Schematizing the Observer and the Epistemic Content of Theories[†]

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I dedicate this paper, with affection and admiration, to Howard Stein, mentor and friend, one of the few people I have met in my life to deserve the honorific 'philosopher' in the deep Platonic sense.

> So I suggest that the principal difficulty is not that of how to leave the theory outside the laboratory door, but that of how to get the laboratory inside the theory. ¶ Well, how *do* we do it? For of course we do put theory and experiment in relation to one another; otherwise it would be impossible to test theories, and impossible to apply them. It would also, I should add, be impossible to *understand* a theory, as anything but a purely mathematical structure—impossible, that is, to understand a theory *as* a theory of physics—if we had no systematic way to put the theory into connection with observation (or experience).
>
> Howard Stein
> "Some Reflections on the Structure of Our Knowledge in Physics"
> (italics are Stein's)

[†]This paper is forthcoming in *Studies in History and Philosophy of Modern Physics*, 2020, with the following changes: what appears in this version as §5 ("What Measurements and the Observer Are") and §11 ("Appendix: Précis"), will not appear in the published version due to length constraints; otherwise the two versions are identical (except for trivialities noted in the text). I thank Jeremy Butterfield for delightfully edifying discussion and for his usual detailed, extensive and penetrating comments on a draft. I thank Adam Caulton, Neil Dewar, Robert DiSalle, Michael Friedman, Bill Harper, Sebastian Lutz, Lydia Patton, Chris Pincock, Chris Smeenk, and Jim Weatherall for many enjoyable and illuminating conversations covering all the topics I treat here. I thank an anonymous referee for a superogatory report that helped me clarify a few important matters. I am, as always, grateful to Howard Stein for many conversations over many years on these topics, and for having written papers that continue to inspire me. This paper owes a clear and great debt especially to his "Some Reflections on the Structure of Our Knowledge in Physics". I am not certain that Stein would endorse all my arguments and conclusions, but I have hopes he would have sympathy with the overall thrust. Gefördert durch die Deutsche Forschungsgemeinschaft (DFG) – Projektnummer 312032894, CU 338/1-1. (Funded by the German Research Foundation (DFG) – project number 312032894, CU 338/1-1.)

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ABSTRACT

Following some observations of Howard Stein (1994), I argue that, contrary to the standard view, one cannot understand the structure and nature of our knowledge in physics without an analysis of the way that observers (and, more generally, measuring instruments and experimental arrangements) are modeled in theory. One upshot is that standard pictures of what a scientific theory can be are grossly inadequate. In particular, standard formulations assume, with no argument ever given, that it is possible to make a clean separation between, on the one hand, one part of the scientific knowledge a physical theory embodies, *viz.*, that encoded in the pure mathematical formalism and, on the other, the remainder of that knowledge. The remainder includes at a minimum what is encoded in the practice of modeling particular systems, of performing experiments, of bringing the results of theory and experiment into mutually fruitful contact—in sum, real application of the theory in actual scientific practice. This assumption comes out most clearly in the picture of semantics that naturally accompanies the standard view of theories: semantics is fixed by ontology's shining City on the Hill, and all epistemology and methodology and other practical issues and considerations are segregated to the ghetto of the theory's pragmatics. We should not assume such a clean segregation is possible without an argument, and, indeed, I offer many arguments that such a segregation is not feasible. It follows that an adequate semantics for theories cannot be founded on ontology, but rather on epistemology and methodology.

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1 The Complexity of the World and the Simplicity of Theory

For essentially every physical theory we have (Navier-Stokes theory, general relativity, quantum field theory, *etc.*), we have very little detailed knowledge of the structure of generic solutions. Usually, we know exact solutions only under conditions of perfect or near-perfect symmetry or some other unrealistic assumption (two bodies, no external perturbative influences, . . .), and then argue that we can apply such solutions to real physical systems, because the approximation is adequate in appropriately controlled circumstances. That is to say: we have no real idea *at all*, in a representational sense, "what the world would be like if the theory were true or largely true of it", for any physical theory.

This becomes particularly clear when one stops to think, to really think hard, about the mind-blowing complexity and richness of texture of real physical systems in the world. Take this glass of water resting on the table, seemingly in equilibrium with its environment. The water does not in fact have a constant, static density, pressure, temperature, shear-stress, heat flux, fluid velocity and volume, as we would naturally ascribe to it in a basic treatment using Navier-Stokes theory, the classical theory of viscoëlastic, thermoconducive fluids. There are tiny temperature gradients from the edge of the glass to the interior, and likewise tiny pressure gradients. These drive microscopic flows and eddies and vortices, which generate fluctuations in the shear-stress. There is evaporation of the water at the surface, adsorption of a thin layer of particulate matter at the surface, and thereby absorption of particulates from the air and of air molecules themselves. There is thus exchange of thermal energy at the boundary, complicated by the little air bubbles and particulate matter suspended there, and inhomogeneities in the glass at the boundary. The water itself, at a finer level of detail, consists of a hyper-dense, stereometrically complex network of Hydrogen bonds with rich characteristic symmetry patterns as web to the weft of instances of dozens of species of ionic combinations of $\mathtt{H}$ and $\mathtt{O}$ ($\mathtt{H_2O}$ being not even a majority, only a plurality). The water is pervaded and perturbed by the ambient electromagnetic field, itself composed of endless linear superpositions of radiation from the antennæ of radio stations playing Berg's "Lulu" and the BBC news, mobile phones all over the Earth, magnetic induction from small movements of power cords, the Earth's own magnetic field, infra-red radiation and radio waves from Sagittarius A* (the supermassive black hole at the center of the Milky Way), the cosmic microwave background radiation, the jiggling of the water's own molecules, the rotation of the iron core of some planet 30 light-years away, and the gamma ray bursts from distant astronomical cataclysms. The water is inundated by cosmic rays. In every atom of every molecule, continual weak and strong nuclear reactions manifest themselves. And on and on and on.

I think we tend to forget in philosophy, we lose sight of, how complex real physical systems are and what a miracle it is that our almost naively, recklessly simple-minded theories, and the childishly sketched models we construct in those theories, can still capture them with astonishing accuracy, and do so in ways, moreover, that seem to give us real understanding of the nature of the world in a broader sense. And correlatively, we forget how much distance there is between those simple models we do know in any given theory and the real physical systems they purport to represent, and so we forget how many of the theory's other models would do the representational tasks required and *prima facie* do them better (by adding ever more finely grained detail, for example), if one of them does at all—how many of those models that we have no idea how to construct in any way graspable by the human mind, or indeed even how to identify if someone gave us one gift-wrapped.[1]

The standard view today in much if not most of philosophy of science in general, and philosophy of physics in particular, however, especially since the seminal work of Suppes (1960; 1962), presupposes that a theory is characterized in a strong sense by its family of "models", where a model is always meant to be something that, at a minimum, captures the essence of a solution to the theory's equations of motion or dynamical field equations. Crucially, moreover, it is presumed that those models have intrinsic physical significance accruing to their formal structure independent of any consideration of how the theory is applied in practice. It presumes, in other words, that we can cleanly separate one component of the scientific knowledge a theory embodies, that encoded in the theory's formal machinery, from all other components, including that involving the practical application of the theory by experimental scientists.

This is true whether one hews to the semantic view of theories (Suppe 1974; van Fraassen 1980) or the Best-Systems picture (Cohen and Callender 2009) or a semantics based on possible worlds (Lewis 1970b; Butterfield 2018), or one is a neo-Carnapian (Demopoulos 2013), or a structuralist (Stegmüller 1979; da Costa and French 2005), or a neo-Kantian (Friedman 2001), or one tries to reconcile the syntactic and the semantic views by the use of category theory (Halvorson and Tsementzis 2017), or one uses category theory directly to embody the models of the theory (Weatherall 2017), or one champions a sophisticated syntactic view—declared dead many times during and after a long period of mordant vilification—on its own (Lutz 2014), or one adopts any other of the contemporary popular accounts of scientific theory. The presumption of the clean separation of different components of knowledge comes out clearly in every one of these examples. The semantic view treats its models as having "empirical content" while explicitly denying that those models need to represent actual experiments in order to have such content. The syntactic view introduces reduction sentences or coordinating principles or correspondence rules to relate explicitly demarcated languages, the theoretical and the observational. Those who have used category theory have treated the objects and morphisms of the relevant categories as representing physical systems and their behavior based solely on the postulation of a few minimal interpretive principle that scrupulously avoid mention of experiment. And so on.[2]

1. To guard against misunderstanding, I want to emphasize the difference between, on the one hand, knowing—articulating, identifying, grasping—individual solutions to a theory's equations and, on the other, knowing propositions that state general properties of classes of solutions. We know a great many such propositions for every theory in physics. Although we know a vanishingly small number of exact solutions to the Einstein field equation in general relativity, we also have in hand a great number of deep, extensive and powerful theorems that characterize generic properties of large classes of solutions, none of which need be known in its peculiarity. The singularity theorems of Penrose, Geroch and Hawking are excellent examples: any spacetime that satisfies a few generic conditions (positivity of energy, for example, and lack of causal pathology) possesses incomplete, inextendible causal geodesics. (See, *e.g.*, Curiel 1999 for a detailed exposition of the theorems, geared towards philosophers.) Such propositions do not help the proponent of what I will call the standard view—sketched immediately below—for they do not allow one to grasp individual models one does not already know.

2. I find, moreover, that philosophers of physics who do not work directly on the semantics and structure of

In light of my remarks about the paucity of our knowledge of actual solutions to our theories' equations, therefore, to accept the standard view is to accept one of two options:

1. we stipulate by fiat that there is a family of actual (or possible, in some sense) physical systems, though we cannot identify most of them in practice, that are the ones represented by the solutions, and those are the models;
2. we stipulate by fiat that the family of solutions themselves (*e.g.*, vector fields on phase space), though we cannot represent them in anything like closed form or even identify almost any of them if they were to bite us on the ass, are the models.

Call this 'the standard view's dilemma'. In both cases, we are saying that the overwhelming majority of the content of the theory is something that we do not know, are in fact nowhere near knowing, and have good reason to think we will never know in anything resembling thoroughness and detail—epistemologically speaking, either choice is an act of faith.[3] In other words, it is—should be recognized to be—utterly mysterious how the models are supposed to have intrinsic physical significance. Nonetheless, the story goes, there is a preternatural relation of subsistence among the formalism, the models, and the physical systems that unambiguously fixes them; and that in turn gives rise to a magical relation of "reference" or "representation" that zaps out from the formalism to the world and latches on to the salient physical systems in a physically significant way. We, in our supernal cognitive puissance, just *know* this.

We are talking here of a Fichtean faculty of pure intellectual intuition that grasps without cognitive, perceptual or practical mediation the Kantian *Ding an sich*. Or more precisely, we are talking of nothing at all. A semantic relation that is not known and cannot be known by humans in our current epistemic circumstance has no possible use in actual science and has no possible bearing on a fruitful and illuminating analysis of scientific knowledge.

My rhetoric may make it sound as though only the realists are subject to my wrath, but that is not so. I intend my wrath to be directed as well at all philosophers beholden in any way to something like the standard view, whether of a realist or an anti-realist bent. What is this fairyland of formal models that are the semantic tools the instrumentalist uses to make his predictions? All those who hew to something like the standard view, no matter how they think of the character of the semantic relations between a theory and the world in detail, have the same general problem: what is the family of formal structures that "characterizes" the theory, what are the physical systems the theory appropriately and adequately treats, what is the relation between them, and how can we grasp all this in a comprehensive way?[4] That is to say, which formal structures of the theory are even such as to be appropriate to serve as adequate representations of parts of the world, and how can we know this, given our woeful lack of knowledge about the details of

theories, but rather work on problems that either directly or implicitly require an account of a semantics, without hesitation, indeed usually without comment or (I suspect) thought, employ the standard view. It is in this sense as well that I intend the claim that the view I sketch is "standard" today. It may well be that very few today believe the standard view as I sketch it—but then I cannot understand the arguments they do make, *e.g.*, in trying to articulate a criterion for theory equivalence based on formalism alone, with vague gestures at how it is all to make contact with empirical content as an afterthought.

3. I thank an anonymous referee for this marvelous phrase.

4. I characterize my technical notion of "appropriate and adequate" in §8 below—for the time being, I use it as a term of art. I use 'propriety' as the nominal form of 'appropriate' when I intend it in this (to be characterized) technical sense.

the formal structures and of the world? For not all formal models in the theory can designate or represent in a meaningful way all physical systems we naively think they should be able to, though we have no way of determining this in general. *What is the theory?*

We need a mechanism or device for bringing a precisely characterizable situation in the world of experience into substantive, physically significant contact with the mathematical structures of our theories. This mechanism would be an intermediary that identifies the junctions where meaningful connections can be made between the two and embodies the possibility of the epistemic warrant we think we construct for our theories from such contact and connection. That mechanism, I shall argue, is the schematic representation of the observer. To quote Stein (1994, pp. 649–650), who introduces the idea:

> In actual fact, the experimental physics is treated separately as a discipline in its own right, that is partly an art: an affair of both knowledge and manipulative and perceptual skill. But the possibility of connecting this art with the theory is closely connected with a certain possibility within the mathematical structure that is the theoretical framework: ... the possibility of representing experiments, and of representing the observer, "schematically." ... I want to speak ... of "schematizing the observer within the theory"; ... the intention is ... to secure empirical content—content within experience—for an abstract structure.

The standard view gets right the fact that the family of physical systems a theory appropriately and adequately treats is a central, essential component of the epistemic content of a theory. The standard view goes wrong in thinking that it makes sense to characterize this family in the abstract, without recourse to and reliance on all the scientific knowledge we have pertaining to the theory and its applications. To identify that family of systems, we need to demarcate the theory's regime of applicability. To do that, we need to fix error tolerances, acceptable levels of precision, *etc.*, in our experimental practice. To do that, we need real models of actual experimental techniques, representing the actual tools we have at our disposal for performing such measurements, for the regime of applicability changes over time with advances in practical knowledge—we need to schematize the observer.[5]

I give more detailed and extended arguments in §3, and throughout the paper after, to defend these claims. I give here a simple, short, yet powerful one. If demarcating the regime of applicability did not depend on practical knowledge of the kind that changes, and that cannot be encoded in

5. Although the recent wave of philosophical work on practice in science, exemplified by such thinkers as Margaret Morrison and Hasok Chang, has much in common with the views I propound here, I do not explicitly engage with the literature. It is too vast, and often the motivations and aims of the work differ too much from those of this paper, for me to do so without expanding the scope of the paper beyond what it can profitably bear. For the same reason, though it pains me, I do not directly engage with the literature on complexity in science, such as the marvelous work on generative entrenchment, levels of organization and causal thickets by William Wimsatt. And again, I cannot engage with the interesting recent work being done on measurement by philosophers such as Eran Tal and Alistair Isaac. As to why I am not situating this work in the current literature in a more standard way, but rather blowing it all off with brusque regret, I can say in my defense only two things to the anonymous referee who pushed me on it: first, this paper faces many more large issues taken at once than is standard in a philosophy of science paper these days; second, it most often does so in idiosyncracy. It would take a book for me to work out how these ideas compare to—in their similarities and differences, in their virtues and demerits—the most influential contemporary accounts of all these issues.

the pure formalism of the theory—the current reach of experimental technique that determines fixed levels of precision and accuracy, the availability of approximative theoretical techniques and heuristic forms of argument to make calculations tractable, and so on—then there would be a fixed spatial length, once and for all—say 3.14159265358... $\texttt{x}10^{-3}$ `cm`—at scales smaller than which the representational capacities of Navier-Stokes theory breaks down. But that is simply false. It breaks down at different scales for different types of fluids under different conditions. The increasing acuity and resolution of our measuring instruments, moreover, in conjunction with our increasingly powerful control of experimental error, and the increasing sophistication of the models we can construct of our measuring devices and the fine details of their interactions with different Navier-Stokes fluids under different conditions, all mean that such numbers become ever smaller. They are not fixed *sub specie æternitatis*.[6] (I explain the ideas of a breakdown scale and the regime of applicability of a theory more fully in §8.)

Thus, neither is the family of physical systems that a theory appropriately and adequately treats fixed *sub specie æternitatis*. I think this is right. A scientific theory is not a Platonic form. It does not exist independently of our actual epistemic state.[7]

This is a long and dense paper, and the overall argument has not leant itself to a clean, simple, linear exposition. I therefore sum up in an appendix (§11) the main claims of the paper, along with my main criticisms of what I call the standard view of theories.[†]

6. Although it is, lamentably, a theory that has received almost no philosophical attention, I will use Navier-Stokes theory as a recurring example to illustrate and substantiate my arguments throughout the paper. I do this for several reasons, among the most important to my mind being that: it provides excellent models of complex, real phenomena we are all familiar with (water—$\mathtt{H_2O}$—under normal circumstances, for example); it is, with respect to physical content, a well understood theory; and it is a straightforward theory, in the sense that its physical content can be explicated without the use of any heavy technical machinery. (The *loci classici* for the general theoretical treatment of hydrodynamic phenomena are Lamb 1932 and Landau and Lifschitz 1975, and that for issues of stability and equilibrium in particular is Chandrasekhar 1961; see, *e.g.*, Pope 2000 and Foias et al. 2001 for a treatment of non-equilibrium fluid phenomena such as turbulence.) Nonetheless, the full richness and complexity of the theory, both mathematically and physically, lie well beyond us—we have only a poor grasp of the mathematical character of generic solutions to the Navier-Stokes equations, and many of the phenomena most characteristic of Navier-Stokes fluids, such as turbulence, remain among the most puzzling and mysterious that we are aware of. As such, its philosophical contemplation readily throws up interesting questions and problems that philosophers' obsession with quantum theory and general relativity blind them to.

7. Even though I insist on the importance of attending to the complexity of the world in contraposition to the simplicity of our theories, I reject work such as Cartwright (1999), which wants to do away with theories in favor of something like a patchwork of models. Formal frameworks and theories *unify* all those disparate models, providing a common structure they all instantiate, which can serve as the groundwork for both philosophical and physical investigation. This is one important role for formalism. My discussion, in §4, of the many possible models of the black hole SgrA* illustrates how this works: without general relativity serving as the common workroom in which all the models are constructed, examined and then put to further use, providing the common context that allows each to be compared in a meaningful way with all the others, it would be difficult if not impossible to understand them all as models of the same physical system or as making reference to the same physical quantities in different ways.

†. This paragraph, and the appendix, do not appear in the published version of the paper, due to length constraints.

