On the Propriety of Physical Theories as a Basis for Their Semantics[†]

Erik Curiel[‡]

The vague accuracies of events dancing two
and two with language which they
forever surpass

William Carlos Williams
Paterson, I.2

In theory, there's no difference between theory and practice. In practice, there is.

Yogi Berra

[†]This paper has been submitted to *British Journal for the Philosophy of Science*, Feb. 2014.

[‡]I thank John Norton and the group of Fellows at the Center for Philosophy of Science at the University of Pittsburgh, fall, 2008, and also colloquium audiences at the London School of Economics, fall, 2009, and at the University of Leeds, spring, 2011, for spirited discussions of earlier drafts of this paper. I thank Steven French for lively and helpful discussions about the semantic view of theories. I thank Chris Pincock for detailed comments, insightful suggestions and hard questions on earlier drafts, and for many enjoyable, illuminating conversations about these things in general. I thank Bill Demopoulos for enjoyable and helpful conversations on Carnap in particular and the structure of theories in general. I thank David Malament for many helpful suggestions on the paper's organization and style. Finally, I thank Howard Stein for many fruitful conversations over many years touching on all sorts of matters related to the issues I address here, and, in general, for more than I can well say. **Author's address**: Munich Center for Mathematical Philosophy, Ludwigstraße 31, Ludwig-Maximilians-Universität, 80539 München, Germany; **email**: `erik@strangebeautiful.com`

ABSTRACT

I argue that an adequate semantics for physical theories must be grounded on an account of the way that a theory provides formal and conceptual resources appropriate for—that have propriety in—the construction of representations of the physical systems the theory purports to treat. I sketch a precise, rigorous definition of the required forms of propriety, and argue that semantic content accrues to scientific representations of physical systems primarily in virtue of the propriety of its resources. That propriety largely consists in the satisfaction of a subset of the relations a theory posits among the quantities it treats, *viz.*, the theory's kinematical constraints, rather than in the predictive accuracy of its equations of motion. In particular, the adequacy (soundness, accuracy, truth, ...) of a theory's representations plays no fundamental role in the determination of a representation's semantic content. One consequence is that anything like traditional Tarskian semantics is inadequate for the task.

Contents

1 The Problem with Contemporary Semantics

Carnap (1942, ch. B, §7, p. 22) concisely expresses the seductive intuition that grounds essentially all contemporary thought on the semantics of scientific theories: "...to understand a sentence, to know what is asserted by it, is the same as to know under what conditions it would be true." This intuition underlies programs as diverse as rigid designation and the causal theory of meaning on the one hand, and the sort of Beth semantics van Fraassen requires for his constructive empiricism on the other, for it is as elastic as the notion of truth (adequacy, accuracy, reference, ...) itself.

As appealing as this idea is, however, its straightforward application leads to severe problems. This is so no matter the details of the architectonic form of one's account of a theory and its semantics, whether it falls, *e.g.*, under the purview of either the syntactical or the semantical account of scientific theories and their semantics,[1] or some other view entirely, so long as the foundation of that view takes as ineliminable a concept such as truth, adequacy, accuracy, reference, ..., that must be grounded on accuracy of prediction—for without a minimal accuracy in prediction, one has no grounds for postulating any such properties or relations.

The heart of the problem is that, according to any view that founds semantic content ultimately on the accuracy of prediction, a theory tells us what the world would be like if the theory were a sound representation of it—what the world would be like if the theory were true of it, and nothing more. But in fact, a physical theory in general tells us far more about the world than that. A theory can tell us much about the character and nature of physical systems for which it does not give accurate representations, systems, in other words, it cannot soundly represent in totality, cannot be true of. One way to see this is that such accounts cannot differentiate inaccuracy from inapplicability as a defect in a theoretical representation of a physical system: a semantics grounded on a notion like truth, which itself must be founded on accuracy of prediction, can rule a model of a system inadmissible only on the grounds that it does not represent the behavior of the system accurately enough. That, however, is too coarse-grained a measure of the way theories can fail to provide semantically sound representations of physical systems. In consequence, such a semantics fails to capture much that fundamentally informs and embodies the empirical meaning of terms and propositions in theories.

Consider the example of a representation of a body of liquid as provided by the classical theory of fluid mechanics. When the liquid is not too viscous, is in a state near hydrodynamical and thermodynamical equilibrium, and the level of precision and accuracy one demands of the representation is not at too fine a spatiotemporal scale, then the classical theory yields excellent models of the liquid's behavior over a wide range of states and environments. When the state of the liquid, say, begins to approach turbulence, the representation the theory provides begins to break down. It does so, however, in a subtle way, one that cannot be wholly accounted for by adverting merely to the fact that the theory becomes predictively inaccurate. In particular, there is a regime in which the theory's dynamical equations of motion no longer provide accurate predictions by any reasonable measure, and yet all the quantities the theory attributes to the liquid (*e.g.*, shear viscosity, mass density, hydrostatic pressure, shear-stress, *et al.*) will still be well defined, and all the kinematical constraints the theory jointly imposes on those quantities (*e.g.*, the constancy of shear viscosity, the continuity of mass-density, the conservation of energy, the symmetry of the shear tensor, *etc.*), will still be satisfied. In a strong sense, then, the theory can still provide a meaningful—and appropriate—model of the liquid even though that model is not adequately accurate in all its predictions. This sort of situation, where the theory's dynamics are no longer adequate but its kinematics are still appropriate, shapes and provides at least part of the physical meaning of terms like 'mass density' and

[1] See, *e.g.*, Brading and Landry (2006) for a concise, elegant statement of the two positions, and Suppe (1974) and da Costa and French (2005) for more thorough exposition and elaboration.

'shear'—physical meaning that *ipso facto* cannot be captured by a semantics that grounds meaning on predictive accuracy, for a semantics whose fundamental terms require, by way of relation to empirical phenomena, accuracy in prediction, cannot admit such models as part of the theory, period, for the models are not accurate. Indeed, more than just informing the meaning of such terms as 'shear', one can use the theory to say much of substance about the shear tensor of the fluid in states the theory is predictively inaccurate for, and thus much of substance about the behavior of the fluid's shear in those states (as I discuss in §4 below).

More precisely, a view based ultimately on predictive accuracy is inadequate for (at least) two reasons. First, it does not allow us, within the scope of the theory itself, to understand why such models are not sound 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 surely 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; 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 semantics that rejects the inaccurate models in which the term still is well defined will not be able to account for that part of the term's meaning. (I examine and argue for these claims in some detail in §§4–5 below.) Thus, an adequate semantics for physical theory must be grounded on notions of meaning derived from relations in some sense prior to the accuracy of 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.

My gripe is not with the idea of accuracy of prediction itself—only a fool or a philosopher would deny that it must play some important role in the way meaning accrues to physical theories. I oppose only its use as the foundation of meaning. My argument, then, is with accounts of semantics that make semantic content devolve in the end upon the predictive accuracy of a theory's representations, irrespective of how exactly it is that the accuracy comes into play in fleshing out the theory's semantic relations and content. Tarskian semantics, for example, as deployed in much contemporary work, is the archetype of such an account.

2 The Problem of Semantics

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, semantic content accrues to a scientific theory in no small measure through the successful construction in its terms of representations of physical systems. At bottom, then, what secure semantic content a scientific theory has must rest on the meanings expressed in the sound articulation of experimental knowledge. This requires at a minimum that we be able, at least in principle, to construct appropriate 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 particular, actual experiments, not just representations of physical systems *simpliciter*, in abstraction from actual experiments.[2]

Because this is not a standard view,[3] I shall spend a moment explaining why I hold it. There are three basic reasons. 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. An example is 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 (*e.g.*, from the diurnal rotation of the Earth) 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.[4] The second reason is a middling deep one. The quantitative results of all measurements and observations inevitably deviate from those predicted by theory, even if only by a small amount; likewise, there is an inevitable imprecision in the measured values. The errors and imprecision largely accrue to measurements on account of systematic errors arising from the idiosyncratic nature of the particular experimental apparatus used and the way it is actually deployed during the measurement process. 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 inadmissibly large 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. 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.[5]

The third reason is a deeper one. In order for theory to be able to provide guidance to experiment in the design of new types of tools for probing novel sorts of phenomena and in the design of new types of tools for probing known phenomena in novel ways, and conversely for experiment 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

[2]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.

[3]Fraassen (2008, ch. 12), for example, explicitly argues for the opposite conclusion.

[4]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.

[5]See, *e.g.*, Benedict (1969). It is not the most up-to-date reference with regard to the international agreement on defining the standard, practical methods for the determination of temperature (for which see, *e.g.*, Lide 2005), but I have found no better guide to the nuts and bolts of thermometry.

prediction, theory must be able to represent the fine details of the apparatus as actually used in the experiment. This particular interplay between theory and experiment, in theoretical guidance in the construction of instruments and in experimental constraint on the soundness of theory, 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 empirical 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. During most of the career of the investigation, Hertz had very little idea what sorts of arrangements of what sorts of physical system 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 finely detailed models both experimental and theoretical, each guiding the other in turn, of different proposed methods of coupling of the electromagnetic field to its environment and instruments to try to realize those couplings.[6]

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, immediately raises a serious problem, one that Stein (1992, p. 12) 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 semantics of the theories of theoretical 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 semantics of scientific theories.

Stein is not entirely pessimistic about the possibility of the construction of a semantics adequate for the empirical application of physical theories. In the same place as he sketches the problem, he implicitly suggests one possible approach to it (*ibid.*, p. 14; emphases are mine):

[6]See Hertz (1893, *passim*), including the preface by Helmholtz, for an absorbing account. See also the philosophically rich essays by Maxwell (1871, 1876) himself on this necessary sort of interplay between theory and experiment.

