

On the Formal Consistency of Theory and Experiment, with
Applications to Problems in the Initial-Value Formulation of
the Partial-Differential Equations of Mathematical Physics[†]

DRAFT

Erik Curiel[‡]

June 9, 2011

[†]This paper is a corrected and clarified version of Curiel (2005), which itself is in the main a distillation of the much lengthier, more detailed and more technical Curiel (2004). I make frequent reference to that paper throughout this one, primarily for the statement, elaboration and proof of technical results omitted here.

[‡]I would like to thank someone for help with this paper, but I can't.

Abstract

The dispute over the viability of various theories of relativistic, dissipative fluids is analyzed. The focus of the dispute is identified as the question of determining what it means for a theory to be applicable to a given type of physical system under given conditions. The idea of a physical theory's *regime of propriety* is introduced, in an attempt to clarify the issue, along with the construction of a formal model trying to make the idea precise. This construction involves a novel generalization of the idea of a field on spacetime, as well as a novel method of approximating the solutions to partial-differential equations on relativistic spacetimes in a way that tries to account for the peculiar needs of the interface between the exact structures of mathematical physics and the inexact data of experimental physics in a relativistically invariant way. It is argued, on the basis of these constructions, that the idea of a regime of propriety plays a central role in attempts to understand the semantical relations between theoretical and experimental knowledge of the physical world in general, and in particular in attempts to explain what it may mean to claim that a physical theory models or represents a kind of physical system. This discussion necessitates an examination of the initial-value formulation of the partial-differential equations of mathematical physics, which suggests a natural set of conditions—by no means meant to be canonical or exhaustive—one may require a mathematical structure, in conjunction with a set of physical postulates, satisfy in order to count as a physical theory. Based on the novel approximating methods developed for solving partial-differential equations on a relativistic spacetime by finite-difference methods, a technical result concerning a peculiar form of theoretical under-determination is proved, along with a technical result purporting to demonstrate a necessary condition for the self-consistency of a physical theory.

Contents

1	Introduction	4
2	Relativistic Formulations of the Navier-Stokes Equations	10
2.1	The Three Forms of Partial-differential Equation	10
2.2	Parabolic Theories and Their Problems	10
2.3	Hyperbolic Theories	11
2.4	The Breakdown of Partial-Differential Equations as Models in Physics	12
3	The Kinematical Regime of a Physical Theory	15
3.1	Kinematics and Dynamics	17
3.2	Constraints on the Measure of Spatiotemporal Intervals	19
3.3	Infimal Decoupages	23
3.4	The Kinematical Regime	25
4	Physical Fields	30
4.1	Algebraic Operations on the Values of Quantities Treated by a Physical Theory	31
4.2	Inexact Scalars	37
4.3	Algebraic Operations on Inexact Scalars	41
4.4	Inexact Scalar Fields and Their Derivations	51
4.5	Inexact Tensorial Fields and Their Derivations	58
4.6	Inexactly Linear Operators	65
4.7	Integrals and Topologies	67
4.8	Motleys	71
5	Physical Theories	73
5.1	Exact Theories with Regimes and Inexact, Mottled, Kinematically Constrained Theories	75
5.2	Idealization and Approximation	83
5.3	An Inexact, Well Set Initial-Value Formulation	92
5.4	A Physically Well Set Initial-Value Formulation	97
5.5	Maxwell-Boltzmann Theories	101
5.6	The Consistency of Theory and Experiment	105
6	The Soundness of Physical Theory	106
6.1	The Comparison of Predicted and Observed Values	106
6.2	Consistent Maxwell-Boltzmann Theories	110
6.3	The Dynamical Soundness of a Physical Theory	112
6.4	Theoretical Under-Determination	114
7	The Theory Is and Is Not the Equations	116

It is a capital mistake to theorize before one has data.

Sir Arthur Conan Doyle
“A Scandal in Bohemia”

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

Yogi Berra

Theory without experiment is philosophy.

Allison Myers

1 Introduction

In this paper, I intend to investigate a series of questions on the complex interplay between the theoretician and the experimentalist required for a mathematical theory to find application in modeling actual experiments and, in turn, for the results of those experiments to have bearing on the shaping and substantiation of a theory. On the one hand, we have the rigorous, exact and often beautiful mathematical structures of theoretical physics for the schematic representation of the possible states and courses of dynamical evolution of physical systems.¹ On the other hand, we have the intuitive, inexact and often profoundly insightful design and manipulation of experimental apparatus in the gathering of empirical data, in conjunction with the initial imposition of a classificatory structure on the mass of otherwise disaggregated and undifferentiated raw data gathered. Somewhere in between these extremes lie the mutual application to and qualification of each by the other.

It is one of the games of the experimentalist to decide what theory to play with, indeed, what parts of what theory to play with, in planning experiments and designing instruments for them and modeling any particular experimental or observational arrangements, in light of, *inter alia*, the conditions under which the experiment will be performed or the observation made, the degree of accuracy expected or desired of the measurements, *etc.*, and then to infer in some way or other from the exact, rigorous structure of that theory, as provided by the theoretician, models of actual experiments so that he may explicate the properties of types of physical systems, produce predictions about the behavior of those types of systems in particular circumstances, and judge whether or not these predictions, based on the schematic models constructed in the framework of the theory, conform to the inaccurately determined data he gathers from those experiments. It is one of the games

¹I follow the discussion of Stein (1994) here in my intended use of the term *schematic* to describe the way experiments are modeled in physics. That paper served as much of the inspiration for the questions I address in this paper, as well as for many of the ways I attempt to address the questions. Besides to that paper, I owe explicit debts of gratitude for inspiration to Geroch (2001), Stein (npub, 1972, 2004), with all of which, I hope, this paper has affinities, in both method and conclusions.