2 What Is a Theory?

What is the theory of general relativity? Here is one way to make that question somewhat more precise: what is the more or less secure fund of scientific knowledge that constitutes the theoretical and empirical content of the theory—its *epistemic content*?[8] This formulation of the question is certainly not a standard one in philosophy of science. On that standard view, the emphasis is on constructing formal models corresponding to something like solutions to the field equations or equations of motion of a theory, and perhaps asking when two such families of models (or other formal structure) are isomorphic in a (supposedly) relevant sense, with perhaps a gesture at the possible "representational capacities" of the formalism as an afterthought.[9]

I do not like a formulation of the question that admits as an appropriate answer such a formal structure, along with perhaps the fixing of something like a Tarskian semantics or a sketch of representational capacities. A formulation of that kind assumes, with no argument ever given, that it is possible to make a clean separation between, on the one hand, one part of the scientific knowledge general relativity embodies, *viz.*, that encoded in the pure formalism and, on the other, the remainder of that knowledge. The remainder includes at a minimum what is encoded in the practice of modeling particular systems, of performing experiments, of bringing the results of theory and experiment into mutually fruitful contact—including all the *in*exact mathematical methods that cannot be formalized, approximative and heuristic techniques motivated by loose physical arguments and principles not part of the formalism or of any interpretive postulates, and justified only by practical success—including, in sum, real application of the theory in actual scientific practice.[10] This assumption comes out most clearly in the picture of semantics that naturally and usually accompanies the standard view of theories: semantics is fixed by ontology's shining City on the Hill, and all epistemology and methodology and other practical issues and considerations are segregated to the ghetto of the theory's pragmatics.

We should not assume without argument that the epistemic content of a theory can be cleanly and exhaustively divided into two parts, one consisting of what is encoded in the pure formalism alone and the other a catch-all for the rest, including the mess of real application. Much less should we assume without argument that two such parts, no matter how characterized in detail, can be cleanly and exhaustively segregated from each other in an adequate analysis of that epistemic content. That, however, is what the standard view does. Call this the 'segregation problem'.

If one accepts my formulation of the question as fruitful and interesting, then it becomes plausible that the theory of general relativity does not—can not—in any interesting sense consist

8. If one is permitted to speak of explicating a question, one may consider this a first step on the way to a precise explication of the question "What is a physical theory?"

9. Weatherall (2017) is a classic example. I do not single this paper out because I think it is a poor one; to the contrary, I think it is a beautiful paper, rich with important insights. It does, nonetheless, serve as a useful foil to my views, in part because of its clarity and depth.

10. Because I shall often cite "approximative techniques" as an exemplar of scientific knowledge that cannot be encoded in a theory's formalism, I want to clarify what I mean. There are often rigorous and well developed methods of approximation available that lend themselves to comprehensive formalization (*e.g.*, Fillion and Corless 2019). One sometimes even has formalized methods for controlling the errors such approximative methods introduce (*e.g.*, Fillion and Corless 2014). Nonetheless, not all approximative and heuristic arguments can be formalized. I discuss in §6 an example, to wit, arguments to the effect that Navier-Stokes theory is essentially an equilibrium theory.

of the theory of Lorentzian 4-manifolds along with a few interpretive postulates. Now, it may turn out that a compelling analysis attempting to answer my formulation will show that, in the end, the standard assumption of the clean segregation of the relevant domains of knowledge—the "theoretical" and the "practical", for lack of better terms—is a good and justified one. I personally doubt it.

I will argue that the interplay between theory and practice (experimentation, observation, modeling, heuristic and approximative forms of argument, . . .) is far more subtle, nuanced, complex and rich than many if not most philosophers steeped in formal traditions tend to appreciate, and in particular that the two are inextricably intermingled in a strong sense. The sum total of scientific knowledge constituted by the theoretical and practical content of the theory—its epistemic content—includes knowledge we have gained, and more importantly could have gained only, through practical modeling, experimentation and observation, heuristic and approximative techniques of argument, and in particular the application of representations and models of such experiments and observations in our theory by use of heuristic and approximative techniques of argument. It contains such knowledge, moreover, in a way that cannot at bottom be cleanly segregated from any knowledge grounded in the theory's pure formalism. That is why I think my formulation of the question based on the idea of knowledge is a fruitful sharpening and refinement, for it is this sum of epistemic content that distinguishes general relativity as a *physical* theory from a merely mathematical structure. (It also, in the end, is what must serve as the decisive basis for any criterion by which we judge general relativity to be both a different and a better theory than, *e.g.*, Newtonian gravitational theory.) I emphasize from the start that my conception of knowledge here is not limited to anything like "justified true belief", but includes more importantly comprehension and understanding of the sort that often accompanies explanatory practice and inspires novel discovery (on which, more below).

I will further argue that a crucial component of that epistemic content consists in knowing how to schematize the observer in theoretical representations of experiments, and cannot be grasped without models of experiments in which the observer is schematized. By "schematize the observer", as will become clearer, I mean something like: in a model of an experiment, to provide a representation of something like a measuring apparatus, even if only of the simplest and most abstract form, that allows us to interpret the model *as* a model of an experiment or an observation.[11] The force of the segregability problem thus lies in the claim that one cannot characterize and account for any of the epistemic content of a theory without schematic representation of the observer. As I discuss below, even such "trivial" schematic representations as a line of sight for telescopic observations in astronomy can allow one to determine and illuminate important and substantial parts of the way that theory and experiment come into contact, in this example by way of grounding an analysis, *e.g.*, of stellar aberration. Such schematic representation, moreover, is not possible in a physically

11. van Fraassen (2008, ch. 12), for example, an exemplar of what I am calling the standard view, explicitly argues for the opposite conclusion, even though he attempts to construct what he argues to be a pragmatist account of representation of physical systems by formal theory. Indeed, in van Fraassen (2012) he claims, rightly to my mind, that the empirical grounding of theoretical quantities as they appear in a theory's models must involve an account of how those quantities are "related to measurement" (p. 773). He then argues that criteria for what counts as a measurement must display the way that that measurement procedures are theory-dependent. He does not, however, explain how this can be done if the theory's models do not at least have the capacity to represent measurements.

significant and cognitively substantive sense without recourse to all the other components of what I am calling the practical part of scientific knowledge, whose explication I give in §6 below.

3 Why We Need to Schematize the Observer

Because the meaning of scientific terms and propositions must rest on the knowledge we have of the physical world, and most of all on the knowledge we have gained through controlled observation and measurement, that is, through experiment, epistemic content accrues to a scientific theory in no small measure through the construction of empirically successful representations of physical systems. At bottom, then, what secure epistemic content a scientific theory has must rest in large part on the meanings expressed in the sound articulation of experimental knowledge, for that is the final arbiter of empirical success. This requires at a minimum that we be able, at least in principle, to construct appropriate and adequate representations of actual experiments and observations in the frameworks of our best scientific theories, that is, representations of physical systems and experimental apparatus in relation to each other as required by actual experiments, not just representations of physical systems *simpliciter*, in abstraction from experimental practice.[12] It is characteristic of the ways that such models are constructed and applied, moreover, that they require methods and warrant—forms and aspects of knowledge—that go far beyond what can be claimed to be captured by the theoretical formalism of a theory alone, no matter how many representational capacities one ascribes to it or interpretive postulates one affixes to it.

What is at issue here is not the relation of representation itself, which can at most tell us "what the world would be like if the theory were true or largely true of it", but rather the understanding and comprehension we have of the world in so far as we are warranted in thinking of our theories' models *as* representations, which goes far beyond a description of how the world would be under given conditions (even if one generously admits "nomological structure" for inclusion in the description). Such understanding and comprehension come from arguments and investigations that cannot always be "represented" by the formalism of "the theory itself", but rather requires for their elucidation a rich and nebulous halo of inexact mathematical techniques and heuristic physical principles applied in ways that do not lend themselves to clean formalization.

There are, as I have intimated, several reasons why I claim that schematizing the observer is required for an adequate philosophical analysis of the structure and semantics of theories. I discuss now a few of them in more detail. The first is a shallow but still important one: sometimes the nature of the observational process itself results in "distortion" of the magnitudes measured, and a proper computation of the "real" values of the magnitudes of the system's properties requires explicit modeling of the interaction between the measuring instrument and the system itself to correct for the effect.[13] Call this 'the problem of seeing through a glass darkly'.[14] An example is

12. Indeed, it is our incapacity to do this in a consistent way in the context of quantum theory that lies at the bottom of the Measurement Problem. This alone shows the importance of the idea.

13. For simplicity, construe this claim only in the context of non-quantum theories; the same reason applies in many cases in quantum mechanics, but there an appropriate analysis requires far more mathematical, physical and conceptual sophistication and subtlety than I have room for in this paper, and, more to the point, than I feel capable of giving a sound philosophical articulation of, given our current lamentable state of understanding of quantum theory.

14. I thank Jeremy Butterfield for suggesting this excellent moniker.

stellar aberration: when light from a star enters a telescope, the motion of the telescope transverse to the path of the light while the light traverses the telescope (because of the diurnal rotation of the Earth, the orbital motion of the Earth around the Sun, and so on) makes the star appear displaced from its actual position in the sky; in order to correct for the effect, one must compute the actual motion of the measuring device, which requires an explicit representation of it in one's model of the observation.[15]

The second reason is a middling deep one. The quantitative results of all measurements and observations, even the best ones, deviate from those predicted by theory, even if only by a small amount; likewise, there is an inevitable imprecision in the measured values. Such deviations largely accrue to measurements on account of systematic errors arising from the idiosyncracy of the particular experimental apparatuses used and the ways they are configured and deployed during the measurement process; the imprecision is an inevitable artifact of the limited acuity of any probe.[16] In order to compute reasonable values for the expected errors and imprecision (so as, for example, to be able to say when a measured result differs by an inadmissible amount from a theoretically predicted result), one must often take account of fine details of the measuring apparatus and the particulars of its coupling to the system under study in one's model of the experiment. Call this 'the expected error problem'. Thermometry provides an excellent example: different sorts of thermometers (bulbs of gas, pyrometers, inhomogeneous thermocouples, *et al.*) couple to systems in radically different ways, the fine details of which must be handled on a case-by-case basis in order to correct for the effects of such phenomena as convective currents in fluids. (I discuss the example of temperature at greater length at the end of this section.)

The third reason is a deeper one. Theory must be able to provide guidance to experiment in the design of new types of tools for probing novel sorts of phenomena, in the design of new types of tools for probing known phenomena in novel ways, and in the design of the experimental configuration itself, *i.e.*, how the instruments are used; conversely, experiment must be able to provide guidance to theory in modeling practically constructed novel ways of coupling to systems known and unknown so as to place constraints on the possible soundness of theoretical description and prediction. In order for each to carry out these tasks, theory must be able to represent the fine details of the apparatus as it is to be used in the experiment. This particular interplay between theory and experiment, in theoretical guidance in the construction of instruments and the design of experimental configuration on the one hand, and in experimental constraint on the soundness of theory on the other, is one of the most profound ways that theory and experiment are able to make contact with each other; without it, it is difficult to see how any epistemic content could accrue to theory in the first place.

The search by Hertz for ways to produce and detect free electromagnetic waves as predicted by Maxwell's theory provides a beautiful illustration of the delicate dialectic required between theory and experiment, especially in the construction and modeling of instruments in the attempt to produce and probe a phenomenon so poorly understood.[17] During most of the career of the investigation, Hertz had little idea what sorts of arrangements of what sorts of physical system

15. See any good book on astrometry, such as Kovalevsky and Seidelmann (2004), for a full treatment of aberration and the details of computing corrections for it.

16. I ignore error arising from unknown sources, "noise", which raises its own fascinating set of problems, well beyond the scope of this paper.

17. See Hertz (1893, *passim*), including the preface by Helmholtz, for an absorbing account.

would produce electromagnetic waves in the first place, and even less of what sorts of instrument could reliably detect them. His search necessarily included the construction of detailed models both experimental and theoretical, each guiding the other in turn, of different proposed methods of coupling the electromagnetic field to its environment and different instruments to try to realize those couplings.[18] Of course, everyone at the time knew that one could derive the wave equation for the electromagnetic field from Maxwell's equations, and they knew the plane-wave solutions and the principle of linear superposition for electromagnetic fields, but I would say that, at that point in the history of Maxwell theory, the theory did not include a characterization of electromagnetic radiation with substantive epistemic content. In one sense, as Hertz famously remarked, Maxwell's theory may be his equations, but the epistemic content, including the practical knowledge, that characterizes those equations *as* a theory of *electromagnetism* does not exist in a meaningful way until we understand it well enough to use it to make testable predictions—but a prediction is not testable if we don't know in principle how to test it![19] This is not verificationism about meaning. It is rather the simple point that, if we do not know how to bring a theoretical structure into contact with the world by experimentation or observation, then we have little or no grounds for trying to understand that structure as representing anything in the world.[20]

The fourth reason is the deepest, and depends in part on the previous three. Without an understanding of the fine details of the way that theories do and do not successfully model actual experiments on the kinds of physical systems they purport to treat, we have no way to demarcate the regime in which the theory does in fact appropriately and adequately apply for the representation of those kinds of systems—its *regime of applicability*. It is only by dint of the practical forms and aspects of scientific knowledge, in other words, that we can determine a theory's regime of applicability—when, *e.g.*, what we pre-theoretically conceive of as a body of fluid, manifesting some dynamical behavior (say, what looks like turbulence), is appropriately and adequately treated by Navier-Stokes theory and when it is not. Knowledge of that regime, however, embodies perhaps the most important part of the epistemic content of a theory—without it, we have no warrant at all for trusting our use of the theory, much less for understanding it *as* a theory of a particular class of physical systems. Nothing in the theory's formalism can tell us this, without the input of knowledge that can be had only by experimental practices that require schematizing the observer in theoretical models.[21] Much of the rest of the paper will be dedicated to working this idea out.

18. Buchwald (1994) provides a detailed and comprehensive exposition of Hertz's search for an instrumental design and a theoretical representation that jointly would do the job. There is a close affinity between my own arguments and Buchwald's conclusion that, in the end, Hertz required only a highly abstract, "schematized" representation of the instrument—the dipole oscillator—for his purposes, and yet he still did need an explicit representation of it in his theoretical work, no matter how "simple" and abstract. I take the lesson to be that Hertz needed that schematized representation in order for the theoretical and the experimental parts of his work to make substantive contact with each other.

19. This all too brief discussion of Hertz bears profitable comparison with Harry Collins' idea of the experimenter's regress; see, *e.g.*, Collins (1992, ch. 4). In contradistinction to Collins, however, I consider this relation between theory and experiment to be not only unproblematic, and in particular not circular in the sense of leading to a regress, but rather necessary for the progress of science. I am thus more aligned in some ways with the views of Franklin (1986), many of whose criticisms of Collins I endorse.

20. See also the philosophically rich essays by Maxwell (1871; 1876) himself on this necessary sort of interplay between theory and experiment.

21. Although it does not bear on the question of how the schematic representation of the observer informs and is required by work in physics itself, I feel it is important to note that deep philosophical investigation of the foun-

4 What a Theory Is

What, then, is the theory of black holes? It cannot be the subset of Lorentzian 4-manifolds with non-trivial event horizon.[22] That cannot tell us that the Milky Way has at its center a black hole of about 4 million Solar masses, about 88 million kilometers across, referred to by astronomers as 'Sagittarius A*' (abbreviated 'SgrA*', pronounced 'saj ay-star')—even putting aside the fact that it is likely that the thing has nothing associated with it resembling an event horizon in the standard, formal sense. Even if we throw in interpretive postulates or a Tarskian-like semantics, it cannot tell us how we know this, why we think we are justified in believing it, the ways we may be justified in using it as evidential warrant for further knowledge claims, and so on—all of which counts as part of the epistemic content of the theory.

To this observation, an advocate of the standard view may reply that a particular prediction of the theory starting from particular initial data is not "part of the theory" in any interesting sense—we must, she will say, sharply distinguish between, on the one hand, the formal structures of the theory that encode its form of nomic possibility and, on the other, the possibly realized individual physical systems characterized by contingent initial or boundary conditions. Nonsense, I say. First, an *ad hoc* argument: on the standard view, SgrA* *is* one of the Tarskian models, possible worlds, or what have you, that characterizes the theory. Thus scientific knowledge that we have about SgrA* forms part of the theory even on the standard view. That leads to the deeper response, to point once again to the question whether one can cogently articulate the character and content of that knowledge in isolation from the other forms or aspects of scientific knowledge, the more practical ones, we have about SgrA*: one cannot identify the putative Tarskian-like model of the theory, or find the correct application of the interpretive postulates required to identify a solution to the field equations as representing SgrA*, in isolation from the practical knowledge used to characterize it as a physical system amenable to representation by general relativity. Standard interpretive postulates such as "freely falling particles traverse timelike geodesics" are inadequate for the job.

The problem for the standard view is even more severe than these remarks suggest. Criteria for what we are to count as part of SgrA* and what not to count for the purpose of tallying the theory's models are never specified by advocates of the standard view. Should we include the accretion disk? The mini-cluster of rapidly orbiting stars immediately surrounding it? The frozen magnetic field surrounding the horizon and ionized plasma jets shooting along it? Does adding or subtracting each of those yield a new model of the same target system or just a model of a different target system? Does including inhomogeneities in the accretion disk or the magnetic field

dations of physical theories also at times relies on it. The work of Geroch (1977), Malament (1977), and Manchak (2009) on predictability in and observational indistinguishability of relativistic spacetimes, and the work of Malament (2002, 2003) on defining relative rotation in relativistic spacetimes, are exemplary in this regard: the analyses and arguments of each requires schematizing the observer as a timelike worldline. The arguments could not be made without reference to the possible observations of such schematized observers.

22. I put aside here the fascinating question of what physicists mean in the first place by the idea of a black hole, the manifest fact that different fields of physics, and often even different physicists in the same sub-field, have different, often mutually inconsistent, but still closely related definitions of 'black hole' (Curiel 2019). This phenomenon, by the way—the existence of different, even inconsistent definitions for the "same type of thing"—is ubiquitous in physics. To my mind, this strongly suggests already that a semantics for theories based on designation as the fundamental semantic relation cannot suffice for a representation of our knowledge in physics.

or the plasma count as refining an existing model or as creating (or identifying) a new model? What about accounting for the tidal deformation of the orbiting stars? And so on. And in each case, once we have decided, then what type of model are we to use to represent the phenomena in question? One using magnetohydrodynamics to model the accretion disk, or a statistical mechanics of charged plasma? Both are encompassed in the framework of general relativity.