> Now, Carnap's scheme for philosophical analysis is admirably suited to just this situation. It is exactly the theories with a highly mathematical structure—the typical theories of physics—that lend themselves, *ipso facto*, to construction as Carnapian frameworks. The question of the empirical application of such a framework becomes a question of pragmatics. I do not know how, systematically, a general theory of such empirical application might be made; but at least I think the problem, in [this] neo-Carnapian form . . . , finds a suitable locus and an intelligible formulation as a problem. And I think it reasonably clear that *to just the extent that we know in practice how to talk about the empirical application of specific physical theories, we can formulate what we know how to say in terms of the pragmatics of a Carnapian framework.*

I think it *is* reasonably clear that, from a knowledge of how we apply theories in practice to model experiments, we can describe how we do in practice move meaningfully between the two. We can then found a semantics on the basis of our comprehension of the pragmatics: if we can explain how we do represent experiments and the knowledge we gain from them in a theory, and how we in turn apply this knowledge in practice, we will have *eo ipso* characterized the pragmatics of that theory, and concomitantly have grasped the semantic nature of the representations that the theory affords us of the phenomena it treats. We can then work our way upwards to the refinement of a generic model of semantics, in a process analogous to the construction of a scientific theory by the abstraction of formal structure from a collection of experimentally derived, phenomenal propositions. (I discuss this idea, of founding semantics on pragmatics, in §7.)

As I gestured at in section §1, I do not believe an account of semantics grounded on notions such as truth or referential relations, which must in the end devolve fundamentally upon the predictive accuracy of theoretical representations, can suffice. In this paper, I shall sketch the beginnings of another way to try to explicate the semantics of a theory, one founded on the idea of the meaning of predicates and propositions as a relation semantically prior to any concept that requires accuracy in predictive representation. Meaning accrues to the elements of a physical theory, I shall argue, by dint of the *propriety* of that theory's conceptual and formal apparatus for the production of possibly sound schematic representations of the physical systems the theory purports to treat.[7] One can think of propriety, in part, as what a theory must have for it to have the capacity to produce propositions whose truth-value can be cogently investigated—not a fixation 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

[7] I use 'schematic representation' rather than the briefer 'model' because the latter has acquired over the past couple of decades manifold meanings and connotations in various debates and schools and issues, none of which are relevant here. I intend the notion in only the thinnest of senses, something like a (generally complex) proposition that renders in a theory an abstract, skeletal representation of a type or token of physical system. Indeed, often one can think of such a thing, for my purposes, just as the theory's equations of motion for the system and the kinematical constraints it imposes on representations of the system, perhaps along with a concrete set of initial data if one treats a particular situation of an individual system. I shall hereafter sometimes use 'model' for the sake of brevity, hoping that this footnote will suffice to keep the reader from reading too much into the term.

phenomena with propriety. It follows that one can not even entertain questions about the truth of many sorts of propositions until one has determined that the theory has the apparatus, conceptual and formal, to represent the system at issue with propriety, *i.e.*, until meaning already has accrued to the structures of the theory. Thus the notion of propriety is intended to capture the knowledge we have *in practice* of how to talk about the empirical application of specific theories, and so will found the pragmatics I claim can be used to ascend to the semantics.[8]

Though it may sound surprising, I am able to go some way towards making the idea of propriety precise and rendering to it substantive content with manifest physical significance: propriety comes from the satisfaction of certain equations, the local kinematical constraints (which I define and discuss below in §3), by the values of the system's physical quantities; it does not, for example, *contra* contemporary semantical and structuralist accounts of scientific theories, demand even partial isomorphism of any structures in a theory's model of a system and empirically determined data-sets describing the behavior of the system. In this paper, however, I can give only a sketch of the precise explication of the notion. In a full, rigorous treatment, the semantics would be formulated by treating physical theories as something like Carnapian frameworks, as sketchily explicated, *e.g.*, in Carnap (1956) and more fully articulated in Carnap (1962).

How the physical world works in detail must shape our understanding of the way that we theoretically represent and model the details of that world; I find it implausible that we can learn substantive lessons about how we understand the world from study of theoretical structures alone, in isolation from experimental knowledge and how the two hook up in practice.

3 Kinematics and Dynamics

It is often useful when contemplating a physical theory, or as I will often say, theoretical framework (or just 'framework'), to distinguish its kinematical from its dynamical components. Because the peculiar character of each plays an important role in the account of semantics I sketch, I begin with a general account of this.

The difference between the kinematic and the dynamic manifests itself naturally in the family of quantities a framework ascribes to a type of system. On the one hand, there are the quantities that can vary with time and place while the system remains otherwise individually the same; these are the dynamic quantities. On the other, there are the quantities that one assumes, for the sake of argument and investigation, remain constant as the system dynamically evolves, on pain of the system's alteration *in specie*; these are the kinematic quantities. This classification belongs to kinematics. A state of a system is the aggregation of the values of its physically significant properties

[8]Parts of my account bear fruitful comparison to some of the ideas of Putnam in the 1970s and early 1980s (especially as laid out in Putnam 1975a, 1975b, 1975c, 1983a, 1983b) on the sociolinguistic aspects of meaning, and in particular the fact that to know the meaning of a word is not necessarily to have explicit knowledge of the truth or even truth-conditions of particular propositions, and the fact that it is at best questionable to demand an interpretation in a Tarskian sense of a language whose use is already fixed. Having said that, I want to emphasize that I use those ideas as inspiration while disregarding the ideas associated with his proposal of the rigidity of reference, including essentialism and the causal theory of meaning, with which I utterly disagree.

at an instant; it is represented by a proposition encapsulating all that can be known of the system physically, at least so far as the theoretical and experimental resources one relies on are concerned. If one can distinguish the values of the properties of the system at one time from those at another time by the available resources, then the system is in a state at the first time different from that at the second. A state, therefore, can be thought of as a set of the values of quantities that jointly suffice for the identification of the species of the system and for its individuation at a moment. As such, the state is the most fundamental unit of theoretical representation of a system *as* a unified system, rather than just as (say) a bunch of random, unrelated properties associated with a spatiotemporal region. The characterization of a system's state belongs to kinematics. Every known physical system has the property that at least some of its quantities almost always change in value as time passes, which is to say, the system in general occupies different states at different moments of time. The collection of states it serially occupies during an interval of time forms a *kinematically possible evolution* (or just 'possible evolution' or 'dynamical evolution'). The characterization of possible evolutions belongs to dynamics.

Roughly speaking, then, kinematics comprises what one needs to know in order to fix the type of system at issue (is it a viscous fluid? an electromagnetic field?), and to give a complete description of its state at a single moment—complete, that is, with respect to the framework at issue, *i.e.*, a consistent ascription of values to all the quantities it bears that are treated by a model of it in the framework. Dynamics comprises what one needs to know in order to individuate a system and to describe its behavior over time, in order to conclude, for example, that one's model represents this system right here by the determination of the values that a particular set of its quantities respectively takes over the next 5 minutes, given both its state at the initial moment and the state of its environment (the forces, if any, it is subject to) at that moment and over the course of those 5 minutes.

Kinematics does more than classify the quantities of a type of physical system into the kinematic and the dynamic. It also imposes fixed, unchanging relations of constraint among their possible values, both constraints that must hold at a single instant and those that must hold over the course of any of the system's possible evolutions. More precisely, there are two kinds of kinematical constraints a framework may comprise: the *evolutive*, in which the relations include dynamical derivatives; and the *static* ones, in which the relations among the quantities are strictly algebraic or contain derivatives that are, in a technical sense, "non-dynamical".[9] Each of these two types is further subclassified into *local* and *global* constraints: a local constraint, whether static or evolutive, involves only quantities that can be attributed to a single state of the system, such as position; a global one involves a quantity that cannot be attributed to any single state of the system, such as the period of an orbiting body.[10]

[9] Geroch (1996, p. 10) makes a similar distinction based on algebraic and differential relations among quantities, though he does it in the service of differentiating the dynamical equations of a theory from the kinematical, not differentiating among kinematical constraints.

[10] There is a subtlety here. Any kinematical constraints that involve derivatives, strictly speaking, depend on values of quantities at more than one state, even for local constraints; some global constraints, moreover, can be formulated by laying down conditions that must hold at individual states (*e.g.*, that a Newtonian orbit be an ellipse can be

In order to be able to formulate and evaluate any kinematical constraint, of course, the quantities themselves in the terms of which the constraints are formulated must be well defined in the framework. For this to be the case, it is necessary that one be able to formulate the *local* kinematical constraints and verify that they hold. It is in general not necessary that one be able to do the same for global constraints. I shall at the moment only sketch my reasons for asserting this; further arguments will have to wait for §4 below. Without the satisfaction of the local kinematical constraints, the entire idea of the individual state of a system as represented by that framework disintegrates—individual quantities do not stand in the minimal relations to each other required by the theory—and without the idea of a state of a system, one can do nothing in the framework to try to treat the system. More to the point, if the local kinematical constraints are not satisfied, one has no grounds for believing that the system at hand is one of the type the framework treats. Many different kinds of system, for example, have shear and stress—Navier-Stokes fluids, elastic solids, ionically charged plasmas, electromagnetic fields, *et al.* To say that a system has a quantity represented by a shear-stress tensor is not to have said very much. One must also know, among other things, whether the shear-stress tensor must be symmetric, or divergence-free, or stand in a fixed algebraic relation to another of the system's quantities such as heat flux, and so on. Each such possible condition is a kinematical constraint; and each different type of system that has a quantity appropriately represented by a shear-stress tensor will impose different constraints on that tensor. It is those constraints that differentiate types of physical systems, and not their dynamics. Think of all the kinds of systems whose dynamics obey the equation of a simple harmonic oscillator (pendulum, spring, vibrating string, electrical circuit, orbiting planet, trapped quantum particle, . . .)—without question what differentiates them cannot be the form of their dynamics. It is only the forms of the kinematical constraints one demands be obeyed by the quantities entering into the equations of motion.

To illustrate the idea of a local static constraint, consider a few billiard balls on a frictionless pool table. In order to apply Newtonian mechanics to model the system, we demand, for example, that the linear momentum of a ball at a point stand in linear proportion to the velocity of its center of motion at the same point, with the ratio being the inertial mass. This algebraic relation, indeed, is definitive of linear momentum (or, if one likes, of velocity) in the Newtonian mechanics of rigid bodies. If this relation does not hold between the linear velocity and linear momentum, as it will not for example in a system with pronounced viscöelastic or relativistic effects, then one cannot appropriately apply the Newtonian mechanics of rigid bodies to represent that system.

