leads us to think bromides may behave similarly to chlorides.

Expositors of the Semantic Conception give few details of the kind of informal inductive reasoning that is required to apply theoretical models to new situations. Giere's naturalistic version, however, suggests that psychological accounts of the structure of concepts may help describe how scientists jump from one application of a theory to another one. He also distinguishes between the similarity relations operating across folk categories and the more abstract structural resemblances that inform scientists' classification schemes. [1999, pp.100-106]

To sum up, on the SC our standard locutions about the predictive power of successful scientific theories have to be dropped or reconstrued.

The theories that scientists admire are, as Cartwright puts it, “toolkits” for building successful models. The theories themselves do not describe where they apply, but through experience in modeling similar situations the scientific community develops informal guidelines for their deployment.

What does the SC say about explanation?

Let us now turn to current philosophical accounts of explanation and ask how competing models of explanation fit in with the SC. As long as we focus our attention on a single model of a theory, it would seem that the SC can speak of covering law explanations in a very limited sense. In his textbook Giere continues to talk about Newton’s Laws. They are what animate the Newtonian models and tell us, for example, when Halley’s Comet will return. So the Newtonian model of Halley’s Comet both predicts and explains the observed positions of this celestial body by deriving them from general statements about the Comet-sun system. But since refutation on Giere’s account logically impacts only the particular model, not the theory as a whole, then by a parallel argument explanation would subsume the motion of Halley’s Comet only under a specific two-body model, not under a general Newtonian account of the motions of celestial and terrestrial bodies.

In his more sophisticated philosophical account *Science Without Laws*, Giere argues that Newton's Laws are more accurately denominated as "equations" or perhaps the more honorific "principles," in the light of their ubiquitous role in Newtonian models. [1999, p.94] He emphasizes that they do not function as universal generalizations, not even if we add provisos. Giere does believe that there is a relationship of physical necessity between the length and period of a simple pendulum clock, one that even supports counterfactuals, but this is again an addendum to the SC, not part of the core account.

If we look at causal accounts of explanation, the SC would appear to lend itself most naturally to talk of singular causes. If we like, we can trace causal trajectories within one model, but, as was the case with prediction, generalizing the causal account to other systems works only through analogy. The contrast with the Statement View emerges if we look at how each approach handles narrative explanations.

Consider, for example, a so-called Rube Goldberg machine. Here is a description of one of his classic cartoons:

"Picture Snapping Machine: As you sit on pneumatic cushion (A), you force air through (B) which starts ice boat (C), causing lighted cigar butt (D) to explode balloon (E). Dictator (F), hearing loud

report, thinks he has been shot and falls over backward on bulb (G), snapping picture.”

[<http://www.rubgoldberg.com/html/picture%20snapping.htm>]

On the Statement View each step of the machine’s operation would be explained using the appropriate causal law from a wide variety of scientific theories – weight on a confined gas increases its pressure, objects move more rapidly when the friction is low, animals are startled by unexpected noises, etc. Our understanding of each step inherits the epistemic and explanatory weight accruing from the fact that it falls under a covering law that has been confirmed in a wide variety of cases.

The Semantic Conception, by contrast, will provide a composite model constructed by referring to models that have worked well in closely analogous situations. So the reference model for the first step might refer to previous experience with pooh-pooh cushions, not general gas laws, the ice-boat model will be analogous to models of moving hockey pucks, etc. Understanding on the SC approach comes from similarity to the nearest neighboring cases, which may in turn be connected to less similar cases. Of course, there are scientific situations in which the only kind of understanding that is available to us is through a network of family resemblances. But it

seems cognitively preferable to have global connections made salient when they exist and this is exactly what the Statement View provides.

The unification account of explanation, though usually presented in terms of statements, incorporates some of the spirit of the SC approach to science. For example, Kitcher presents early genetics as a series of deductive schemata. To account for the distribution of phenotypes in what he calls “pedigree problems” one fills in slots for genotypes, traits, dominance, etc. and then derives the expected distribution. If the given biological system does not fit the Mendel schema, one can move to the Morgan schema or the Watson-Crick derivation pattern. As in the SC approach, one does not test an overall theory; rather one hunts around until one finds a schema that fits. Kitcher also emphasizes that some theories may not contain universal laws; instead they consist of a variety of “mini-laws” each figuring in their own deductive schema. [1989, p. 447]

However, there is this basic philosophical difference between the two approaches. For Kitcher, unification, namely presenting a small number of schemata that will cover a large number of phenomena, is the defining feature of scientific explanation. But on the Semantic Conception, as we saw above, scientists’ preference for an economical set of models appears to be a question of pragmatic convenience, not a central cognitive requirement.

