How We Dapple the World*

Paul Teller†

This essay endorses the conclusion of Sklar's "Dappled Theories in a Uniform World" that he announces in his abstract, that notwithstanding recent attacks foundational theories are universal in their scope. But Sklar's rejection of a "pluralist ontology" is questioned. It is concluded that so called "foundational" and "phenomenological" theories are on a much more equal footing as sources of knowledge than Sklar would allow, that "giving an ontology" generally involves dealing in idealizations, and that a transfigured "ficitonalism" provides an (in many respects) better model of scientific knowledge than the model of "foundational truths."

1. Introduction. In "Dappled Theories in a Uniform World" (2003)[1] Lawrence Sklar has defended some traditional ways of thinking about "foundational" theories in physics against recent attacks by Cartwright and others. Sklar acknowledges that science as a whole has a deeply pluralistic methodology. But he argues, "[n]othing in the admitted variety of our conceptual and explanatory schemes. . .by itself is good reason for denying the universal domain as the appropriate domain for the truth of foundational physics" (441). And to the question, "Does the seemingly ineliminable plurality of explanatory schemes imply a plurality of ontologies as well?" (428) he concludes: Definitely not!

I have two aims in the present essay. First I will critically examine Sklar's arguments. I will agree with the argument for universal applicability of foundational physics, in a sense to be clarified. But I will question the argument against a pluralist ontology.

Sklar's way of seeing this subject is widespread, familiar, and deeply entrenched. Dislodging it can't be accomplished simply by picking at flaws. Thus my second objective will be to sketch an alternative, showing how it arises naturally from attention to difficulties with the familiar arguments that Sklar presents. Pivotal in this development will be the

*Received May 2003; revised December 2003.

†To contact the author write to Department of Philosophy, University of California, Davis, CA 95616; e-mail: prteller@ucdavis.edu.

1. Unless otherwise indicated, all page numbers are to this work.

Philosophy of Science, 71 (October 2004) pp. 425–447. 0031-8248/2004/7104-0001$10.00
Copyright 2004 by the Philosophy of Science Association. All rights reserved.

https://doi.org/10.1086/423625 Published online by Cambridge University Press

idea that what is described when one "gives an ontology" is usually, if not always, an idealization. Recognizing "ontologies" as idealizations will reconcile the conflict among alternative ontologies that Sklar thinks stands in the way of endorsing a pluralist attitude towards our characterizations of what there is.

Before beginning I need to issue a caveat about what is here to be understood by "foundational physics": Is it the foundational physics we now have, or "our desired ultimate theory" (439)? On pages 440–441 it appears that Sklar may have the latter in mind. Much of his essay leaves this open, and his detailed descriptions of present theory may leave one with the impression that it is foundational theory, as it now exists, that is in question. There need be no dispute about this issue, for there are really two quite separate interpretive projects that are in question: (1) Giving an interpretation of the science we actually have or any science that resembles it with respect to the sorts of imperfections on which I will focus. (2) Giving an interpretation that would apply to the sort of ultimate exactly correct "theory of everything" that has long been the dream of physics. My project is exclusively the former. Insofar as Sklar has the latter in mind, my current work misses his intended object. Even so, I trust that what I have to say will be of interest to those who take interest in the former project, as the arguments Sklar gives can be read as pertaining to science as it actually exists, and the discussion will lead to a line of interpretation of currently existing science that I hope many will find of worth considering.[2]

2. The Questions. Sklar agrees that the overall methodology of science is deeply pluralistic:

> First let us all agree that the explanatory and predictive methods of science do, indeed, constitute a diverse plurality. The generalizations we invoke for explanation and prediction in sociology, anthropology, linguistics, evolutionary biology, and even in chemistry for the most part, are not the principles invoked in our foundational physics. Even in the realm of physics itself, most of our explanations are not framed in terms of the laws of the foundational theories. When we predict the behavior of falling rocks, moving fluids or energy transforming steam engines, we don't use relativistic quantum field theory. We use

2. Carl Hoefer (2003), drawing on some of the same considerations to which Sklar also appeals, argues that the success of current theories (and their approximate truth in certain domains at least) gives us reason to believe that there is one (or more) exactly true theory waiting to be discovered.

https://doi.org/10.1086/423625 Published online by Cambridge University Press

> instead the Newtonian theory of rigid bodies, hydrodynamics or thermodynamics. (428; cf. also 429, 430, 433, 440, 441)

Sklar asks: What follows from this pluralist methodology? Sections 1 through 4 raise a range of interconnected (and not always clearly distinguished) more specific issues, of which I will focus on the following three:

1. Questions about the domain of foundational theories: "Are the pretensions of foundational physics to be a universal description of how everything is legitimate pretensions?" (430).
2. "But what are the legitimate conclusions that can be drawn about ontology from this admitted methodological pluralism?" (429). Sklar appears to presuppose that foundational theories specify a specific ontology. He then asks, why suppose that this ontology is a "universal structure for the world" (427), "A universal ontology of the world as a whole" (429); or is the acknowledged methodological pluralism grounds for "some kind of genuine plurality of ontology in the world?" (430, cf. also 427). Sklar pretty clearly intends that the ontology of foundational theories is universal in the strong sense that it excludes the conflicting ontologies of phenomenological theories. For example, he insists that

> foundational theory includes within its domain of applicability as a describer of what things there are and how they are, all the things and features within the domain of the phenomenological theory. That the phenomenological theory might in practice be the only epistemic route into finding the connecting links between the theories doesn't in any way entail some autonomous ontological domain for its application. (437)

3. The foregoing strong reading of "universal ontology" is also encouraged by Sklar's attitude towards the third issue, of "whether the 'special' ontologies of the phenomenological sciences are to be understood not as meant to characterize 'how things are' at all, but to serve as 'useful fictions' in some sense"(429).