As an example of a local evolutive constraint, let us examine the notion of a billiard ball's velocity in a little more detail. We demand that the ball's velocity at a point and at the states kinematically

formulated as a constraint on the value of the spatial derivative at every point of the orbit, or on the sum of the distances from the foci at each point); this seems superficially similar to some local ones, *e.g.*, conservation of angular momentum, which can also be formulated as a relation among derivatives at a point. Whether a constraint, then, is global or local, may depend on whether one can formulate the condition over arbitrarily short periods of a possible evolution, which one can for conservation of angular momentum (the system satisfies angular momentum, say, during one part of an evolution but not another), but not for whether a planetary orbit is an ellipse (where, by definition, one must wait an entire orbital period before one can say the condition is satisfied or not).

reachable from that point after a short temporal interval jointly satisfy a strict relation (except, perhaps, at isolated, singular points), as follows. The kinematic velocity of a ball in any individual state, as a quantity borne by that system, does not depend on the spatial position that state represents the ball as occupying: a ball at any point of space may have any velocity, irrespective of its position (ignoring constraints that external systems may impose). From a physical point of view, however, the velocity of the ball is the rate of change of the ball's position with time, including its direction of change and its change in speed. We require of the ball's evolution, in order to be kinematically possible, that its velocity at a point as computed by direct measurement of spatial and temporal distances traveled be, in the limit of smaller and smaller spatial and temporal distances over which one measures, an ever better approximation to the kinematic velocity the state of the evolution ascribes to the ball at that point. In other words, we require as a local evolutive kinematical constraint that the velocity, as a physical quantity in its own right, be the temporal derivative of position, and thus that it as well can be represented as an element of a real, three-dimensional, Euclidean vector space. The physical operation of composing velocities (through collision, say) also manifests just the sort of linear, additive structure as vectors. We require that our kinematical representation respect, indeed that it manifestly encode, these relations. A representation of Newtonian velocity by a real number, say, rather than by a vector would lack propriety in this precise sense, for a real number cannot be a temporal derivative of a position in a three-dimensional space.

Although there is much more to say about the dynamical structure of a physical theory, for the purposes of this paper I must rest content with remarking that it includes in general a rich and deep lode of topological, geometrical, analytical and algebraic structures on the space of states that in particular encode relations among entire classes of dynamic evolutions; those relations often take in part the form of a set of partial-differential equations expressed in terms of the kinematic and dynamic quantities, the solutions to which represent the totality of the system's kinematically possible dynamic evolutions starting from all kinematically possible initial states. These equations are known as the system's *equations of dynamical evolution* (or *equations of motion*). The canonical example is Newton's Second Law: a Newtonian body accelerates in direct, fixed proportion to the net total force applied to it, the ratio of the acceleration to the total force being the kinematic quantity known as the body's inertial mass.

Now, kinematical constraints are differentiated from equations of motion by the fact that the particular, concrete form of a kinematical constraint is fixed once and for all, irrespective of the interactions the system may enter into with other systems (such as a measuring apparatus in the laboratory). By contrast, the particular, concrete form of a system's equations of motion depends essentially on the particular interaction (if any) the system enters into with another system in its environment—*e.g.*, what external forces, if any, act on the system. According to this characterization, the first two Maxwell equations,

$$\begin{aligned} \boldsymbol{\nabla} \cdot \mathbf{B} &= 0 \\ \dot{\mathbf{B}} &= -\boldsymbol{\nabla} \times \mathbf{E} \end{aligned} \tag{3.1}$$

those governing the magnetic components $\mathbf{B}$ of the electromagnetic field, are both local kinematical constraints, the former static (because the only derivative that appears is spatial, *i.e.*, not dynamical) and the latter evolutive. They are kinematical constraints and not equations of motion because neither changes form no matter the environment the electromagnetic field evolves in (ignoring the possibility of magnetic monopoles). Indeed, even though one of the equations includes the time-derivative of another quantity, making it look like an equation of motion, I claim that from a physical point of view one must think of them both as kinematical constraints. The crux of the matter is that the electromagnetic field couples with other systems only by way of their manifestation of electric charge ρ or current $\mathbf{j}$, but those quantities when present change the form only of the other two Maxwell equations,

$$\begin{aligned} \boldsymbol{\nabla} \cdot \mathbf{E} &= \rho \\ \dot{\mathbf{E}} &= \mathbf{j} - \boldsymbol{\nabla} \times \mathbf{B} \end{aligned} \tag{3.2}$$

those governing the electric components $\mathbf{E}$ of the electromagnetic field. In effect, the difference between the two pairs of relations shows that, in a precise sense, the magnetic field couples directly with no physical quantity of any other system in that the presence of electric charges and currents does not alter the form of its two defining equations. (The magnetic field does couple to electric current "to second order" by way of the second of equations (3.2), whence Ampère's Law.) Thus the form of equations (3.1) does not depend on the particular dynamical evolution the system manifests at any given time. Nonetheless, not just any old thing counts as a magnetic field no matter how it evolves and no matter what relations hold among its quantities at different points; only those things that behave like magnetic fields can be magnetic fields, which in this case means the identical satisfaction of the first two Maxwell equations. Again, that is why satisfaction of the local constraints is necessary for the definition of the state of a system: without their satisfaction, one has no reason to think that the state represents that species of system at issue.

The discussion has implicitly drawn out one of the most fundamental and, for my purposes, salient differences between the kinematic and the dynamic: frameworks do not predict kinematical constraints; they demand them. I take a prediction to be something that a theory, while meaningfully and appropriately modeling a given system, can still get wrong. Newtonian mechanics, then, does not predict that the kinematical velocity of a Newtonian 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. If the equations of motion as one has modeled them are not satisfied, however, that may tell one only that one has not taken all ambient forces (couplings) of the system with its environment into account; it need not imply that one is dealing with an entirely different form of system. Even in principle, one can never entirely rule out the mere 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 accurately 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. This is a serious difference in physical significance among the types of proposition a theory contains, which, among other things, should be reflected in the way an account of the meaning of a theory assigns significance to the theory's structural elements. Thus satisfaction of the local kinematical constraints is required as a precondition for the appropriate application of a theory.[11] This is not true of the dynamical relations the framework posits. As we will see, a framework 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 (and thus when, according to the standard conception of semantics, the schematic representations of those phenomena cannot contribute to the semantic content of the terms occurring in those representations).

4 The Breakdown of Models and the Kinematical Regimes of Propriety

Now, a system may initially be treated with accuracy by a theory but then slowly fail to do so as its environment or its state changes; in other words, a framework's model of a system may be adequate under some conditions, but come increasingly not to be so as those conditions change. I now consider some of the ways that a previously adequate model may break down and fail to provide an empirically substantive representation of the phenomenon at issue. The difference between, on the one hand, the propriety of a theory and its models in representing a class of phenomena and, on the other, their empirical adequacy in doing so shows itself clearly in those sorts of failures. First, I will state in schema three relevant relations in which a framework purporting to treat a system may stand towards it with respect to the framework's representational resources, or, put differently, three relevant regimes in which a system may manifest itself with respect to a framework's representational resources. I will then work in some detail through two examples to argue that these three are appropriate for explicating one fundamental notion of propriety required for semantics. (In §6 below, I shall introduce one last regime to add to this list, and concomitantly a sharper notion of propriety required for a complete account of semantics.)

As a matter of brute scientific fact, a theoretical framework has at least three regimes:

regime of impropriety all systems for which not all local kinematical constraints are satisfied: the theory's quantities are not explicatively defined in the context of the framework; *a fortiori* the equations of motion cannot even be formulated

regime of kinematical propriety all systems for which all the local kinematical constraints are

[11] The claim is not that these propositions are "empirically unrevisable" along the lines, say, of a Reichenbachian coordinating definition; indeed, these propositions are not only empirically revisable but are in fact revised from time to time (for example, as with the relation of position and velocity in special relativity, in quantum mechanics, . . .); when they do fail to hold empirically for a given system—as they will in some circumstances—then any theory that demands them is inapplicable. That is all.

satisfied: all the theory's quantities are well defined; some global kinematical constraints may not be satisfied; and the equations of motion are not satisfied

regime of adequacy all systems for which everything is well defined and satisfied: a state of harmony and bliss in which systems and theories move together hand in hand with the equanimity of the blesséd gods

A framework 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 spatial and temporal scales appropriate for the representation of the relations among the quantities manifested in the phenomena at issue. Thus, because we need only the definitions of the quantities to formulate and solve all the equations the theory poses, kinematical propriety is equivalent to the having of good definitions of all quantities *simpliciter*, without worrying about whether they satisfy all the global kinematical constraints or the equations of motion. In order for the quantities to be well defined, they must satisfy a subset of the kinematical constraints, the local ones, which by definition are constraints on values of magnitudes accruing to individual states of the system, the fundamental unit of representation the theory provides; being able to formulate and evaluate the local kinematical constraints so as to have their solutions within the admissible deviance from observed values for them, as allowed by the theory, is in fact the precise and rigorous definition of kinematical propriety. This, by the way, is why kinematical propriety is characterized in terms of *kinematical* constraints—why I give preference to the local kinematical constraints rather than to some other form of relation the theory imposes, demands or predicts holds among the values of a system's quantities: because the cogency of the representation of single states is the necessary and sufficient condition for a theory's being able to represent a system as a system of its type, and the satisfaction of the local constraints is the condition that guarantees not only that all quantities are well defined, *viz.*, that they have the capacity to individuate single states and identify them at a moment of time, but even more that they are quantities associated with *that kind of system*.[12] (I shall sketch in §5 below another reason for the primacy of kinematical constraints when it comes to the meaning of theoretical terms and structures.)

As an example, consider first Navier-Stokes theory, the classical theory of viscoëlastic, thermoconducive fluids, such as liquid water or nitrogen gas. Using the notions of §3, I first sketch the kinematics and dynamics of the theory by giving the kinematic and dynamic quantities it predicates of them and gesturing at the kinematic and dynamic relations obtaining among those.[13] The theory ascribes three kinematic quantities to any physical system it treats: bulk viscosity, a measure of resistance to compression and expansion; shear viscosity, a measure of resistance to deformation that does not change the fluid's volume; and thermal conductivity, a measure of the rate at which thermal energy disperses throughout the substance. If we cannot meaningfully predicate these quantities of a physical system, then it cannot be a Navier-Stokes fluid. The dynamic quantities standardly

[12] I thank Chris Pincock for pushing me on this point.