Here is my evaluation so far of how well the Semantic Conception of theories fares when it is taken as the foundation of a general theory of scientific inquiry: It gives no direct rationale for the scientific practice of seeking a variety of evidence and novel test cases – in fact its resemblance account of how scientists apply old models to new instances points in just the opposite direction. It gives only a weak rationale for why crucial experiments between theories (as opposed to between competing models within a single theory) should have the epistemic weight that scientists attribute to them. To say that Einstein is preferred to Newton, or oxygen to phlogiston, or evolution to creationism simply because their models are more widely applicable is too faint praise. The localism of the SC approach that limits the scope of refutation and turns prediction into a search for analogues also yields a feeble account of scientific explanation although I have suggested that it might be strengthened by incorporating some sort of unification view.

A critical look at the prima facie advantages of the SC

I have argued above that the Semantic Conception of theories is hard pressed to account for some of the most basic features of scientific inquiry such as the role of attempted refutation, variety of evidence, and crucial

experiments in the search for theories with great predictive and explanatory power. Nevertheless, there are aspects of scientific practice where the Semantic Conception on the face of it has the upper hand. One of these can be dealt with quickly. It is in fact true that scientists spend lots of time tinkering with models and generally blame them instead of the overall theory when discrepancies between data and prediction occur. This phenomenon can be rationalized in a variety of ways – Lakatos’ Methodology of Scientific Research Programmes was motivated by this very issue. But the core of any Statement View response will rest on the distinction between models of initial conditions (what Lakatos has in mind) and the sorts of theoretical models invoked by the Semantic Conception.

The Semantic Conception also gives a smooth account of the fact that scientific representations rarely fit the data perfectly and that scientists often promiscuously flit back and forth between models according to what is convenient – sometimes earth-centered, sometimes heliocentric. Sometimes chemists use a valence-bond approach, other times they postulate molecular orbitals. There is a quick Statement View response to the first example – scientists use analyses they know to be false as convenient calculating devices, but they rely on the theory they consider to be true (or closer to the truth) to tell them when the old theory can be employed as an instrument.

However, the VB/MO clash is more problematic and it leads us to the biggest challenge that the SC poses: What can the Statement View really say about fields that lack a single, detailed overarching theory? Does not the SC give better advice in such cases? A prime example would be Evolutionary Theory, which Popper at one time construed as unfalsifiable and hence falling on the metaphysical side of his Demarcation Principle. It is no accident that there is strong adherence to the Semantic Conception among philosophers of biology.

I will come back to biology later. But let us first begin with the case of chemists' apparently promiscuous use of Valence Bond descriptions of molecules (e.g., the configuration of methane is tetrahedral because the bonding electrons of carbon are in sp^3 orbitals) versus Molecular Orbital accounts (e.g., the spectrum of benzene is best explained in terms of a sea of electrons on each side of the flat ring). Is this not a clear example of model fitting where there are no theoretical statements underwriting the activity?

Well, this is a sort of messy mixed case. It is true that chemists do not simply model initial conditions and derive predictions from Quantum Mechanics - this is *not* a chemical equivalent of the Halley's Comet problem. Nevertheless, there does exist an overarching theory that guides chemical explanations even when it does not figure directly in derivations.

Thus it is the Pauli Exclusion Principle which tells chemists the properties of the available bonding electrons. Theory thus puts enormous constraints on the VB or MO models that chemists build. Theory also tells us that the electrons in every molecule are differentially localized. Valence Bond approaches work best when there is a high degree of localization; Molecular Orbitals reflect the other extreme. There are independent theoretical considerations that allow chemists to predict for a new molecule which model will give the best fit. The choice is not just one of trial and error or reasoning by analogy.