In our current state of knowledge—given the actual epistemic content of general relativity as we know it—there is no single, unambiguously correct answer to those questions. Different ways of modeling the phenomena yield different families of solutions to the Einstein field equation in general relativity, none of which are related to each other in any simple way: none of the families "reduce" to the others as one lets some parameter smoothly go to zero, none of the families are subsets of the others, none of the families non-trivially intersect all the others, and so on. One can, for example, depending on one's purposes, treat the accretion disk as an uncharged dust or fluid or as a charged plasma, and, if the last, one can either treat the plasma as a source or treat it as not a source ("test matter") for the ambient magnetic field. This will be justified, *e.g.*, by a solid practical understanding of the aspects, features and components of the system one's observational instruments will couple to in the kinds of measurements one plans to make. For some wavelengths of electromagnetic radiation one aims to detect, one can ignore the inhomogeneities and anisotropies in the magnetic field arising from treating the plasma as a source, and for others not, depending, *inter alia*, on whether those inhomogeneities and anisotropies are comparable in scale to the wavelengths one plans to study. Whether one treats it as charged is irrelevant, for example, if one is making measurements in the gamma-ray wavelength spectrum, as the electromagnetic radiation of that type produced by the system is insensitive to the differences. Similarly, when one treats the constituents of the accretion disk as uncharged, then whether one models it as a dust or as a fluid is irrelevant if one is making observations in the x-ray band, as the system's generation of x-rays is insensitive to the differences.[23]

In all these cases, it may seem as though the one type of model is just a simplification of the other, as (say) one allows some quantity or coupling to go to zero. This is not so. One may think that the models based on dust are just the fluid models in which the pressure and viscosity are taken to zero. The procedure just described, however—"let pressure and viscosity go to zero"—is ambiguous, as there are many ways to construct such a limiting family in general relativity that each yields a different kind of spacetime as the limit (Geroch 1969). In any event, the "obvious" ways of doing it give the wrong answer in some cases: it is a theorem of Ellis (1967) that if a pressureless dust is shear-free, then either its twist or its expansion must be zero. Many fluid models of accretion disks, however, are both shear-free and have non-vanishing twist and expansion, but the theorem tells us that there are no dust models that correspond to such behavior. (Recall that the Einstein field equation is non-linear, so the behavior of the limits of such procedures as "take this parameter to 0" cannot always be simply "read off" the mathematics in a naive way.) There is no systematic relation between the dust and the fluid models of the accretion disk.[24]

23. I struggled to find a few references to cite to substantiate these claims. I failed. This is not the sort of knowledge explicitly presented in research papers nor in textbooks (though it is often implicitly there, if one knows how to read between the lines). I learned it, as all those in the field do, by asking questions at advanced research seminars and colloquiums and having detailed discussions with knowledgeable practitioners. The discussion in §6 of the relevance of how one learns different forms of knowledge will clarify the import of this remark.

24. Indeed, even just from a mathematical perspective, the situation is more complex than I have sketched, and

In sum, there is no single, canonical model that represents SgrA*, nor even a single naturally inter-related family of models. We rather have several different, not unambiguously related families of models, each of which captures some aspect, feature or component of the system not captured by the others, and each of which is appropriate and adequate for the different investigative purposes one may have—each gives what we believe to be excellent answers to kinds of questions that the others answer terribly.[25] The propriety and adequacy of the model for many given purposes, moreover, is determined in large part by the kind of knowledge that can be had only by comprehension of the practical part of the theory—what sorts of instruments with what sorts of sensitivities are appropriate and adequate for probing and studying those different aspects, features and components of the system, in which states and under what sorts of conditions, and the kinds of arguments (almost never articulable strictly in the theoretical formalism) that show this.

There is also the fact that we do not think general relativity is at bottom correct as a theory of spatiotemporal structure and its relation to matter; at some point, presumably, quantum effects must be taken into account, and we have little to no idea how to do that. In that sense, *no* model general relativity produces can possibly be "the right one" for SgrA*. Physicists almost universally ignore this fact in their work, and with good reason: even were we to have a satisfactory theory of quantum gravity, it is almost certainly the case that we would be unable to use it to construct models of SgrA* to answer the astrophysical questions about it we are interested in.

Indeed, a crucial part of the epistemic content of the theory is the knowledge of the physical regime in which it is appropriate and adequate for representing a given class of phenomena at all. As Geroch (2001, pp. 6–7) concisely puts it, in discussing attempts to formulate a relativistic version of Navier-Stokes theory,

> The Navier-Stokes system [of equations], in other words, has a "regime of applicability"—a limiting circumstance in which the effects included within that system remain prominent while the effects not included become vanishingly small.

The physical quantities modeled by a theory's equations have a regime in which they are simultaneously well-defined, satisfy the theory's equations, and have values stable with respect to fluctuations and other effects not representable in the theory (such as quantum effects in classical general relativity, or molecular-scale fluctuations in a theory of fluids). That regime itself forms part of the core of the theory, in so far as it determines what the scope and depth of the theory's empirical content can be. As I'll discuss below in §§8–9, moreover, until one has specified a regime

more dire for the standard view. What, *e.g.*, is the level of differentiability we demand or require of solutions? Depending on the answer to the questions, we may or may not be able to represent such phenomena as shock waves. It is still, in fact, an open theoretical and mathematical question whether generic shock waves in the standard sense (Landau and Lifschitz 1975) can be represented in general relativity outside the regime of exact spherical symmetry (Reintjes and Temple 2020). So, are representations of shock waves in non-symmetrical systems part of the theory of general relativity itself? Please do not say there is an answer to that question in the Platonic heaven of the space of formal solutions to the Einstein field equation. That would not answer the question about the physics., for it would implicitly assume that there is such a thing as "the way" that models in general relativity represent systems in the real world, irrespective of how complex the models and the systems may be, and irrespective of the methods at our disposal for probing them.

25. It is perhaps a commonplace now in philosophy of science that the same target system can have multiple models, each in a different theory or "semi-autonomous" or "phenomenological" and all in manifest tension in some way with each other, as forcefully argued, *e.g.*, in Morrison (2011). My point is different, and, to my mind, much more striking: this happens within one and the same theory.

of applicability for the theory, such questions as I've raised about SgrA* cannot be answered, but to specify such a regime requires of necessity knowledge that can in part be had only by the construction of models of experiments that include schematic representation of the observer. One cannot determine the regime of applicability by examination of the formal structures on the one hand and a clearly articulated family of interpretive postulates on the other. The interpretive postulates on their own cannot do the job, for they require the systems to which they apply to fall already within the regime of applicability; they do not determine it.

That we feel secure in claiming that SgrA* is a black hole of that mass and size—that we believe this to be *scientific* knowledge—depends in large part on the confidence we have in the experiments and observations that delivered the data that allow us to identify and characterize SgrA* as a black hole, and moreover as *that* black hole. That confidence in turn depends on the confidence we have in the methods we use to model those experiments and observations and to manipulate and impose structure on the raw data collected so as to put it into a form that can make fruitful, physically significant contact with the abstract, formal structures of pure theory. And much of the knowledge that underlies the experiments and observations and constitutes their results—the confidence we have in those results, the way we use those results as evidence in turn for other knowledge claims, the confidence we have in doing that—both derives in large part from and consists in large part of the knowledge that comes from constructing a model of the experiments and observations themselves, including schematizing the observer.[26] This also necessarily involves epistemic content from other theories—those, *e.g.*, we use to model the measuring instruments and the environment in which the experiment or observation is taking place—and the knowledge we have of how the epistemic content of those other theories interacts with that of the theory at issue. That itself forms a critical part of the epistemic content of the theory we are considering, how it admits input from other theories.

A theory in my sense thus includes such things as accounts of experimental devices appropriate for studying the relevant kinds of physical system, good practices for employing them, sound techniques for the collection of raw data and for the statistical and other analysis and organization of the same, reliable methods of approximative and heuristic reasoning for constructing models and solving equations, guidelines for determining whether a system of the given kind is in a state and experiencing interactions with its environment such as to be amenable to appropriate and adequate representation by the theory (*viz.*, whether or not the system falls into the theory's regime of applicability), and, more generally, good principles of argumentation for regimenting all these and bringing them to bear on each other.

When I speak of "principles of good argumentation" as forming part of a framework, I do not mean to imply that I think that first-order predicate logic thereby must be part of it. I mean rather: forms of argument *peculiar to the domain of physical systems the framework purportedly treats*. For some argumentative purposes, some "standard" forms of mathematical and heuristic argumentation will not be appropriate, even though they work well in other fields of physics. An

26. See Genzel et al. (1997) and Ghez et al. (2000) for accounts of the detailed infrared studies that nailed once and for all our confidence that SgrA* is indeed a black hole, and in particular the way that models of the experimental apparatus and the observations themselves were crucial to the arguments. Collmar et al. (1998) and Eckart et al. (2017) provide illuminating discussions, almost all by the physicists directly involved in the work, of the fascinating methodological and epistemological problems associated with trying to ascertain that what we observe astronomically when we point our telescopes at SgrA* is indeed a black hole.

example is how perturbations are treated, when understood as leading terms in a (truncated) power expansion in some dynamically relevant parameter. That works well for systems that are well behaved in an appropriate sense, but not for others, *e.g.*, chaotic systems. Thus, the argument in the 19th Century that the Solar System is stable because it is so up to second order in a standard expansion representing the perturbative effects of the planets' gravitational forces on each other (according to the standard methods of classical celestial mechanics as inaugurated by Laplace and Lagrange at the end of the 18th Century), though unobjectionable in and of itself as a standard form of perturbative reasoning, fails because, as Poincaré showed in his epoch-making work *Les Méthodes Nouvelles de la Mécanique Céleste*, the third-order terms exhibit chaotic instabilities (as he explained, with an eye towards the sort of issues I am concerned with here, in the popular essay Poincaré 1897). The lesson is that one must take care in applying perturbative analysis to any Newtonian gravitational systems of more than two bodies. The recognition and respecting of the required care is part of the canon of "good argumentative forms" for that theory. Another good example is the use of the linearized Einstein field equation as a truncated perturbative expansion: one must take care to ascertain that the phenomena at issue are in the appropriate "weak field regime" before one is justified in using the linearized form in general relativity. Even then, one must be careful of the sorts of arguments one will employ the approximation for, for it will not be appropriate for many sorts of claims one will want to investigate. The exterior of "large" black holes can have curvature as close as one likes to zero, and so will satisfy any criterion one reasonably lays down for a "weak-field regime", but solutions to the linearized Einstein field equation on Minkowski spacetime can never replicate global features of black holes, such as the presence of an event horizon.

Similarly, with regard to standards of what can count as good evidence, only what is peculiar to the framework or theory forms a part of it in my sense. An example is stellar aberration. Before telecopic observational prowess, in conjunction with calculational capacity, had developed to such a degree that the phenomenon could be measured and controlled for, worrying about it did not form a part of good evidentiary standards peculiar to the theory of astronomy as formulated in the context, *e.g.*, of Newtonian physics; afterward, it did.

In sum, a theory must include all the practices and principles that allow one to meaningfully bring formal theoretical structure and practical experiment into contact with each other so that the former may be used to interpret the latter, and the latter may be used to constrain the former, *i.e.*, so that data may be structured in such a way as to be comparable with, and even identified with, *e.g.*, formal solutions to the equations of motion. This will include, *inter alia*:

- mathematical structures, relations and formulæ over and above the abstract equations of motion or field equations (*e.g.*, in Newtonian mechanics that $\vec{v} =_{\mathrm{df}} \dot{\vec{x}}$, that mass is additive, that spacetime has a flat affine structure, and so on);
- standards of good argumentation (accepted approximative techniques for solving equations for the purpose of addressing particular kinds of questions, methods for stability analysis of perturbative results, sound heuristics for informal arguments, and so on);
- families of accepted experimental and observational practices for systems of different types;
- rules for connecting experimental outcomes with formal propositions (principles of representation, guidelines for the construction of concrete data models from raw observations, rules

for reckoning expected experimental precision and error, and so on);

- rules of evidential warrant (what can be evidence, how to apply it, reckoning of error tolerance, and so on);
- and guidelines for judging the legitimacy of proposed modifications, extensions, and restrictions of all these.

(I make no pretense that this is an exhaustive list, only a sample of characteristic components of a theory's epistemic content.) Most of this will be difficult if not impossible to articulate and record in an exhaustive and precise way, so as to lend itself to use in formal philosophical investigations. We must trust that all such collateral principles and practices are there, and can be, now and again, each more or less precisely articulated as the occasion demands. The same holds true, however, for formal reconstructions of all forms of reasoning in science (*e.g.*, the 'auxiliary hypotheses' of the hypothetico-deductive method, which always hide an unruly mob of philosophical sins)—which, I believe, those formal reconstructions themselves lull us into forgetting.[27]

On this view, the entirety of a theory is a dynamic entity, evolving over time as new theoretical and experimental techniques and practices are developed and accepted. I believe this is the right way to think about these matters for many if not most purposes in those parts of philosophy of science studying scientific theories. The contemporary practice of treating theories as static, fixed entities, especially in work of a more technical and formal character, can lead to serious philosophical error. An adequate semantics of a theory, for instance, should reflect and accommodate its dynamic nature.

Before moving on, I must emphasize that I am not opposed to the use of formal machinery in philosophy of science in general or in the study of theories in particular, far from it. I am opposed only to a certain conception of how formalism can be fruitfully conceived of as representing or capturing (part of) the structure of scientific knowledge, *viz.*, as cleanly segregable from all practical concerns.[28]

In the end, the standard view is a philosophical idealization. Unlike idealizations in physics, however, its use is not susceptible to justification or defense by the use of a quantitative measure of the expected error it introduces, and the subsequent judgement whether that error falls within one's error tolerance. The kinds of error the standard view introduces are not controllable in that fashion. One must argue for the goodness of the idealization on a case by case basis, in a way that respects the peculiar demands of the given philosophical context. There are cases in which it is manifestly a good idealization. A good example is Earman's (1986) study of different explications of the idea of "determinism" and whether or not different physical theories respect them. Relying only on the mathematical formalism in conjunction with a few interpretive postulates, Earman's constructions and arguments tell us something of great conceptual and physical interest about

27. See Curiel (2020) for a discussion of these kinds of informal practices and principles that necessarily attend real application of theoretical structure in a particular form of logical argument deployed in physics.

28. As is so often the case, Suppes' (1993) discussion of this issue is insightful. I am pleased to say my views have a great deal in common with his across the board, from the role of formal Tarskian-like models to the need for analysis of the pragmatics of experimental evidence. I fancy many not directly familiar with his work may be surprised to hear this, as he is often caricatured as the formalist *par excellence.* He elaborates on his thoroughly pragmatist predilections and views, in most illuminating ways, in Suppes (1998) and Ferrario and Schiaffonati (2012).

those theories. Specific conclusions about what such explorations can teach us, however, must be drawn with account taken of the fact that one ignores much if not most of the epistemic content of a theory while exploring. There are cases in which it is manifestly not—just open any book on analytic metaphysics.

5 What Measurements and the Observer Are[†]

The relevant notion of measurement and observation at play here is a "non-primitive" one, whose explication in the context of a particular theory, or as an adjunct to a particular theory, is always at least in part mediated by the theory.[29] I mean something like: "measurement as coupling between two individual systems, usually of different types", such that:

1. one can distinguish the target system (the one being measured) from the measuring system, in the sense that there are some quantities of the target system and some quantities of the measuring system that dynamically evolve independently of each other;
2. there is at least one fixed quantity (the *measured quantity*) borne by the target system whose value will be determined by the measurement;
3. there is a set of quantities (possibly a singleton, but not empty, the *measuring quantities*) borne by the measuring system that jointly couple to the quantity being measured, in the sense that the measured quantity and possibly some of its derivatives are algebraic functions of the measuring quantities;[30]
4. the minimal temporal interval required for a measurement to take place is small compared to some relevant, characteristic time-scale associated with the dynamics of the state of the measured system during the time of measurement.

Some simple examples will clarify what is meant. For a gas-bulb thermometer, the gas's volume (the measuring quantity) is an algebraic function of the temperature of the system being measured; the equilibration time, *i.e.*, the time it takes for the volume of the thermometer to achieve its final value directly proportional to the temperature of the measured system, is small compared to the characteristic time-scale in which the measured system will change its temperature in response to any external influences that may be present. When the acceleration of a test-particle of known mass and charge is used to measure the vectorial value of an ambient, static electric field, the

†. This section does not appear in the published version of the paper, due to length constraints.

29. It is, therefore, exactly the kind of measurement that does not pertain to the Measurement Problem in quantum mechanics.

30. *Cf.* Geroch (1996, pp. 3–4, arXiv version):

> Roughly speaking, two fields are part of the same physical system if their derivative-terms cannot be separated into individual equations; and one field is a background for another if the former appears algebraically in the derivative-terms of the latter. The kinematical (algebraic) interactions are the more familiar couplings between physical systems.

In this sense, some quantities borne by some types of physical system cannot be measured. Entropy is an example: entropy mediates no physical interaction, does not couple with the quantities of any other type of physical system. Its value can be determined only by measuring (in this sense) other quantities (*e.g.*, temperature and free energy) and then calculating it from those values. That is why there is no such thing as an entropometer.

particle's acceleration is an algebraic function of the value of the electric field; in this case, the acceleration is an "instantaneous" response to the intensity and orientation of the electric field. Components of the Riemann tensor are algebraic functions of the differential accelerations of a collection of nearby freely falling particles, in accord with the equation of geodesic deviation, and those differential accelerations serve as measuring quantities for those components.

It is a deep and difficult question, to ask how one "reads off" the results of a measurement from the values of the measuring quantities. Naively, it seems as though the process must either continue in an infinite regress or terminate in a measuring quantity whose values are amenable to direct inspection by the ordinary human sensory apparatus. But such a conclusion comes from too quickly accepting the raw empiricist's naiveté—I think rather that the distinction drawn in discussions of empiricism and realism, between "theoretical" terms and "observable" terms, is a red herring. Our sensory organs are instruments, and we know—through theoretically grounded experimental investigations—that they are often unreliable ones, and we know, moreover, in quantifiable ways, experimentally determined under precisely specifiable conditions, how they are unreliable: the ways and the magnitudes of the errors. We know the same thing about other instruments of measurement we use in experiments, often in greater, more precise and more quantitatively exact detail. It is only knowledge gained through such theoretically controlled means, *i.e.*, gained using instruments that we have some measure of a theoretical grip on the inaccuracies of, that can contribute in the strongest form to the epistemic content of a theory. Thus, the relevant distinction for the analysis of scientific knowledge and the nature of scientific theories is between purely theoretical propositions and experimental propositions based on measurements using well understood instruments, whether those be sensory organs or electron microscopes. So far as *scientific* knowledge is concerned, the epistemological status of the different kinds of instruments is the same, so long as their various forms of inexactitudes and systematic errors are amenable to theoretical and experimental characterization and testing—which I shall refer to as "the epistemic primacy of experiment".[31]

The results of perception are *not* more epistemically fundamental, primitive or secure *with respect to* the constitution of an evidentiary basis for a scientific claim. *That* is a true dogma of empiricism, that perception is the most epistemically secure source of knowledge. Both the results of perception and those of experimentation must themselves be secured—their reliability, veracity, accuracy, precision, scope, fallibility, *etc.*—by scientific investigation, each every bit as much as the other.

31. My view has some affinity with that of Helmholtz (1870, p. 12, from the English translation given in the bibliography reference):

> When we measure, we only effect with the best and most reliable aids known to us the same thing that we ordinarily ascertain through observation by visual estimation, judgment by touch, or pacing off. In these cases, our own body with its organs is the measuring instrument that we carry about in space. Our compasses are now the hand, now the limbs; or the eye, turning towards all directions, is our theodolite, with which we measure arc-lengths or surface-angles in the visual field. ¶ Every comparison of magnitudes of spatial relations, whether by estimation or by measurement, thus proceeds from a presupposition about the physical behavior of certain natural bodies, whether our own body or the measuring instruments employed; a presupposition which, moreover, may possess the highest degree of probability, and may stand in the best agreement with all other physical relationships known to us, but which in any case reaches beyond the domain of pure spatial intuition.

