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INTRODUCTION

Ontology & methodology

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Philosophers of science have long been concerned with the question of what a given scientific theory tells us about the contents of the world, but relatively little attention has been paid to how we set out to build theories and to the relevance of pre-theoretical methodology on a theory's interpretation. In the traditional view, the form and content of a mature theory can be separated from any tentative ontological assumptions that went into its development. For this reason, the target of interpretation is taken to be the mature theory and nothing more. On this view, positions on ontology emerge only once a theory is to hand, not as part of the process of theory building.

Serious attention to theory creation suggests this is too simple. In particular, data collection and experimentation are influenced both by theory and by assumptions about the entities thought to be the target of study. Initial reasoning about possible ontologies has an influence on the choice of theoretical variables as well as on the judgments of the methodology appropriate to investigate them.

But what do these interrelations between ontological and methodological assumptions really involve, and how do they constrain each other in determining the final form a theory takes? Debates as to whether we should be theory firsters, method firsters, or data firsters arise across the scientific landscape (biology, physics, economics, chemistry, statistics). Each of these positions offers a distinct view of the role of preliminary ontologies in theory construction and how we should extract ontological claims from mature theories. The interplay between ontology and methodology presents a number of questions:

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- What philosophical assumptions underlie debates over the priority of theory, method, and data, how do these assumptions relate to claims about ontology, and how can philosophers of science illuminate these questions?
- Should we distinguish an ontology- from a methodology-driven perspective?
- What insights into the role of ontology in methodology or into the role of methodology in the interpretation of a theory can be obtained from philosophy of experiment and philosophy and history of science?
- Might our current ontology prevent understanding the actual (or at least the best hypothetical) ontology underlying scientific episodes?
- Do the interrelationships of ontology and methodology differ in different fields? (e.g., molecular biology vs. ecology, organic vs. quantum chemistry, economics vs. social science) Do they differ with different types of methodology?
- How do distinct methodological constraints and aims of scientific investigation intertwine with initial conjectures about the entities and processes under scrutiny? How does the interplay between these in turn constrain the theories we accept and reject?
- Does successful methodology presuppose an ontology, or can holders of radically different ontologies (or even agnosticism about ontology) share the same methodology, and agree on empirical regularities?
- Does successful methodology depend on a single correct interpretation for theoretical variables? Or can plural interpretations actually contribute to scientific knowledge?

The papers in this special issue approach these questions from a diversity of disciplinary perspectives. They emerged from the "Ontology & Methodology" conference held at Virginia Tech in May of 2013. That conference brought together prominent philosophers whose expertise spans a wide range of sciences from biology to quantum physics, as well as different levels of method including local experimental hypotheses, statistical inferences, and empirical modeling.

1 Realism, interpretation, and representation

On the occasion of the 25th anniversary of Arthur Fine's *The Shaky Game* (1996), Laura Ruetsche argues in "The Shaky Game +25, or: on locavoracity" that the Natural Ontological Attitude (NOA) is the basis of an alternative to Naturalism, the view that "takes the only reputable metaphysics to be the metaphysics 'read off of' our best scientific theories" (2014, Sect. 1). Ruetsche argues for a set of morals derived from NOA, morals that tell against the "no miracles" argument from successful scientific explanation to realism. Ruetsche argues, instead, for localized realism, based on interpretations of theories within certain contexts in which they are employed. She argues for the disunity of virtues ["The nature of the virtues constituting [a theory's] success, invoked in premise 1 of the Miracles Argument, can't be decided in abstraction from [that theory's] employment in ongoing science," (2014, Sect. 3.3)], and for the dis-integration of virtue ["There may be no single interpretation of a particular theory T under which it exhibits all the virtues cited in the first premise of the Miracles Argument," (2014, Sect. 3.3)]. Ruetsche's account, following on her work in *Inter-*

preting Quantum Theories (2011), concludes with an analysis of "locavoracity" as a positive thesis, investigating problem cases for realism including the Unruh effect and the Hawking effect. Like the no miracles argument and deployment realism, Ruetsche argues, locavoracity can best be supported by the cases in which it is successful as an explanation.