Theory and Experiment

of the theoretician to abduct exact, rigorous theories from the inaccurately determined, loosely organized mass of data provided by the experimentalist, and then to articulate the rules of play for those theories, by, *inter alia*, articulating the expected kinds and strengths of couplings the quantities of the theory manifest and the conditions under which they are manifested, leaving it to the experimentalist to design in light of this information probes of a sort appropriate to these couplings as manifested under the particular conditions of experiments. Jointly, the two try to find, in the physical world, common ground on which their games may be played. No matter what one thinks of the status of these sorts of decisions and articulations in science—whether one thinks they can ultimately be explained and justified in the terms of a rational scientific methodology or whether one thinks they are, in the end, immune to rational analysis and form the incorrigibly asystematic bed-rock of science, as it were—it behooves us, at the least, to get clearer on what is being decided and articulated, and on how those decisions and articulations bear on each other, if, indeed, they do at all .

I will not examine the actual play of any current or historical theoreticians and experimentalists in their attempts to find common, mutually fruitful ground on which to engage each other. I leave those issues, fascinating as they are, to other, more competent hands. Neither will I examine all the different sorts of games in which they engage in their respective practices, rather treating only those played in one small part of their common playground, that having to do with the comparison of predicted and observed values of a system as it dynamically evolves for the purposes of testing and substantiating a theory on the one hand, and refining experimental methods and design on the other. [*** For this latter, *cf.* the suggestion by Lee and Yang of the experiments that showed violation of parity; differentiate these more explicitly from the construction of theoretical models that only use well-founded theory to predict, with no thought of substantiation, such as planning the moon-shots ***]. I do not deal explicitly with others, such as predictions that have nothing to do with comparison to observations (for instance, the use of Newtonian gravity in calculating trajectories during the Apollo project’s flights to the Moon), or the calculation of fundamental properties of physical systems based on theoretical models (for instance, the use of the quantum theory of solids to calculate the specific heat of a substance). [*** Distinguish “comparison to observation” from “use of observation” in these examples—for in the moon-shots they surely also compared the observed results of previous moon-shots to, among other things, refine their methods of prediction and characterization for future ones ***]. The extension of the methods and arguments of this paper to those and other practices strikes me as straightforward, but the proof is in the pudding, which I do not serve here, and have, indeed, not thought much about preparing, so I will say nothing more about it.

I will examine in this paper only what one may think of as the logical structure of the relations between the practice of the theoretician and that of the experimentalist, and, *a fortiori*, of those between theory and experiment. I do not mean to claim that there is or ought to be a single such structure *sub specie aeternitatis*, or indeed that there is any such structure common to different branches of physics, or indeed even one common to a single branch that remains stable and viable over arbitrary periods of time, in different stages of the scientific enterprise. I intend to investigate

Theory and Experiment

only whether one can construct such a structure to represent some idealized form of these relations. I am not, in this paper, interested in how exactly the experimentalist and the theoretician may make in practice the transitions to and fro between, on the one hand, inaccurate and finitely determined measurements, and, on the other, the mathematically rigorous initial-value formulation of a system of partial-differential equations, whether their exact methods of doing so may be justified, *etc.* I am rather concerned with the brute fact of its happening, whether there is indeed any way at all of constructing with some rigor and clarity a model of generic methods for doing so. Having such a model in hand would show that there need be no gross logical or methodological inconsistency in their joint practice (even if there is an inconsistency in the way physicists currently work, which I would not pretend to hazard a guess at). Indeed, it is difficult to see, on the face of it, how one may comprehend these two to be engaged in the same enterprise in the first place, difficult, indeed, to see even whether these two practices are in *any* sense consistent with each other, since it is not even clear what such consistency may or may not consist of.² While I seriously doubt that any formal analysis of the relations between theory and practice I or anyone else may propose could answer this question definitively with regard to a real physical theory and its experimental applications, the sort of analysis I attempt to outline here, if successful, would perhaps have the virtue of underlining the sorts of considerations one must take account of in judging the consistency of a real theory and its application to the world. This may seem a Quixotic project, at best, on the face of it, but I think I can say a few words in defense of its interest in the remainder of the introduction. In defense of its feasibility, I offer the paper itself.

Without a doubt, one can learn an extraordinary amount about a physical theory (and about the world) by examining only its structure in isolation from the conditions required for its use in modeling phenomena, as is most often done in philosophical discussion of a technical nature about physical theories in particular, and about the character of our understanding of the physical world in general. I will argue, however, mostly by example, that comprehensive understanding of a physical theory will elude us unless we examine as well the procedures whereby it is employed in the laboratory, and, moreover, that comprehension of the nature of such knowledge as we may have of the physical world will similarly elude us without a serious attempt to understand both the theoretical and the practical characters of that knowledge. In particular, the question I plan to