[13] See, *e.g.*, Lamb (1932) or Landau and Lifschitz (1975) for a comprehensive treatment of Navier-Stokes theory.

attributed to a Navier-Stokes fluid are mass fluid density, gross fluid velocity, hydrostatic pressure, the flux of heat, and the distribution of stress and shear.

The kinematic and dynamic quantitities jointly satisfy five fixed kinematical constraints, and a sixth idiosyncratic to each different species of fluid, the equation of state. An example of one of the fixed constraints is the requirement that the shear-stress tensor be symmetric, which means physically that any stress as measured across a (perhaps only imagined) surface in the fluid in a particular direction and any shear along a surface in any particular direction will be counterbalanced by an equal stress and shear across and along, respectively, the same surface in the directions opposite those of the first. This is a static, local constraint. Another is the equation of continuity. It states that the flux of fluid into any region must equal the flux of fluid out of that region plus the rate at which the amount of fluid in the region increases or decreases; it states, in other words, that no fluid can be created *ex nihilo* or destroyed *ad nihilum*, and, correlatively, that the fluid can be appropriately modeled as a continuous distribution of matter. This is an evolutive local constraint. The dynamical structure consists largely of the two Navier-Stokes equations, the theory's equations of motion, jointly governing the dynamical evolution of the mass density, the fluid velocity, the shear-stress and the heat flux. To fix the values of all 3 kinematic quantities is to define a species of Navier-Stokes fluid—or, in more loaded terms, to define a "natural kind" of fluid in the only sense the theory can support. Anything that has the same values for all kinematic quantities as does water, *e.g.*, *is* according to Navier-Stokes theory the very same stuff as water, for it is indistinguishable both kinematically and dynamically from water: the same initial conditions yield the same dynamical evolutions.

Now, every species of physical system as represented by a given framework has a characteristic length (or characteristic interval of time, or characteristic energy, and so on), the *characteristic scale*, beyond which the terms designating the system's quantities lose definition in the framework. Typically there is only one such length, at which all quantities simultaneously lose definition, for a Navier-Stokes fluid being a few orders of magnitude greater than the length of the mean free-path of the fluid's constituent particles (the average spatial distance a particle travels between collisions with another), in this case sometimes called the hydrodynamical scale.[14] At this length scale, one cannot even formulate much more verify the local constraints. The shear-stress, *e.g.*, stands proxy for the mean acceleration of the fluid's constituent molecules, but below the hydrodynamic scale the population samples of molecules are too small and vary too much for this average to have any statistical significance. Physically, this means that we cannot design instruments that appropriately couple to the quantity below that scale: different sorts of experimental devices for coupling to molecular acceleration, all with sensitivities below the hydrodynamic scale, will record markedly

[14]There is no *a priori* reason why the definitions of all the different quantities, both kinematic and dynamic, that appear in the Navier-Stokes system—bulk viscosity, shear viscosity, thermal conductivity, temperature, pressure, heat flow, stress distribution, and all the others—should fail at the same characteristic scale, even though, in fact, those of all known examples do, not only for Navier-Stokes theory but for all physical theories we have. This seems, indeed, to be one of the *markers* of a physical theory, the existence of a single characteristic scale of length (equivalently: time, energy) for its kinematic *and* dynamic quantities. Why should this be?

different "shear" and "stress" depending on properties of the joint system that one can safely ignore at larger scales, for instance the exact distribution of collisions of the fluid's molecules with the device's. In such a case, whatever the devices measures, it is not a quantity that conforms to the notion of shear-stress as represented by Navier-Stokes theory—for one thing, it will not satisfy a physical condition of the sort captured by the symmetry of the shear-stress tensor, for that symmetry exactly encodes the fact that we are treating a regime where the relevant molecular averages have statistical significance. The definitions of the other quantities fail in similar ways.

It is not obvious at first glance, but the satisfaction of the kinematical constraints of Navier-Stokes theory requires that the system at issue exist in conditions close to hydrodynamical and thermodynamical equilibrium. This follows from the definition of the characteristic scale, which makes clear the possibility that a turbulent or otherwise strongly disequilibrated fluid can manifest higher-order molecular effects, making themselves felt in dynamical pathology in the behavior of the fluid's quantities, even while the quantities themselves remain defined in at least experimental terms, so to speak.[15] The first constraint, symmetry of the shear-stress tensor, for example, requires that the distribution of the accelerations of the fluid's molecules does not vary too strongly and erratically over regions several orders of magnitude larger than their mean free-path. No quantity that does not manifest the behavior encoded in the symmetry of the shear-stress tensor, however, can be *in the context of Navier-Stokes theory* shear-stress: it cannot be modeled by Navier-Stokes theory and it cannot be measured experimentally in any way relevant to Navier-Stokes theory, for, among other problems, Navier-Stokes theory can give no appropriate guidance in the construction of experimental tools for its unambiguous determination. This follows from the fact that the conditions under which the shear-stress begins to lose symmetry are of the same sort as those in which devices that couple to molecular acceleration in different ways begin to provide markedly different readings, as happens for devices with sensitivities below the characteristic scale. At these scales, the quantitative artifact resulting from measurements by a particular kind of device may still have (some of) the crude characteristics of a shear-stress, but it cannot be explicatively represented by Navier-Stokes theory. We are in the regime of impropriety. The laboratory cannot get into the theory.

The second constraint, the principle of the continuity of mass-density, does not hold of all fluids under all conditions. If a strong enough current passes through water, some of the water will denature by electrolysis into hydrogen and oxygen. A similar effect would occur by the application of a heat shock to the water while keeping it under great pressure. Bombarding the water with dense enough sprays of neutrons of high enough energy will rupture the hydrogen and oxygen atoms themselves, resulting in clouds of electrons, protons and neutrons, as well as momentary, transient bursts of more exotic particles such as π-mesons. Under some of these conditions, the fluid will have no well defined mass density as a measure of the amount of *water* at hydrodynamic scales, and so there can be no question of its continuity, and *a fortiori* no question of the applicability of Navier-Stokes theory for its representation. Under similar but milder conditions, the constraint will fail in a weaker way, one

[15] 'Higher-order' here refers to the moments of the distribution function and correlatively of the Maxwell-Boltzmann collision equation one must take account of in giving an adequate treatment of the system in molecular kinetics. See Sommerfeld (1964, ch. v, pp. 41–43, 293–318) for a discussion.

in which the mass density manifests gross discontinuities at the hydrodynamic scale, say in those localized regions where the molecules denature, or just in the presence of certain kinds of shock waves or turbulence, and so will not satisfy the equation of continuity everywhere. This latter, milder form of the failure, however, need not imply that the mass density itself, as a physical quantity borne by the fluid, loses either experimental definition on its own or even explicative representation in Navier-Stokes theory. It may still have experimental definition, for example, in so far as different sorts of devices purporting to measure it will return consonant results across most of the fluid most of the time, and so one can still model the quantity using the theory, since all the couplings the theory allows for—all those different devices—still more or less agree among themselves. This is an example of a weak failure of an evolutive, local constraint that does not invalidate the definition of the quantities entering into it, showing that the division into three regimes for a given system and framework is not always possible without adverting to pragmatic concerns, such as the nature and purposes of the investigation at hand. We may still be in the regime of kinematical propriety; the determination whether or not is a pragmatic affair, depending on the sorts of considerations I discuss in §7 below.

Finally, Navier-Stokes theory can fail when the equations of motion themselves do not hold even though all the system's kinematical constraints are satisfied (and so all the theory's quantities are well defined). In other words, there may be systems in states or environments for which the equations of motion predict values different from those measured by an amount greater than the admissible deviance of predicted from observed behavior. The model provides for such a system complete, appropriate representations of all quantities, both kinematic and dynamic, throughout the entirety of the system—in particular all kinematical constraints are satisfied—but the system falls outside the model's acceptable deviation from accuracy in prediction.[16] If I apply a heat shock to a glass of wine, for example, Navier-Stokes theory cannot handle the resulting temperature gradient and the relaxation effects (the higher-order terms from the distribution function), in the sense that the equations of motion yield solutions that do not represent the physical evolution of the system to any reasonable degree of accuracy. Nonetheless, all the theory's quantities can be well defined and the kinematical constraints satisfied by any reasonable measure one wants to use, if the heat shock is not too great. We are in the regime of kinematical propriety.

According to a semantics that requires predictive accuracy, such as a Tarskian one, the preceding discussion of Navier-Stokes theory would, strictly speaking, be meaningless. I hope it to be clear that it is *not* meaningless.

The reader may worry that these suggested definitions and arguments implicitly turn on the peculiar character of hydrodynamical theories and their relation to underlying theories of molecular kinetics. To assuage that worry, I shall now show that Newtonian gravitational theory has the same set of regimes, characterized by the same conditions. To make the case, I examine the history of the investigation of the precession of the perihelion of Mercury's orbit, an important example of

[16] In this case, the system's dynamical evolution does not constitute a model in the sense of Tarskian semantics, for the values of the quantities do not satisfy all the semantical requirements for being an interpretation of the model. In particular, it does not satisfy the predicate representing the system's equations of motion.

an actual case in which a system's falling in a theory's regime of propriety but not in its regime of adequacy had profound consequences for the development of physical theory.[17]

Before diving into the details, a few words about terminology are in order. The orbit of a body revolving around a central one *precesses* about the central body if, at the completion of one period of the orbit, the form of the orbit remains more or less unchanged but a major axis has rotated from its starting inclination. Say, for a planet in an elliptical orbit about the sun, that its major axis when continued in a right line points at a given moment in the direction of the North Star, and then at the completion of the planet's orbit starting from that initial moment it points in a direction oblique to the original line by 1°; then the orbit precesses by 1° per orbital period.[18] Because the major axis of an elliptical orbit contains the perihelion (the point in the orbit closest to the central body) and the aphelion (the point in the orbit farthest from the central body), we also say that the perihelion and the aphelion themselves precess, and do so at the same rate. An *arcminute* is $1/60^{\text{th}}$ of an angular degree, and an *arcsecond* is $1/60^{\text{th}}$ of an arcminute, or $1/3600^{\text{th}}$ of a degree; the expression '$4'\ 16''$' designates a measure of 4 arcminutes and 16 arcseconds. The Solar System's planets' actual precessions are so slight as to make it more perspicuous to express their magnitudes in arcminutes and arcseconds.