In "Methodological Realism and Modal Resourcefulness: Out of the Web and into the Mine," Lydia Patton evaluates methodological realist arguments—in particular, those which are used to argue further for "robust scientific realism" about entities, and not just structures or relations. Such arguments have been contentious since Laudan's confutation of convergent realism, which brings into question the ontological status of posited scientific entities, including the ether, phlogiston, and caloric. Psillos, Kitcher, and Leplin have defended a sophisticated form of deployment or methodological realism, which divides those entities that are deployed in scientific explanation (working posits) from those that are dispensable (idle wheels). Patton raises a problem for robust deployment realism, the accretion problem. Theories and principles that are explanatory or otherwise empirically successful independently may not be successful or even intelligible when combined. If the best explanation of the empirical success of theories and principles is that they are true, why would combining two empirically successful theories or principles result in failure? Patton argues that Laura Ruetsche's recommendation that theories allow for modal resourcefulness, defended in *Interpreting Quantum Theories* and in Ruetsche's essay for this issue, solves the accretion problem. Modal resourcefulness is the recommendation "that a theory be able to function as a guide in varying modal contexts, without requiring a unifying physical interpretaton of the theory as a depiction of reality." (2015, p. 1).

In "Scientific misrepresentation and guides to ontology: the need for representational code and contents", Shech (2014) picks up on the question of how theories depict the way things are, and of how this reflects scientific methods and the epistemic status of theories and inferences. Shech's contribution takes a close look at the nature of scientific representations as vehicles of inference and as tools to tell us what the world is like. Specifically, he urges us to recognize a variety of features that any suitable account of scientific representation should capture. Given that scientific representations are first and foremost *epistemic representations*—surrogates for reasoning, he draws upon non-scientific accounts of representation to argue that scientific representations are "intentional objects" that come with not only a denotational scheme mapping parts of the representational vehicle to aspects of its target, but with a richer semantic content—that which the representation is about—and what he calls a "code," a system for extracting claims about the target from facts about the representational vehicle. This view of representations implies a number of distinctions that tend to be overlooked. The denotational scheme along with the representational 'code' provide a norm for valid inference. Such inferences can constitute sound representations or misrepesentations depending on whether they actually yield true conclusions about the target. Lastly, we can distinguish an additional sort of misrepresentation, one which emphasizes the distinct epistemic and ontological roles of a representation. A representation, he says, can provide many sound inferences yet fail to describe a target physical system as it is. That is, the representation can fulfill its epistemic duties while failing to provide any sort of guide to ontology. For instance, representing phase transitions via functions exhibiting discontinuities licenses many sound inferences, but in some sense fails to reflect the nature of finite physical systems which cannot exhibit discontinuous changes in properties.

By showing where prominent accounts of representation fail to recognize or account for one or more of these distinctions, Shech provides a sort of roadmap for constructing a better theory of scientific representation. In doing so, he identifies the problem of content determination as a central issue in the philosophy of science. If he is right that denotation is insufficient for representation and that we need further to specify content, then we are led to the significant question of how a representational vehicle comes to have the content it does. Stepping back from questions about the nature of scientific representations, it is also an interesting question whether Shech's distinction between faithful and unfaithful representations (those that can and cannot serve as guides to ontology) can be maintained in an ontologically neutral way (i.e., does it presuppose a strong realism of sorts?).

2 Methods, statistics, and economics

In "Drift Beyond Wright–Fisher," Hayley Clatterbuck provides a case study in the perils of ignoring methodology when drawing ontological conclusions. She does this in the context of the debate over evolutionary drift. Roughly speaking, drift denotes those changes in trait frequencies that cannot be accounted for by processes that move populations in predictable directions (2014, p. 2). The central point of contention amongst philosophers and biologists is whether evolutionary drift is a causal process in its own right, on par with natural selection, mutation, migration, and other evolutionary forces, or whether it is instead a mere statistical truism, a mathematical consequence of random sampling in reproduction. According to the causal theorists, drift is like a force that, when altered, results in changes in evolutionary trajectory. According to the statistical theorist, drift is a statistical summation over genuinely causal properties, and the relations between population size and the probability of significant deviation from expectation are necessary mathematical relations, not nomic causal connections. Clatterbuck points out that both sides of the debate have been working from the same mathematical model of drift, the Wright–Fisher model. But other predictively inequivalent models exist and do a better job accounting for some actual populations. Recognition of the broader class of models suggests a number of novel ways in which one might intervene on a population to alter the process of drift in such a way that one also systematically alters population-level dynamics. This, says Clatterbuck, is strong evidence that drift is in fact a genuine causal process. Whether or not this is the case, the fact that methodological assumptions have led the community to focus on one out of a broad class of models casts doubt on the ontological conclusions drawn from that single model.