²Indeed, in my use of the terms ‘experimentalist’ and ‘theoretician’ throughout this paper, I am guilty of perpetuating the crudest of caricatures—as though the two lived in separate worlds, and had to travel some distance and overcome great obstacles even to meet each other. Physicists such as Newton and Fermi, masters of both theory and experiment, give my caricature the lie. Still, there is a grain of truth in the caricature—which is to say, that it *is* a caricature, and so *a fortiori* it does strike home somewhat. When I was at the Relativity Group in the Fermi Institute at the University of Chicago, the other graduate students and I used to say, only half jokingly, that other groups of theoreticians—those studying quantum field theory, or solid-state physics, for example—spoke a different language than the one we spoke, and one had to work hard at translation to avoid a complete breakdown of communication. One may extrapolate our feelings about experimentalists: “if experimentalists could speak, we would not understand them.” (To which the experimentalist replied, “When you speak, I cannot understand you!”) [*** Even those physicists whose research focuses on one to the exclusion of the other yet share much in common—ways of thought, methods of argumentation, standards of proof, funds of knowledge, overarching goals—with those on the other side of the aisle. ***].

address is not how one gets to a system of exact partial-differential equations from inaccurate data; nor is it how one gets from exact solutions of partial-differential equations to predictions that may or may not accord with actually observed, inaccurate data (though this latter will be touched upon *en passant* to some degree). It is rather a question of the consistency of, perhaps the continuity between, the two—a question, if you like, of whether the theoretician and the experimentalist can be understood as being engaged in the *same enterprise*,³ that of modeling and comprehending the physical world, in complementary, indeed mutually supportive, ways. Another way of putting the point: philosophers, when having tried to understand the relation between theory and experiment, tend to have been vexed by the problem of how a theory gets into (and out of!) the laboratory, often framed in terms of the putatively inevitable “theory-ladenness” of observations; I am concerned here with what one may call the converse problem, that of getting the laboratory into the theory, and the joint problem, as it were, whether the theory and the laboratory admit at least in part a consistent, common model. Along the way, I will present an argument, in large part constituted by the body of the construction itself, that the initial-value formulation of the partial-differential equations of a theory provides the most natural theater in which this sort of investigation can play itself out. Later in the paper, after the construction has been sketched, I will have more to say explicitly on the privilege, as I see it, accruing to the role of the initial-value formulation in the comprehension of physical theory.

I will focus the discussion around the idea of the *regime of propriety*⁴ of a physical theory (or *physical regime* or just *regime*, for short). From a purely extensive point of view, a regime of a physical theory, roughly speaking, consists of the class of all physical systems *cum* environments that the theory is adequate and appropriate for the modeling of,⁵ along with a mathematical structure used to construct models of these systems, and a set of experimental techniques used for probing the systems in a way amenable to modeling in the terms of that structure. It can be represented by, at a minimum

1. a set of variables representing physical quantities (“the environment”) not directly treated by the theory but whose values in a given neighborhood are relevant to the issue of the theory’s propriety for use in modeling a particular physical system in that neighborhood, along with a set of algebraic and differential expressions formulated in terms of these variables, representing the constraints these ambient, environmental quantities must satisfy in order for physical systems of the given type to be susceptible to treatment by the theory when they appear in such environments
2. a set of algebraic and differential expressions formulated in terms of the variables and constants appearing in the theory’s system of partial-differential equations, representing the constraints the values of the quantities represented by those constants and variables must satisfy in order for the system bearing those quantities to be amenable to treatment by the theory; these

³I model use of the word *enterprise* in this paper on its use in Stein (2004).

⁴I owe this term to Geroch (2001).

⁵It is immaterial to my arguments whether one considers the class to comprise only existant systems or to comprise as well possible systems, in whatever sense one wishes to give the modal term.

Theory and Experiment

expressions may include as well terms from the set of variables representing relevant environmental quantities (“kinematical constraints”)

3. a set of algebraic expressions formulated in terms of variables representing the measure of spatiotemporal intervals, constraining the character of the spatiotemporal regions requisite for well-defined observations of the system’s quantities to be performed in; these expressions may include terms from the set of variables representing relevant environment quantities, as well as from the set of variables and constants appearing in the theory’s system of partial-differential equations (“constraints on the system’s characteristic spatiotemporal scales”)
4. a set of methods for calculating the ranges of inaccuracy inevitably accruing to measurements of the values of the system’s quantities treated by the theory, depending on the sorts of experimental techniques used for probing the system, the environmental conditions under which the probing is performed, and the state of the system itself (including the stage of dynamical evolution it manifests) at the time of the probing—these methods may include, *e.g.*, a set of algebraic and differential expressions formulated in terms of the variables and constants appearing in the theory’s system of partial-differential equations, the variables representing the relevant environmental factors, and the variables representing the measure of spatiotemporal intervals
5. a set of methods for calculating the ranges of admissible deviance of the predictions of the theory on the one hand from actual measurements made of particular systems modeled by the theory on the other, depending on the sorts of experimental techniques used for probing the system, the environmental conditions under which the probing is performed, and the state of the system itself (including the stage of dynamical evolution it manifests) at the time of the probing—these methods may include, *e.g.*, a set of algebraic and differential conditions formulated in terms of the variables and constants appearing in the theory’s system of partial-differential equations, the variables representing the relevant environmental factors, and the variables representing the measure of spatiotemporal intervals

The idea of a regime is perhaps best illustrated by way of an example. For the theory comprising the classical Navier-Stokes equations to model adequately a particular body of fluid, for instance, elements of its regime may include these conditions and posits:

1. the ambient electromagnetic field cannot be so strong as to ionize the fluid completely
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%

Theory and Experiment

5. a deviance of less than 3% of the predicted from the observed dynamic evolution 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

I neither promise nor threaten to offer in this paper a definitive analysis of the concept of a regime or indeed of any of its constituents. I will rather sketch one possible way one may construct a (moderately) precise and rigorous model of the concept, with the aim of illuminating the sorts of questions one would have to answer in order to provide a more definitive analysis. The hope is that such a model and correlative demonstration may serve as a constructive proof of the formal consistency of the practice of the experimentalist and the practice of the theoretician in physics, indeed, as a construction of the common playground, as it were, of the two, playing with the toys and rides and games of which we may pose precise questions of a technical nature about the interplay between theory and experiment, and attempt to answer such questions, at least in so far as one accepts the viability of the sort of formal model I construct.