3. Two Things to Keep an Eye on. In framing this third question Sklar presupposes a sharp dichotomy between descriptions intended to describe "the way things really are" versus merely "useful fictions." Again, he writes:

> How are we to determine if the characterization of a system within science is meant to describe the real system or if, instead, it is merely intended to provide a "useful fiction" that is meant to have predictive value but not to characterize how things "really are?" (431; cf. also 429, 438)

https://doi.org/10.1086/423625 Published online by Cambridge University Press

Second, throughout much of the essay Sklar writes of foundational theories as providing true descriptions. For example, he refers to "applicability, in the sense of truth, of foundational physics" (438) and to a law's "applicability not for 'practical' purposes of prediction and explanation, but applicability in the sense of truly governing the behavior of a system" (434). He asks: "So why should we believe the foundational quantum theory is true of [things like rocks, trees, and cats]?" (435). He concludes that "But, once again, such work in no way tells us that the foundational laws do not truly apply to the systems in question" (437–438). At the very end Sklar takes back this talk of truth: "Do I believe that our current best foundational theory is universal in its domain? No, because I don't believe that it is true of any phenomena at all," (440) though two paragraphs later he reverts to exposition in terms of 'truth':

> Nothing in the admitted variety of our conceptual and explanatory schemes, even if that variety is admitted to be intrinsically ineliminable for adequate description and explanation of the world, by itself is good reason for denying the universal domain as the appropriate domain for the truth of foundational physics. (441)

Many of Sklar's arguments depend for their cogency on the assumption that it is truth, not some kind of approximation to truth, that is in question. In particular, I will be calling the presupposed dichotomy between descriptions of the "way things really are" and "useful fictions" into question by showing how its apparent robustness turns on the presumption that descriptions that count as telling us "the way things really are" are true descriptions. I will then examine in detail the consequences of letting go of both these sharp contrasts.

4. The Argument for the Universal Domain of Foundational Physical Theories. We can understand Sklar's claim that foundational physical theories provide "a universal description of how everything is" as comprising the following two more specific claims: First,

> the claim that all of the things of the world are composed of the elementary constituents described by the fundamental theory. It is because they believe that trees and rocks, stars and planets, monkeys and galaxies are made up out of the elementary particles that those who claim a universal domain for fundamental physics make such a claim. (433)

And second:

> the laws of the fundamental theory are as true of these objects as

https://doi.org/10.1086/423625 Published online by Cambridge University Press

> they are of the carefully isolated systems of small numbers of particles constructed in the laboratory. . . (433)

Sklar is concerned with

> the skeptic who notes both our inability actually to use the foundational laws for predictive and explanatory purposes outside extremely limited laboratory situations and our ability to make predictions and offer explanations for the behavior of the systems outside the scope of foundational physics for these purposes by using autonomous conceptual schemes and lawlike regularities appropriate to the systems in question.. . . What right, then, does "science itself" give us to think that [a system composed of a vast number of complexly related constituents] is governed by the foundational laws? (434)[3]

Sklar offers a number of considerations. There are cases in which the laws of fundamental micro-theories are tested in the macroscopic domain—Sklar cites macroscopic superposition as an example (435). He also cites the use of foundational theories to explain the success of phenomenological theories:

> [T]here are major programs of science itself that are specifically designed to account for the fact that we are so successful in meeting our aims of prediction and explanation when we use theories that the fundamental science tells us cannot possibly be really correct in the contexts in which they are applied. (435)

More generally, I think I can summarize much of what Sklar has in mind with the thought that, were foundational microtheories to fail, we would expect them to fail for complex objects. But every effort to find some such limitation—every effort to falsify such theories in the macroscopic domain—has come to naught. While detailed predictions from foundational micro-theories of the behavior of macroscopic objects may be few and far between, there are many indirect consequences of these theories for the macro-domain, and none of these consequences have proven false.

I agree that such considerations provide very good reason for taking our foundational physical theories to have a universal domain in the sense of the forgoing two more specific claims that "the laws of the fundamental

3. Cartwright, clearly Sklar's target, has retracted the positive claim that the world is "dappled," though she may still not agree with the specific arguments summarized in this in section. See Teller (forthcoming, section 5) for a summary of her argument, and her more recent qualifications.

https://doi.org/10.1086/423625 Published online by Cambridge University Press

theory are as true" of the microconsituents of macroscopic objects as they are of microconstituents isolated in the lab. This comes to agreeing that there are, "in principle" Hamiltonians that, when put into the Schrödinger equation, describe the behavior of macroscopic objects as well as the Hamiltonians that we supply to describe laboratory situations work in those very restricted cases. The situation for relativistic quantum field theory is more complex and will be examined below.

5. Sklar's Rejection of a Pluralist Ontology. The foregoing arguments, and the conclusion with which I have agreed, do not themselves directly bear on Sklar's rejection of a "pluralist ontology," or the ontologies of phenomenological or "special" sciences. A further argument is needed:

> Certainly no one could maintain that we ought to think of each and every mode of conceptualization and explanation applied to a system as genuinely characterizing "what that system is really like." (430)
>
> Other ways of characterizing systems in our higher-level sciences do seem incompatible with foundational physics. If we treat a physical system as being a continuous fluid, we can't be at the same time treating it as made up of discrete particles. But we have already emphasized the fact that we simply cannot take all of the conceptual schemes under which we subsume systems as applying to it "really" in any naively realistic way. Multiple incompatible schemes applied to one and the same system, each one of which has its legitimate descriptive and explanatory use, cannot all be intended to be genuinely "true" of the system. We must allow for "convenient fictions" in our characterization of our methods even if we are realists and not the kind of very radical pragmatist that views all such conceptualizations as *equally* "fictional" (whatever that would mean). (438)

I take this argument to be: Where ontologies conflict, at most one can be right. Foundational theories are true, phenomenological theories merely false, "useful fictions," so we should accept the ontology of true, foundational theories and not those of false phenomenological theories. But if this is the argument, or if the argument turns in any way on the truth of the foundational theories, the argument is compromised by Sklar's acknowledgement (440, cited above), that all of our current foundational theories are false. It goes without saying that there cannot be several literally true but conflicting ontologies. But in light of the foregoing acknowledgement, this shows nothing about the ontologies of the theories we actually have. Thus, if Sklar means to be drawing conclusions against pluralist ontologies of our actual theories, a further argument is required.