To my mind, the theoretical in science, as supported and substantiated by controlled experimentation, is on a more sound epistemic footing than the perceptual. To be clear, I mean that the theoretical both has more epistemic warrant and plays a more fundamental role in the construction of epistemic warrant than the perceptual *with regard to their respective roles in scientific enterprises*, precisely because of the substantive contact of the theoretical with the experimental, in the relation between which we have many powerful methods of measuring, tracking, manipulating, controlling for, and justifying essentially all the kinds of approximation, idealization and error that such investigations must suffer, none of which we have for perception. And, in any event, the perceptual is almost entirely irrelevant for the philosophical comprehension and understanding of the nature of science and of scientific knowledge, as is shown by the fact that, outside physiology, no science has the theoretical capacity to include sense organs in its models. This, I think, has been one of the main and most pernicious sources of error in much of Twentieth Century philosophy of science, inherited from Kant as worked through the mangle of the Logical Empiricists: that the most relevant epistemic distinction in the analysis of science is the theoretical versus the observable. It's not. The most important and fundamental distinction is between the theoretical and the experimental, and the most important correlative question is, how does it happen that these two dramatically different representations of the physical world not only come into fruitful contact with each other, but indeed need and rely on each other in such intimate ways in order to be able to come into contact with the physical world in the first place. (My one-sentence sketch is, of course, a caricature of both Kant and the Logical Empiricists, but it's one that, astonishingly, continues to guide much contemporary philosophical work.)

Science itself shows us how unreliable our perceptual faculties are, with great precision and rigor. If we accept those findings—and I think we must, if we are to accept anything in science at all, as those findings are grounded in and derive from the same sorts of experimentation, reasoning, *etc.*, as all our most deeply held scientific results—then it follows *a fortiori* that perception and the deliverances thereof are less epistemically secure than experimental knowledge as represented in our theoretical formalism, for we rely on experiment to characterize the epistemic reach, precision and accuracy of perception.

In any event, how a real human comes to know the result of a measurement is an independent question from that of what *constitutes* a measurement and what constitutes an epistemically warranted judgement of a sound and accurate *outcome* of an experiment. That we see a pointer on an instrument indicating the value measured as a result of an experiment to be 3 is a fact of little to no relevant epistemic import. How and that we have good reason to believe that the result we expect is 3 and that the reading '3' is the result of the correct functioning of the experiment as a whole is what matters, and that comes entirely from our reasons for believing that our experiment is latching on to the germane parts or aspects of the world in the right way and that our theoretical representations of the experiment appropriately and adequately captures that latching on to.[32]

32. Compare the remarks of Popper (1959, part II, ch. 5) to the effect that an observation sentence is not an autobiographical report, but a conventional statement of socially accepted scientific fact. Also compare the remarks of Suppes (1998, p. 237) concerning the fact that

> experimentalists are not in any sense searching for epistemic certainty in designing their experiments or reporting their observations. . . . [T]he fantasies of philosophers about certainty of observations is not at all the right model of how to think about what experimental physicists are doing.

We can teach a person to record the temperature of soup with a given thermometer, even if that person has no knowledge of temperature, thermometers or even soup: "stick this rod in the container of this liquidy stuff, in such a way that this bulb at the end is completely submerged; hold everything still for one minute, then write down the number that appears at the other end of the rod".[33] Such a person, however, can reach no epistemically relevant judgment on any scientific question we may want to raise about the experiment: was the result not valid because, *e.g.*, of the presence of a confounding external factor that disrupted the proper working of the experiment? We can be as thorough and detailed as we like in giving instructions to the ignorant soul we entrust with the job, but there is always ambiguity, always something we will not mention explicitly that can bear on whether the recorded outcome is trustworthy, and which the amanuensis will be in no position to recognize as relevant, in her ignorance. Did we forget to mention not to hold the thermometer in place with heated tongs? Most likely. Only someone with practical knowledge of how the instruments work, informed by the right sort of theoretical knowledge about both the instruments and the target system, can judge such matters with epistemic warrant—and they can do so even if their perceptual faculties are grievously impaired.

For all these reasons, I want to speak of experimentation, measurement and observation as practices and processes that can be characterized independent of the direct experience of humans, though certainly not independent of the current epistemic state and capacity of humans. By "schematize the observer", therefore, I mean something like: in a model of an experiment, to provide a representation of something like a measuring apparatus, even if only of the simplest and most abstract form, that allows us to interpret the model *as* a model of an experiment or an observation. Such schematic representation, moreover, is not possible in a physically significant and cognitively substantive sense without recourse to all the other components of what I will call the practical part of scientific knowledge, as spelled out in the next section.

6 The Different Forms and Aspects of Scientific Knowledge

> In theory, there's no difference between theory and practice.
> In practice, there is.
>
> – Yogi Berra

An adequate semantics should be able to support the articulation of, to represent the meaning of, all knowledge claims a theory embodies, for all the forms and aspects of knowledge thus embodied. As the discussion of SgrA* in §4 highlights, my formulation of the question "what is a theory?" brings into focus what I consider another shortcoming of the standard view—that,

33. Compare the function of the illiterate ancient and medieval scribes who scrupulously duplicated manuscripts one letter at a time, having no knowledge at all of what each squiggle meant, perhaps not even aware that each squiggle was a conveyor of semantic content, nor indeed of how to syntactically individuate squiggles; which example, moreover, shows how important such workers, who have no knowledge of what they're doing, can nonetheless be in any human enterprise, even the intellectual ones. We would have no works of Plato, none of Archimedes, Aristotle, Aeschylus, of any of those whose thought currently forms one of the cornerstones of the knowledge and culture of the present age, without the toil and travail of those now nameless and forever hapless laborers.

according to it, all scientific knowledge is essentially of one undifferentiated kind: approximately true (or at least epistemically justified) propositions about states of the world, and perhaps about the nomic structure of the world independent of the state the world is actually in. This comes out most clearly in the picture of a theory's semantics that naturally accompanies the standard view of theories, that it is fixed by ontology.

This is not an accurate picture of scientific knowledge. Scientific knowledge has many forms and aspects that the picture cannot well capture. An account of scientific knowledge that characterizes a theory as a *physical* theory of the types of physical systems it purports to treat must include *all* forms and aspects of knowledge the theory has endowed us with, including that gathered in the context of experimental practice and expressible only by using resources that go beyond the theory's formalism. A semantics that captures only "potential knowledge" that it is impossible for humans to know, such as that embodied in the "family of all models or possible worlds" of the theory, is useless as a semantics on which to ground an analysis of the nature of scientific knowledge and its epistemic warrant, because science is a human enterprise. If the semantics captures only what it is impossible for humans to know, then what good does it do? And why should we care about it? What problems will it solve, or even illuminate? Ones of a purely formal character, no doubt.

A useful classification of what I have been calling the theoretical and the practical forms of scientific knowledge is suggested by the following observation of Stein (1994, p. 637, emphases his):

> It is hard, but possible, to learn theory by self-study from books; it is surely much harder to learn experimental techniques without a teacher to help one acquire skills; but what I suspect to be impossible is to learn the principles of experiment without *actual* experience with the relevant *instruments*.

How one is able to learn different parts of the epistemic content of a theory may seem at first glance irrelevant to an analysis of the character of the knowledge to be learned. To the contrary, I think Stein has put his finger on something deep about epistemic content: how it is possible to come to learn something shapes and constrains the kinds of epistemic warrant we can have for it, and thus constrains how we may bring it to bear in possible evidential relationships to other pieces of potential knowledge. The nature of the object of knowledge determines how one may learn about it, and so that constraint itself can teach us something about its nature. Thus, if one wants to understand the structure and character of our knowledge, in all its forms, one of the deepest and most powerful clues we have is what we do and can learn, and how we do and can learn it, for how knowledge can be learned tells us how it can (and cannot) be represented—and thus whether it can be accommodated by a representational regime consisting only of formalism in conjunction with "coordinating principles" or "representational capacities". One of the most important distinctions here is that between a subject matter's being *teachable* and its being *learnable*.[34]

Theoretical knowledge of the sort Stein describes in the first part of the quote has the peculiar and remarkable property of being representable by a well arranged sequence of propositions (a

34. It is instructive in this regard to compare Stein's remarks about how one can and cannot teach and learn different forms of knowledge in physics with Plato's discussion, in *Protagoras*, of how one can and cannot teach and learn virtue: Plato concludes (as I read the dialogue) that virtue is learnable but not teachable (at least not based solely on the output and intake of discursive language use); one must practice it in order to understand and master it.

well written textbook, say). If it did not have that property, we could not learn it from such a sequence. Epistemic warrant for knowledge acquired in that way has its strengths and weaknesses. It relies on a particular form of trust in the scientific community,[35] and on a belief that one's own idiolect shares enough in common with that of the author's (or, at least, that one can learn enough of how the author uses language) to be certain that one has understood the epistemic content the author is trying to convey. It also, however, admits of being made precise and clear in a way that conduces to verification and defense, and to application in further argument.

Practical knowledge cannot be effectively represented in this way.[36] An anecdote will perhaps show this better than an argument. During the 2017 run of the Event Horizon Telescope (`https://eventhorizontelescope.org/`), when a worldwide team of astronomers were co-ordinating the use of several observatories around the world to simultaneously record observations of SgrA* (for later collation and structuring) in the attempt to record the first direct image of a black hole, the head of the organization, Shep Doeleman, masterminded the proceedings in a control center two doors down from my office at the Black Hole Initiative. Naturally, I spent some time in the room watching them, as this was scientific history in the making. One evening, while the team was deciding whether or not to make observations that night, I witnessed the following exchange between Shep and an instrumentalist at ALMA, the Chilean observatory (5000 meters above sea level), who was having trouble with one of the specially designed instruments—it seemed to be malfunctioning in a particular way, but the source of the problem was not immediately clear. Shep said, "Oh, yeah, I've see this before. Here's what you do. Remove the oscillators and take'em down to 1000 meters [above sea level], and leave'em for a few hours; they'll settle down, and then when you take them back up, they'll work." Chilean instrumentalist: "Why does that work?" Shep: "I have no idea, but it does." This is a part of the practical knowledge associated with observational relativistic astronomy that I doubt anyone will ever find written in any book—that sort of malfunction is likely the result of the quartz crystal oscillators suffering from pressure-related issues associated with high elevation. Moreover, even if written, it is the sort of thing one could not *learn* from just having read the book. Of course, one could learn that particular trick—but to learn how to *play* with the instruments so as to acquire the feel for them necessary to figure out such tricks for identifying and solving problems—that is something that cannot be learned from a book, as Stein so beautifully points out in the passage I quoted.[37]

35. Of course, not only the learning of theoretical knowledge requires such trust: "When scientists present the results of an experiment they take responsibility for those results by attaching to them the most precious coin of the scientific realm: the individual scientist's pledge to speak the truth" (Lockman 2005, p. 2).

36. Practical knowledge in Stein's sense, then, and as I will further develop the idea in this section, is close kin to the notion of "knowing how" much discussed in standard epistemology since its introduction by Ryle, and even closer to its more specialized form, "tacit knowledge", as introduced by Polanyi in the context of studying the epistemology of engineering and technology. The literature on those notions is too vast to engage with here.

37. Before making the anecdote public, I emailed Doeleman to ask his permission, asking him to read as well the remarks I made surrounding it. He wrote me back an interesting elaboration on the anecdote, explaining the process by which he acquired the knowledge about the oscillator, which illuminates the theme even more:

> [The anecdote you're relaying concerns] a quartz crystal oscillator we had running at Chile for a while that then went on the fritz. After we brought it back to the SAO [Smithsonian Astrophysical Observatory] it seemed to return to normal, and we attributed that to the high elevation and low pressure (up at 16,500 ft). That's not to say that the standard operating procedure should be to send such crystals to a low elevation 'spa' for rejuvenation every so often. It was more just me speculating about what was going on. However the general point you are making is very valid. As with

I think this lesson generalizes: how it is possible to learn different pieces or areas of scientific knowledge is one important criterion for determining the kind of knowledge one is dealing with. This practical form of knowledge, an inextricable part of the epistemic content of the theory one considers, can not be formalized or even just well captured in book form; most theoretical forms of knowledge can. The latter is, to speak crudely, propositional in a way the former is not. That does not, however, *ipso facto*, imply that it is more important for a proper understanding of the character and content of the sum of knowledge that a theory embodies, that characterizes it as a *physical* theory of *that* sort of family of physical systems.

The proponent of the standard view may well reply that it's an amusing anecdote, but tells us nothing of the epistemic content of general relativity. I disagree. It is knowledge of this sort, and the trust we have in experimentalists and instrumentalists of the calibre of Doeleman and his collaborators, that gives us the epistemic warrant we have for believing that our astronomical observations provide us understanding and comprehension of the physical systems that general relativity provides us models of—that justifies our belief that general relativity is a good theory of those systems, and that we know what the mathematical formalism represents, and know in particular which parts of the formalism represent which parts of the world. More importantly, this shows how the two types of knowledge are not cleanly segregable—it is only practical knowledge of the kind had by the experimentalists commingled with the formal theoretical apparatus that allows us to understand the formalism as part of a physical theory.[38]

> any profession, we develop intuition about the instruments and systems we work with, just as any craftsman understands the feel of good tools working good materials. The lesson in the anecdote you quote is that the quartz oscillator in question was brought up to high altitude at the ALMA site as a reference to check the performance of the hydrogen maser, which is critical for VLBI. When the crystal-maser comparison went South, there was some concern that it might have been the maser that was the problem, but after working with these atomic clocks for so long, I was sanguine. There was a greater probability that it was the crystal (with a power supply not designed specifically for long-term operation at high altitude) that was at fault. When I brought a second crystal to Chile to check it out, that was indeed the case.

Doeleman then recommended that I read Lockman (2005), which examines why going to the telescope and being there with the instruments in person can have great benefits, both epistemic and practical, over remote observing. I did read it, and now I recommend that you do as well. Here is a gem from it (p. 3):

> Someone who actively plays with the equipment, tries out various combinations of things, and constantly iterates on technique, not only gets data and a sense of its correctness, but also develops skills which can make the next data better. The way to become a skilled observer is to participate in the observations as completely as possibly, to seek active control or understanding of every phase of the process; to try to recognize the difference between the basic limitations of an instrument and those limitations which are rooted in style or tradition.

38. I do not want to leave the reader with the idea that I think all theoretical knowledge is articulable in propositions and learnable from books, nor that all practical knowledge can be had only by experience. The situation is far more nuanced than that, and a complete discussion would take us too far afield. To get an idea of how the simplistic picture I have sketched falls short, consider all the inexact techniques theoreticians learn and master in order to solve their problems, and how, moreover, they can in general learn to do so only by practice, by trial and error, not from textbooks—for example, the "feel" for when a given perturbative or approximative technique is the right one, how it should be applied, when it is legitimate, when illuminating, where it breaks down, *etc.* I have not found a good discussion of this with particular regard to theoretical physics, so I invite the reader to look at the great geometer William Thurston's remarkable discussion of what mathematics is and what it does (Thurston 1994), particularly his account, in §4 of that paper, of how he came to learn what a "proof" in mathematics really

To make this argument a little more precise, I again follow Stein (1994, p. 635, italics his), now in distinguishing among different aspects of scientific knowledge:

> I am construing the word knowledge in a wide and ambiguous sense. The reflections I am proposing have as their object (a) our knowledge in physics as an *achieved result*: knowledge as *the knowledge we have of X*; (b) our knowledge as susceptible of *justification* or *defense*—that is, as involving a structure of "evidence" for its asserted contents; and (c) knowledge—science—as (to appropriate a word of Isaac Levi's) an *enterprise*: an activity aimed at increasing our knowledge in sense (a), by means appropriate to the constraints of (b).

I would add to this list a fourth, closely related to, but distinct from, Stein's (b): knowledge as ground for epistemic warrant for other knowledge claims and for furthering the enterprise of science, *i.e.*, knowledge that is suited to playing the role of evidence for other assertions. Call this '(d)'.[39]

The standard view, I claim, captures at most one of these aspects, knowledge as achieved result, and that at best only in part. The standard view's picture of the knowledge content of a theory, entirely captured by indicative propositions about the state and nomological structure of the world, does not allow one even to ask how such claims are justified, how their justification depends on the knowledge we have in virtue of other theories, the limitations on the scope of such claims, and so on, for the practical knowledge I have been discussing informs and provides much of the body of the theory's knowledge in all these aspects, and the standard view does not allow this knowledge to be represented. Thus, it cannot explain the epistemic warrant we have for trusting and using the theory, and thinking that the understanding and comprehension it seems to give us of the world *is about the world*.[40]

The standard view, however, is deficient, I claim, even with regard to knowledge of aspect (a) (knowledge as achieved result), in so far as it precludes fundamental understanding and comprehension we gain about parts of the world that can be had only by heuristic, informal arguments that cannot be formalized. A good example is the way that one of the fixed relations among quantities in Navier-Stokes theory, *viz.*, that heat flux is always independent of the pressure gradient (Landau and Lifschitz 1975, ch. v, §49), partly encodes the fact that Navier-Stokes theory is valid only for fluids quite close to hydrodynamical and thermodynamical equilibrium of a certain kind. (This relation is not one of the equations of motion, the so-called Navier-Stokes equations; it forms part of what I call the kinematical constraints of a theory, which I describe in §8 below.) The only arguments I know of for the fact that Navier-Stokes is essentially an equilibrium theory are not only heuristic and approximative, but indeed are, strictly speaking, incorrect in parts, incoherent as a whole, and even inconsistent in several ways (all with respect to the formalism of the theory).[41]

is and how it is he thinks that mathematicians produce and check them.

39. Not all knowledge is suited to playing this role, and its being suitable may depend on facts about the wider scientific context, beyond what pertains only to the narrow investigation at hand. It would take us too far afield to go into this now. I thank Chris Smeenk for helping me to see how subtle this business can be.

40. *N.b.*: nothing I am saying commits me to any form of realism about theories—I can believe that our best theories tell us a lot about the world, and even that those theories provide us with deep understanding of some aspects of the world, without also believing that those theories are exactly or even approximately true of the world in any deep or strong representational sense.

41. I do not know of any published arguments for the proposition. It is a fact widely known among physicists

Here is a sample of some of the propositions such arguments rely on:

- that a "particle" of fluid could "switch flow lines" only if the line it traversed crossed another (whereas in fact this can happen also if two lines merely osculate, or, in technical terms, at a point at which a Jacobi field of the flow vanishes);
- that flow lines can "cross" in a sense, even in the absence of turbulence, by converging to a single, isolated point, a "singularity" (as happens, more or less, when water runs down a narrow drain);
- that, strictly speaking, however, flow lines can never cross, whether in turbulent flow or smooth, as flow lines lose their definition the moment they converge.