The structure of the paper is as follows. In order to illustrate the sort of considerations that motivate and found my proposed definition and analysis of the idea of a regime, I begin, in §2, by briefly analyzing the dispute over hyperbolic reformulations of the theory of relativistic Navier-Stokes fluids, as the dispute illuminates the issues nicely. The points drawn from this analysis lead naturally into the introduction in §3 of the notion of a regime, and the sketch of a construction of a formal model purporting to represent the notion. In §4, I offer a mildly technical analysis of the mathematical representation appropriate for the modeling of physical fields by theories with regimes, necessary for the culmination of the paper in §5, in which I analyze the initial-value formulation of the partial-differential equations of theoretical physics (as opposed to that of those in pure mathematics) based on my analysis of the idea a regime, and draw several consequences from the analysis, and in §6, in which I discuss the criteria one may want to demand a theory satisfy in order for it to be thought empirically adequate. One of the most interesting of the results of this discussion describes a peculiar form of theoretical under-determination necessarily attendant on a physical theory, in so far as the theory possesses a regime in the idealized sense proposed in this paper.

For the most part, I will deal only with the case of the interaction of theory and experiment when both the theoretical structures and the experimental practices are well worked out and well understood; the investigation of those relations when one is dealing with novel theory, novel experiments, or both, presents far too many difficult and unavoidable questions for me to treat with any adequacy or depth here.

The entire paper, if you will, may be considered an exercise in approximation and idealization in the philosophy of physics in the attempt to work out part of its regime of propriety.

2 Relativistic Formulations of the Navier-Stokes Equations

2.1 The Three Forms of Partial-differential Equation

[*** Briefly characterize the elliptic, parabolic and hyperbolic forms of partial-differential equation, *à la* Sommerfeld (1964). Define ‘hyperbolization’ of an elliptic or parabolic system of equations. ***]

2.2 Parabolic Theories and Their Problems

It is sometimes held that parabolic systems of partial-differential equations, such as the Navier-Stokes system or Fourier’s equation of thermal diffusion, do not have well set initial-value formulations.⁶ This, of course, depends on one’s formulation of the idea of an initial-value formulation. The following is known, for example, about the Navier-Stokes system in non-relativistic physics (Temam 1983, *passim*):

1. for appropriate initial data on a 3-space of absolute simultaneity, say $t = 0$, there exists a $0 < \tau < \infty$ such that there is a unique, regular solution in the interval $[0, \tau)$
2. for appropriate initial data, a distributional solution exists for all future time and, in the two-dimensional case at least, this solution is unique

Whether global distributional solutions for the three-dimensional case are unique is apparently not known. Leray (1934) conjectured that global uniqueness does not hold (though, of course, he did not phrase this conjecture in the language of distributions), arguing that the break-down in uniqueness is associated with the onset of turbulence, which, he held, is not representable by the Navier-Stokes equations. Recently, Ruelle (1981) has attempted to argue that global uniqueness does hold, and that the onset of turbulence should rather be associated with the existence of strange attractors in the phase space of the Navier-Stokes system. So far as I know, no firm conclusions either way are known, and, in any event, this issue has not been treated in the context of relativity to any comparable depth.

What is indisputable is that, in parabolic systems, roughly speaking, although the solutions to boundary-value problems vary continuously with the specified boundary-values, perturbations in initial conditions can propagate with unbounded velocities in initial-value problems. In other words, there is no guarantee that the solutions to parabolic systems will not violate the causal strictures of relativity theory, no matter how exactly one poses those strictures. This observation underlies the sense theoretical physicists have of the inadequacy of parabolic partial-differential equations. Another, related problem with them involves the stability of their solutions. In the particular case of the relativistic (parabolic) Navier-Stokes system as formulated by Landau and Lifschitz (1975), for example, Hiscock and Lindblom (1985) found a solution that grows exponentially over microscopic time-scales in any coordinate system in which the representation of the net momentum-flux of the

⁶See, *e.g.*, Geroch (2001).

fluid is not zero.⁷ Kostädt and Liu (2000) have disputed the admissibility of this solution, claiming that it arises from an ill-set initial-value formulation. They conclude that Landau and Lifschitz’s parabolic formulation is in fact viable as a mathematical representation of a physical theory, at least in so far as such objections go.

These discussions and arguments are exemplary of the problems faced by theoreticians when attempting to model novel systems, or systems that can be investigated only with great difficulty. Of particular relevance for our study is the focus on whether or not the initial-value formulation of the partial-differential equations of a theory is well set or not. This notion will play an indispensable role in the characterizations we offer, in §5, of a regime of propriety and a theory possessing one.