In the body of the essay Sklar does appear to have the ontologies of our current theories in mind, for he responds to this worry:

https://doi.org/10.1086/423625 Published online by Cambridge University Press

> The very distinction between a descriptive scheme as "straight-forward" and one as "merely fictive" is, to be sure problematic. How, in detail, such a way of thinking about things is to be made clear is an open matter. Yet there is something very different between characterizing an atomic nucleus as a complex system of neutrons and protons, with these composed of quarks bound by gluons, and with the neutrons and protons bound by a van der Waals residual effect of the quark-quark binding, and a characterization of a fissionable nucleus as a "liquid drop" held together by a "surface tension."
>
> The former description is at least a part of a structure "on the road" to our desired ultimate theory. The latter is intended, from the start, as nothing more than a weak model adequate only in the most restricted ways to characterizing what is really going on.
>
> Much more needs to be said about such distinctions, and making all of this clear is, again, by no means a trivial matter. But that need not reduce us immediately to an "its all a bunch of fictional models" approach to our understanding of the aims of theory. (439)

Why should we endorse the ontologies of atomic and quark theory, but not, for example of the liquid drop model of the nucleus or the continuous fluids of hydrodynamics? Because, it is now said, the former are "'on the road' to our desired ultimate theory" while accounts like "[t]he latter[are] intended, from the start, as nothing more than a weak model adequate only in the most restrict ways to characterizing what is really going on." This argument turns on the metaphor of being or not being "on the road to our desired [I presume Sklar means true] ultimate theory." To evaluate the argument I will look at a number of ways that this metaphor might be elaborated.

First elaboration: One might take our current theories to be false simply out of caution, given the historical record, so that presuming them false is a way of taking our current theories to be provisional. On this reading the proposal is that we can provisionally accept the ontologies of theories of which we do not yet know that they are, or at least do not yet know specifically the ways in which, they are false. But as Sklar acknowledges, we know that all our current foundational theories are incorrect and in fact know in a great deal of detail, some of which I will summarize below, how and in which ways they fail.

Second elaboration: The quotation suggests that we have given up on some theories—they are no longer "'on the road' to our desired ultimate theory." The ones classified as useful fictions are dead ends, of limited use. The ones still "on the road" are thought to be improvable ultimately to the correct theory. Making out this proposal turns on making out what

https://doi.org/10.1086/423625 Published online by Cambridge University Press

is now the metaphor of being "[still] 'on the road' to our desired ultimate theory."

Should we say that a theory counts as "still on the road" because we do not yet know what specifically is wrong with the theory? But we do know—that was the proposed first elaboration. The difference between the "useful fictions" and the currently imperfect foundational theories that, nonetheless, are intended to tell us "how things really are" can at best be a matter of degree—not a difference between dead ends and live options but a matter of being less or further "down the road" to the truth.

But how can a difference among false theories, or being further or less far "down the road" to the truth found a preferential attitude towards ontology when all the theories in questions have known failings? Perhaps it is because, although we know our foundational theories to be quantitatively inaccurate in various respects, we have no reason to suspect their "qualitative" ontologies. On the one hand there are theories that are seen as basically sound, describing what things there "really are" although we acknowledge that the quantitative description that our theories give of these things is not quite right. Presumably our best candidate for such a theory would be our most accurate micro-theory—relativistic quantum field theory and its specific realization in the "standard model" of the electro-weak theory and quantum chromodynamics (QCD). The claim is that, although we know, for example, QCD not to be completely accurate, we have the best of scientific reasons to believe that the entities it postulates—the quarks and gluons—do exist. In particular, the quarks have been detected in deep inelastic scattering experiments. Other "phenomenological" theories are ones for which we have reasons to believe, not just that their descriptions are not completely accurate, but to believe that the entities they postulate do not really exist.

In sum, the proposed contrast is between ontologically sound but quantitatively not completely accurate descriptions of things that (we have good reason to believe) in fact exist, and useful but ontologically erroneous descriptions such as ones appealing to point particles and continuous fluids. I will mark the purported distinction as one between approximations and idealizations.

Beyond giving examples (Sklar repeatedly cites the example of the continuous fluids of hydrodynamics as "useful fictions" (429, 438)), how can this contrast be made out? Original intent will not do, the foregoing quote from page 439 (cf. also 429, 430, 431) notwithstanding. Sklar insists that when it comes to the problem of "of deciding which descriptive schemes are to be taken as straight-forward and which as useful fictive modes of characterizing the world . . . much useful and sometime very hard and very brilliant science is devoted to just that question" (438–439; cf. also 431).

https://doi.org/10.1086/423625 Published online by Cambridge University Press

Making the needed generally applicable and principled distinction between approximations and idealizations promises to be difficult. But perhaps we can, following Sklar himself, let science itself render well-defended conclusions in specific cases:

> How are we to determine if the characterization of a system within science is meant to describe the real system or if, instead, it is merely intended to provide a "useful fiction" that is meant to have predictive value but not to characterize how things "really are"? . . .
>
> These questions can only be answered, I claim by consulting the science itself. . . . Science is replete both with schemes intended to truly characterize "how things are" and with other schemes intended only as knowingly false but useful models of the real situation. But only the science itself can do the job of explicating the intended purpose of its own descriptive and explanatory schemes. (431; cf. also section 4, passim, and 439)

However, this approach is still troubled. Sklar reports that science itself declares the continuous fluids of hydrodynamics to be merely "useful fictions" (429, 438) while he takes science to be telling us that there really are quarks and gluons (439). Yet our current best theories clearly tells us that the latter are idealizations every bit as much as the former.