The propositions wear their individual incorrectness and incoherence, and their mutual inconsistency, on their sleeves. Nonetheless, they serve their purpose adequately in practice, allowing the formulation of an argument that sketches a picture of a fundamental part of the epistemic content of a theory of a genus of physical phenomena (Navier-Stokes fluids), a picture that in essence conveys the important physical features of the phenomena at issue. In particular, such arguments convey the kind of understanding and comprehension Navier-Stokes theory yields of the systems it treats. The nature of the arguments, moreover, are such that they cannot be made sense of, much less concluded successfully, within the confines of the formal apparatus of the theory alone and any epistemic content that could accrue to it from a pristine semantics based on ontology. In any event, "close to hydrodynamical and thermodynamical equilibrium" is not a concept that lends itself to precise formalization once and for all, independent of context. It is, however, of the most fundamental importance in coming to understanding the epistemic content of Navier-Stokes theory that it is essentially an equilibrium theory. No model in the theory represents this fact. No formal, rigorous proposition can capture this idea once and for all.

Both theoreticians and experimentalists employ that sort of loose, heuristic argument as a matter of course in their workaday activity, as one of the most important tools in their respective tool-boxes. To my mind, the most striking and salient feature of such arguments lies in the fact that, strictly speaking, one cannot prove them either correct or incorrect, in so far as, strictly speaking, several of the terms and relations used in such arguments lose their definition at various points. (The sentence "A bears the property ϕ" can be neither true nor false if neither 'A' nor 'ϕ' nor 'bears the property' has an unambiguous definition.) One cannot analyze the meaning of the propositions of such arguments by analyzing their possible respective truth-conditions in isolation from those of the others, by judging relations of reference to pure ontological models for example. And yet those propositions do have meaning, albeit not precisely determined, and can be more or less appropriate and adequate. One must base the meaning of the propositions on the import of the argument as a whole, and in particular on what we know to be the sort of "mild" incoherencies and inaccuracies we can allow ourselves in contexts like this, as determined and verified by the experimental probing and practical fixing of the theory's regime of applicability. We are back in the neighborhood of needing to know how to schematize the observer in order to grasp and articulate large and important parts of the theory's epistemic content.[42]

practicing in the relevant areas, part of the common lore of the field, passed down from teacher to student in lectures and laboratory apprenticeships and such.

42. None of this has to do with meaning holism in anything like Quine's sense, as expressed, *e.g.*, in the famous

I have argued that the standard view gives too little as an answer to the question of the scope of a theory's epistemic content. In another sense, however, the standard view gives too much—for it says that every question one can pose to the theory has a determinate answer. In a formal sense, this may be correct. Navier-Stokes theory is a continuum theory, so in principle it answers questions about how fluids behave at the scale of 100^{-100} `cm`. But if one wants to know whether the answer given is *relevant to or even just possibly meaningful in the real world*—whether the answer counts as possible scientific knowledge in the senses, for instance, of Stein's (b) and (c), and my (d), and indeed even for the factual kind in Stein's (a)—then one has to know, among other things, the scope of the theory's regime of applicability, *i.e.*, the spatial, temporal and energetic scales (*e.g.*) in which it is a good theory and those where it breaks down. For that, one must have recourse to knowledge that one can come by only through experimentation and the application of knowledge embodied by other theories, and thus, more importantly, knowledge that is not and cannot be encoded in the formalism of the theory itself. Our knowledge of quantum theory tells us that the answer to any question about how fluids behave at the scale of 100^{-100} `cm`, well below the Planck scale, is not so much false as *meaningless*—we have good reasons to think that ordinary spatial concepts such as "fraction of a `cm`" are not meaningful past a certain point. According to the standard view, however, all the answers Navier-Stokes theory gives us about the behavior of fluid at such scales are simply false, albeit meaningful.

In so far as knowledge of the regime constitutes knowledge in Stein's aspect (b), moreover, as being susceptible to justification by involvement in a network of evidentiary relations, it cannot be given as a physical interpretive postulate in any standard sense (*e.g.*, a Reichenbachian coordinating principle, a Carnapian reduction sentence, or contemporary gestures at representational capacities), *i.e.*, one that encodes (in part) or at least makes contact with knowledge cleanly segregable from any knowledge represented only by the theory's formalism.[43] Our knowledge of where and how Navier-Stokes theory, *e.g.*, breaks down is in large part based on and justified by experimental results as represented in and put into meaningful contact with the mathematical formalism. A physical interpretive postulate, therefore, that encoded such knowledge would have to make inevitable reference to such facts that are known only by experimentation, and thus require knowledge of how the observer and measuring devices are represented in the theory. Whether a given question, therefore, that seems in the abstract to be one the theory can address is in fact one the theory can give a physically relevant answer to—whether it is about a system that the theory appropriately and adequately treats—can be determined only by recourse to knowledge that goes well beyond that which can be encoded only in the formalism or that can be expressed by propositions making no reference to detailed experimental knowledge. Nonetheless, such knowledge ineliminably involves as well theoretical terms whose definitions rely in part on some of the formal structure of the

(and glib) aphorism, "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (Quine 1980, p. 41). The meaning of the—strictly speaking—incoherent and inconsistent propositions in the kinds of arguments I discuss indeed cannot be fixed without reliance on a body of other propositions. Here, however, the required propositions concern either the propriety and adequacy of such descriptions in experimental models of fluids, or the way such propositions can be treated as approximations, as it were, of rigorous and precise theoretical propositions. There is no implication that *all* the propositions of theory face the tribunal of experience as a corporate body.

43. This, I think, is what is right about the claims of Friedman (2001) to the effect that, in so far as the relativized *a priori* provides the grounds for endowing theoretical formalism with empirical content, it itself is not susceptible to justification by experimental means.

theory. It is knowledge, in other words, that cannot be cleanly split into a formal part and a practical part. The standard view cannot apply.

Much of this discussion has implicitly relied on a picture of knowledge that goes far beyond what can be captured in propositions expressing true, justified beliefs. The most important component of the epistemic content of a theory, on my view, is more akin to what the (notoriously problematic) concepts of "comprehension" and "understanding" try to get at. I shall not attempt to give a systematic account of these ideas, but I can say a few things to try to convey my sense of them. "Understanding" is something like "the capacity to operate successfully in the scientific enterprise, in all its aspects and parts": to use our representations and instruments as the basis for the fruitful continuation of the enterprise, as part of evidential warrant in testing, as basis for predictions and characterizations, as inspiration for potentially fruitful new investigations, as the grounds for conceptual clarification and innovation in foundational work; and perhaps most of all to grasp how our representations and our instruments relate to, inform, and contribute to the constitution of each other, and to grasp that in such a way that it grounds the work succesfully continuing the enterprise.

Before moving on, I want to address a question that my discussion has raised. One may well object that the kind of knowledge shown by, say, observational astronomers in good manipulation of their instruments may be part of the epistemic content of *some* theory, but surely not that of general relativity. That may well be right in some sense, although I suspect that one can never impose a clean, sharp demarcation, once and for all, between a given theory and auxiliary theories needed by experimentalists in the study and use of it. What then can we say, if anything, about criteria for whether a particular piece of knowledge has been delivered by a particular theory in the relevant sense, so as to count as part of *its* epistemic content, rather than, say, as part of the content of an auxiliary theory used to model an experimental apparatus? I don't know. At a minimum, it should be a piece of knowledge that we could not, even in principle, have without using the theory at issue, at least as determined by the context of our current state of achieved knowledge and our understanding of the web of evidentiary relations that support it. In principle, one should always be able to perform experiments of different kinds, with instruments modeled by different theories, so as to determine whether or not the theory at issue makes an ineliminable contribution to the epistemic content of the knowledge, and so to attempt to isolate that contribution. A theory, however, at bottom, is a nebulous congeries. I doubt that there can be a comprehensive criterion by which one could judge all attempts to answer any question about whether this or that bit of knowledge exclusively belongs to the epistemic content of this or that theory, an answer relevant and appropriate for all foreseeable philosophical and scientific purposes, much less for all those we cannot yet imagine.

In the end, all the problems I discussed in this section arise from the same deficiency in the standard view, to wit, its inability to allow schematization of the observer in a way relevant to the articulation of the epistemic content of a theory: all the problems devolve upon the need to bring theoretical and practical knowledge into fruitful contact, and it is the capacity to schematize the observer that allows that to happen.

7 How Theory and Experiment Make Contact with and Inform Each Other

That we must have the capacity in our theories to construct explicit models of complete experimental situations including instruments and the actual methods of their deployment in order to represent actual observations, however, raises a serious problem, one that Stein (1992, p. 290) trenchantly poses:

> . . . we have no language at all in which there are well-defined logical relations between a theoretical part that incorporates fundamental physics and any observational part at all—no framework for physics that includes observational terms, whether theory-laden or not. . . . I cannot think of any case in which one can honestly deduce what might honestly be called an observation. What can be done, rather, is to represent . . . "schematically," within the mathematical structure of a theoretically characterized situation, the position of a "schematic observer," and infer something about the observations such an observer would have.

In other words, we do not have a formal account of the epistemic content of the theories of physics even minimally adequate for any account of their actual empirical application; this is not to say that such applications in real scientific practice have no foundation or are unjustified, only that we have no adequate comprehension of the process. Forget how we get the theory into or out of the laboratory—how do we get the laboratory into the theory? This, I think, is the fundamental issue one must address in trying to give an account of the epistemic content of scientific theories and how its components bear on each other, and in particular in answering the question whether they are mutually extricable in the way demanded by the standard story told by philosophers about the character of scientific theories, and relied upon by them in their work. (See again the long passage from Stein 1994 that I quote on page 6.)

In order to address this problem, in this section I explain with a little more precision what I mean by 'theory', with the goal of drawing a—schematic!—picture of how experimental results are placed in physically significant contact with formal theoretical structures, and what the nature of that contact is. For my purposes, it suffices to provide only a rough characterization sketched in broad strokes.[44]

A theory, as implied by the discussion up to this point, is a system that allows one, among other things, to formulate propositions and affirm them in principled ways based on evidence gathered according to good principles, to apply them in turn as evidence for other propositions, and to use

44. I discuss here only one way that theory and experiment make contact, when one has already in hand a developed and articulated theory. There are other ways, other roles for each of theory and experiment in bearing on the other, that I do not discuss because they are not relevant for my purposes, such as the use of experiment in guiding the development of new theory, and in expanding understanding. I think excellent case studies for exploring those kinds of roles would be Newton's investigations on light and color (which have not received the philosophical attention they deserve—see Stein unpublished(a), unpublished(b) for a discussion of those investigations with particular regard to these issues, and Stein 2004 for an analysis of how they illuminate questions of understanding in both physics and philosophy) and the role of Fizeau's experiments in the development of special relativity (for which, see Patton 2011). Patton (2012) provides an insightful discussion of the problem more generally.

them as the inspiration and basis for new investigations.[45] A theory in this sense is a system that allows for the unified representation and modeling of a particular kind of physical system so as to render that kind amenable to investigation by scientific reasoning and practices of all forms. A particular kind of physical system is one such that all individuals falling under the kind bear the same physical quantities whose properties are characterized by and whose behavior is governed by the same set of equations of motion and collateral mathematical relations. A theory in my sense also includes such things as accounts of experimental devices appropriate for the probing of the relevant kind of physical system, good practices for employing them, sound techniques for the collection of raw data and statistical and other analysis and organization of the same, reliable methods of approximative and heuristic reasoning for constructing models and solving equations, and guidelines for determining whether a system of the given kind is in a state and experiencing interactions with its environment such as to be jointly amenable to appropriate and adequate representation by the theory (*viz.*, whether or not the system falls into the theory's regime of applicability), and so on.

Before diving into the details of how I think that all these components hang together so as to make it possible for the theoretical and the experimental parts of a theory to make fruitful contact with each other, I shall need further distinctions with regard to "level" of theoretical representation; it will be useful to draw them by laying down terminology. I shall not attempt to give formal, precise definitions. These ideas are for my purposes adequately clarified by examples.

Theories are often formulated in the context of a framework. Newtonian mechanics, the heart of which is embodied in the definitions and the three laws Newton lays out in his *Principia*, is perhaps the exemplar *nonpareil* of a framework. Newton's Second Law in the abstract is not the equation of motion of any particular kind of physical system. It is rather the template that any equation of motion for any particular kind of system treated by the framework must instantiate. Newton's theory of gravity is a theory formulated in the framework of Newtonian mechanics. It treats that kind of physical system characterized, *inter alia*, by the possession of inertial mass, of gravitational mass (equal in magnitude to the inertial mass), of spatial position, and of a velocity expressible as the temporal derivative of spatial position, such that its dynamical evolution is governed by Newton's gravitational force law. Navier-Stokes theory is another theory formulated in the framework of Newtonian mechanics. It treats that kind of physical system characterized, *inter alia*, by the possession of inertial mass, shear viscosity, bulk viscosity, thermal conductivity, fluid velocity, heat flux, pressure, and shear-stress, all satisfying among themselves fixed relations of constraint (*e.g.*, that heat flux is always independent of the pressure gradient), and whose dynamical evolution is governed by the Navier-Stokes equations.

45. My conception of a theory is in many ways similar to, and indeed in part inspired by, Carnap's (1956) conception of a linguistic framework, particularly in the way that a theory in my sense serves to define a fixed sense of physical possibility relevant to the kinds of system the theory treats, just as a Carnapian framework does for the types of entity whose existence is analytic in the framework. Carnap's conception is too broad and vague, however, to do the work I require of it. Stein (1992) provides an insightful and illimunating, albeit brief, discussion of the differences between a Carnapian framework in the original sense and a theory in the sense of a formal structure in theoretical physics related to, but more restricted in scope than, the type I am sketching here. Lakatos (1970) has some affinity with the gist of this view, in particular his notion of the "hard core" of "research programs", though, again, the differences in detail outweigh the similarities. There is perhaps more affinity with the "research traditions" of Laudan (1977), in so far as different ones can share and swap important methodological and theoretical principles, as can happen with theories in my sense.

The template in a framework for equations of motion and other mathematical relations I call *abstract*. Canonical examples are Newton's Second Law, the Schrödinger equation in non-relativistic quantum mechanics, and so on. Structure and entities at the highest level of a theory formulated in a given framework I call *generic*. In particular, generic structure has no definite values for those quantities that appear as constants in the theory's equations of motion and other mathematical relations. The symbol 'k' appearing in the generic equation of motion of an elastic spring modeled as a simple harmonic oscillator, $\ddot{x} = -\frac{k}{m}x$, denotes Hooke's constant (the coefficient of proportionality of a force applied to the spring and the resulting diplacement from its equilibrium position), but possesses no fixed value, and the same for the mass m. It is important to keep in mind, however, that all these formal representations of physical quantities at the generic level in a theory do have determinate physical dimensions, for Hooke's constant, *e.g.*, $\frac{\mathtt{m}}{\mathtt{t}^2}$ (*i.e.*, "mass over time-squared"). Otherwise, one could not say that this is the generic equation of motion for, say, a spring rather than a pendulum or oscillating string or electric circuit or any other type of system whose dynamical evolution is governed by the equation of motion for a simple harmonic oscillator.

One can, in the same way, write down generic solutions to the generic equations of motion; these are formal representations of the dynamically possible evolutions such systems can manifest. One generic solution to the equation of motion of an elastic spring is $x(t) = \cos(\sqrt{\frac{k}{m}}\,t)$, where one may think of 'k' and 'm' as dummy variables, not determinate real numbers. Generic structure defines a *genus* of physical system, all those types of physical system the theory appropriately and adequately treats.

One obtains *specific* structure by fixing the values of all such constants in generic structure, say $m = 1$ and $k = 5$ (in some system of units) for the elastic spring. This defines a *species* of physical system of that genus, all springs with those values for mass and Hooke's constant.[46] One now has a determinate space of states for systems of that species, and a determinate family of dynamically possible evolutions, *viz.*, the solutions to the specific equations of motion, represented by a distinguished family of paths on the space of states. A path in this family is an *individual model* (or *individual solution*) of the specific equations of motion; one common way to fix an individual model is to stipulate definite initial conditions for the specific equations of motion. An individual model, as the name suggests, represents a unique physical system of the species, that whose dynamical quantities satisfy the initial conditions.[47] In the case of the spring, that may mean the unique one whose position and momentum at a given time have the values given by the initial conditions.[48]

46. One can as well consider mixed systems, with, say, a fixed value for mass but indeterminate value for Hooke's constant. These raise interesting questions, but they are beside the point here.

47. Everything I say so far applies only to deterministic theories that represent the dynamical behavior of its target systems with something like partial-differential equations. The discussion is easily modified to accommodate, *e.g.*, stochastic theories, and theories such as classical thermodynamics that have no paths on a space of states representing physically possible processes. It is not clear to me what the limits of this picture are in this regard—what its regime of applicability is, if you will. It would be an interesting exercise to find an example of a physical theory that does not conform to it, and see whether one could emend the picture to as to account for the theory or not. Either way, one would learn something of interest.

48. I have deliberately taken my terminology from biological taxonomy, inspired by the remark of Peirce (1878, p. 605):

> Now, the naturalists are the great builders of conceptions; there is no other branch of science where so much of this work is done as in theirs; and we must, to great measure, take them for our teachers

Finally, I call a *concrete model* a collection of experimentally or observationally gathered results structured and interpreted in such a way as to allow identification with an individual model.[49] (I shall sometimes also refer to a concrete model as *structured data*.) If I continually measure the position and momentum of a spring with known values for m and k, oscillating in one dimension for an interval of time equal to its period of oscillation, and graph the results "in a natural way", *e.g.*, as a curve parametrized by time on a Cartesian plane whose x-axis represents position and y-axis represents momentum, then I shall produce a curve that can be, "in a natural way", identified with exactly one dynamically possible evolution on the space of states used to represent that species of spring. Kepler's organization and structuring of the planetary ephemerides into parametrized ellipses satisfying the Area Law and the Harmonic Law is a marvelous example from the actual history of physics of this procedure.[50]

It is this identification of individual and concrete models that embodies the substantive contact between theory and experiment required to comprehend the full epistemic content of a theory; as we will see, moreover, in general this cannot be done without schematizing the observer.[51] There are many fascinating and deep problems associated with characterizing the ways that experimental and observational results are collected and appropriately transformed into structured data, but I put them aside.[52] Here, I shall give only a (schematic!) outline of how I envisage the procedure

in this important part of logic.

There is much of insightful relevance in the lead-up to this remark, about how one individuates and characterizes genera and species of physical systems in my sense, which it would be illuminating to discuss, but it would take us too far afield. I am tempted to describe structure at the level of a framework as *phylar*, and to call the family of all physical systems treated by a framework a *phylum*—and so all physical systems would fall under the kingdom of physics—but I suspect it would just be distracting to the reader.

49. This idea bears obvious and interesting comparison with the distinction between data and phenomena as drawn by Bogen and Woodward (1988). If I understand their distinction, their idea of data more or less corresponds with my idea of "experimentally or observationally gathered results", but their notion of phenomena, in so far as it seems to try to capture something like general patterns in the world, does not square with my conception of a concrete model, which is the result of structuring the results of a single experiment (or family of related experiments). A complete discussion is beyond the scope of this paper.