Now, Newton (1726, Bk. III, Prop. XIV, Scholium) knew that his theory predicted that the perihelion of Mercury's orbit precesses. His theory when applied to the known observational data predicted its precession at $4'\ 16''$ a century, or about $2.5''$ a year. At the same time, astronomers knew the (sidereal) period of Mercury's orbit with a precision of $1/10,000^{\text{th}}$ of a Terran day, or about 8.5 seconds (Newton 1726, Bk. III, phenomenon IV), and with perfect accuracy, according to today's best datum: 87.9692 Terran days, the same down to the ten-thousandth part as the most current value given by NASA.[19]

In the event, though, Newton's prediction was not sound; it had to await Le Verrier (1859) for the accurate determination of the rate of the precession of Mercury's perihelion, and the calculation that it inadmissibly differed from any value derivable from the Newtonian theory.[20] Le Verrier (1845) had suspected already that the precession of Mercury's orbit posed problems for Newtonian gravitational theory, with an observed period of $8'\ 47''$ per century and a predicted value of $9'\ 26''$,

[17]See Harper (2011) for an extended and philosophically rich discussion of the topic touching on many questions of relevance to us here.

[18]In this case the orbit is not even a closed curve, much more an ellipse, but the amount of actual precession in the orbits of the planets in the Solar System is so slight that it is an excellent approximation to treat them as ellipses.

[19]This accuracy is all the more astonishing when one learns of the difficulties attending attempts to observe Mercury from the Earth: in the lower latitudes it is visible in its own right (as opposed to being visible as a negative image during transits of the sun) for only a short time every few years, and then for only a few hours in the very early evening and the very early morning; in the higher latitudes, such as those of England, it is visible even less of the time. In a country whose meteorology is so hard on the astronomer as England's, these difficulties are exacerbated. See, for example, Flamsteed (1835, *passim*). Copernicus himself is reported to have expressed regret that he never had the opportunity to directly observe Mercury at all.

[20]It is amusing (and perhaps not surprising) that this Le Verrier who heralded the first unequivocal failure in Newtonian gravitational theory's adequacy is the same who had achieved perhaps its greatest triumph since Newton's own time, the theoretical prediction of the existence of Neptune based on deviations of Uranus's observed orbit from its theoretically predicted form.

a discrepancy of 39″ per century, but at that time he could not rule out the possibility that the discrepancy arose from problems with his models.[21] (In the language I used earlier, he could not have said definitively the discrepancy did not come from his not having taken into account all the couplings of Mercury with its environment, rather than from the impropriety of the framework for modeling Mercury's motion.) It was not until 1859 that he had satisfied himself that the best models constructible in Newtonian gravitational theory as applied to the best data available could not explain the discrepancy, not even by the postulation of hitherto unobserved celestial bodies or any other such *ad hoc* devices. Indeed, by the end of the nineteenth century the inexplicability of the aberrant precession was such a great embarrassment that many eminent physicists had already concluded that Newtonian gravitational theory could not be fundamentally correct, even before the development of special relativity (a historical fact that seems to be not so well known as it ought).[22]

Back-of-the-envelope, geometrical computation shows that, in light of the level of precision and accuracy of the knowledge of the temporal period of Mercury's orbit in Newton's day, the position of Mercury's perihelion could in principle have been measured to an accuracy of about 0.1″ a year, well within striking range of the verification of the prediction of its precession at the end of the nineteenth century, even if one allows for an error of an order of magnitude. This is not to say that they had in hand at Newton's time the amount and quality of data actually to perform the requisite computations. Their level of precision and accuracy in the ascertainment of the period of Mercury's orbit did not derive from direct observation, but rather from sophisticated analysis of the cumulative data from thousands of years of less precise measurements that were not suited to the task of the ascertainment of the changing position of Mercury's perihelion with respect to the fixed stars.[23] The most important point for our purposes, though, is that no fundamental lack in the experimental understanding and expertise of the day stood in the way of Newton's and his contemporaries' having made such measurements.[24]

Thus, although Newton did not know it, his theory *by its own lights* cannot adequately model the full dynamics of Mercury's orbit. Nonetheless, Newton had used the observationally determined orbit of Mercury as part of the foundation of the derivation of the universality of gravity in Book III

[21] Newton's deduction of the precession of Mercury's aphelion as 4′ 16″ per century is thus off by a factor of 2, remarkably accurate given the difficulties in the observation of Mercury and the concomitant meagreness of the data available to him. Le Verrier is said to have remarked, ≪ Nulle planète n'a demandé aux astronomes plus de soins et de peines que Mercure, et ne leur a donné en récompense tant d'inquiétudes, tant de contrariétés. ≫

[22] See Newcomb (1895a, 1895b, 1905) for an extended discussion and summation of the experimental knowledge of the aberrant precession at that time, when the anomalous amount of Mercury's precession was finally fixed at 43″ per century, and see Freundlich (1915) for an exhaustive argument that Newtonian gravitational theory could not account for it. To get a sense of how small the angle 43″ is, imagine the appearance of the diameter of a penny from a distance of about 30 miles. This makes an angle of that measure. It is a testament to the profound entrenchment of Newtonian gravitational theory at the time that a discrepancy of this infinitesimal angle *per century* caused such consternation in and provoked such labor from the leading lights of the scientific community for almost a century.

[23] See Herz (1887, 1894) and Dreyer (1906) for expository and critical discussion of those analyses.

[24] Indeed, several of the aberrant features of Mercury's orbit, as compared to the behavior of the orbits of the other planets, was well known already to Ptolemy (1998, Bk. IX, §§6–10). (Of course, Ptolemy discussed the anomalies in the behavior of Mercury's perigee and apogee, the points in its orbit closest to and furthest from the Earth, rather than those in that of its perihelion and aphelion.)

of *Principia*. I claim that he was justified in doing so, even from the perspective of one who knows his theory to be inadequate for Mercury's orbit, because one can formulate the local kinematical constraints the theory requires and verify that they hold for all the quantities associated with Mercury's orbit for the data available to Newton and the machinery of his theory as developed at the time. For example, it is straightforward to verify from the data Newton had available to him that at any point of Mercury's orbit, its angular velocity stands in linear proportion to its angular momentum (as measured by the area swept out in a unit of time by a line joining the planet to the sun), with the ratio being given by its inertial mass, all to well within the experimental accuracy of the data. The only proposition encoding some of Mercury's dynamics that Newton used as an assumption in his derivation of universal gravity, moreover, the so-called Harmonic Rule of Kepler, also held to within the limits of observational accuracy available at the time.[25] The Harmonic Rule states (in modern terms) that the squares of the orbital periods of the planets are directly proportional to the cubes of the semi-major axes of the orbits. Thus, to apply the Rule, one needs to be able to make sense of two global quantities associated with the planet's motion, the period of the planet's orbit and the length of the orbit's semi-major axis, and to be able to verify that a fixed relation holds between them, both of which Newton could do and did. Thus, even though Newtonian gravitational theory cannot adequately treat the full dynamical details of Mercury's orbital motion, *i.e.*, even though Mercury falls outside the regime of adequacy of Newtonian gravitational theory, that theory still can represent that motion in a way that is not only meaningful in itself, but, more importantly, in a way that can have such profound consequences as its being able to be legitimately invoked in such a weighty investigation as Newton's in Book III of the *Principia*. And that is precisely because Mercury falls within the regime of propriety of Newtonian gravitational theory: its motion satisfies the theory's kinematical constraints and even some of the theory's dynamical propositions, even though it does not satisfy the theory's full equations of motion by the theory's own lights.

5 Kinematical Propriety and Meaning

Now, if a system falls within a framework's regime of propriety but not in its regime of adequacy, the model the framework provides for representing the system has at least the semantic content accruing to it in virtue of the fact that it is an appropriate representation of the system: the terms referring to the system's quantities are well defined and have at least so much semantic content as accrues to them from the fact that they jointly satisfy the framework's kinematical constraints. If the system crosses over into its regime of adequacy, that is, if the model becomes an adequately accurate representation of the system, then the model gains no semantic content *independent* of that already having accrued to it from its propriety. To see this, assume the model were to gain new, independent semantic content when the system passes into its regime of adequacy, that semantic content encoding the fact that it was not before but is now an accurate model of that system, which, *ex hypothesi*, is independent of the semantic content it already had in virtue of being in

[25]See Harper (2011) for an argument to this effect.

the framework's regime of propriety. Thus the model must have had some independent semantic content accruing to it before designating the fact that it was inaccurate as a model of that system. It is also (almost certainly) true, however, that the model does not accurately represent the growth of a tree or the passage of neutrinos through interstellar space or the sinking of the Titanic, but its lack of accuracy in these cases does not by itself contribute to or otherwise inform its semantic content. The lack of accuracy of the model in its representation of a physical system bears on its semantic content *only in so far as* we already know or have good reason to suspect that the model has propriety in the representation of *that* sort of system. Because the model does not represent the growth of a tree with propriety, its lack of accuracy as such a representation does not bear on its semantic content at all, or, more precisely, the model does not give us the tools to investigate whether it represents the growth of the tree, and so it has no semantic content that pertains to its possibly being a model of the tree, accurate or not. The inaccuracy of a model, and *a fortiori* its accuracy, can inform its semantic content *only if we already know the model does in fact represent the system with propriety.* The propriety of a model must be already in place for its accuracy or lack of accuracy to inform its semantic content in any way. It follows that propriety is semantically prior to accuracy, *i.e.*, any semantic content accruing to a model encoding its accuracy must depend on the semantic content it already has in virtue of its propriety.[26] I emphasize: this is not to say that the fact that a model's predictions are accurate does not contribute any semantic content to the theory at all; only that such content as it does contribute depends on the semantic content the model already has in virtue of its belonging to the theory's kinematical regime. Thus one cannot found a semantics on the accuracy of a framework's models.[27]

Indeed, one can say more, and more precisely, about how the meaning that accrues to a theory in virtue of the satisfaction of the kinematical constraints must be prior to whatever meaning comes from the accuracy of predictions made by its equations of motion. I noted briefly, in §3 above, that in general there are relations among a system's possible dynamical evolutions that encode its equations of motion. In fact, those relations are formulated on the system's space of states by the use of global structures that are kinematical in nature, both in the sense that they are fixed once and for all for all systems of the relevant species and, even more importantly, in the sense that they encode kinematical constraints themselves. Those structures, moreover, are exactly what one uses to formulate the equations of motion for the system. One familiar example of such a global kinematical structure is the canonical symplectic structure on the space of states of a Hamiltonian system, which is encoded in the Poisson-bracket structure on the family of all possible Hamiltonian flows for the system.[28] That structure, among other things, encodes the kinematical constraint of

[26]In fact, I think a stronger claim is true: because judgments of soundness are pragmatic to the core, accuracy is not a semantic property of the model at all, but is rather a meta-linguistic property of the model's semantic content (*i.e.*, of the model's propriety). I do not have room to go into these matters here.