2.3 Hyperbolic Theories

While the problems mentioned in §2.2 have served as stimulus for finding a hyperbolic extension of the relativistic Navier-Stokes system, in the attempt it was suggested that there may be two other perhaps even stronger reasons to find a viable such extension. In particular, it was suggested that, contrary to early assumptions, the hyperbolic theories might produce predictions differing from those of the parabolic system in certain tightly constrained circumstances in which both were applicable, offering the possibility of experimental tests of the hyperbolic systems.⁸ Even more enticingly, it was suggested that the hyperbolic theories could be applied in circumstances in which the parabolic system becomes in one way or another inapplicable. I will briefly discuss how it is hoped that the hyperbolic systems may resolve the problems mentioned in §2.2, but my primary focus for the majority of the section will be on the two novel suggestions just mentioned.⁹

In order to discuss these issues further, it will be convenient to be more precise than there has yet been call for. Fix a relativistic spacetime¹⁰ (\mathcal{M}, g_{ab}) . Then a *relativistic Navier-Stokes fluid* (or just *Navier-Stokes fluid*, when there is no ambiguity) is a physical system such that:

1. its local state is completely characterized by the set of dynamical variables representing the

⁷Their solution has its origin in the fact that Landau and Lifschitz (1975) define the mean fluid velocity by the net momentum-flux—the so-called *kinematic velocity*—rather than by the flux of the particle-number density—the *dynamic velocity*. Whereas in classical physics these two quantities are identical, this is not generically the case in relativity, though it may be in particular cases, such as for a system in complete thermodynamic equilibrium. See Earman (1978) for a discussion.

⁸Of course, given the profound observational entrenchment of the parabolic Navier-Stokes system, one of the conditions demanded of such hyperbolic extensions will be that they (more or less exactly) recapitulate the predictions of the original system under appropriate conditions.

⁹For arguments in support of both suggestions, see Müller and Ruggeri (1993a), Herrera and Martínez (1997), Anile, Pavón, and Romano (1998), Herrera, Prisco, and Martínez (1998), Herrera and Pavón (2001a), Herrera and Pavón (2001b) and Jou, Casas-Vázquez, and Lebon (2001), *et al.* For attempts actually to conduct such studies, see, *e.g.*, Müller and Ruggeri (1993b), Zimdahl, Pavón, and Maartens (1996), Herrera and Martínez (1998), Jou, Casas-Vázquez, and Lebon (2001), Eu (2002) and Herrera, Prisco, Martín, Ospino, Santos, and Troconis (2004).

¹⁰For the purposes of this paper, a spacetime is a paracompact, Hausdorff, connected, orientable, smooth differential manifold endowed with a smooth Lorentz metric under which the manifold is also time-orientable. The imposition of temporal orientability simplifies presentation of the material dealing with the dynamic evolution of systems. It could be foregone by restricting all analysis to appropriate subsets of spacetime.

mass density ρ , particle-number density ν , mean fluid velocity¹¹ ξ^a , heat flow q^a , and shear-stress tensor σ_{ab} , jointly satisfying the four kinematic constraints

$$\sigma_{ab} = \sigma_{(ab)} \quad (2.3.1)$$

$$\xi^m \sigma_{am} = \xi^n q_n = 0 \quad (2.3.2)$$

$$\nabla_m (\nu \xi^m) = 0 \quad (2.3.3)$$

$$\nabla_m ((\rho + p)\xi^a \xi^m + pg^{am} + 2q^{(a} \xi^{m)} + \sigma^{am}) = 0 \quad (2.3.4)$$

2. in the same physical regime, there are equations of state (specified once and for all), expressed in terms of the dynamical variables characterizing the state, defining the pressure p , temperature τ , thermal conductivity θ , shear-viscosity α and bulk-viscosity β
3. in the same physical regime, these quantities jointly satisfy the two equations of dynamic evolution

$$q_a + \theta \rho [(\delta^m_a + \xi_a \xi^m)(\nabla_m \log \tau) + \xi^n \nabla_n \xi_a] = 0 \quad (2.3.5)$$

$$\sigma_{ab} + \alpha \rho (\delta^m_{(a} + \xi_{(a} \xi^m) \nabla_{|m|} \xi_{b)}) + \rho (\beta - \alpha/3)(g_{ab} + \xi_a \xi_b) \nabla_n \xi^n = 0 \quad (2.3.6)$$

Equation (2.3.3) represents the conservation of particle number (all possible quantum effects are being ignored), (2.3.4) the conservation of mass-energy, (2.3.5) the flow of heat, and (2.3.6) the effects of viscosity and stress.

2.4 The Breakdown of Partial-Differential Equations as Models in Physics

Classically, every Navier-Stokes fluid has a characteristic length (or equivalently, characteristic interval of time), the *hydrodynamic scale*, below which the description provided by the terms of the theory breaks down. Typically there is only one such length, of the order of magnitude of the mean free-path of the fluid’s particles; at this length scale, the thermodynamic quantities appearing in the equations are no longer unambiguously defined. Different sorts of thermometers, *e.g.*, with sensitivities below the hydrodynamic scale, will record markedly different “temperatures” depending on characteristics of the joint system that one can safely ignore at larger scales—the transparency of each thermometric system to the fluid’s particles, for instance. The other quantities fail in similar ways.¹²

¹¹I leave it purposely ambiguous as to which definition of fluid velocity appears, the so-called kinematic or dynamic, as nothing hinges on it here.