QCD works by quantizing normal modes, what are called the positive and negative frequency solutions of a wave equation. The quanta—in particular the quarks and gluons of QCD—are then supposed to be "excitations" of these normal modes. But in any model that characterizes space-time as irregularly curved, as do our most accurate models of the structure of space-time, there are no positive and negative frequency solutions of the field equations. States described by such solutions are idealizations every bit as much as the idealization of a liquid as a continuous medium. Thus we may paraphrase Sklar's grounds for taking continuous fluids to be useful fictions, when he writes that

> We do, of course, speak of flowing fluids as continuous media, say. And it is certainly true that if they really are such things, where 'continuous' really means just that in its fullest sense, they cannot be the sorts of things described by the foundational physical theory. (429)

insisting that, equally,

> [I]f they really are such things, where ['normal mode'] really means just that in its fullest sense, they cannot be the sorts of things described by the foundational physical theory [of irregularly curved space-time]. (429)

https://doi.org/10.1086/423625 Published online by Cambridge University Press

While my examination has not, of course, been exhaustive I tentatively conclude that the metaphor of "farther down the road to the truth" won't help with driving a wedge between acceptable and unacceptable ontologies. Questions of ontology may be taken to be sharp—either there are such things or there are not. With the question thus understood it is hard to see how being "further down the road to the truth" is a relevant difference. Or questions of ontology are (somehow) not sharp. Below I will suggest that what have been described as "competing ontologies," the ontologies of continuous hydrodynamics and of QCD alike, are alternative idealizations used in characterizing what there is in the world. None of these are completely accurate. But a great many of such competing "ontologies" have complementary virtues in informing us about the world.

Thus far in examining Sklar's argument I have accepted the assumption underlying the metaphor of theories ordered as farther or less far "down the road" to the truth, the assumption that theories can be ordered as to being closer or farther from the truth. Even on the assumption that there is such an ordering we have not yet been shown how that assumption applies to reject a pluralist ontology for our currently existing theories. But the assumption itself requires critical scrutiny.

6. Is There a Road to the Truth? General Considerations. The assumption in question is that there is an intelligible and useful notion of approximate truth, variously called "verisimilitude," "closeness to the truth," and "truthlikeness"—I will use these expressions interchangeably—that orders false theories in an absolute, context independent manner as to how "close to being true" they are supposed to be. There is a vast literature attempting to make out this notion, and an equally vast literature critical of such attempts. Of course, Sklar does not need a numerical measure of closeness to the truth, or that all theories can be compared as to which is closer; he needs only that in certain important cases at least a qualitative but context independent comparison makes sense. But even so much an assumption is problematic. As I have written on this question elsewhere, I will here briefly summarize my conclusions relevant to this discussion.[4]

David Miller has provided examples in which, for a given measure that orders statements as to closeness to the truth and two statements, the first closer to the truth by that measure than the second, there is a transformation on the statements that produces two new statements that are, respectively, logically equivalent to the first two, but for which the closeness to the truth by the measure has been reversed. I provide an example,

4. See Teller 2001, section 5, for the details and relevant references.

https://doi.org/10.1086/423625 Published online by Cambridge University Press

mimicking Miller's infamous "weather" example, that shows that this linguistic transformation reflects a contrast in respects in which statements can be compared for closeness to the truth: Of two statements, one can be closer to the truth in one respect, or with respect to one consideration, while the second statement may be closer in another respect. The example also indicates that the issue cannot be evaded by retreating from a linguistic formulation to the content carried by a statement. Peter Smith gives a similar example for a continuous case. The examples pretty clearly generalize.

Room does not permit going over these examples and arguments here, but the reader can get a qualitative idea of the argument from the following considerations: The point is, at bottom, really the same as that about similarity. Any two objects are similar in countless respects and dissimilar in countless others. For respects that come in degrees, such as size, of two objects one may be more similar to a third in one respect, while the second is more similar to the third in another respect. The point about closeness to the truth is really the same. Characterization of an object or situation is "close to the truth in a respect" just to the extent than an object or situation as described by the characterization is similar to the actual object or situation in the respect in question.

Some may want to maintain that certain respects are privileged in some way. If so, we are owed an argument why we should take any such privileging to be other than interest relative.

So on general grounds, there is no road to the truth. Comparisons among false statements for truthlikeness makes perfectly good sense when they are relativized to a respect or kind of characteristic. But this makes comparison among false statements for truthlikeness an indefinitely various affair, not a simple or even partial ordering.

7. Are We on the Road to the Truth? Consideration of Specific Cases. We need to be clear headed about what we have in our "foundational theories," about what they actually give us.

To illustrate with what is not now considered a foundational theory, what does Newtonian physics give us, say, with the equation $F = ma$? The equation, $F = ma$, is, by itself, extremely non-specific, really not a description but a description schema. To get much of a description of any real situation one must fill in the force function and then solve the resulting equations. The general information tacitly carried by "$F = ma$" itself is, at best, no more than the claim that, for any system, there is a characterization of the forces on the constituents of the system and that the resulting equations have approximate solutions correctly describing the motion of the system. (The n-body problem in Newtonian mechanics provably has no closed solution for $n > 2$!)

https://doi.org/10.1086/423625 Published online by Cambridge University Press

For the reasons outlined above, in this respect I have no disagreement with Sklar that we have excellent reasons for taking "the universal domain as the appropriate domain for the [approximate!] truth of foundational physics" (441). (As Sklar has agreed two paragraphs before this statement that all our foundational theories are at best approximate, the "approximate" has to be inserted, and we may as well include Newtonian theory as foundational along with its successors.)

What does this kind of conclusion tell us about ontology? For reasons outlined in Section 5 and elaborated below, we know that all actual and even "in principle" resulting descriptions are inexact and idealized. So they do not give us any *exactly* correct answer to Sklar's question, "What is there in the world, and what is like?" (424). They do, of course, in many respects give us a great deal of relevant information. But the same can be said about the more specialized "ontologies" of theories like continuum hydrodynamics.

Let us move on to our improved foundational physical theories of the microscopic. The story plays out in very much the same sort of way for conventional quantum mechanics as it did for Newtonian mechanics, with the Hamiltonian playing the same role as did the force function. But when we look at the structure of relativistic quantum field theory and its current realization in QCD and the standard model the case for universal approximate applicability is weaker than for the prior cases, and the content is even more limited.[5]

Such theories begin by writing down an equation for the free "quantum field," that is what the field would be if, contrary to physical necessity, there were no interactions. This equation can be solved, giving the "operator valued quantum field." This solution is not a description of any specific state of even the counterfactual world without interactions. Roughly speaking it is really no more than a restatement of the equations with commutator conditions imposed.[6] Next one adds an interaction term to the free field equations, guessing at the interaction term, usually with the simplest possible candidate that is consistent with various symmetry constraints. No one has any idea of how to solve the equations with interaction included, or even if there are solutions. Instead one uses approximation schemes, for the most part based on a power series expansion in terms of the solutions of the free field equations.