[27]One of the virtues of my account is that it brings to light a serious problem with traditional confirmation theory—a prejudice inherited from Popper—that any violation of theory is viewed as a disconfirmation. But that is deeply wrong, from an epistemological standpoint. Often, an experimental violation rather serves only to show the boundary of the theory's regime of applicability. This comes out clearly on my view of semantics.

[28]See Curiel (2014) for a discussion of the example and proof of the claims made below.

conservation of energy and that of the Poission-bracket commutation relations that hold among any complete set of canonical variables, configuration and momentum, that characterize the system; that is part of the physical significance of the symplectic structure, part of the meaning that an adequate semantics of Hamiltonian mechanics should attribute to it. It is also the structure that one uses to formulate Hamilton's equation itself. In a very real sense, therefore, one cannot check the validity of the Hamiltonian equations of motion—for one cannot even formulate them—unless one is already in a position to verify that those kinematical constraints hold, which is to say, unless one already knows how to define the symplectic structure on the space of states. Knowing how to define the symplectic structure, however, and being able to verify that the kinematical constraints hold, does not imply that the system's evolution satisfies the Hamiltonian equations of motion.

To take another example, consider a generic Newtonian system, *i.e.*, one whose motion satisfies Newton's Second Law; it is simple to show that the family of all vector fields representing possible dynamical evolutions on the system's space of states naturally accrues the structure of an affine space, modeled on the vector space of vector fields representing all possible interactions the system can enter into with other systems (all possible couplings to its environment).[29] That affine structure precisely encodes the kinematical constraint that the system's velocity-like quantities (*e.g.*, ordinary velocity for a Newtonian particle or angular velocity of a planet in its orbit) are dynamical derivatives of its configuration-like quantities (in this case, the particle's spatial or planet's angular position). As in the Hamiltonian case, it is also the case here that this global kinematical structure is required for the formulation of the system's equations of motion. When one constructs the representation of the system in the Lagrangian framework, it turns out that the affine-space structure of the space of solutions to Newton's Second Law suffices to characterize on the tangent bundle of configuration space a tensor field—the almost-tangent structure—that is the geometrical entity the Euler-Lagrange equation is constructed out of, just as the symplectic structure is for Hamilton's equation. Once again, then, it is the case that one must know how to define a global kinematical structure, one that encodes fundamental kinematical constraints, before one is even in a position to construct the equations of motion much more to check to see whether or not they are satisfied.

Such global kinematical structures have manifest physical significance, and so should be treated by a semantics for the theories comprising them. We need these structures in order to be able to formulate the equations of motion, even if the equations of motion are not adequately satisfied—we need to be able to formulate the equations in order to check whether or not they are in the first place. And so we must know the meaning of those global structures in order to check whether the equations are satisfied. This shows that satisfaction of at least some of the kinematical constraints is a necessary condition for the formulation, and *a fortiori* for the satisfacton, of the equations of motion. This provides further reason why priority must be given to kinematical constraints in a theory's semantics.

Still, one may think that traditional accounts of semantics should be able to accommodate the point of view I advocate, perhaps after some minor revision.[30] On the face of it this should be feasible,

[29] Again, see Curiel (2014) for a discussion of this example and proof of the claims made here.

[30] I thank Miklos Redei and Clark Glymour for pushing me on this point.

since my program still does ground its fundamental terms on the accuracy of a set of relations. In this case, the accuracy is that of the kinematical constraints of a theory, not its equations of motion, but surely that makes no nevermind. This misses the profound semantical difference between kinematical constraints and equations of motion, however. As I have stressed, a theory requires the satisfaction of its kinematical constraints as a precondition for its own applicability; it does *not* predict them, as it does the results of its equations of motion. To show the profound difference this makes from a logical point of view, consider an attempt to capture the program I propose in the framework of Tarskian semantics. The kinematical constraints, as constant semantical content common to all models—something like Carnapian *L*-sentences, but with non-trivial semantic content—, should be fixed as part of the initial interpretative stage, in which the designata of the elements of the syntax are given. The fixing of the interpretation of the syntax in a Tarskian semantics, however, involves only the fixing of the designation of constants and predicates and the fixing of the range of bound variables, *etc.*; my conception of propriety, however—that the kinematical constraints be identically satisfied—demands the satisfaction of actual propositions, which goes far beyond the scope of giving a standard interpretation to a syntactical system, and so nothing like a traditional Tarskian semantics can be used to formalize the sort of semantics I advocate. More fundamentally, there is the fact that nothing like a Tarskian semantics can accommodate models in which some of the propositions are false, such as those of systems that are in a theory's regime of propriety but not its regime of adequacy.

Before moving on, this is a good place to remark on another type of global structure that has semantical content that can be accommodated by the kind of account I advocate here, but cannot be so accommodated in any straightforward way by the semantical view of theories. It is tempting to think of the regime of adequacy as something like the set of the theory's Tarskian models, but that will not do. Entire families of models (classes of solutions to the equations of motion) may have on their own semantic content that forms part of the semantic content of the theory, but which cannot be expressed in terms of the semantic content of individual models. For example, the claim that the equations of motion have a well set initial-value formulation in the sense of Hadamard indubitably informs part of a theory's semantic content, but it is one that, in its essence, consists of relations among models and cannot be reduced to the interpretation of a single model. *A fortiori*, that semantic content cannot be captured by a Tarskian semantics, which by definition excludes the possibility that semantic content resides in relations among models.

6 Dynamical Propriety and Meaning

In the opening sentences of his posthumously published essay "Thought", Frege remarks that, as the concept "good" shows the way in ethics and "beautiful" in æsthetics, so must "true" in logic. I am not entirely sure what he meant, but I think it was something related to this: we must look, in deductive logic, for forms of reasoning that preserve truth in moving from antecedents to consequents. In the same vein, I say that "propriety" shows the way in the semantics of scientific theories: in order to explicate the notion of meaning in terms of propriety, we must find forms of reasoning that preserve propriety from initial propositions in a model to derived propositions in the model, and, more generally, that preserve propriety from initial models to other models derived from them.

The notion of propriety I have so far treated places only weak constraints on the acceptability of a theory for modeling phenomena. That a model has kinematical propriety for representing a system signifies, as its name suggests, only that, so far as the resources of the framework are concerned, it is possible that a system exists whose kinematics the model represents with propriety. Nothing I have said about it so far implies any restriction on the propriety of the dynamics of a model. We want a fuller-blooded notion of propriety to complete our semantics, one that expresses the fact that the framework represents with propriety both a system's kinematics and its dynamics, whatever that may come to for dynamics—in other words, we want a notion of propriety that captures the semantic content that does accrue to the model in virtue of the fact that it is accurate. In the event, the characterization of a form of reasoning that preserves propriety in the derivation of propositions and models from others will at the same time illuminate the character of the richer form of propriety we require, as the investigation of logical deduction at the end of the nineteenth and beginning of the twentieth centuries led to new ways to comprehend the semantic character of truth.

Now, one cannot ask for a clearer, conciser summation of what a good, accurate model in science consists of than that given by Hertz (1899, intro., p. 1):

> We form for ourselves images or symbols of external objects; and the form which we give them is such that the necessary consequents of the images in thought are always the images of the necessary consequents in nature of the things pictured.

Appropriate, accurate models, in other words, represent physical systems, albeit with this peculiar proviso: the construction of representations of the system in the theory's terms at a moment *commutes* with the physical evolution of the system as determined by experimental measurement. Say that one first constructs an appropriate model of a physical system, constructs a proposition in that model to designate the state of the system at a given moment and then represents the dynamical evolution of the system starting from that initial state by the application of the model's equations of motion to the proposition so as to yield a proposition designating the same system after, say, five minutes; next one prepares an actual system of that type in the initial state at issue, lets the system evolve for five minutes from that initial state, and then, by the use of experimental observations on the system in that final state to determine the values of the system's quantities, constructs in the theory's terms a proposition that designates the final state. Then, if the model represents the system with *dynamical propriety*, as I will call it, the proposition derived by the former, theoretical procedure will be in some important sense the same as the proposition derived by the latter, experimental procedure.[31] Of course, these two procedures will never yield quantitatively identical

[31] If one likes, a dynamically appropriate model is a "functor" from "the category of scientific representations of a class of physical systems" to "the category of a class of physical systems", where the "objects" of the first are states of a system as modeled in the given representation, and those of the second are the physical states of the system, and the "morphisms" of the first are the rules of derivation the representation makes available, and those of the second

results, so what is to count as "the same"? The argument of the paper points to a ready answer: the two procedures must yield representations that are the same in the sense that they are both propositions, in the same appropriate model, that represent with kinematical propriety the same state of the same physical system, to within the model's admissible inaccuracy. They are appropriate, adequate descriptions of the same single state of the system. It follows that the theory must model the system with kinematical propriety at every stage of its evolution as well, to ensure that the local kinematical constraints are satisfied and so the quantities well defined, if there is to be nothing physically or theoretically special or distinguished about the moments one chooses to make one's determinations.