In "The ontological status of shocks and trends in macroeconomics," Kevin Hoover (2014) begins by quoting Nancy Cartwright: "Metaphysics and methodology should go hand in hand. Metaphysics tells what something is; methodology, how to find out about it. Our methods must be justified by showing that they are indeed a good way to find out about the thing under study, given what it is" (Cartwright 2007, p. 132).

Hoover endorses this approach, and aims to respond to “Cartwright’s challenge” to the causal modeling enterprise: that the causal connections among variables should not be presumed to be deterministic. He points out that this epistemological challenge leads to a methodological one: how to find ways to justify ontological claims based on causal modeling. Can causal modeling support claims for the reality of unobservable or even indeterministic processes? Hoover takes economic modeling as his chosen context in which to explore how this challenge can be met.

The paper begins by recognizing that economists are “committed implicitly to an ontology of causes”, since they “privilege representations [of data] that support counterfactual analysis and provide a map of effective interventions in the economy”, not just representations that save the phenomena. Hoover focuses on a discussion of shocks, which are “residuals” in structural equations of macroeconomics, represented by error terms. Such error terms represent the discrepancy between the actual value of an equation and the expected value given the background theory or model. These residuals, if they are not a result of misspecification or of measurement error, could represent omitted causes or fundamental indeterminacy. If so, they are called “shocks”, which are “causally significant, unobserved or unobservable real features of the world”. “The cumulation over time of such shocks forms a (stochastic) trend.”

Are economic shocks and trends real? The jury is out. In the case of trends, Hoover urges that “short-run and long-run causal structures can have distinct ontologies and may, in fact, pull in opposite directions”. Moreover, he argues that the well-known strategies of seeking “variety of evidence (i.e., independent methods of indirect measurement that provide essentially the same values for the shocks)” and “transferability (i.e., a shock identified in one context appears to be causally efficacious in another)” are more difficult to use effectively than many who employ causal modeling imagine. “Difficult, however,” he concludes, “is not impossible.” Cartwright’s challenge may not yet be answered, but it is not unanswerable in principle. Hoover concludes that many economists are not yet well enough aware of the deep methodological and ontological puzzles and problems associated with their causal models.

Aris Spanos’s and Deborah Mayo’s (2015) “Error statistical modeling and inference” is a sustained analysis of the difference, in practice, between statistical and substantive models. They have argued in earlier works that this difference is fundamental to statistical methods including misspecification testing, confidence intervals, prediction intervals, and significance testing (Mayo and Spanos 2011). The question of whether the statistical model is independent of the substantive model is central to the discussion. On their account, current problems of replication in statistically modeled sciences tend to blur the question of whether an observed effect is genuine and replicable with the question of whether a claim made by a theory is justified or substantive. Spanos and Mayo defend the view that the two types of models are ontologically distinct, and call for distinct methods of appraisal.

Focusing on econometrics, they argue that justifying the assumptions of a statistical model—a question that, in their view, is largely ignored in replication research—is the most effective way to ensure replicability. The question of whether an effect is replicable, or whether an experiment is a reliable or severe test of an hypothesis, is assessed via the construction and analysis of a statistical model. Whether the entities or processes referred to by the background theory are real, on the other hand, is a matter to be investigated via the construction and testing of a substantive model.

The final sections of the paper focus on the advances that can be made in the replicability discussion by focusing on the misspecification of statistical models. Here, Spanos and Mayo, like Hoover, focus on the error term. Hoover argues that the error term itself can contain substantive information, including unobservable or indeterministic causal processes. In a related but quite distinct vein, Spanos and Mayo observe that instead of probing the source of statistical misspecifications, econometricians often suppose that they can "correct" statistical violations by tinkering with the error term. The resulting model "fits" the data, but as the method had no chance to uncover numerous other models that would fit as well, the inferred correction fails to pass severely. Spanos and Mayo recommend, instead, that one should distinguish between a substantive and a statistical error term with the latter capturing the non-systematic statistical information in the data. Hence, one should regard the original model as an incorrect parametrization of the process underlying the data (Spanos and Mayo 2015, Sect. 4.3) unless the statistical error term is "white-noise", or more formally, a martingale difference process. When this is not the case, the statistical model should be re-specified so that it is adequate to describe the process in question, rendering the error term "white noise". Only then, once we have a statistically adequate model, can the ontological commitments associated with the structural error term, such as causal claims, autocorrelated shocks and omitted confounding factors, be reliably tested.