Most of the so-called foundational theory is constituted by the ap-

5. The following description applies to all "practical" relativistic quantum field theories, such as quantum electrodynamics and quantum chromodynamics. Axiomatic quantum field theory has an entirely different structure. I do not consider it here since its application to experimental practice is extremely limited and, at best, schematic.

6. This gloss cuts some corners. See Teller 2002 for details.

https://doi.org/10.1086/423625 Published online by Cambridge University Press

proximation schemes—including most of our understanding of the quarks, gluons, and other quanta. The resulting 'theory', that is the approximation schemes, is breathtakingly exact for certain very special questions, but also in a great many respects severely limited in what it covers. The direct application of the principle approximation scheme is to scattering problems, that is, descriptions of what will happen in the "infinite future" to simple probes described as coming in from the "infinite past." While we know how consistently to apply this scheme to calculate predictions for individual scattering experiments, if the scheme is taken to provide a unified picture describing at once scattering phenomena and the existence of real quarks and gluons between preparation and measurement, the scheme is formally inconsistent (Haag's theorem—see Teller 1995, 122–124 for a summary of this issue). Scattering amplitudes can be calculated for individual scattering phenomena, but cannot consistently be fitted together to describe a unified physical process, making the direct application of the scheme as good a candidate as one could imagine for an "instrumentalist" theory.

There is also an enormous amount of further approximative application of the theory. The methods of effective field theory provide ways of suppressing or summarizing aspects of the theoretical description that are significant only at high energies, resulting in various lower energy approximative field theoretic descriptions with various technically desirable and convenient characteristics. Then any and all of these field theoretic approaches can be harnessed as a means of estimating various parameters that are plugged back into more familiar but less "foundational" theoretical devices, such as the Dirac equation or even the non-relativistic Schödinger equation. None of this works by deriving results from "first principles."

How does this sketch square with Sklar's example of the account "characterizing an atomic nucleus as a complex system of neutrons and protons, with these composed of quarks bound by gluons, and with the neutrons and protons bound by a van der Waals residual effect of the quark-quark binding. . ." that is lauded as "at least a part of a structure 'on the road' to our desired ultimate theory" (439)? The quarks and gluons are creatures of the approximation schemes insofar as our understanding of them is largely hostage to these schemes. One has confidence in the postulation of these quanta and their properties because the postulation dramatically refines agreement between the scattering theory, as sketched above, and experiment. Once one has the foregoing evidence for the existence and properties of quarks and gluons, their characterization is used in fashioning refined models of protons, neutrons and nuclei as outlined above, usually by fitting the features gleaned from the approximation scheme

https://doi.org/10.1086/423625 Published online by Cambridge University Press

into more manageable and familiar, but hardly "fundamental" theoretical devices, such as the Dirac and Schrödinger equations.

What, then, can we say about the "ontology" to be gleaned from relativistic quantum field theory? The foregoing shows the severe limits on what the theory tells us about the basic constituents of physical matter. In addition we must include here the point mentioned earlier, that the whole quantization scheme turns on the idealization of normal oscillatory modes, the "positive and negative energy solutions" to the underlying (free!) equation. The quarks, gluons, and other quanta are excitations of these idealized oscillatory modes. But the oscillatory modes are well defined only when we model space-time as flat or regularly curved.

Why then should we accept the "ontology" of quantum field theory, qualifying as "part of a structure 'on the road' to our desired ultimate theory," and reject the continuum ontology of hydrodynamics as "nothing more than a weak model adequate only in the most restricted ways to characterizing what is really going on" (439) when both cases similarly involve idealizations that distort the "kind" of object in question?[7]

8. Royal Road to the Truth, or a Multitude of Limited Probes of Reality? I have briefly reviewed general considerations that show that "closer to the truth," "more accurate," and the like make perfectly good sense, but only in a relational way, relative to things like aspects and features that in turn may be variously evaluated in relation to our interests. I have also reviewed in greater detail ways in which one of our most detailed "foundational" theories exhibits severe limitations right along with its breathtaking strengths. Thus the whole metaphor of the caravan of science proceeding down a royal road towards the truth has no application. Individual theories approach the truth in some respects. Others approach it in others. Below I will suggest that there are additional considerations that ought also be taken into account in any sensible talk of "closeness to the truth."

In face of the considerations I have reviewed we may ask how we all have come to be so gripped by the image of a royal road to the truth. Perhaps it is this: We have found, in a wide range of cases that, for each description, we can find refinements. From which we might reasonably conclude that, for each description and each relevant characteristic, there is a further description that refines the first with respect to the characteristic. But it is a fallacy (I call it the UEEU fallacy — from $\forall\exists$ inferring $\exists\forall$) to infer from the last conclusion that there is *one* description that,

7. In the text Sklar gives the latter characterization of the bag model of the nucleus, not continuum hydrodynamics, but the context pretty clearly indicates that he intends the epithet to be general.

https://doi.org/10.1086/423625 Published online by Cambridge University Press

for all characteristics covers all refinements. To endorse the metaphor of a royal road to the truth is to embrace the search for such a "desired ultimate theory." But the evidence supports, at best, indefinite refinability, in indefinitely many diverse ways, a network of complementary and always limited probes into those parts of "reality" that are humanly accessible, not the Holy Grail of some "final theory."

We command an improved understanding of these considerations when we bear in mind that the evaluation of "truth" and various kinds of "truthlikeness" are characteristics of our representations. Our representations function as our guides to what we take to be a world independent of us. None of these guides are flawless. So they need to be evaluated as to how well they succeed. Again, in making such evaluations we must bear in mind that success and failure come not only in degrees, but also in a multitude of respects. We must not be misled by the metaphor of fit to the circumstances that "fit" is not just in a fixed respect. We also must not be blinded to the relevance of other essential considerations when evaluating the extent to which a representation does what we seek from it.