So what at first appears to be opportunistic modeling turns out to incorporate elements of the conception of theories as generalizations about a domain. The Statement View illuminates more scientific practice than might at first appear. I will now argue that the wider applicability of the Semantic Conception is actually a mark against it because it allows too much to count as scientific inquiry.

A common mode of analysis in literature is to point out structural similarities between plots. These can range from the simple “boy meets/loses/gets girl” recipe for B-movies to Vladimir Propp’s elaborate structural theory of folktales. Recalling the emphasis on polarities and analogies in Greek science and the proliferation of oppositions that are

“good to think” in Levi-Strauss’ structural anthropology, it is evident that the human mind delights in mapping abstract models onto the variegated manifold of experience. In this respect the SC resonates well with very common cognitive strategies. It places Propp’s theory of fairy tales on one end of a continuum that has Newtonian models on the other.

But is this a mark in favor of the SC? I think not. Any philosophy of science worthy of study, even a thorough-going naturalism, needs to criticize modes of thinking that lead us astray. From a Popperian point of view, if there is no falsifiable claim about the structure of folktales, then merely pointing to formal similarities where they can be found (thus ignoring stories which don’t fit the pattern or imposing the pattern on them in an *ad hoc* way) has no explanatory power. We should be prepared to mount similar criticisms of rational choice theories. It is easy to look around at human behavior and pick out examples of people maximizing expected utility. On the SC this is a perfectly acceptable scientific activity. But on the Statement View we would urge scientists to specify the domains where they expect rational choice to work as well as to theorize about what sorts of factors tend to vitiate the rational choice approach. Although the SC offers social scientists a veneer of respectability – just like physical scientists they are

finding places where models fit the world – it comes with a loss of cognitive clout.

But now it's time to talk about the SC's poster child, biological science. How well does biology fare if we require tougher standards than those inherent to the SC? I have neither the space available nor the expertise to whip out a detailed Popperian analysis but I will record a couple of suggestions. My first take on the adaptationist research program would be identical to the above gloss on rational choice theory. One needs to provide some sort of general account, even if it starts out with only a laundry list of problems that organisms have to "solve" in order to survive, that gives at least some guidance to which traits are likely to be adaptations.

The Statement View can be tolerant of weak, incomplete, or vague theories as long as one is trying to improve them. What is not impressive is the opportunistic matching of templates to phenomena where only successes are recorded.

Much of biological research can be viewed as a highly ramified and generalized version of the narrative explanations discussed above. For example, the various steps of the Krebs Cycle fall under different laws of biochemistry; for example, one early step is an case of oxidative decarboxylation. But unlike the Rube Goldberg machine discussed above,

the metabolic mechanism described by Krebs operates in most higher animals. So one can also attempt generalizations about where the Krebs chain of reactions will be found. Current work in evolutionary biology, known as “evo-devo,” also attempts to analyze complicated causal mechanisms and make generalizations about where they occur.

There is no doubt that explanatory practices in biology appear more complicated than that which is presented in textbook physics. And, as we have seen, in such cases it is tempting to fall back on the SC because it makes any sort of modeling activity count as full-fledged science. But when we look more carefully at the achievements of biology and why they are considered great, I think we find the tougher methodological values associated with the Statement View operating everywhere. Biologists do not need to espouse the SC in order to be counted as first-rate scientists! Both the form and the content of scientific theories will depend on the nature of the phenomena they are trying to describe and explain.

But perhaps we should give the SC points for providing us with a congenial way to talk about pre-paradigm science. In a bookstore I once leafed through a hefty volume that presented 27 different theories of personality. (It may have been Burger 1993.) I suppose one could view this largess either as a testimony to the creativity of psychologists or as a

reflection of the complexity of the human psyche. However to me it was a classic indication of the fact that we don't yet have a good scientific understanding of personality. When scientists first explore phenomena they often resort to curve fitting and sometimes develop models with quite limited applicability – one recalls the two equations for Black Body radiation, those of Wien and Rayleigh-Jeans (Kuhn, 1978). But it was only with the development of Plank's theory that one had both a single formula for all wave lengths and a picture of the underlying mechanism that explained it. A similar scenario applies to spectra before the Bohr atom.