Erik Angner (2014) considers, in "To navigate safely in the vast sea of empirical facts," how behavioral economics manages to retain key assumptions of neoclassical economic theory while simultaneously replacing them with more psychologically plausible foundations. Angner begins by pointing out that "behavioral economics is motivated by the belief that actual human beings deviate in significant, systematic, and therefore predictable ways from the assumptions of neoclassical economics and aims to replace them with more empirically adequate, or 'realistic,' assumptions borrowed largely from contemporary psychology" (Angner 2014, Sect. 1). The driving force behind behavioral economics is the desire to replace the "great," but "unrealistic," assumptions classical economics makes about human conduct. Angner's account supports the thesis, instead, that with respect to behavioral economics, neoclassical economics supplies a conceptual apparatus that functions as a set of ideal types, especially the ideal type of the "rational agent." Here, Angner draws on Max Weber's notion of ideal types, such as the "capitalistic entrepreneur," "economic action," or "charismatic authority". Even if there is no human being who behaves perfectly as a capitalistic entrepreneur, and even if there not exactly any such thing as charismatic authority, Weber's ideal types yet may be useful to build models describing economics and human conduct (Angner 2014, Sect. 2). As Angner explains, with a final quote from Weber, "ideal types are not assessed by reference to their empirical adequacy. Rather, they are assessed by reference to how successful they are at attaining the goals for which they were constructed"' (Weber 1949, p. 92).

When economic agents differ from rational agents, for instance in exhibiting loss aversion or time inconsistency, behavioral economics often will measure that deviation as a deviation from the neo-classical prediction (Angner cites Tversky and Kahneman as an example of such a method). Angner develops an account according to which the development of behavioral economics is influenced by, or even grounded by, orthodox economic reasoning.

Angner concludes that “neoclassical economics is not helpfully criticized for the mere use of abstract, idealized models and mathematical, formal methods, and that behavioral economics is not usefully criticized for being insufficiently guided by economic theory” (Angner 2014, Sect. 5). Rather, neoclassical economics often is preserved as a “limiting case” of behavioral economics, suggesting that the pictures of human behavior, of economics, and of rationality described by neoclassical and by behavioral theories are linked, not only in their descriptions, but in their methods and dynamics.

3 Ontologies, interventions, and natural kinds

James Woodward’s “Methodology, Ontology, and Interventionism” (2014) begins with explicit definitions of methodology (“how we ought to go about investigating, learning, and reasoning about various aspects of nature, about what sorts of theories we should construct, and about how we should reason about various important concepts in the scientific enterprise”, given our epistemic and practical limitations), and of ontology (Woodward 2014, Sect. 2). Ontology, on Woodward’s account, splits into two approaches: one approach, $ontology_1$, focuses on “what are taken to be the most basic entities or properties or structures in some area inquiry or the most useful or perspicuous way of classifying or conceptualizing these” (Woodward 2014, Sect. 2). Another, $ontology_2$, focuses on certain questions like “what are the Truth-makers’ or grounds’ for causal claims (laws of nature?, powers and dispositions?, relations of necessitation between universals?)” (Woodward 2014, Sect. 2). Woodward uses this account to motivate defenses of interventionism against criticisms by Strevens, Baumgartner, possibly Glymour, Hiddleston, and Reutlinger. These criticisms, on Woodward’s account (Woodward 2014, Sect. 5), stem from asking that interventionism provide an $ontology_2$. But, Woodward responds, while interventionism “may be $ontology_2$ unilluminating... I contend that it is methodologically illuminating.” Drawing on “a recent influential econometrics text, Angrist and Pischke (2009)” as representative of broader scientific practice, Woodward provides an account according to which “hypothetical experiments can figure as a regulative and clarifying ideal in causal inference,” which provides a methodological “benchmark” or “standard” for interventionist accounts. Woodward concludes by enlarging upon his account of scientific practice, including economics, statistics, epidemiology, and so on, for which “it is strategies that fail to provide truth-makers in the metaphysicians’ sense and that are non-reductive that turn out to be the useful and clarifying ones” (Woodward 2014, Sect. 9).