¹²It may seem that this sort of constraint on the definition of physical quantities manifests itself only as one shrinks the germane spatial and temporal scales, but this is not so. Imagine the difficulties involved in attempting to define what one means by *the* temperature of a cloud of gas three billion light years across. How will one, for instance, calibrate the various parts of the thermometric apparatus, each with the others? There are many possible ways one could conceive of doing it, with no guarantee that they will all yield the same answer. What if one wants to compute the total angular momentum of the cloud “in a particular direction”? Or even just to compare the values of the spin “in a particular direction” of two distant Hydrogen atoms? In general relativity, in a generic, curved spacetime, there is no natural notion of “the same direction” at different points, and so *a fortiori* no natural method to identify the same “particular direction” at different points of the spacetime to use in taking such averages or making such comparisons.

Theory and Experiment

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, beyond which the definitions of them all fail.¹³ Clearly, if there were different characteristic lengths at which the definitions of different quantities in the system broke down, the system itself would fail at the greatest such length-scale. Any phenomena that are observed at scales greater than the largest length at which one of the thermodynamic quantities becomes ill-defined are said to belong to the *hydrodynamic regime*; any observed below that scale are part of the *sub-hydrodynamic regime* (or *the regime of molecular effects*).¹⁴

As Geroch (2001) points out, the Navier-Stokes system can fail in another way, at a length-scale logically unrelated to the hydrodynamic length-scale, one at which the equations themselves may fail to hold even though all the system’s associated quantities remain well-defined. In other words, there may be a characteristic length-scale at which the expressions on the left-hand sides of the equations (especially the last two) may differ from zero by an amount, *e.g.*, of the same order as that of the terms appearing in the equations, while the equations remain valid at scales greater than that length-scale. I will refer to such a length-scale as the *transient scale*, gesturing at the fact that it is reasonable to expect that any such failures would have their origins in the dissipative fluxes of the fluid’s quantities transiently settling down as the quantities themselves approach their equilibrated, hydrodynamic values. This idea, in fact, inspires the preferred interpretations for the novel terms introduced in the hyperbolic theories. I will refer to the greatest length-scale at which the system for any reason is no longer valid—whether because the quantities lose definition or because the equations no longer hold—as the *break-down scale*, and I will refer correlatively to the regime below this scale as the *break-down regime*.

Geroch (2001) points out a possible complication in the notion of a characteristic length-scale at which the system of equations breaks down (for whatever reason). The system may fail in a way more

¹³One could perhaps try to argue along the following lines to try to explain this fact. The definitions of all quantities, kinematic and dynamic, fail at the same characteristic scale reflects the fact that these quantities and the relations among them encoded in theory’s equations of motion are all higher-level manifestations of the same underlying phenomena, whatever hidden structure, beyond the reach of our theory, lies at the foundation of the phenomena at issue. In the case of Navier-Stokes fluids, the underlying phenomena are those evinced by the statistical dynamics of the molecular constituents of the fluid. The fluid density reflects the spatial, numerical distribution of the fluid’s constituent molecules; changes in the fluid density arise from local, relative changes in the numerical distribution. Pressure reflects the distribution of the molecule’s velocities, and changes in pressure, including the fluxes manifested as stress and strain, arise from local, relative changes in that distribution. The distribution of kinetic energy among the molecules and its local, relative change evince temperature and heat. And so on. The relations among these higher-level quantities encoded in the Navier-Stokes equations reflect the relations governing the statistical mechanics of the quantities thus associated with the underlying distribution of the fluid’s molecular constituents.

¹⁴This is also sometimes called the *Knudsen regime*, after [*** get cite ***].

Theory and Experiment

complicated than can be described by a single, simple spatial or temporal length, or spatiotemporal interval. As an example, he points out that relativity itself imposes constraints on the experimental applications of the theoretical model: the model must fail at every combined temporal and spatial scale, t and s respectively, jointly satisfying

$$s^2 \lesssim \chi t \tag{2.4.1}$$

and

$$s > ct \tag{2.4.2}$$

where χ is the value of a typical dissipation coefficient for the fluid and c is the speed of light. Instead of a characteristic break-down scale, this requirement defines a characteristic break-down *area* in the t, s -plane. Note that the complement of this region of the plane, that is, the region in which the system remains valid (at least so far as these conditions are concerned), includes arbitrarily small s -values and arbitrarily small t -values (though not both at the same time!).

In this terminology, proponents of hyperbolic theories contend that the examples they exhibit are of relativistic, dissipative fluids for which the parabolic system adequately models the equilibrium behavior, yet which have transient scales measurably greater than their hydrodynamic scales, manifesting them in disequilibrated states—in other words, in certain kinds of disequilibrium, the quantities in the equations are well-defined, but the equations themselves fail to hold to a degree that, for one reason or another, whether theoretical, experimental or pragmatic, is unacceptable. Geroch (2001), in turn, contends that there are no such fluids not even, as he puts it, any known *gedanken* fluids.¹⁵ This is why such fluids represent an intriguing possibility: they would provide unambiguous examples of systems (presumably) amenable to theoretical treatment by the hyperbolic theories and (perhaps) accessible to experimental investigation.

Geroch (2001) offers an illuminating example of a particular way a system of equations may fail while the quantities in terms of which the equations are formulated remain well defined. I call it the *problem of truncation*, and, again, the hyperbolizations of the relativistic Navier-Stokes equations provide excellent illustrations. The hyperbolizations work, as we have said, by introducing terms of second-order or higher,¹⁶ purportedly representing transient fluxes of the ordinary quantities treated by the parabolic Navier-Stokes system. There is, however, no natural, *a priori* way to truncate the order of terms one would have to include in the new equations to model the systems accurately enough, once one began including any higher-order terms, for the scales at which second-order effects become important, for instance, seem likely to be the same at which third-order, fourth-order and 839th-order terms also may show themselves. It is, so far as I can see, a miracle and nothing more that there are physical systems capable of being accurately modeled by the first-order Navier-Stokes equations, ignoring all higher-order effects.