50. I am making a serious over-simplification here. Not every concrete model contains data for a sufficient number of dynamic quantities to enable the identification of the concrete model with a unique individual model. A set of data may contain, *e.g.*, data only for the pressure of a Navier-Stokes fluid, which allows one to identify the concrete model only with a family of individual models, all those with those values for pressure, but having any values for all the other dynamic quantities Navier-Stokes attributes to fluids. I also ignore the fact that, in light of the inevitability of expected error and experimental imprecision, one can *always* identify a given concrete model with a family of individual models, *viz.*, all those whose values collectively lie within the error and imprecision intervals associated with the data. I shall not discuss these issues, as they raise too many problems orthogonal to my purposes here, though the problems are of great interest in their own right. I assume from hereon that a concrete model is such as to allow identification with a unique individual model. In particular, I assume it contains values for enough dynamic quantities to determine individual states—at least as many as the "dimension" of the state space–and that determinate, individual values have been fixed for all data, not just intervals defined by the range of expected error and imprecisions.

51. This classification of levels of theory and data and the concomitant idea of identifying individual and concrete models have obvious similarities with the proposals of Suppes (1960; 1962), and indeed much of my own thought has been inspired by my reactions to Suppes' extensive work on these questions (not just those two papers).

52. As is, again, so often the case, Suppes (1960, p. 297) sums the matter up with elegance and concision:

> The maddeningly diverse and complex experience which constitutes an experiment is not the entity which is directly compared with a model of a theory. Drastic assumptions of all sorts are made in

of bringing theory and experiment into contact, based on the concepts I just articulated. I give a more finely grained analysis of the procedure in the next section.

The identification—I think essentially always—comes down in the end to the brute comparison, one by one, of the values of theoretically calculated quantities on the one side with the values of the analogues of those quantities in the structured data on the other, with the choice being made each time whether the values are close enough, given one's understanding of the errors and imprecisions in measurement, the introduction of skew in statistical reconstruction, one's pragmatically chosen error tolerance, and other such factors. An excellent example is provided by the comparison of the observed to the calculated values for the planetary orbital periods and the semi-major axes of the orbits in Book III of the *Principia*, in Newton's claim that the orbits of the planets and of Jupiter's and Saturn's satellites all respectively satisfy Kepler's Harmonic Law. His explicit discussion of the errors involved in constructing the concrete models, primarily due to limitations in the observational instruments, and the errors involved in constructing the individual models, primarily due to idealizations, and why they can all safely be ignored when comparing the data to the theoretically calculated results, exemplifies what I am trying to get at. (See especially Newton's discussions of Phenomenon I, II, and IV, and Harper 2011 for a comprehensive and detailed discussion of Newton's analysis of the data.)

Newton's discussion also exemplifies perhaps the most important feature of such identification, at least for my purposes, *viz.*, the thoroughly and fundamentally pragmatic character of the whole affair. One does not evaluate the truth value of the proposition "all the values are in close enough agreement". One rather chooses to accept (or not) the theoretical structure as an appropriate and adequate representation of the structured data, and one does this for any of a number of reasons, one of which may be that the differences in values one focuses on fall within one's pragmatically chosen error tolerance. The pragmatic nature of the acceptance is shown most clearly by the fact that, even when all the values do *not* fall within one's error tolerance, there may be other principled grounds on which one can reasonably choose to accept the identification of theoretical structure with concrete model, *e.g.*, to serve as the ground of further, more refined investigation. This is what Newton did in his studies of the moon's librations and other irregularities in its motion—he knew his initial, simple theoretical models (2-body interaction ignoring the Sun, circular orbit) were inadequate (in the sense of predictive accuracy) by any reasonable measure, but he relied on them nonetheless in attempting to construct better ones, taking into account ever more finely grained details of the system (*Principia*, Book I, Proposition LXVI, and Book III, Propositions XVII, XXII, and XXV–XXXV; see also Smith 1999a, 1999b)—and he had damn good reason to do so. In such a case, there can be no question of acceptance based on the evaluation of the truth value of a proposition characterizing the accuracy of the data.

With regard to the exact relation between theory and experiment in making such identifications, it is worth noting that one can "Keplerize" the planetary ephemerides without Newton's Laws or any of the other most characteristic theoretical concepts of his system, such as inertial mass and force. One needs only the pre-theoretical ideas of "ellipse", "area swept out in given time", "distance to sun", "orbital period", and so on. One constructs the structured data using these pre-theoretical

reducing the experimental experience, as I shall term it, to a simple entity ready for comparison with a model of the theory.

concepts, and then *shows* that it can be *interpreted* using the concepts of the Newtonian framework in such a way as to support identification of the concrete model with a particular individual model. And indeed that is what Newton did.[53]

To try to reassure the reader that I am not cherry-picking particularly apt examples from the Greatest Hits in the History of Science, I will also briefly discuss the recent detection of gravitational waves by LIGO (Abbott *et al.* (LIGO Scientific Collaboration and Virgo Collaboration) 2016). This example also shows in a clear light where theory enters into the design of the experiment and the construction of structured data, and where it does not. I give only a wildly simplified, schematic description of the way observations are transformed into structured data and compared with theory in the experiment.[54]

1. Design the instruments and their relative configuration so as to be susceptible kinematically to coupling with the kinds of physical phenomena one is interested in, according to the models of one's theories—in this case, differential stress-strains caused by "distortion" of spacetime as gravitational waves pass, in the persona of the relative acceleration of contiguous portions of continuous bodies (the instruments).
2. Arrange the instruments in the desired configuration, fire them up, and begin collecting raw data.
3. In the first pass through raw data, discriminate between "noise" and "signal", throwing out "bad measurements", winnowing signal pollution from other sources, ignoring mistakes and instrument malfunctions, and so on. (This will have to be done at several points later in the process as well.)
4. Match and collate different numerical "data streams" of raw stress-strain (in this case, using time-stamps of the measured stress-strain coming from instruments in geographically separated locations).
5. Perform numerical interpolation, extrapolation, and manipulation (best-fit statistical analysis, *etc.*) on the raw data so as to produce a continuous curve of aggregated stress-strain plotted against time; it is now structured data.[55]

53. My use of "pre-theoretical" here bears fruitful comparison with that of Hempel (2001).

54. I ignore many subtleties, such as the fact that radically different kinds of theoretical models and calculational methods are used to model different parts of the single process of in-spiral, coalescence and ring-down, and also the fact that the stress-strain is determined by the use of Michelson interferometers, and so optical theories are required as well as mechanical theories in the design and performance of the experiment and the structuring of the data. There are many fascinating philosophical problems of methodology and epistemology here. See, for instance, the project being run by Lydia Patton on the problems associated with parameter estimation in LIGO (`https://www.researchgate.net/project/LIGO-and-parameter-estimation`), and her recent paper Patton (2020). See also the current doctoral work of Jamee Elder (Elder 2020), whose PhD thesis explores the whole area with great insight.

55. This description is too simplistic, even at the broad-brush level I am working at: theoretically generated models are partly used already here to separate signal from noise. This, however, is controlled for by residuals testing, *i.e.*, subtracting the best-fit waveform from data, and then comparing the residue to normal detector noise. It is also controlled for by testing for phenomenological deviation, by comparing data with templates generated by analytically deforming templates away from general relativity. In this way, one effectively removes dependence on general relativity from this stage of the process. See Abbott *et al.* (LIGO Scientific Collaboration and Virgo Collaboration) 2016 and Elder (2020) for more thorough discussion.

6. Using the theory of general relativity, construct models of the production of gravitational waves in different phases of the process (in-spiral of a binary black hole system, coalescence, ring-down of the resultant black hole to equilibrium); calculate the waveforms as they would appear when passing through the Earth.
7. Based on those waveforms, construct continuous graphs representing the theoretically predicted responses of the instruments to the stress-strain resulting from them.
8. Compare the structured-data graph with the theoretically constructed graphs.
9. Decide, based on calculation of expected errors, statistical analysis of the rate of expected false positives, and so on, whether the structured-data graph matches one of the theoretical graphs to within one's error tolerance.
10. If the match is good enough, use the physical principles of general relativity to interpret the measured stress-strain in the instruments as the result of passing gravitational waves produced in the coalescence of a binary black hole system.
11. Identify the structured data with an individual model (or family of models) of a coalescing binary black hole system.

One of the most interesting points for our purposes is where the dynamics of general relativity, *viz.*, the Einstein field equation itself, enters: only in steps 6 and 7.[56] Step 1 may appear already to depend on the Einstein field equation, but the appearance is illusory, arising from the description I used. The instrument needs only to be sensitive to stress-strain induced by "gravitational forces", irrespective of how one interprets, conceives or represents such a thing. Of course, one must know how to interpret the relevant notion of "gravitational force" in the conceptual framework of general relativity, and how to represent stress-strain, for all of which the theory's kinematics suffices. In particular, one needs to know only how to measure the relative acceleration of contiguous parts of continuous bodies (induced stress-strain in the instrument), and to ensure that the stress-strain does not arise from any source other than "gravitational" (electromagnetic forces, *e.g.*, or mechanical disturbances arising from vibrations in the Earth's surface caused by passing semi trucks). None of that depends on the assumption (or lack thereof) that the Einstein field equation holds for the matter fields one is working with.

The way that the rest of general relativity comes into play, moreover, in steps 1, 10 and 11, does not make the experiment "theory-laden" in any interesting sense, because it is only the pre-theoretical concepts of stress-strain, relative acceleration, and so on, that ground the construction of the instruments and the structuring and analysis of the data used to test the predictions.[57] The Einstein field equation comes into play only in producing predictions to compare to observed results, and then the rest of the conceptual framework of general relativity comes into play in interpreting the results by the identification of a structured data model with an individual model (or family of models) in the theory.[58]

56. It may also come into play in step 9, in the determination of expected errors, but that discussion is far beyond the scope of this paper.

57. These concepts are pre-theoretical with respect to general relativity, not with respect to the theories (*e.g.*, of the continuum mechanics of solid bodies) that are used to construct detailed models of the instruments themselves and their reactions to imposed forces.

58. At this point, the experiment has not ruled out the possibility that other theories of gravity besides general

I hope it to be clear how all 4 aspects of both types of knowledge (as I sketched them in §6) come into play in inextricable ways throughout the entire process. The construction of the graphs representing theoretical predictions of instrument responses, for example—a necessary part of building the evidentiary web of relations that constitutes the confirmation of general relativity by the experiment—requires input from both experimental and theoretical sources. One cannot, however, point to the "experimental part" and the "theoretical part" of the graphs. Nor can one understand the graphs' empirical import using the formalism of the theory in conjunction with minimal interpretive principles—among other reasons, to do so can give no reason to believe that the graphs have the capacity to represent systems in the theory's regime of applicability.

One may well ask, however: if the only theoretical input to the design of the instruments came from pre-theoretical notions, how can I claim (as I did in §3) that the guidance of instrument design is one of the most profound ways that *theory* and experiment make contact with each other? The answer is twofold. First, even though "stress-strain" as used here is pre-theoretical with respect to general relativity in the sense that one need not use general relativity to characterize it, it is still an essential part of the conceptual framework of general relativity. One must be able to ensure that the way one characterizes stress-strain in general relativity is consistent with, and indeed identifiable with, the pre-theoretic notion one explicitly used in designing the instrument. In that way, the conceptual structure of general relativity, its kinematics, did indeed guide the design of the instrument. Second, the design of the configuration of the experiment, how the instruments are deployed and used, must respect both the theory's kinematics and its dynamics.[59]

Stein (1994, p. 639, emphases his) speaks of "*a mathematical structure [of a theory] discernible in the world of phenomena, of observations, of experience*." I endorse this idea by the requirement that structured data be identifiable with formal theoretical structures. I mean, however, something quite weaker than what many contemporary philosophers would assume on reading Stein's phrase in the absence of any context. I mean only that the theory facilitates the successful identification of individual and concrete models in such a way as to make it possible for its entire formal and conceptual apparatus to come into play and form the basis for further successful reasoning about the systems at issue. In particular, I make no claims that structures in the individual models and in the concrete models, much less "in the world itself", are "isomorphic" or "homomorphic" or "similar" in any way above and beyond the fact that they are relevantly identifiable with each other.[60] (I do not even know what it means to speak of an isomorphism or homomorphism or similarity between a mathematical structure and "the world itself".[61])

relativity might also produce predictions appropriately and adequately matching the structured data. This fact highlights the important point that different theories—*e.g.*, general relativity and many alternative theories of gravity—can share a substantial amount of epistemic content, *viz.*, that embodied in various pre-theoretical concepts and general principles they obey, and that encoded in the structured data formed from observations that can be modeled by and can be used to test theoretical propositions in the different theories. It should thus be clear that I reject the "incommensurability" of theories in any deep or interesting sense.

59. I explicate "kinematics" and "dynamics", and further discuss this point, in §8.

60. See the discussion in the final section of Curiel (2014) for examples of how one can identify the same concrete model with individual models in different theories (the Lagrangian and Hamiltonian formulations, respectively, of the theory of an oscillating spring) without any physically significant morphism between any of the models at issue.

61. Jeremy Butterfield countered this claim in conversation by adverting to work on the correspondence theory of truth, such as Davidson (1969), in which there seems to be a straightforward sense of isomorphism between a formal semantic structure and parts of the world. I can accept the cogency of such work but still deny that the

Indeed, the sense in which there may be any structure in the concrete model isomorphic with structure in the individual model is etiolate at best. In the case of LIGO, the extrapolated continuous curve of stress-strain data is isomorphic with the analogous curve theoretically constructed on the basis of the individual model general relativity yields, but that is only the minimal consistency condition required in order to justify the identification of the two. (Note, moreover, that the theoretical curve "isomorphic" to the data curve is, strictly speaking, not even part of the individual model in the sense of the standard view!) The additional structures in general relativity one needs in order not only to justify the identification but further to give empirical content to the individual model—additional structure such as the relation of the relative acceleration of neighboring non-geodetic curves to the Riemann tensor, *e.g.*—have no correlate, isomorphic or otherwise, in the concrete model. The concrete model has no affine structure.

In any event, the identification is, strictly speaking, not a relation between "different structures" at all, in the sense of being representable by a proposition whose truth value one can evaluate based only on consideration of theory and data alone—it is, as I have already said, a pragmatic choice: does one accept that the two can be identified with each other or not? It is a pragmatic choice in the same way as use of a framework in the sense of Carnap (1956) is decided by pragmatic choice. This is why pragmatics and epistemological concerns, not ontology, lie at the ground of semantics, as I will argue in §9 below. Nonetheless, I shall continue to speak of this identification as a "representation relation" for convenience, though I emphasize again that by "representation" I do not mean a relation whose content can be exhausted by reference to the theory and data alone, nor even by reference to theory, data and theory-users (*à la* van Fraassen)—it is a relation that relies on schematizing the observer.

One can agree with this analysis of how theory and experiment make contact with each other while remaining agnostic about all issues pertaining to realism and anti-realism. Indeed, none of this has anything to do with any issue pertaining to structural realism in particular, nor to debates about realism and anti-realism in general. There is no claim made or needed that the structure manifest in the structured data is "really" part of the furniture of the world, in some deep metaphysical sense. That it is manifest in the structured data suffices for the soundness of the representational relations at issue. Those relations are agnostic about realism, as any good epistemic relation should be.

I conclude this section with a summation of where in all this the schematic representation of the observer enters. In the case of Newton's investigations, they appear explicitly in his defense of the way he identifies his theory's models with the Keplerized orbits, by explaining the way, *e.g.*, one expects variation in the observed size of Jupiter's apparent diameter, and thus in the

isomorphism at issue consists of anything more than the kind of bare, thin agreement between formal semantic structures and parts of the world captured by the identification of concrete models and individual models in my sense. For the claim that there is a cogent sense of isomorphism between a mathematical structure and the world to be true in a substantive and interesting sense, I would want to see an example like the following: there is an isomorphism between the (standard) topology of $\mathbb{R}^4$, as used in flat FLRW cosmological models in general relativity, and the topology of the real world. I do not know, however, what "the topology of the real world" means. I can certainly envisage ways to operationalize its meaning, but different operationalizations, each with manifest physical content, will yield different topologies. One obvious way will yield the standard manifold topology (at least locally). Another, however, based on time functions, will yield a different topology (Geroch 1970). So which one is "the" topology of "the world itself"?

distance its satellites appear to be from its center, when using telescopes of different sizes and resolving powers (Book III, Phenomenon I). In the case of LIGO, they appear in several places, perhaps most notably for our purposes in the calculation of the response of the instrument to applied stress-strain from passing gravitational waves. It is true that often, if not indeed almost always, explicit representations of the observer and the instruments will not appear in the concrete models—they get washed out in the structuring of the data. They are still there implicitly in the background, however, and the eventual identification of individual and concrete model, including the interpretation of the pre-theoretical terms in the structured data using the concepts of the full theory, can happen only because of them.

At a deeper level of analysis, schematic representation of the observer is what grounds the circumscription of the regime of applicability of a theory, because different kinds of instruments, naively modeled and deployed, give different outcomes for "measuring the same thing" near the boundaries of the regime. Circumscribing the regime, however, grounds all other aspects of what one may want to call the semantics of a theory in particular and its epistemic content more generally, so it is to this problem that I now turn.

8 The Breakdown Scale and the Regimes of a Theory

This shaking keeps me steady. I should know.
What falls away is always. And is near.
I wake to sleep, and take my waking slow.
I learn by going where I have to go.

– Theodore Roethke
"The Waking"

It is characteristic of an appropriately unified kind of physical system, one treated by a theory in my sense, that there exist a set of scales at each of which all quantities the theory attributes to the kind of system simultaneously lose definition. In other words, every theory, in so afar as it treats an appropriately unified kind of physical system, not only has a regime of applicability, but it has a *single, unified* one, bounded on all sides by scales characterized by the values of different combinations of its physical quantities. For classical fluids, for example, the definitions of their pressure, fluid flow, viscosity, and all the rest break down at spatial and temporal scales a few orders of magnitude greater than those of the mean free-path of the fluid's constituent particles. All those quantities also lose definition when the fluid enters a strong enough state of turbulence, which can be characterized by (*inter alia*) a ratio of the fluid's kinetic energy to a measure of its viscous damping—a scale independent of the small spatial and temporal ones characterized by the mean free-path. There is no *a priori* reason why the definitions of all the different physical quantities represented by the theory should fail at the same set of characteristic scales, even though, in fact, those of all known examples do, not only for theories of classical fluids but for all physical theories we have.[62] This seems, indeed, to be one of the markers of a physical

62. This is a fact that deserves philosophical investigation. I discuss it in more detail in Curiel (2017b), but with no pretense of even touching on all the interesting questions this fact raises.

theory, the existence of a set of characteristic scales for its physical quantities, at each of which all the theory's physical quantities simultaneously lose definition—"places" where all the kinematically and dynamically relevant structures of the theory break down all at once, in the sense that the theory becomes inadequate for an appropriate treatment of any system beyond the determined boundaries.[63] Because all the quantities a theory attributes to the systems it treats lose definition at the same time when the system passes beyond a breakdown scale, the idea is unambiguous.