In “Goal-dependence in (scientific) ontology,” David Danks (2015) argues for a startling consequence of the interaction of ontology and methodology: as long as we are precluded from knowing the world’s objects directly, we must accept a goal-dependent pluralism in our scientific ontologies. Assuming science aims understanding how the world “really” is, at producing theories which accurately describe or depict the world, then progress toward that goal can only be assessed indirectly by evaluating how well a theory fits goals such as predictive accuracy, usefulness in the design of experiments and interventions, ability to explain other phenomena, etc. Given this variety of goals against we we must assess our theories, it is at least a conceptual possibility that these goals provide conflicting evidence – while predictive success, for example, may favor theory A over theory B, usefulness in designing interventions to achieve desired outcomes (usefulness in controlling the world) may favor theory B.

Danks provides a pair of case studies that demonstrate this sort of situation is more than an idle possibility. The first, drawn from theoretical and simulation work by Steven Fancsali (2013), considers two scientifically plausible goals: (i) prediction of the value of a target variable T given passive observation of some set of variables in a system, and (ii) prediction of T given an intervention from outside of the system to set or change the values of some of the other variables. Suppose we are given a set of "low-level" variables $\mathbf{R}$ and wish to use observations of these variables to construct the "right" ontology, the set of "high-level" variables $\mathbf{C}$ defined in terms of $\mathbf{R}$ that best satisfies our goals. What Fancsali showed and Danks stresses is that the answer provided by each goal is distinct. In other words, accurate prediction of T given passive observation suggests one set of high-level variables, while accurate prediction under intervention suggests another, distinct set. This pluralism remains even in the limit where the simulated learner receives infinite information about the values of variables in $\mathbf{R}$. The second case study is drawn from climate science. Here, we find two conflicting criteria for carving the ocean into regions that act in some way like distinct "objects." One criterion concerns the internal coherence of each region—regions for which interior properties are highly correlated. The other criterion involves interaction with other ocean and terrestrial regions—the more an ocean region is stably related to long-range climate impacts, the more object-like it is. Danks cites recent work to demonstrate that in the actual world, these two criteria pull in different directions. The upshot of both case studies is clear. Assuming that we reject a strong eliminativism and recognize that some things above the level of fundamental phsyics warrant some ontological status (there really are birds, for instance), then real-life science presents us with ineluctable pluralism.

Finally, in "Projection, symmetry, and natural kinds," Benjamin Jantzen (2014) urges a view of natural kinds that suggests a data-driven approach to ontology construction. Specifically, Jantzen takes on the problem of "natural kinds" in its older epistemic guise. That is, he is concerned with the problem of identifying kinds that are projectible in Goodman's sense of supporting induction. He begins by distinguishing two questions: (i) what, if anything, distinguishes those kinds which are projectible from those that are not; and (ii) by what methods can we efficiently identify kinds that are likely to be projectible? As Jantzen points out, a variety of metaphysical accounts of natural kindness have been advanced to answer the first question, but none have much to say about the second. For instance, essentialism about kinds—the view that membership in a natural kind is determined by possession of a necessary and sufficient set of intrinsic properties—may explain why some kinds are projectible and others not. Since essences are inhomogeneous, however, essentialism offers no advice with respect to finding natural kinds. Assuming that "any theory of projectibilty that can answer both (P1) and (P2) is superior, ceteris paribus, to a theory that answers only one, and a theory that answers them for both system and kind induction is superior to a theory that doesn't," Jantzen argues for a superior alternative, one with significant methodological implications.