As Geroch says,

¹⁵He does exhibit an instructive but ultimately unsuccessful attempt to construct an example of such a Navier-Stokes fluid.

¹⁶The *order* of a term here refers roughly to the moment of the distribution function one must calculate to express it.

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

Geroch (2001, pp. 6–7)

The quantities modeled by the parabolic Navier-Stokes equations have a regime in which they are simultaneously well-defined, satisfy the equations and have values stable with respect to higher-order fluctuations. One cannot assume this for any amended equations one writes down, with novel terms purportedly representing higher-order effects. One must demonstrate it. On the face of it, this would be a fool’s quest to attempt by a strictly theoretical analysis; in practice, it could be accomplished only through experimentation.

3 The Kinematical Regime of a Physical Theory

Philosophical analysis of particular physical theories, such as non-relativistic quantum mechanics, often focuses on the more or less rigorous mathematical consequences of the structure of the theory itself, in abstraction from the necessary laboratory conditions required for application of the theory in modeling the dynamic evolution of particular, actual systems. To clarify what I mean, consider the usual schema of a Bell-type experiment considered by philosophers: an undifferentiated source of pairs of electrons in the singlet state, and an inarticulate, featureless Stern-Gerlach device to measure the spin of the electrons. This indeed constitutes a model of a physical system, but only in an abstract, even *recherché*, sense. No consideration is given to the structure of the source of the electrons, the exact form of the coupling between the system under investigation (the pairs of electrons) and the instrument used to measure the relevant quantities of the system (the Stern-Gerlach device), or to the regime of propriety of the model they are using for this kind of system’s coupling with that sort of measuring apparatus—it is a schematic representation of the experiment, in the most rarefied sense of the term.

It is taken for granted, for instance, that

1. the ambient temperature is not so high or so low as to disrupt the source’s output of the electrons
2. the electrons are not traveling so quickly (some appreciable fraction of the speed of light), nor are the primary frequencies of the photons composing the magnetic field so high (having, *e.g.*, wave-lengths of the order of the Compton length), as to require the use of quantum field theory rather than standard non-relativistic quantum mechanics in order to model the observation appropriately
3. the spins of the electron are measured using a Stern-Gerlach type of mechanism whose physical dimensions are such as to allow its being treated as a classical device (as opposed to one whose dimensions are of such an order—a quantum-dot device, *e.g.*—for which the “measurement of

Theory and Experiment

the electron’s spin” would become ambiguous, as one would have to account for the quantum properties of the measuring device as well in modeling the interaction)

4. the metric curvature of the region in which the experiment is being performed ought not be so great as to introduce ambiguity in the assignment of correlations among the spin-components of different directions at the different spacetime points where the spin of each electron is, respectively, measured¹⁷

In the literature in general, no effort is put into determining how such restrictions may, if at all, affect the expected outcome of the experiments.¹⁸ While we are perhaps safe in blithely ignoring these sorts of issues in the case of Bell-type experiments (and I am not even convinced of that), the study of theories of relativistic, dissipative fluids provides a clear example of a case in which we may not safely ignore them, not only for reasons pertaining to the practice of physics but also, I feel sure, for reasons pertaining to the production of sound philosophical argument, as I intend to show in what follows.

The analysis of the debate over theories of a relativistic Navier-Stokes fluid shows that, at a minimum, the propriety of a theory for modeling a set of phenomena is constrained by conditions on the values of environmental quantities, the values of the quantities appearing in the theory’s equations, and the measure of spatial and temporal intervals: a theory can be used to treat a type of physical system it putatively represents only when the system’s environment permits 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 envisioned phenomena. In this section, I will propose a possible model for dealing with these considerations precisely, *the kinematical regime*, requiring (in brief) with regard to the observation and measurement, and hence to the well-definedness, of the quantities treated by a theory:

1. a set of constraints on the measure of spatial and temporal intervals, and perhaps as well on the behavior of the metric in general (*e.g.*, that some scalar curvature remain bounded by a given amount in the region)
2. a set of constraints on the values of the theory’s quantities in conjunction with correlative constraints on environmental conditions
3. a set of methods for calculating the ranges of inevitable inaccuracy in the preparation or measurement of those quantities using particular sorts of experimental techniques under particular environmental conditions

¹⁷If the curvature were so great that parallel transport of a tangent vector along different paths from the point of measurement of the spin of one of the electrons to the point of that of the other would yield markedly different resultant tangent vectors, then the question of the correlation of the spins along “opposite” directions at the two points becomes incoherent.

¹⁸The analysis of Fine (1982) perhaps comes the closest in spirit in trying to take account of these sorts of issues with regard to Bell-type experiments. The discussion of Stein (1972) presents a much richer, somewhat complementary account to the one I sketch here.

I will not attempt to formulate any of the notions I discuss with rigor or to treat them to any depth. For those interested in the development of a rigorous technical apparatus for treating all these issues, as well as for treating the issues raised in §§4 and 5, see Curiel (2010b). The importance of the regime, as I will argue later, is that much if not most of the semantic content of a theory derives from the continual interplay between the theoretician and the experimentalist involved in its aboriginal working out and the continual, ongoing work in its development as the theory matures and in its refinement as the theory settles into its maturation. That interplay constitutes the relevance and importance of being able, at least in principle, for a theory to have the resources for the modeling of actual experiments, including the apparatus used in them—it is only that process that renders to a theory one of the most important components of its semantic content. This is why I think it has been to the detriment of philosophical comprehension of scientific theories that philosophers have not focused more on the modeling of actual experiments, or, at least, have not focused more on the collateral requirements involved in the construction of actual experimental models in the examples they tend to use in their philosophical argumentation.