This discussion suggests an explication for the idea of dynamical propriety. Harking back to our brief discussion of velocity in §3, the point of the proposition that vectorial addition appropriately represents the physical addition of velocities is not that physically and mathematically adding the velocities of the cue balls always yields the same answer quantitatively—we do not require that the evolved system and its derived representation in the model be the same up to a fixed, desired degree of accuracy. We rather demand that the two commute in the sense that the same kinematical structure still accrues to them, within a given kinematical regime—they both yield a representation the same with respect to the potential to have accuracy, even if the model's regime of adequacy does not comprise the system. The framework continues to embody all the same structures as are manifested in the phenomena over the course of the system's evolution. The kinematics and the dynamics in their entirety must mutually respect each other: the semantic designation of the values of the system's quantities and all the allowed theoretical combinations of the designations of these values (*e.g.*, the algebraic addition of one with another), as semantic operations, commute with all physical combinations of the fields and the measurement of the relevant quantities as physical operations modeled by the framework. The order in which one performs the physical and the semantic operations and evaluates the physical and the semantic attributes and relations, whether one mixes them or performs them sequentially, does not affect the representation one produces of the final state of the system. A model for which this is true has *dynamical propriety.* The straightforward continuation of the discussion in §4 shows that a theory can have dynamical propriety in modeling a system even when the system is not in the theory's regime of adequacy, for dynamical propriety requires the framework's kinematical constraints be satisfied over finite stretches of the system's dynamical evolutions, which does not by itself imply that the system's behavior satisfies the framework's equations of motion. (Obviously, the system must be in the framework's regime of kinematical propriety.) Thus I add to the list on page 13 another regime coming after that of kinematical propriety and before that of adequacy, *viz.*, that of dynamical propriety, to construct the complete list of all semantically relevant regimes a framework possesses:

regime of impropriety all systems for which not all local kinematical constraints are satisfied: the theory's quantities are not explicatively defined in the context of the framework; *a fortiori*

are the actual dynamic evolutions of the physical systems. This idea provides the basis for one way to make precise the idea of designation in the context of a semantics of the sort I propose.

the equations of motion cannot even be formulated

regime of kinematical propriety all systems for which all the local kinematical constraints are satisfied: all the theory's quantities are well defined in individual states; the equations of motion can be formulated but they are not satisfied

regime of dynamical propriety all systems for which all kinematical constraints are satisfied in the strong sense that theoretical representation commutes with experimental determination over the course of finite stretches of dynamical evolutions: all quantities are well defined over the course of dynamical evolutions, and the equations of motion can be formulated but they are not satisfied[32]

regime of adequacy all systems for which everything is well defined and satisfied: a state of harmony and bliss in which systems and theories move together hand in hand with the equanimity of the blessèd gods

In conclusion, I give the briefest of sketches of a way to explicate meaning in the account I have developed. The semantics I propose bases the meaning of structures and terms in a scientific framework on the propriety of the representation of systems by models in the framework, and, more specifically, on the conditions under which a model represents a (type of) system with, respectively, each of the two kinds of propriety, kinematical and dynamical: *we know the meaning of a model when we know the conditions under which it represents systems with kinematical and dynamical propriety.* It is illuminating to compare Carnap's (1942, ch. B, §7, p. 22) characterization of meaning, with which I began the paper: "...to understand a sentence, to know what is asserted by it, is the same as to know under what conditions it would be true." For me, the meaning is the same as to know under what conditions it is sensical to investigate the formulation of possible conditions of its truth, for this can be done only in so far as one already knows the theory represents the system with propriety. The problem of meaning now becomes: given a model in a framework, how does one determine its conditions of propriety? From a pragmatic point of view, to know those conditions is the same as to know the family of systems the model represents with propriety, which is to say, those systems the model, as a semantic element of the framework, designates. This is the solution to our problem, stated in experimental terms.[33]

[32] It may happen for some frameworks that there is no difference in extension between the regimes of kinematical and dynamical propriety.

[33] I do not have room to discuss the matter here, but I remark in passing that the account of propriety I have given here carries over intact to the semantical analysis of at least some quantum theories. For example, the non-commutative algebraic structure manifested in our experimental comprehension of the organization of and relations among the spectral lines of Hydrogen as represented in experimental models such as those used by Rydberg and encapsulated in the Ritz Principle illustrates the point, for the representation of those phenomena by the models quantum theory constructs in the terms of the non-commutative algebra of Hermitian operators on a Hilbert space has both kinematical and dynamical propriety in my sense. See Bohr (1954) for discussion. The interested reader should also consult Connes (1994, ch. 1, §§1–2, pp. 33–43) for an exposition of closely related points (though he does not mean to address issues of the sort I do).

7 From Pragmatics to Semantics

Now that I have sketched the semantics, it is time to take a step back to make explicit its relation to pragmatics, how it is grounded upon our knowledge of how to apply theories and frameworks in actual practice.[34] This will pay the promissory note I drew in §2.

If I were developing a semantics that at bottom grounded semantic content on something like truth as characterized by predictive accuracy, I could at this point call upon Stalnaker's admirably elegant and succinct summation of the appropriate relation between pragmatics and semantics in such systems (1981, pp. 44–45, emphases mine):

> Now that we have found an answer to the question, "How do we decide whether or not we believe a . . . statement?" the problem is to make the transition from belief conditions to truth conditions; that is, to find a set of truth conditions for statements . . . *which explains why we use the method we do use to evaluate them.*

In other words, the relation of pragmatics and semantics in a system that grounds meaning in truth conditions should reflect the relation between epistemology and metaphysics: one's formal truth conditions (semantics by way of metaphysics, as truth conditions ought not depend on the context of the individual knower) should relate in the appropriate way to belief conditions (pragmatics by way of epistemology, as belief conditions depend on the context of the individual, actual believer). This must hold at a minimum if our beliefs are even to have a shot at tracking truth. In a system such as the one I advocate, by contrast, in which meaning cannot be grounded on truth conditions, such as accuracy in prediction, but is rather grounded on the way a framework provides appropriate structures for the representation of a given (type of) system, the relation between pragmatics and semantics should remain silent about the relation between epistemology and metaphysics; in the best of cases, it should be compatible with many possible such relations.

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 adequacy—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, the analogue to Stalnaker's, is how to move from an understanding of how to check whether or not a theory's kinematical constraints are satisfied 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. As Stalnaker says, moreover, our account of this process should "[explain] why we use the method we do use to [verify] them."

Now, as a practical affair, how to verify whether or not the kinematical constraints of a theory hold will differ from system to system, and even from state to state for the same system. The constraints on the definability and measurability of a quantity in a given theory, and in general the understanding of how to check the accuracy of the kinematical relations posited to hold among

[34] See Stein (1994) for an extended and extremely illuminating discussion of the differences between the kinds of theoretical and experimental knowledge that must come into play in the sorts of circumstance I discuss here.

the quantities, depend on the parameters of particular types of systems under certain kinds of conditions; one cannot characterize them generically in an attempt to constrain the definability and measurability of that quantity and that relation once and for all, without qualification. For example, in checking to see whether the conservation of energy as formulated in Navier-Stokes theory (one of its kinematical constraints) holds of a particular body of fluid, one may require the following conditions to hold and make the following posits, *inter alia*:

1. the ambient electromagnetic field cannot be so strong as to ionize the fluid
2. the gradient of the fluid's temperature cannot be too steep near equilibrium
3. only thermometric systems one centimeter in length or longer are to be used to measure the fluid's temperature, and the reading will be taken only after having waited a few seconds for the systems to have settled down to equilibrium
4. the chosen observational techniques to be applied, under the given environmental conditions and in light of the current state of the fluid, yield data with a range of inaccuracy of $\pm 1\%$, with a degree of confidence of 95%
5. a deviance of less than 3% of the predicted from the observed behavior of the system's temperature, taking into account the range of inaccuracy in measurement, is within the admissible range of experimental error for the chosen experimental techniques under the given environmental conditions, in light of the current state of the fluid

Once one is satisfied all such conditions are met and all such posits appropriately implemented, all part of the preparation of the system for experimental study, one can begin measuring the fluid's quantities to see whether conservation of energy as formulated in Navier-Stokes theory holds for the fluid.

Now, the statement of the conservation of energy as formulated in the theory of Navier-Stokes fluids accounts for only a small number of the types of energy a physical system may possess. It does not account, for instance, for radiative energy and certain types of chemical energy. If one throws a chunk of pure sodium into a bucket of water, Navier-Stokes theory cannot model the explosive result and so, *a fortiori*, cannot keep track of the chemical energy released. So far as the theory is concerned, energy in that reaction is not conserved. In order to satisfy the principle of the conservation of energy as encoded in the kinematics of Navier-Stokes theory, the fluid must exist in a condition close enough to thermodynamical and hydrodynamical equilibrium so that the great majority of its free energy exists in a form dependent only on the quantities Navier-Stokes theory makes available for the modeling of its state, *viz.*, the gross fluid velocity, the hydrostatic pressure, the heat flux and the shear and stress distribution in the fluid. In order, therefore, to ascertain whether the relation as formulated by the theory holds, one measures the contributions from all these terms separately, combines those experimentally determined values in the theoretically required way and checks whether or not the resultant number is close enough to zero. If it is, the fluid satisfies conservation of energy as formulated by Navier-Stokes theory. If it does not, then there are other energetic processes occurring in the fluid that the representational resources of Navier-Stokes theory cannot account for, such as an exothermic chemical reaction (the exploding sodium). In this case, because the system does not satisfy the theory's kinematical constraint, Navier-Stokes theory does not have propriety in representing it. This all forms part of the pragmatics of the theory. None of the propositions one formulates during the entire procedure is "representational", for one is still trying to determine whether or not the theory is appropriate for the representation of the fluid in the first place.

Say that one has in fact managed to verify that all the kinematical constraints hold over finite stretches of time. One now knows that Navier-Stokes theory does have propriety in representing the system, since the system is in the theory's regime of dynamical propriety, and so the propositions encoding the kinematical constraints acquire representational force. One has moved from pragmatics to semantics in the application of the theory. Among other consequences of that move, one is now in a position to attempt to ascertain whether or not the Navier-Stokes equations of motion accurately model the fluid's behavior, *i.e.*, whether the system is in the theory's regime of adequacy. Note, however, that, even before that determination is made, the theory does in fact represent the system in a semantically rich and robust sense. If it then turns out that the theory also accurately predicts the dynamical evolution of the fluid, then the theory's representation of the system gains that further semantic content; but even if it does not accurately predict the dynamical evolution of the fluid, I hope it to be clear by now that the theory's terms still meaningfully designate the fluid's quantities and the theory meaningfully represents at least that much of the nature of the system encoded in the relations among those quantities captured by satisfaction of the theory's kinematical constraints, for one now knows the theoretical species of system one is dealing with.