Jantzen argues that the way forward is to focus on causal structure, but only indirectly. He points to a range of examples from a diversity of sciences, including chemistry and particle physics, to motivate the view that natural kinds are characterized not by any particular causal structure but by special properties of causal structure. Specifically, he suggests that members of the same natural kind are collections of causally connected variables that are identically indifferent to a set of interventions he calls 'dynamical symmetries'. More specifically, a dynamical symmetry with respect to a variable, V, is an active transformation of a system with a special property: it makes no difference whether one applies the symmetry transformation and then intervenes on V, or intervenes on V and then applies the symmetry transformation since the final state is the same either way. In classical mechanics, examples of dynamical symmetries with respect to time include rigid translations and rotations. Symmetry transformations compose—a symmetry followed by another is itself a symmetry transformation. A set of dynamical symmetries along with a particular pattern of behavior under composition constitutes what Jantzen calls a 'symmetry structure'. A set of causal systems sharing a common symmetry structure is a natural kind. As Jantzen points out, if symmetry structures characterize natural kinds, then a plausible answer to question (ii) is that we efficiently identify suitable scientific kinds by spotting symmetry structures. If this is correct, there are methodological consequences. The final section of Jantzen's paper is dedicated to sketching out a path toward algorithmic discovery of natural kinds by detecting dynamical symmetries.

4 Conclusion

The papers in this collection fall rather naturally into three sections: those concerned with realism and the way theories represent the world, those dealing with statistical methods and confirmation, and those focused on experimental intervention and on identifying variables and natural kinds. Within this classification, we can identify a set of more precise questions and themes raised by the contributions.

Quantum physics is known to be a hard case for robust scientific realism. More generally, certain theories and phenomena, including quantum theories and the phenomena associated with phase transitions, are known hard cases. Shech develops an account of representation according to which representations associated with scientific theories do not have to be reliable representations of ontology to function as epistemic guides. Ruetsche's paper is a recommendation of "locavoracity," of a preference for local and context-motivated explanations over global ones. Patton's essay, in the same vein, recommends evaluating theories on their "modal resourcefulness," on their ability to guide research in distinct modal contexts. Both argue that these preferences have consequences for our account of scientific representation. These papers evaluate the significance of scientific practice, methods, and history for our account of how scientific theories represent.

At least since Carnap, Popper, and Hempel, philosophy of science has had an enduring interest in the theory of confirmation. In practice, confirmation theory is allied with statistical methods in science. The papers by Hoover and by Spanos and Mayo raise the question: what is to be done when a theory or model fails? If there is an error term that appears to encode substantive information, for instance, how is the theory or model to be revised to take the discrepancy into account? Spanos and Mayo argue that any steps taken should rely first on an analysis of the statistical model in use. For instance, is the statistical model misspecified, that is, is it inadequate to represent the data in play? Only then should the substantive model, and inferences from that model, be evaluated.

While their papers also deal with confirmation and statistical methods, Clatterbuck and Hoover focus on the ontology of causal processes. Clatterbuck's chosen question is the use of mathematical models to represent "drift" in evolutionary biology, while Hoover homes in on the question of whether economic "shocks" and "trends" are deterministic causal processes, and whether they can be modeled as such. Angner brings a broader scope to the discussion of economics, addressing the relationship between classical and behaviorist economics as a case of the use of idealizations (in this case, 'ideal types') in scientific modeling.

All the papers, in one way or another, deal with the use of models and theoretical structures in drawing inferences and in scientific practice. In the final section, Woodward, Danks, and Jantzen consider this question specifically and in detail. How can interventionism and inference from experiment support a kind of ontology (Woodward)? What are the consequences for ontology of the fact that scientific progress can only be assessed against indirect goals rather than directly against success at representing reality (Danks)? How are the variables of interest of a scientific theory chosen, and how are structures that characterize those variables used to support claims that a theory has identified a natural kind (Jantzen)?

The papers cluster around a central set of issues, including scientific practice, representation, modeling, confirmation, and inference. Readers may find it illuminating also to look for dissimilarities among the approaches. For instance, the approaches based on confirmation of theory by evidence do not focus as closely on the question of how theories represent reality. Is it possible to bring these approaches together?

The papers that deal with the question of representation suggest a novel concern with fragmentation or fracture of the scientific picture of reality. This is a contemporary phenomenon, belied by the "web of belief" or coherentist views espoused by Quine and others—which raises questions of its own, including the extent to which science can function as a reliable guide to epistemology, much less ontology, if its image of reality is broken. The development of the new, fragmented theory is driven by practice and by history, but what are the consequences of adopting such a view?

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