Our representations are intended as a guide to the world. But our guides are imperfect—they speak to us fallibly. So their virtues as guides to the world cannot be simply evaluated in terms of the dichotomy, true or false. Rather a sensible objective is degree of fit in respects that are of current interest. Search for "the truth" is not a search for something that is today at hand. If we must settle for something less than truth of our representations then we must make choices as to the respects in which our representations do well. But then accessibility (also a metaphor) is an additional essential consideration when what is at issue is providing human access to an independent world. If our interest is in, say the fluid properties of water, what is the truthlikeness of our foundational theories in this regard? Our foundational theories give us constraints on any relevant Hamiltonian, constraints that can help us in understanding how a false theory of continua can nonetheless provide a sound guide to the fluid properties of water—a guide, we should add, not just for making useful predictions of the "phenomena," but also for providing humanly accessible understanding of the fluid properties and behavior of water. Foundational theories do not give us, nor do we otherwise know how to formulate, the Hamiltonians that could underwrite answers to the questions about fluidity that interest us. And even if we did have such a Hamiltonian, we would have to solve a differential equation with 10^{25} to 10^{27} variables and then, somehow, makes sense of the solutions.

In short, our foundational theories deliver little humanly accessible information about the questions that interest us about fluidity. Consequently, in this regard, relative to the evaluation of "closeness to the truth"

https://doi.org/10.1086/423625 Published online by Cambridge University Press

of humanly accessible representations, our fundamental theories can fairly be said to have very little truthlikeness. Keeping this in mind, and when it is the fluid properties of water that are of interest, a hydrodynamic characterization of water may be fairly evaluated as much more "truthlike" than a quantum mechanical description, let alone any humanly accessible characterization in terms of quantum field theory.

I can put the point in question here the other way around. In Sklar's opening words, to give an ontology is to provide an answer to a question of the form, "What is there in the world and what is it like?" (424). I take seriously the claim that our foundational theories do provide answers to such questions, and that in spite of the fact that they are seriously flawed, in particular suffering the same kind of flaws exhibited by continuum hydrodynamics and many other "phenomenological" theories. But if our foundational theories can provide answers to questions about what there is in the world and what is it like, then non-foundational theories can do likewise, notwithstanding their having similar flaws. In considering this suggestion, we keep in mind that both foundational and non-foundational theories invoke severe idealizations as well as simple numerical inaccuracies. In addition, we keep in mind that "better" must always be evaluated in a complex multitude of ways including considerations of human accessibility. In view of these considerations it cannot be said that foundational theories uniformly provide better answers for questions of the form, "What is there in the world, and what is it like?"

9. Pluralist Ontologies as Systems of Idealizations. Let us dwell, for a moment, on this idea of ontologies as idealizations. There are no quanta any more than there are continuous fluids. Both are idealizations, known not to be realized in the real world. What then is it to give an 'ontology'?

We can fairly be said to be in the position of the three blind men and the elephant, respectively in contact with the elephant's trunk, side and leg, reporting a snake, a wall, and a tree. In one respect this ancient metaphor threatens to mislead because it includes the elephant, so that the metaphor still encourages the idea of getting it all exactly right. To have cognitive access to the world is to represent it. Consequently, "direct (that is non-representational) cognitive access" is a contradiction in terms. So we cannot (logically cannot) get "outside" our representations to "see" what the elephant—the world—is "really" like. In addition we know that virtually all, if not absolutely all our representations are imperfect in the sense that there will always be aspects with respect to which the representations can be improved, "improved" in turn understood in the sense that there can be alternative representations that we can have good reasons to prefer as giving us better characterizations with respect to the aspects in question. So we are stuck in the situation of the three blind men.

https://doi.org/10.1086/423625 Published online by Cambridge University Press

The project of getting our ontology "right" was predicated on presuming that some of our accounts are, exactly speaking, true, or at least that they can be ordered as to closeness (farther down the road) to the truth. (In this regard, review the quotations from Sklar, 438 and 439, and above.) When we give up this presumption we see that we can embrace "conflicting" ontologies, being careful not to invoke them "at the same time," or, more carefully, not to take them to represent the same aspects of the not completely accurately represented objects of our representations. Thus we may take a pluralist ontology to consist of a collection of idealized descriptions that, when deployed with care in cognizance of their limitations, can be consistently applied as complementary rather than conflicting. Short of arriving at the limit of enquiry, such a pluralist ontology provides a much richer understanding of the world than any univocal alternative.

10. Unification and Reduction? I have not yet addressed a major objection often cited against views like the one I have been proposing. Progress in our foundational theories has provided a multitude of striking unifications in the understanding of what otherwise appear to be entirely disconnected phenomena. To deny that a unifying account is, in a context independent sense "closer to the truth" than what it unifies, would appear to be denying that it in fact provides the claimed unifying explanations.

I also feel the force of this line of thinking. But it conflicts with the considerations I have put forward. Here I will review the conflict and conclude that there must be unresolved aspects of this issue that we are very far from understanding.

Why does an instance of unification seem so compelling? This is easy to understand if unification is thought of in terms of truth of the unifying theory. Typically there will be a number of phenomena that would appear to be unrelated. We discover that these disparate phenomena can be characterized as manifestation of some one, intuitively unified characterization or "mechanism," in a very broad sense. In most such cases it also transpires that the prior descriptions of the ununified phenomena are not exactly correct—water is not a continuous medium, Boyle's law is not exactly true, hydrogen and oxygen do not combine in exactly the ratio of two to eight by weight. The unifying theory generally provides more accurate descriptions in certain respects, and also helps us understand both the limitations of and why the idealized phenomenological laws work as well as they do: On the hypothesis that the truth of the matter is that there really are atoms, characterized classically, or quantum mechanically, or . . . then we can explain the accuracy—and the failings—of the unified phenomena. Thus our feeling that the consideration of unification is so compelling is also closely tied to the appearance of reductions.