References

Benedict, R. (1969). *Fundamentals of Temperature, Pressure and Flow Measurements*. New York: John Wiley & Sons, Inc.

Bohr, N. (1954). Rydberg's discovery of the spectral laws. In *Proceedings of the Rydberg Centennial Conference on Atomic Spectroscopy*, Volume 50 of *Acta Universitatis Lundensis*, pp. 15–21.

Brading, K. and E. Landry (2006, December). Scientific structuralism: Presentation and representation. *Philosophy of Science 73*, 571–581. Originally delivered as a talk as part of the Symposium "The Semantic View of Theories, Scientific Structuralism and Structural Realism", at the 20th biennial meeting of the Philosophy of Science Association, 2004, Vancouver, BC.

Carnap, R. (1942). *Introduction to Semantics*, Volume I of *Studies in Semantics*. Cambridge, MA: Harvard University Press.

Carnap, R. (1956). Empiricism, semantics and ontology. In *Meaning and Necessity: A Study in Semantics and Modal Logic* (Second ed.)., pp. 205–221. Chicago: The University of Chicago Press. An earlier version was published in *Revue Internationale de Philosophie* 4(1950):20–40.

Carnap, R. (1962). *Logical Foundations of Probability* (Second ed.). Chicago: University of Chicago Press.

Connes, A. (1994). *Non-Commutative Geometry*. San Diego, CA: Academic Press. Based on an English translation of Connes' *Géométrie non Commutative* (InterEditions, Paris, 1990) by S. Berberian, and greatly expanded by Connes.

Curiel, E. (2014). Classical mechanics is Lagrangian; it is not Hamiltonian. *British Journal for the Philosophy of Science*. Forthcoming 2014, advance access online at doi:10.1093/bjps/axs034 (`http://bjps.oxfordjournals.org/content/early/2013/05/11/bjps.axs034.abstract`).

da Costa, N. and S. French (2005). *Science and Partial Truth: A Unitary Approach to Models and Scientific Reasoning*. Oxford: Oxford University Press.

Dreyer, J. (1906). *History of the Planetary Systems from Thales to Kepler*. Cambridge: Cambridge University Press.

Flamsteed, J. (1835). *An Account of the Rev$^{\underline{d}}$ John Flamsteed, the First Astronomer-Royal; Compiled from His Own Manuscripts, and Other Authentic Documents, Never before Published*. London: Lords Commissioners of the Admiralty. F. Baily (Ed.). Reprinted by Dawsons of Pall Mall, London, 1966.

Fraassen, B. v. (2008). *Scientific Representation*. Oxford: Oxford University Press.

Freundlich, E. (1915). Über die Erklärung der Anomalien im Planeten-System durch die Gravitationswirkung interplanetarer Massen. *Astronomische Nachrichten 201*(4803), 49–56.

Geroch, R. (1996). Partial differential equations of physics. In G. Hall and J. Pulham (Eds.), *General Relativity*, Aberdeen, Scotland, pp. 19–60. Scottish Universities Summer School in Physics. Proceedings of the 46th Scottish Universities Summer School in Physics, Aberdeen, July 1995.

Harper, W. (2011). *Isaac Newton's Scientific Method: Turning Data Into Evidence about Gravity and Cosmology*. Oxford: Oxford University Press.

Hertz, H. (1893). *Electric Waves, Being Researches on the Propagation of Electric Action with Finite Velocity through Space*. New York: Dover Press. Trans. D. Jones. Originally published as *Untersuchungen über die Ausbreitung der elektrischen Kraft* (Leipzig: J. A. Barth, 1892). This is a 1962 reprint of the English edition which first appeared in 1893, published by Macmillan and Sons.

Hertz, H. (1899). *The Principles of Mechanics Presented in a New Form*. New York: Dover Press. Trans. D. Jones and J. Walley. Originally published posthumously as *Die Prinzipien der Mechanik in Neuem Zusammenhange Dargestellt* (Leipzig, 1894). This is a reprint of the first English edition which appeared in 1899.

Herz, N. (1887). *Geschichte der Bahnbestimmung von Planeten und Kometen*, Volume I. Leipzig: Teubner.

Herz, N. (1894). *Geschichte der Bahnbestimmung von Planeten und Kometen*, Volume II. Leipzig: Teubner.

Kovalevsky, J. and P. Seidelmann (2004). *Fundamentals of Astrometry*. Cambridge: Cambridge University Press.

Lamb, H. (1932). *Hydrodynamics* (sixth ed.). New York: Dover Publications. The 1945 Dover reprint of the 1932 edition published by the Cambridge University Press.

Landau, L. and E. Lifschitz (1975). *Fluid Mechanics* (Second ed.). Oxford: Pergamon Press. An expanded, revised edition of the original 1959 edition. Translated from the Russian by J. Sykes and W. Reid.

Le Verrier, U. (1845). *Mémoire sur les Variations Séculaires des Éléments des Orbites, pour les Sept Planètes Principales, Mercure, Venus, la Terre, Mars, Jupiter, Saturne, Uranus*. Paris: Bachelier.

Le Verrier, U. (1859). Lettre de M. Le Verrier à M. Faye sur la théorie de Mercure et sur le mouvement du périhélie de cette planète. *Comptes rendus hebdomadaires des séances de l'Académie des sciences (Paris) 49*, 379–383.

Lide, D. (Ed.) (2005). *CRC Handbook of Chemistry and Physics* (86th ed.). Boca Raton, FL: Taylor & Francis.

Maxwell, J. C. (1871). Introductory lecture on experimental physics. See Maxwell (1965), pp. 240–255.

Maxwell, J. C. (1876). General considerations concerning scientific apparatus. See Maxwell (1965), pp. 505–522. Originally published in the *Handbook to the Special Loan Collection of Scientific Apparatus*, 1876, South Kensington Museum, London: Chapman and Hall, 1–21.

Maxwell, J. C. (1965). *The Scientific Papers of J. C. Maxwell*. New York: Dover Publications, Inc. W. Niven (Ed.). Two volumes, published as one.

Newcomb, S. (1895a). *The Elements of the Four Inner Planets and the Fundamental Constants of Astronomy*. Washington, DC: US Government Printing Office. Supplement to the American Ephemeris and Nautical Almanac for 1897.

Newcomb, S. (1895b). On the principal fundamental constants of astronomy. *Astronomical Journal 14*, 185–189.

Newcomb, S. (1905). *A New Determination of the Precessional Constant with the Resulting Precessional Motions*. Washington, D.C.: U.S. Nautical Almanac Office.

Newton, I. (1726). *Philosophiæ Naturalis Principia Mathematica* (Third ed.), Volume II. Berkeley, CA: University of California Press. The translation by A. Motte of the third edition (1726), originally produced in 1729, revised by F. Cajori and published in 1934. The first edition of the *Principia* was published in 1686, the second in 1713.

Ptolemy (1998). *Almagest*. Princeton, NJ: Princeton University Press. Trans. G. Toomer.

Putnam, H. (1975a). How not to talk about meaning. See Putnam (1975d), Chapter 6, pp. 117–131.

Putnam, H. (1975b). Is semantics possible? See Putnam (1975d), Chapter 8, pp. 139–152.

Putnam, H. (1975c). Language and reality. See Putnam (1975d), Chapter 13, pp. 272–290.

Putnam, H. (1975d). *Mind, Language and Reality: Philosophical Papers*, Volume 2. Cambridge: Cambridge University Press.

Putnam, H. (1983a). Introduction: An overview of the problem. See Putnam (1983c), pp. vii–xviii.

Putnam, H. (1983b). Models and reality. See Putnam (1983c), Chapter 1, pp. 1–25.

Putnam, H. (1983c). *Realism and Reason: Philosophical Papers*, Volume 3. Cambridge: Cambridge University Press.

Sommerfeld, A. (1964). *Thermodynamics and Statistical Mechanics*, Volume v of *Lectures on Theoretical Physics*. New York: Academic Press. Trans. J. Kestin. Edited and posthumously completed by F. Bopp and J. Meixner.

Stalnaker, R. (1981). A theory of conditionals. In W. Harper, R. Stalnaker, and G. Pearce (Eds.), *Ifs: Conditionals, Belief, Decision, Chance, and Time*, Volume 15 of *The University of Western Ontario Series in Philosophy of Science*, pp. 41–56. Dordrecht: D. Reidel Publishing Co. First published in *Studies in Logical Theory, American Philosophical Quarterly* Monograph Series, No. 2, Oxford: Blackwell, 1968.

Stein, H. (1992). Was Carnap entirely wrong, after all? *Synthese 93*, 275–295. I do not have access to the published version of Stein's paper, but rather only to a typed manuscript. All references to page numbers, therefore, do not correspond to those of the published version. The typed manuscript I have is about 14 and a half pages long, and the published version about 20. Multiplying the page numbers I give by $\frac{29}{40}$ and adding the result to 275 (the number of the first page in the published version) should give approximately the page number in the published version.

Stein, H. (1994). Some reflections on the structure of our knowledge in physics. In D. Prawitz, B. Skyrms, and D. Westerståhl (Eds.), *Logic, Metholodogy and Philosophy of Science*, Proceedings of the Ninth International Congress of Logic, Methodology and Philosophy of Science, pp. 633–55. New York: Elsevier Science B.V. I do not have access to the published version of Stein's paper, but rather only to a typed manuscript. All references to page numbers, therefore, do not correspond to those of the published version. The typed manuscript I have is about 17 pages long, and the published version about 22. Multiplying the page numbers I give by $\frac{17}{22}$ and adding the result to 633 (the number of the first page in the published version) should give approximately the page number in the published version.

Suppe, F. (1974). The search for philosophic understanding of scientific theories. In F. Suppe (Ed.), *The Structure of Scientific Theories*, pp. 3–254. Chicago: University of Illinois Press.