Although we perhaps naively tend to think of scales determined by spatial, temporal and energetic quantities when considering how and where theories break down in their capacity to provide sound representations of phenomena, any quantity in any theory can provide such a measure. Velocity provides a breakdown scale for Newtonian mechanics, and acceleration and scalar curvature provide different breakdown scales for various theories of gravity, such as general relativity. No breakdown scale, moreover, can be a single number holding for all systems the theory treats. Navier-Stokes theory, for instance, becomes inadequate for different fluids at different energies and spatial and temporal scales. Often it is not a bound on a single quantity, such as a value of energy, a value of spatial length, *etc.*: classical Maxwell theory, *e.g.*, breaks down when the ratio of the field's amplitude to its frequency approaches $\hbar$. Nor is it ever the case that there is a single characteristic scale for each theory. Navier-Stokes theory breaks down when various measures of flow complexity indicate the fluid is approaching turbulence, when the fluid is too viscous, when one tries to use the theory to specify behavior at time scales comparable to equilibration time after a sharp disturbance, when temperatures become large enough that heat loss due to emission of blackbody radiation becomes non-neglible, when the ambient electromagnetic field in the environment becomes strong enough that the constituent molecules of the fluid begin to denature and ionize, and on and on—which shows, moreover, that sometimes a breakdown scale is determined by physical quantities not even representable in the theory (such as the electromagnetic field for Navier-Stokes). A breakdown scale, then, is something like the following: a measure of or function of or relation among quantities, such that, when the joint state of the system and its environment imply that the values of some of the system's quantities do not satisfy the measure, function or relation, then the theory can no longer provide good models of the system.[64] (I provide a more precise definition below.)

Breakdown scales can never be determined by analysis of the formalism and theoretical machinery of the theory alone, without input from knowledge acquired by experimentation in particular and empirical investigation in general. They are rather fixed by knowledge that one can gather

63.

Und wir: Zuschauer, immer, überall,
dem allen zugewandt und nie hinaus!
Uns überfüllts. Wir ordnens. Es zerfällt.
Wir ordnens wieder und zerfallen selbst.

– Rainer Maria Rilke
"Eighth Duino Elegy"

64. I gloss over a subtlety here: sometimes approximations used to construct models of particular phenomena, such as surface waves in fluid dynamics, have characteristic breakdown scales different from those of the material in which the phenomena manifest (Lamb 1932, ch. ix). The theory can provide appropriate and adequate models of the systems in the relevant states, only not in the way the approximations definitive of the phenomena at issue require.

only from investigations grounded in that part of the epistemic content of the theory not captured by the formalism by itself. As such, they change with the increasing scope and depth of our experimental reach.[65] Every theory has many breakdown scales, putting constraints on the values of all the quantities the theory treats. For simplicity, however, I shall speak as though all such scales are spatial or temporal.

What does it mean to say that the theory cannot provide good models of systems outside its breakdown scales? One of the most important markers of this is that the quantities the theory attributes to the system lose unambiguous definition. For a Navier-Stokes fluid, for example, different sorts of thermometers that allow spatial discrimination on scales only a couple of orders of magnitude greater than the mean free path of the fluid's molecules will record markedly different "temperatures" depending on characteristics of the joint system that one can ignore at larger scales—the fine details of the fluid's convective flow in relation to the geometry of the thermometric system, for instance, and even the transparency of each thermometer to the fluid's particles.[66] As the fluid approaches turbulence, to take another example, the values of all its quantities begin to vary rapidly in time and eventually cannot be measured by any conventional means—the quantities are no longer well defined. In practice, all that can be measured are averages (usually time averages), leading to the application of the idea of ensemble averages and the development of statistical generalizations of the Navier-Stokes equations to treat the fluid flow (Foias et al. 2001).

This discussion shows how one cannot even define physical quantities—*e.g.*, temperature—without explicit schematic representation of the observer, much less have understanding of how to employ their representations in scientific reasoning in ways that respect the regime of applicability. Without such a definition and such understanding, however, any theoretical proposition referring to temperature can have no epistemic content. The ideal of segregability enshrined in the standard view is illusory.

In order to draw out the full import of the existence of breakdown scales for the characterization of the epistemic content of a theory, however, a few more definitions and distinctions must be

65. This is not to say that one can never find relations among quantities derived from purely theoretical considerations that determine *some part* of the breakdown regime. Geroch (2001, pp. 2–4) points out, for example, that any relativistic theory of dissipative fluids—such as Navier-Stokes theory posed in the relativistic context—must fail on every combined time-distance scale τ-d such that $d^2 < \eta\tau$ and $d > c\tau$, where η is a value of a typical Navier-Stokes dissipation coefficient and c is the speed of light. Instead of a characteristic break-down scale, this requirement defines a characteristic break-down *area* in the τ-d-plane. Note that the complement of this region in the plane, that is, the region in which the system remains valid (at least so far as these conditions are concerned), includes arbitrarily small d-values and arbitrarily small τ-values (though not both at the same time). This all follows from considerations of relativistic constraints on causal propagation alone. Such relations, however, never exhaust the boundary of the complete breakdown scale, and in general form only a small part it. The determination of whether any given system in any given state satisfies such relations, moreover, is still sensitive to the state of experimental prowess—we can always improve the accuracy and precision of our measurements of τ, d, c and η.

66. See, *e.g.*, Benedict (1969) for detailed exposition of the complex interplay among theory, model and experiment one must take account of in attempting to define a physical quantity such as temperature based on the behavior of real measuring devices, that is, to render epistemic content to any representation of it, and to deploy it meaningfully in real applications of theory. It is not the most up-to-date reference with regard to the international agreement on defining the standard methods for the determination of temperature (for which see, *e.g.*, Haynes 2014), but I have found no better guide to the nuts and bolts of thermometry when it comes to developing in a physically illuminating way how the mathematical models and the experimental techniques bear on each other.

made.[67] The most basic distinction is between what I call the kinematical and the dynamical parts of a theory. The kinematical parts, roughly speaking, are all those features of a theory's treatment of a system that remain the same irrespective of the state the system is in and irrespective of the kinds of interaction it has with its environment. The kinematic quantities are, therefore, those which are assumed to be constant. In relativity theory, this includes the speed of light *in vacuo*. In Newtonian particle mechanics, the masses of the particles are assumed to be fixed once and for all. In Navier-Stokes theory, the fluid's shear and bulk viscosities and its coefficient of thermoconductivity are assumed to be constant. The dynamic quantities are those that are allowed to vary as the system evolves, such as position, velocity, momentum, shear-stress, and so on.

All theories, moreover, attribute not only constant quantities to its systems, but also fixed mathematical relations among both its kinematic and dynamic quantities whose form is always the same, what I call its kinematical constraints. In Newtonian mechanics, the velocity is always the time-derivative of position. (This is not so trivial a requirement as it may sound, as there are theories in which it fails, *e.g.*, in some formulations of relativistic fluid mechanics; see Landau and Lifschitz 1975 and Earman 1978.) Navier-Stokes theory requires that the shear-stress tensor be symmetric, and that heat flux be independent of the pressure gradient. The attribution of definite values for the kinematic quantities to a species of system also is a kinematical constraint, *e.g.*, that water under "normal" conditions have the value (approximately) 1000 `µPa-s` (micro Pascal-seconds) for its shear viscosity.

Every theory postulates as well dynamical relations among its quantities, the more familiar equations of motion or field equations. These have the characteristic property that their specific form depends on the interactions the system has with its environment. ($\mathbf{F}$ always equals $m\mathbf{a}$ in Newtonian mechanics, but the functional form of $\mathbf{F}$ may be anything one wants, depending on what forces the system at issue is subject to.)

Now, to be able to bring theory and experiment into physically significant contact by having the capacity to identify individual and concrete models, one must demonstrate that the structures intrinsic to the theory are appropriate and adequate for representing and reasoning about the genus of physical systems the theory purports to treat. I am now in a position to give (somewhat) precise definitions for these terms. The theory is *appropriate* (or has *propriety in representation*) for treating a given system if all the quantities the theory attributes to a system are well defined and they jointly satisfy all the theory's kinematical constraints, given the state of the system in conjunction with the interactions it has with its environment. Thus, a theory can be used with propriety to treat a type of physical system it putatively represents if and only if the system's environment and its own state jointly permit the determination, within the fineness and ranges allowed by their nature, of the system's quantities over the variously relevant scales appropriate for the representation of the relations among the quantities manifested in the phenomena at issue.

Propriety is a property accruing to the representation of individual states, not to the course of a dynamical evolution: one may not be able to identify an entire concrete model with an entire individual model, but, if one is to apply the theory in a meaningful way at all to the treatment of a system, at a minimum one must be able to identify those substructures of each that refer only to single states. Since the kinematical constraints are the same for all states, it makes sense to

67. See Curiel (2017a) for a more comprehensive and detailed discussion of all the issues I treat here.

ask whether they are satisfied so long as the quantities themselves are well defined. Indeed, I now impose a stronger criterion for the presence of a breakdown scale, in the form of a definition.

Definition 8.1 *A* breakdown scale *of a theory is a bound on or function of or relation among its quantities such that the kinematical constraints are satisfied to the required degree of accuracy given the experimental techniques used for probing them if and only if the joint state of the system and its environment imply that the bound or function or relation is satisfied.*

If the kinematical constraints are not satisfied, one has no reason to think that the system at issue is one the theory can treat at all.

It is a brute fact about the actual physical theories we use in real science that they become predictively inaccurate in regimes well separated from all its breakdown scales, *i.e.*, well before the theory's kinematical constraints are no longer satisfied. The theory is *adequate* if its dynamical relations are satisfied to some prescribed degree of accuracy, given the experimental techniques used for measuring it. Thus, a theory is adequate if and only if entire concrete models can be identified with entire individual models. If the theory does not have propriety in representation, it cannot be adequate. It is only now, when we have reached the point when it makes sense to inquire after a theory's adequacy, that the issue of the accuracy of the theory's *predictions*—their "truth"—becomes meaningful.

This discussion suggests the following.

Definition 8.2 *The* regime of propriety *of a theory is the family of physical systems whose states, in conjunction with their interactions with their environments, are bounded on all sides, in all ways, by all the breakdown scales of the theory.*

In this regime, the theory's representational resources are appropriate for modeling the kinds of system at issue, even if they are not predictively accurate. In particular, all quantities are well defined, and all kinematical constraints are satisfied. A system is in the theory's regime of propriety if and only if one can identify those parts of an individual model representing a single state with those parts of a concrete model representing the same.

Definition 8.3 *The* regime of applicability *of a theory is a subset of the regime of propriety, in which the theory's dynamical equations are predictively accurate in their modeling of the behavior of the family of systems at issue—the theory model's are adequate for ordinary scientific usage.*

A system is in the regime of applicability—it is adequate—if and only if one can identify an entire individual model with an entire concrete model.[68]

A theory may appropriately treat a family of phenomena even when it does not model the dynamical behavior of all members of the family to any prescribed degree of accuracy, *i.e.*, even when the equations of motion are not satisfied in any reasonable sense. A theory, that is to say, can and does tell us much about the character and nature of physical systems for which it does not give accurate representations so long as they are in its regime of propriety—systems, in other

68. Identifying the regimes in this way with "the family of all systems such that..." does not fall prey to the problems the standard view faces when it attempts to do something similar: humans will never be able to identify the entirety of the regime of propriety of any theory in practice, but we can determine whether any given system belongs to it or not. That is what the standard view fails to do, blinkered by its own methodological restrictions.

words, it cannot soundly represent in totality, cannot be true of, and so systems that, according to all the standard contemporary accounts of theory structure and semantics, the theory should have nothing to say about at all. I shall now discuss four roles that propriety, even in the absence of adequacy, plays in informing and contributing to the epistemic content of a theory.

Fisrt, theories do not predict kinematical constraints; they demand them. I take a prediction to be something that a theory, while appropriately modeling a system, can still get wrong. Newtonian mechanics does not predict that the velocity of a body equal the temporal rate of change of its position; rather it requires it as a precondition for its own applicability. It can't "get it wrong". If the kinematical constraints demanded by a theory do not hold for a family of phenomena, that theory cannot treat it, for the system is of a type beyond the theory's scope. By contrast, if the equations of motion are not satisfied, that may tell one only that one has not taken all ambient forces on the system (couplings with its environment) into account; it need not imply that one is dealing with a different type of system. Even in principle, one can never decisively rule out the possibility that the equations of motion are inaccurate only because there is a force one does not know how to account for, not because the system is not appropriately treated by those equations of motion. This can never happen with a kinematical constraint. It is either satisfied, to the appropriate and required level of accuracy given the measuring techniques available and the state of the system and its environment, or it is not. Thus, the propriety of a theory constitutes its necessary preconditions of applicability.[69]

Indeed, satisfaction of kinematical constraints is required in general for the equations of motion of a theory to be well posed or even just consistent (the second role for kinematical constraints). The initial-value formulation of the Navier-Stokes equations, for example, is well set (in the sense of Hadamard) only if the shear-stress tensor is symmetric, and it is thermodynamically consistent only if the heat flux is independent of the pressure gradient (Landau and Lifschitz 1975, ch. V, §49). One cannot even formulate Newton's Second Law if velocity is not the first temporal derivative of position. More generally, in a sense one can make precise (Curiel 2014), if the kinematical constraints of Lagrangian mechanics are not satisfied ($\mathbf{v} = \dot{\mathbf{q}}$), then one cannot formulate the Euler-Lagrange equation; and similarly, if the kinematical constraints of Hamiltonian mechanics are not satisfied (the $\mathbf{p}$s and $\mathbf{q}$s do not satisfy the canonical Poisson-bracket relations), then one cannot formulate Hamilton's equation (Curiel 2014). Thus satisfaction of the kinematical constraints is required as a precondition for the appropriate application of a theory in modeling a kind of system.

Third, it is essential that all the kinematical constraints be satisfied for the theory to be able to represent any aspects of the system at all, for it is those kinematical constraints, not the equations of motion, that characterize the genus of system at issue. Many systems have, *e.g.*, shear-stress (Navier-Stokes fluids, elastic continua, Maxwell fields, charged plasmas, *etc.*); what makes a shear-stress the shear-stress of a Navier-Stokes fluid as opposed to that of a Maxwell field is its satisfaction of the Navier-Stokes kinematical constraints, *e.g.*, that the shear-stress be, in a sense one can make

69. In this sense, the kinematical constraints play a role analogous to that proposed by Reichenbach (1965) and Friedman (2001) for what they call the relativized *a priori* of a theory. Kinematical constraints, however, differ in this respect: they are part of the theory itself, not supra-theoretical principles, as are the relativized *a priori* of the neo-Kantians. Also, satisfaction of the kinematical constraints can, indeed must, be *experimentally* verified in order for one to ascertain that the theory represents the system with propriety. To the contrary, satisfaction of the relativized *a priori*, in the neo-Kantian sense, does not admit of empirical verification, but rather grounds the possibility of empirical investigation in a stronger sense.

precise, transverse to the fluid flow. By contrast, you can't throw a rock without hitting a system whose equations of motion are that of a simple harmonic oscillator: if I know only the equations of motion, I cannot in general tell you what kind of system I am dealing with.[70]

Finally, it is the kinematical constraints, not the equations of motion, that guide the experimentalist in the design of instruments for probing and measuring the quantities the theory attributes to the systems it treats. (Recall the general discussion of this issue in §3, and the analysis of this issue in the particular case of LIGO in §7.) An instrument that is to measure Newtonian velocity, for instance, must be sensitive to differences in spatial location at ever smaller measured temporal intervals, even if only indirectly, in accord with the kinematical constraint $\dot{\mathbf{x}} = \mathbf{v}$. Such an instrument, if well designed, does not care about how the system accelerates, *i.e.*, about its dynamics. Similarly, an instrument that would measure shear-stress of a Navier-Stokes fluid must conform to the equality of pressure and reversed sense of shear across imaginary surfaces in fluid that is represented by the symmetry of the shear-stress tensor. Again, the instrument need not care at all about the dynamics of the fluid for it to measure the instantaneous value of the quantity. In this way, kinematical constraints provide the foundation for the operationalization of the meaning of theoretical terms.

There is one more subtlety in the idea of the regimes of a theory that must be addressed. It is in fact never the case that an individual model of a given physical system represents the system appropriately and adequately *in toto*. There is always a sense in which some "parts" of the model represent nothing physical at all. Consider again individual solutions to the Navier-Stokes equations. Because Navier-Stokes is a continuum theory, the solutions to its equations allow one to make, indeed they *necessarily encode*, what seem to be physical predictions at arbitrarily small spatial and temporal scales. We know, however, that the theory is not appropriate for that task, and we know we can ignore those "predictions" of the theory in assessing its adequacy and soundness, for real fluids are not continua. General relativity is scaleless—one cannot formally distinguish a model of a Schwarzschild black hole of radius $44\mathtt{x}10^{-10^{39876}}$ `km` from one of radius 44 million `km`. And yet we also know that the theory is appropriate for treating the latter, not the former. Indeed, even in the case of a Schwarzschild black hole of radius 44 million `km`, which does find appropriate and adequate representation in the theory, not all "parts" of the individual model represent. One finds again the same circumstance as in Navier-Stokes theory: we have good reason not to trust "predictions" general relativity makes in such models about the geometry of spacetime at spatiotemporal scales approaching the Planck scale. Propositions about geometry at such scales formulated in the model have no empirical content. That in no way detracts from the sound epistemic content the model accrues by the propositions it allows one to formulate about geometry at much larger scales.

One comes to recognize and understand these limitations in the representational capacities of theories only by knowledge of the breakdown scales, and correlatively of the regime of propriety. Thus, the knowledge of how the mathematics is to be applied—how it represents and what it represents—is not segregable from the practical knowledge that grounds the determinations of the scales and regimes. This is not a matter of "segregating part of the mathematical structure as representational fluff ('gauge')"—for there just is the solution itself. The fact that one cannot

70. Thus, kinematical constraints are constitutive of the systems the theory treats, also in a way analogous to the relativized *a priori* of the neo-Kantians.

use it to model phenomena happening on small spatiotemporal scales does not mean that "part" of the solution—"that part representing small stuff"—is gauge, for there is no such part cleanly segregable from the rest. It is not like the case of the Faraday tensor and one of its 4-dimensional gauge potentials, two different mathematical entities with a fixed relation between them. And it is not that there is a simple rule, *e.g.*, "ignore everything in cubes whose edge is smaller than the mean free-path of the fluid's constituents", because such rules will in general be incorrect. Sometimes what happens on such scales is physically relevant (a discontinuity in a boundary condition, say, or wild small-scale fluctuations that render coarse-grained statistical averages meaningless). One has to use one's judgment, on a case by case basis, to determine how the formalism encodes potential knowledge, how it successfully represents parts of the world, how it can be used as part of sound, legitimate reasoning.[71]

To hark back to the discussion, in §6, of the different forms and aspects of scientific knowledge, I argued there that that the standard view of theories gives one too little in answer to the question of a theory's epistemic content. Here, by contrast, is a case in which the standard view tries to answer too many questions, tries to embody too much knowledge in Stein's sense (a)—simple, factual knowledge—in so far as it allows one to formulate propositions that can have no empirical content *even about systems falling in the theory's regime of applicability.*

This characterization of the regime of propriety and the regime of applicability makes explicit the ways that schematizing the observer is required for a theoretical structures to make substantive contact with experimentation, and so for a theory as a whole to have non-trivial epistemic content.