But we know that the foundational theories we actually have are also

https://doi.org/10.1086/423625 Published online by Cambridge University Press

idealizations. We cannot think of them as reducing and unifying phenomenological theories by showing us the truth of the matter. I suspect that the occurrence of unifications nonetheless seems so compelling because closeness to the truth is seen as serving as an adequate surrogate for truth. Thinking of the distance of the foundational theory from the truth as small compared to the distance from the truth of the phenomenological theory, when it comes to explaining the errors of the phenomenological theory, the foundational theory is as good as true. And we then look to the verisimilitude program to fill in a measure of "distance from the truth" to make all this precise.

For reasons outline above, I reject the verisimilitude program as unworkable. The idea of relative distance from the truth does make perfectly good sense, but only in a context sensitive way, always relativized to various aspects under consideration. I don't take this conclusion straightforwardly to show that considerations like that of the last paragraph can't be made out. But I do take it show that it is not obvious that they can, and that the whole matter must be reconsidered much more carefully. Given that all our foundational theories themselves are idealizations, the problem is the puzzling one of understanding how one falsehood can help us in understanding another.

There is also an independent respect which complicates too cavalier an attitude towards the import of unification and reduction. Sklar provides what I agree is a fair description of the common attitude towards these questions, but an attitude that I submit will not stand up to a closer examination of the matter. Sklar agrees that the

> [s]impleminded claims to the effect that the older "practical" theories can be simply derived from the newer foundational theories have been long understood to be too easy . . . [and that] . . . in probing the relationship between the foundational theory and some higher-level, phenomenological theory one may need to work 'from the top down' . . . [using] the known laws of the upper level theory to discover the connections of those laws to those of the foundational theory. (436)

But, he insists that this and other limitations on reductive claims

> in no way need vitiate the claim that the connections between the theories so established provide evidence of the truth of the claim that the foundational theory includes within its domain of applicability as a describer of what things there are and how they are all the things and features within the domain of the phenomenological theory. That the phenomenological theory might in practice be the only epistemic route into finding the connecting links between the theories doesn't

https://doi.org/10.1086/423625 Published online by Cambridge University Press

> in any way entail some autonomous ontological domain for its application. (437)

This statement would make it seem that once phenomenological theories have played their part as a tool for discovering just what deductions are needed from some foundational theory, the relevant true descriptions of the successes (and failings) of the phenomenological theories follow from the foundational theory by itself. But development—and I use the word "development" advisedly—of intertheoretic relations rarely proceeds in such a manner. Rather one usually supplements, simplifies, trims, rewrites, augments, and otherwise contorts the foundational theory in squeezing out the phenomenological information.[8] To be sure, ideas and relationships from the foundational theory play an essential role. But it is misleading to suggest that, after the phenomenological considerations have done their epistemic work in telling us just were to look in the foundational theory, the resulting description no longer includes information that goes beyond what was implicitly contained in the foundational theory from the beginning. When $F = ma$ is applied by putting in a frictional force, the resulting description includes information from Newton's laws augmented with phenomenological information obtained by observation of friction. The most charitable reading of the renormalization procedure in all practical quantum field theories, what on the face of it looks like an arbitrary "throwing away of infinite quantities," proceeds by taking the measured values of mass and charge to "cover" for what must be inaccuracies in the "fundamental" theory being used (see Teller 1995, chapter 7). Treatment of intertheoretic relations often proceeds using "semiclassical" approximations, for example using a quantized description of the behavior of particles in a classically described, unquantized, external potential. It is misleading in the extreme to think of phenomenological theories functioning only to help us find the information all of which is "hidden" in the "foundational" theory. My brief summary of the form of practical relativistic quantum field theories shows how restricted the information can be in a foundational theory when it is, strictly speaking, taken by itself.

Nonetheless, all these worries not withstanding, I agree that unifications and reductions show something important about how our theories bear on the world. But I take the worries to show that we are very far from understanding what that "something" is.

11. Fictionalism Transfigured. Is all of this just old fashioned 'fictionalism'? In particular, I would seem to be one of "those who would argue

8. Cartwright insists on this point, in (1999) chapter 8, and elsewhere.

https://doi.org/10.1086/423625 Published online by Cambridge University Press

that every conceptualization is of [the sort "that are deliberately misrepresentative of the 'true' state of affairs"], and that there is no principled way of distinguishing conceptualization meant to represent how things really are with a system and those meant merely to describe 'useful fictions' about the system" (430), who thus advocate an "'it's all a bunch of fictional models' approach to our understanding of the aims of theory" (439) and so exemplifying "the kind of very radical pragmatist that views all such conceptualizations ["Multiple incompatible schemes applied to one and the same system"] as *equally* 'fictional' (whatever that would mean)" (438).

I maintain that any such characterization of the proposal is extremely misleading, turning on the presupposed dichotomy between "conceptualization meant to represent how things really are with a system and those meant merely to describe 'useful fictions' about the system" (430; cf. also 429 and 431), in turn presupposing, or at least encouraged by the presumption of some kind of access or approach to exact truths. If we had exactly true descriptions we could use them to sort the veridical descriptions from the "useful fictions." But since all the theoretical descriptions we get from science fail in one way or another, and succeed and fail in a complex pattern of contrasting respects, thinking in terms of such a distinction, in a context impendent way, badly misleads.

This is not to deny that the terms, "true" and "useful fiction" do have a perfectly good use. We use these contrasting terms unproblematically, so that it may seem that there must, all the foregoing notwithstanding, be such a distinction.

Of course there is. But the impression that the distinction will support conclusions about ontology will melt away as one takes it to heart that the unproblematic use is context sensitive. The situation is analogous to the vicissitudes of our understanding of the idea of "direct observation." We unproblematically characterize some observations as "direct," although we know that 'direct' never literally and exactly applies, so that we must take all observations to be, in one way or another, indirect. Since the indirectness is a (complex) matter of degree, contextual considerations often make "direct observation" an appropriate characterization—my observation of my hand before me is (relatively) direct in comparison to my observation of a paramecium through a microscope. It took several decades of careful analysis to see that this robust usage does not underwrite a sharp, context independent distinction between direct and indirect observation that would support epistemological and possibly ontological concerns.