On the Statement View, equations whose domain of application is restricted for no reason except the brute fact that they don't fit elsewhere are at best viewed as stepping stones to a general, unified account. On the Semantic Conception, however, curve fitting is a paradigm case of model building; mapping limited aspects of a discrete physical system is not only what scientists do best – it's all that they do! Recent advocates of the disunity of science argue that the ideal of a unified science waxes and wanes according to local circumstances, including political considerations. (For a guide to this literature see Cat.) Here is not the place to review those arguments. I would merely say that the SC meshes nicely with a picture of science as a pluralistic, patchwork quilt of partial perspectives.

The nexus is not a necessary one, however. Giere, unlike the more extreme partisans of disunity, advocates that scientists adopt what he calls a one-world methodological rule: “Proceed as if the world has a single structure. In light of this rule, the existence of conflicting models is an indication that one or both types of models fail to fit the world as well as they might.” [1999, p. 83] Giere’s justification of this rule does not rest on metaphysics, he claims. Instead he would point to episodes in the history of science where following such a rule was fruitful. Once again, we see that the SC must be supplemented in order to bring it into line with exemplary scientific practice.

And I submit that it is extremely important that one adhere to the one-world methodology. One of the most important sources of deep problems in the history of science has been clashes between theories, each of which are eminently successful in their own domains, but which give incompatible accounts in areas where they overlap. Standard examples include the conflict between the Copernican astronomy and Aristotelian physics, wave vs. corpuscular theories of light, the early Bohr atom and Maxwell’s equations. But on the SC none of these inter-theoretic inconsistencies need give scientists pause – it simply means that they must judiciously pick one kind of model to account for spectra and another to account for the behavior of

moving charges. Again, we see that the SC needs to be supplemented if it is to serve as a guide to good scientific practice.

In conclusion

Whatever its flaws (and they were many!) the old logical empiricist account gave an integrated account of the structure of science. The “layer-cake” model, as Feyerabend mockingly called it, provided an excellent jumping off point for the analysis of prediction, confirmation and explanation, and the interplay of theory and observation. Popper dropped the logical empiricist account of meaning and criticized the account of induction that often accompanied it but kept the idea of a layered deductive structure of statements intact.

The Semantic Conception pretty much started over again from scratch. Its proponents were very concerned to give a radically different approach to the problem of how abstract scientific theories are related to the natural world. The early attempts to formalize scientific theories using a set-theoretic approach used as examples universal theories from physics. At this point there was no reason to think that the SC would lead to a radically different picture of science – it was simply clearing up the vexed positivist problem of meaning. But people quickly realized that the SC might be adapted to Kuhn’s philosophy of science as paradigms – in fact Stegmüller

enthusiastically discussed the prospect of “Sneedifying Kuhn”. By viewing theories as tools for building maps, one could bypass many of the worries about truth and verisimilitude. The SC also lent itself naturally to the analysis of sciences such as meteorology where the central projects are the development of computer simulations and models of huge sets of data.

By and large Semantic Conception philosophers have neglected giving a systematic account of scientific methodology. Their intellectual forbears such as early 20th century conventionalists always placed a high value on economy of thought and this allowed them to stress the importance of inter-theoretic as well as intra-theoretic consistency and the search for comprehensive theories that made precise predictions over a wide domain. Giere’s account of science as models tries to include these desiderata.

However, in the discussion above I have amassed multiple instances where his version of the SC fails to give an adequate account of scientific inquiry unless it is supplemented with methodological rules that are often in tension with the central tenets of the SC approach. Many current SC philosophers see science as much more fragmented and in some cases actively oppose attempts to unify science. (See the discussions in Galison 1996.)

The basic issue, however, is not unification or pluralism *per se*. The key problem for any account of scientific methodology is to tell us how to

maximize the critical appraisal of scientific claims. As my first logic teacher Alan Musgrave used to emphasize, logical inconsistency is the engine of criticism. Sometimes the inconsistency is between theoretical predictions and experimental results. Even more challenging are inconsistencies between successful scientific theories. On the Semantic Conception one is invited to take mapping or modeling as the basic activity of science and this very starting point makes it much more difficult to talk about the critical processes that are so distinctive of science – severe testing, clashes between theories, the search for deep explanations. We may someday see an account of science that integrates the good features of model-theoretic devices with a robust, critical methodology, but so far the Semantic Conception approaches do not look promising.

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