9 Semantics Is Epistemology, Not Ontology

The conclusions of my discussion of breakdown scales and the regimes of a theory have a deep consequence for how meaning and truth may be related in an adequate semantics for theories. According to the kind of semantics that naturally accompanies the standard view of theories, all of a theory's propositions must be (at least approximately) true of a system in order for the theory to represent it. By my arguments, however, the meanings of a theory's theoretical terms must be fixed by the epistemic content associated with the regime of propriety, not the regime of applicability, *i.e.*, in part by situations in which *not* all of a theory's propositions about the system are true—all those treating the dynamics of the system, all the assertions standardly called "predictions", are false. The meanings of the theory's propositions, therefore, cannot be fixed solely by their truth-conditions in any standard sense—the seductive intuition grounding essentially all contemporary thought on the semantics of scientific theories, as Carnap (1942, ch. B, §7, p. 22) concisely expresses it: "... to understand a sentence, to know what is asserted by it, is the same as to know under what conditions it would be true." One cannot even begin to investigate what the truth conditions of some of the sentences in the theory may be, however—in particular, its

71. This problem underscores the shallowness of much of the debate about realism in science, in particular the almost ubiquitous yet deeply problematic implicit assumption that "realism" is a holistic attitude—a mathematical structure *in toto* "really represents" or it does not. That, however, is too coarse a conception. Some aspects of a mathematical structure may represent, and others not, *e.g.*, the integrated phase differences in a quantum wave-function (Berry phase) may be "real" whereas the phase itself may not, or those "parts" of a Navier-Stokes model representing behavior on scales greater than $10^{-4}\texttt{cm}$ may "really designate" whereas those parts representing smaller scale behavior may not. Halvorson (2019) provides a splendid discussion of related matters.

dynamical predictions—until one already knows enough about the meaning of its terms to ascertain the truth of some of its other sentences, *viz.*, the kinematical constraints, that guarantee that the system at issue falls within the regime of propriety.

One can think of propriety in representation, in part, therefore, as what a theory requires for it to have the capacity to produce propositions whose truth-value can be investigated—not a fixing of truth conditions, but rather the securing of the possibility to investigate whether or how truth-conditions for a given proposition can be determined in the first place. A theory does not possess even the capacity to be accurate or inaccurate in its treatment of a family of phenomena if it does not represent the phenomena with propriety. It follows that one can not even entertain questions about the truth of many sorts of propositions—most of all those depending on the identification of individual models with concrete models—until one has determined that the theory has the resources, both practical and formal, to represent the system at issue with propriety, *i.e.*, until meaning already has accrued to the formal structures of the theory. The fact that the regime of propriety is strictly larger than the regime of applicability, therefore, shows that the fundamental idea of semantics should rather be: to understand a sentence, to know what is asserted by it, is the same as to know under what conditions its constituent terms can be assigned meaning and so allow one to investigate the possibility of ascertaining the truth conditions of the sentence.

Indeed, I believe that all the problems I have discussed with the standard view—in particular, its lack of accounting for all kinds of knowledge, and its incapacity to handle the breakdown of models—boil down to its implicit reliance on something like a truth-conditional semantics, in conjunction with designation as the fundamental semantic relation: semantics is fixed by ontology. This is what the assumed clean segregation of formal from practical spheres of knowledge amounts to in current philosophical work, and, conversely, that clean segregation is implied by such a semantics.[72]

More precisely, a view about the structure and semantics of physical theory based ultimately on ontology, grounded in the assumption of a clean split between the parts of the epistemic content of a theory captured by its formalism and all other parts, is inadequate for (at least) two reasons. First, it does not allow us, within the scope of the theory itself, to understand why models of systems in the regime of propriety but not in the regime of applicability are not accurate even though all the quantities the theory attributes to the system are well defined and the values of those quantities jointly satisfy all kinematical constraints the theory requires. Second, we miss something fundamental about the meaning of various theoretical terms by rejecting such models out of hand merely on the grounds of their inaccuracy. It is part of the semantics of the term 'hydrostatic pressure', *e.g.*, that its definition as a physical quantity treated by classical fluid mechanics breaks down when the fluid approaches turbulence closely enough; because, however, the theory's equations of motion stop being accurate long before, in a precise sense, the quantity loses definition in the theory and long before the kinematical constraints of the theory stop being satisfied, any account of the structure of theories and their semantics that rejects the inaccurate

72. My reliance on the formally characterized distinction between kinematical and dynamical structures (§8) is not inconsistent with my contention that a good semantics allows no clean segregation of one part of epistemic content captured by the formalism from another part that is not. It may still be the case—and in fact is the case—that different parts of the formalism play different roles in the analysis of the epistemic content, indeed in characterizing the integrity of epistemic content itself, not cleanly separable into formal and non-formal parts.

models in which the term still is well defined will not be able to account for that part of the term's meaning. Thus, an adequate account of physical theory must be grounded on notions derived from relations in some sense prior to the theory's representations of the dynamical behavior of the physical systems it treats, relations that govern the propriety of the theory's representational resources for modeling the system at issue. These are the theory's kinematical constraints.

One may think that this discussion based on an analysis of how, where and when theories break down more properly belongs to pragmatics (in the sense of semiotic theory) than to semantics. That is not so. A system of formal semantics that would ground itself in the family of possible physical systems for which it provides sound models cannot even get started until that family is demarcated. But that is to require an investigation of the boundary of the theory's regime of propriety, which is thus logically and conceptually prior to any such system of semantics.[73]

According to a semantics that requires predictive accuracy, such as a Tarskian one and in general almost every one conforming to the standard view, the idea of the regime of propriety is meaningless. Such a semantics cannot explain or even accommodate this fact about our theories, for *predictively inaccurate models cannot be Tarskian models or possible worlds or objects in the category of solutions to the equations of motion of the theory.* Semantics founded on the standard view, therefore, does not exhaust the representational capacity of the theory, and the theory gains non-trivial semantic content from everything it can significantly represent, whether in all accuracy

73. Jeremy Butterfield has suggested to me that an intensional semantics of the sort, *e.g.*, that Lewis (1970a) propounds—which manifestly lends itself to the articulation of a semantics for theories in conformity with the standard view—can handle these pragmatic issues as part of the formal semantics itself, in so far as such pragmatic issues are encoded in the intensions. I do not think that is right. Such a semantics would evaluate a proposition in Navier-Stokes theory purporting to describe the behavior of a fluid at spatial scales of 10^{-100} $\mathtt{cm}$ as false. Such propositions are not false, however. They are meaningless. None of the quantities Navier-Stokes theory attributes to systems are well defined at such scales, so no "proposition" purportedly referring to them can have meaning. It is folly to require that every well formed formula constructible in the terms of the formalism of a physical theory and derivable from its formal principles must have a determinate truth value, much more a meaning. If one could re-tool such a scheme of intensional semantics so that its domain is not "all possible worlds" (or "all possible models" in some other appropriate sense), however, but rather a pragmatically characterized regime of propriety, then it might in fact be a useful tool for trying to define a semantics grounded on propriety in representation. Even if one were to do so, however, one should still recognize that all the heavy lifting is performed by what are commonly considered to be the pragmatic elements of the intension, not by the determined relation of designation. Indeed, those pragmatic elements must be investigated and fixed *before* one can define designation: they are logically and conceptually prior to designation. So one ought also ask: what work does the designation do? What is its cash value (to echo William James)? For the meaning of the terms is, by my lights, already in place once one has succeeded in determining the pragmatic elements that govern proper use—*useful* use—and so, *a fortiori*, govern the understanding that the proper use of the language can foster. Nonetheless, I do endorse the teamwork picture of Lewis (1975)—that the 2 approaches to semantics (Gricean pragmatic versus formal language) are complementary, not contradictory; the problem today is to convince people on each side of the divide to reach out and work with those on the other side, and not to focus exclusively on one's own approach in studies and applications. I do not, however, fully endorse Lewis's conclusion (p. 35) that *both* ingredients are essential. The Gricean/pragmatic approach seems to me essential; the formal approach seems to me useful. The real problem with philosophers like Lewis, and the way they adhere to the standard view of theories, is that they believe more in their non-empirical theory of semantics then they believe in (*e.g.*) quantum mechanics. (See, for instance, the dismissive remarks in the preface to Lewis 1986 about the reliability of quantum mechanics as a guide for trying to understand the world.) The referential relation of word to world required by Lewis's picture and others like his (in order to avoid, *inter alia*, Putnam's 1977 permutation argument) is not grounded in any empirical fact, cannot be exposed or even only studied by any empirical investigation, and yet Lewis believes more firmly in *that* than in the deliverances of physics. I say (to echo Anscombe): I cannot understand such a man.

or not. The set of possible worlds picked out by satisfaction of the equations of motion is not a rich enough family of worlds to express or encode all the information the theory can give us about the possibilities of the actual physical world. The theory tells us more about physical quantities like pressure in the actual world than there would be to learn about it in a world the theory would be true of, in the standard sense.

Thus, the regime of propriety must be included as part of the theory's semantics—at least so far as a real semantics of a real physical theory goes, not just a formal semantics of a formal theory: if I don't know the family of actual and physically (not mathematically) possible systems the theory applies to, I don't, by the lights of the standard view itself, know the semantics; but if I don't know the regime of propriety, I don't know that family; and nothing in the formalism of the theory itself can tell me the regime—I cannot fix the "ontology" by reasoning grounded on a clear segregation of the theory's epistemic content into one part captured by the formalism and "interpretive postulates" and another part corresponding only to practice.

A semantics grounded on pure ontology assumes a kind of Fichtean direct intellectual grasp of the world by our representational systems: the posited relation between our symbolic systems and the "objects in the world" (irrespective of whether one is a realist about such things or not) is unmediated by the actual state of our knowledge and by the practices and techniques we have for probing, experimenting on, and more generally investigating the world and evaluating the results of those investigations.[74] Have we learned nothing from Kant? What sense is there in trying to articulate "truth conditions" that are forever beyond our cognitive grasp? What was wanted was an account of "meaning" analytically connected to truth, completely divorced from human concerns. But this is self-defeating, for such a conception *eo ipso* completely divorces, unbridgeably separates, semantics from the *fundamental sources* of scientific knowledge—experimental knowledge—which in the end must ground the empirical content and significance of our theoretical representations. Relations of "direct designation" serve—can serve—no philosophical or foundational purpose. They can tell us nothing about meaning.[75] They are vacuous chicanery, nonsensical will-o-the-wisps leading us to a morass of philosophical quicksand into which we hopelessly sink, suffocating on our own confusion.

These criticisms assume a link between semantics and knowledge in all its human forms, as

74. These criticims do not apply to the use of any Tarskian-like semantics in logic and mathematics, where one cannot cleanly and unambiguously segregate our symbolic systems from the objects they purport to represent; or, at least, the kind of access we may have there to such objects is mediated only by the symbolic systems, not by experimental practice.

75. Stein (1989, p. 50, emphases his) puts the point forcefully, when he imagines Kant posing the following dilemma to Leibniz's ghost:

> How can you *know* that things are as you say they are? If the claimed "reference" of the theory is something beyond its correctness and adequacy in representing phenomena – if, that is, for a given theory, which (we may suppose) does represent phenomena correctly and adequately, there are still two possibilities: (a) that it is (moreover) *true*, and (b) that it is (nonetheless) *false* – then how in the world could we ever tell what the actual case is?

Stein talks here of representing phenomena "correctly and adequately". His distinction, as I understand it, is related to my own "appropriately and adequately" in interesting ways. For Stein, "correctly" is something like my "appropriately and accurately", and "adequately" is something like covering a large enough extent to think one has latched onto *general* features of the world, not just something peculiar to the idiosyncracy of *this* kind of system studied by *this* kind of experiment. This kind of comprehension in coverage is a fundamental part of the epistemic content of a good theory, but it would take us too far afield to discuss it here.

explicated and discussed in §6. I think this must be right, that there must be such a link between semantics and real human knowledge. A semantics divorced from our actual state of knowledge (as achieved state, as provider of evidentiary relations for epistemic warrant, as guide to future investigation), and from the ways we have of improving our epistemic state, is useless.

We must expel from philosophy the myth of a "human-free" semantics. *All* aspects of scientific knowledge, not just the "ontological", should be reflected to at least some degree in a theory's semantic content, and most of all that knowledge ultimately grounding the semantic content—and that is indubitably, inextricably, inevitably *experimental* in large part. This view may entail a blurring of the lines between the traditional conception of semantics and pragmatics (in the sense of semiotic), but that, I think, is all to the good, since the traditional notions are (*pace* Carnap, Suppes, *et al.*) appropriate for mathematics, not the empirical sciences. In any event, I am not convinced that one needs to lose a sharp distinction between semantics in a formal sense and pragmatics in order to ground the kind of view I advocate—one needs only to characterize semantics in a way that is not wholly "ontological", based on a primitive relation of designation.[76]

As I have argued, in order to know how to investigate whether or not a theory provides an accurate representation of a system—whether that system falls in the theory's regime of applicability—one must be able to verify first whether or not the system satisfies the theory's kinematical constraints, *i.e.*, whether or not it falls in the theory's regime of propriety. Our problem, therefore, is to move in a principled way from an understanding of how to verify whether or not a theory's kinematical constraints are satisfied (which generically involves including schematized representations of the observer in experimental models) to an understanding of how to use the resources of the theory to represent a system once we have verified the kinematical constraints are satisfied. Because, as I argued, the identification of (parts) of a concrete model of a system with (parts) of the formal structures of a theory is a pragmatic choice, the problem is to find justifications for those choices in the success and fruitfulness of the subsequent use we make of theory based on them. Pragmatics, on this picture, still has to do with the way that the contexts of individuals shape the expression of the meaning of their utterances, and that context includes, *inter alia*, the individuals' beliefs; but what individuals believe and express, in any given context, does not bear on the objectivity of the knowledge embodied in the epistemic content of a scientific theory, of which the semantics ought to ground the articulation and representation.

Semantics, therefore, must ground analysis of epistemology, and be grounded in turn by our grasp of it.

NOT:

$$\text{semantics} \approx \text{ontology}$$

$$\text{pragmatics} \approx \text{use}$$

RATHER:

$$\text{semantics} \approx \text{epistemology, methodology}$$

$$\text{pragmatics} \approx \text{acceptance, choice}$$

The Slogan:

Meaning comes before truth.

76. I want to emphasize that I am not opposed to referential relations and the idea of designation itself in a semantics for theories. I am opposed only to the idea that such relations be primitive.

10 Valediction

The miracle of science is that theory and experiment are consonant with each other; the necessity of science is that they are inextricably so—not, however, as equals. Theory plays Boswell to the subtle and tragic clown of experiment's Johnson.

11 Appendix: Précis[†]

This is a long and dense paper, and the overall argument has not leant itself to a clean, simple, linear exposition. I therefore sum up here the main claims of the paper, along with my main criticisms of what I call the standard view of theories.

First, I briefly rehearse the argument of §1 for the inadequacy of the standard view.

1. The standard view is that a theory is characterized by a family of models, where "characterized by" is construed broadly.
2. Correlatively, because those models are assumed to have intrinsic physical significance, it assumes a clean segregation of the theoretical from the practical forms and aspects of scientific knowledge (discussed in greater detail in §2).
3. The "models" are stipulated to be:
   a. either something like a family of (closed form) solutions to the theory's equations of motion or field equations, or a family of some mathematical structures with the right representational capacities;
   b. or else the family of physical systems that the equations' solutions or the theory's other mathematical structures represent.
4. Both possible stipulations are, however, epistemologically speaking, acts of faith:
   a. for almost all known theories we have very little knowledge of the family of solutions (or other relevant structures) because the general problems are mathematically intractable, which makes both stipulations 3a. and 3b. untenable;
   b. for all real physical systems, their true complexity is such that we cannot possibly know to which solutions of which theories they correspond, which makes stipulation 3b. untenable.
5. To grasp either horn of the dilemma, in any event, is to be forced to conclude that we never have real knowledge of any sophisticated scientific theory and how it represents the world.
6. It thus becomes utterly mysterious how the models are supposed to have intrinsic physical significance.

Second, my discontents with the standard view, which are part and parcel its inadequacy.

1. It assumes that only one form (theoretical), in one of its aspects (achieved state as represented purely by formalism), suffices for characterizing a theory (§§6–9).

†. This appendix does not appear in the published version of the paper, due to length constraints.

2. Thus, it cannot demarcate, much less identify, the family of physical systems the theory appropriately and adequately treats (§§2–4, §8).
3. Thus, it cannot explain the epistemic warrant we have for trusting and using the theory, and for believing that the understanding and comprehension it seems to give us of the world is indeed about the world (§7, §9).

Finally, the positive claims I defend.

1. A theory is characterized by its epistemic content, the sum total of all forms of knowledge it embodies, in all their aspects and relations to each other, as determined by our actual current state of knowledge about the world and about how to investigate it. (§2, §4, §§6–7)
2. The total family of physical systems the theory appropriately and adequately treats, a proper subset of all those "possibly representable" by all its formal models, forms an essential part of that epistemic content. (§§3–4, §8)
3. There are (at least) two forms of knowledge (§6):
   a. theoretical (what can be learned from books);
   b. practical (what can be fully understood only by doing).
4. Those forms have (at least) four aspects (§§6–7):
   a. as achieved state;
   b. as susceptible of justification, and so involving a structure of evidential relations;
   c. as ground for epistemic warrant, and so involving a structure of evidential relations;
   d. as an enterprise, an activity aimed at increasing the first aspect as constrained by the second and third.
5. Much of all of those forms and aspects is grounded on and embodied in our knowledge of how to schematically represent the observer in models of actual experiments and observations (§3, §§6–8). Perhaps most importantly, schematization of the observer is needed to:
   a. lay down adequate definitions for physical quantities (§8);
   b. determine the theory's breakdown scales (§8);
   c. demarcate the theory's regime of applicability, the total family of systems the theory appropriately and adequately treats, as delimited by the breakdown scales (§3, §8);
   d. adjudicate whether and, if so, elucidate how a given experiment lends confirmatory support to (some part of) a theory (§7).
6. A good semantics for theory should respect the fact that the epistemic content of a theory is not cleanly separable into formal and practical parts; in particular, semantics should be based on and reflect epistemology, not ontology (§9).

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