Similarly, in many situations, contextual considerations make "(useful) fiction" an appropriate evaluation, though there is no simple sorting of the fictive and veridical aspects of any of our imperfect descriptions. All of our imperfect representations have fictive and veridical aspects. When

https://doi.org/10.1086/423625 Published online by Cambridge University Press

we have the fictive aspects in view, we apply the epithet, "useful fiction." When a description's shortcomings are not of present concern we apply the epithet "true." Like the (context relative) distinction between direct and indirect observation, the context relative distinction between veridical descriptions and useful fictions bears no weight in drawing conclusions about ontology.

Characterizing the present view, as Sklar does, as saying that all our conceptualizations are *equally* fictional is to suggest that the view comes down to holding that all our conceptualizations are false statements that happen, coincidentally, to have some useful "predictive" consequences, or that they are all

Merely useful fictions.

The epithet, "merely" might be warranted if we had contrasting descriptions that were in no way fictive. But we don't. So drop the "merely": Our conceptualizations are

Useful fictions.

And note that they can be useful in a multitude of ways, not just for "predicting the phenomena" but in improving our grip on properties, explanation, theoretical understanding, every aspect of our intellectual encompassing of what there is. (Consider the hydrodynamical example in all these regards). So we can say that the fictions are not just useful but more generally that our conceptualizations are

Informative fictions.

By this point "fiction" has become less and less appropriate. Our imperfect characterizations genuinely inform, our "foundational" theories and theories such as hydrodynamics alike. Just calling them "fictions" thus misleads. But we do want to acknowledge that these characterizations are not simply true. So perhaps we need a characterization such as

Veridical fictions

or perhaps

https://doi.org/10.1086/423625 Published online by Cambridge University Press

Fallible veracities.[9]

12. Conclusion. We have thought that, for example, the hydrodynamic model is a "merely useful fiction" because we know that it does not tell us what is "really" (read that as "exactly") going on. In contrast, we have been supposing, with what we learn from foundational theories. But then we learn that foundational theories themselves only tell us about the world in whatever way that the "merely useful fictions" do. Sklar worries that such facts would drive us to the attitude of some "kind of very radical pragmatist that views all such conceptualizations as *equally* 'fictional' (whatever that would mean)" (438). I suggest that we should take the argument implicit in this worry and run it in the opposite direction: I accept that foundational theories do tell us a great deal about how the world really is. I note that many "phenomenological" theories, such as hydrodynamics, tell us about the world in the same kind of way that the foundational theories do. I conclude that such theories also tell us much about how the world "really" is, not just about the "phenomena," and that "useful fiction" is context sensitive and often misleading.

I have suggested that the "ontologies" given us by any of our actual theories are all idealizations, and so are, strictly speaking, false. In closing I will also note that the content of such false idealizations and theories can, after all, be expressed in terms of some kind of truths. For example, I can truly say that water is, in many respects, *very nearly like* an incompressible continuous medium satisfying Euler's equations. I have thus stated a truth every bit as much as when I say that John is six feet tall, meaning, of course, very nearly six feel tall, not six feet tall exactly. Generally we have presumed that the truth conveyed by such vague statements is parasitic on some presupposed exact correlate, or analysis in terms of exact expressions. But I think that a sober evaluation of the literature on vagueness will show that such a presumption is as illusory as the presumption of exactly true foundational theories on which the first presumption is based. When I say that water is, in many respects, very nearly

9. To the extent that we can trust Arthur Fine's (1993) analysis of Vaihinger's "as if" fictionalism, what has been outlined here would appear to differ in important ways from Vaihinger's, especially by recommending that we abandon our effort to sort out characterizations into the strictly true and not true, and by seeing our "fictions," if we must use that term, as our only way of getting real knowledge about the world. I am not sure Fine would agree, though he makes clear contact with the issues I have been examining: Fine concludes that, "Preeminently, the industry devoted to modeling natural phenomena, in every area of science, involves fictions in Vaihinger sense. If you want to see what treating something "as if" it were something else amounts to, just look at most of what any scientist does in any hour of any working day (16).

https://doi.org/10.1086/423625 Published online by Cambridge University Press

like an incompressible continuous medium, the relativity to respects and the vague "very nearly like" can be mitigated and refined, but never completely eliminated.[10]

So I conclude that it is only through idealizations, and what I have just suggested we can think of as their alter-egos—inexact truths—that we have access to the world. Yes, foundational theories apply to everything. But *how* do they apply? In just the kind of way that Sklar's dreaded "merely useful fictions" apply, often giving us (in many respects) better access to the world than the "foundational" theories. But we haven't lost anything in this. This isn't to retreat to doing no more than predicting "the phenomena" (whatever *that* might mean . . .). This is the way we know about the world. If you like, about "how things really are" (430, 431).

REFERENCES

Cartwright, Nancy (1999), *The Dappled World.* Cambridge: Cambridge University Press.
Fine, Arthur (1993), "Fictionalism", *Midwest Studies in Philosophy* 18: 1–18.
Hoefer, Carl (2003), "For Fundamentalism", *Philosophy of Science* 70: 1401–1412.
Sklar, Lawrence (2003), "Dappled Theories in a Uniform World", *Philosophy of Science* 70: 424–441.
Teller, Paul (1995), *An Interpretive Introduction to Quantum Field Theory*. Princeton, NJ: Princeton University Press.
——— (2001), "Twilight of the Perfect Model Model", *Erkenntnis* 55: 393–415.
——— (2002) "So What IS the Quantum Field?", in Meinard Kuhlmann, Holger Lyre, and Andrew Wayne (eds.), *Ontological Aspects of Quantum Field Theory*. Rivers Edge, NJ: World Scientific.
——— (forthcoming), "The FineWright Theory".

10. In Teller (forthcoming) I have expanded on the suggestion, very briefly made here, of a duality between idealizations and inexact or vague statements, that in somewhat different ways these two forms of description accomplish the same reprentational ends, that they cannot be eliminated from our discourse, and that they intelligibly function as our way of understanding what there is in the world and what it is like. I also tie this duality to the idea of veridical fictions (idealizations) and fallible veracities (vague or inexact truths).

https://doi.org/10.1086/423625 Published online by Cambridge University Press