3.1 Kinematics and Dynamics

[*** Give a brief characterization of both the kinematics and dynamics of a generic theory. ***]

Before starting the analysis proper, we fix some definitions.¹⁹ Given a type of physical system, a *quantity* of it is a (possibly variable) magnitude that can be thought of as belonging to the system, in so far as it can be measured (at least in principle) by an experimental apparatus designed to interact with that type of system, in a fashion conforming to a particular coupling of the system with determinable features of its environment, which coupling may (at least, again, in principle) be modeled theoretically.²⁰ Fix, then, a type of physical system, along with a system of partial-

¹⁹None of these prefatory definitions ought to be considered attempts at even the slovenly rigor, as it were, I aim for in this paper, or, indeed, anything near it. These are rather in the way of marking off the field of play, much as children determine a bit of a meadow as a soccer-field with episodic markers of the boundary (jackets, frisbees, . . .), which is to be interpolated between those markers as the niceness of the occasion demands.

²⁰This characterization of quantity involves (at least) one serious over-simplification. Not all quantities' values can be determined by direct preparation or measurement, even in principle, as this statement may suggest. Some, such as that of entropy, can only be calculated from those of others that are themselves directly preparable or measurable. Other quantities defy direct measurement for all intents and purposes, though perhaps not strictly in principle. Consider, for example, the attempt to measure directly distances of the order of 10^{-50} cm—the precision required of any measuring device that would attempt it would demand that the probes it uses have de Broglie wave-lengths of comparable scale, and so, correlatively, would demand the release of catastrophic amounts of energy in its interaction with another system—think of the energy of a photon whose wavelength was of that scale.

I am not sure whether the analysis I offer in this paper would or would not suffice for the treatment of these sorts of quantity, though, offhand, I see no reason why it should make a difference. Temporal and spatial constraints do not allow me to consider the question here, however.

Note that this is not an *instrumentalist* requirement. These measurements do not define the quantities, at least not in all cases.

Perhaps this is not an inappropriate place to mention, *EN passant*, that, were one to allow oneself the momentary luxury of Saturnalic speculation and wild extrapolation, it would be fun to imagine that the lack of such a thing, even in principle, as an “entropometer”, and correlatively the lack of a unit of measure or scale for entropy, as the Joule

differential equations, which are intended to represent, among other things, the evolution of the system's dynamic quantities over time. An *interpretation* of the equations in terms of the quantities of that type of physical system (or, more briefly: in terms of the physical system itself) is a complete, one-to-one correspondence between the set of variables and constants appearing in the equations on the one hand and some sub-set of the known quantities of that type of physical system on the other, in conjunction with a set of statements describing the coupling of those quantities to known and determinable features of the environment precise and detailed enough to direct the experimentalist in constructing probes and instruments tailored to the character of each quantity, as associated with that kind of system, for its observation and measurement. The system of partial-differential equations *models* the type of physical system if, given an interpretation of the equations in terms of that type of physical system, and given any appropriate set of initial data for the equations representing a possible state of a physical system of that type, the mathematically evolving solution of the equation continues to represent a possible state of that system if it were to have dynamically evolved from a state represented by the initial data of the equations. In other words, the equations model the system if the equations' solutions do not violate any of the system's inherent kinematic constraints. If, for example, a set of partial-differential equations as interpreted by the terms of a given type of physical system predicted that systems of that type, starting from otherwise acceptable initial data, would evolve to have negative mass, or would evolve in such a way that the system's worldline would change from being a timelike to being a spacelike curve, then we would likely conclude that those partial-differential equations do not model that type of system, at least not for that set of initial data. Note, in particular, that modeling is a strictly kinematical notion. The accuracy of predictions produced by the partial-differential equations—whether or not its solutions, under the given interpretation match to an admissible degree of accuracy the actual, dynamic evolution of such systems—has no bearing on the question of modeling at this stage. Let us say, then, that a physical theory *comprises* a system of partial-differential equations if those equations model the types of systems treated by the theory, under the interpretation the theory provides. For example, the theory of relativistic Navier-Stokes fluids comprises equations (2.3.3)–(2.3.6), under their standard interpretation. Finally, by *physical theory*, I intend, very roughly speaking, an ordered set consisting of, at least,

1. a mathematical structure representing the states and the dynamical evolution of the physical systems treated by the theory (*e.g.*, a space of states and a family of vector fields on it, the latter representing the kinematically allowed evolutions of the system)
2. a set of experimental techniques for probing those systems
3. a mapping between the terms of the mathematical structure and the quantities associated with those systems as observed and probed by the experimental techniques (an interpretation of

is to energy, somehow has to do with the fact that the Wigner time-reversal operator in quantum mechanics is not a Hermitian operator but rather is anti-Hermitian. (The issue of the possible existence of an entropometer is, thank goodness, not directly related to the possibility of a Maxwell demon, for the demon does not putatively measure the entropy of a system, but rather only reduces it piece-meal.)

the mathematical structure in empirical terms)

4. the set of data germane to knowledge of those quantities, collected from those systems by the given experimental techniques and analyzed and informed by application of the given mathematical structure

The fifth element I would include in the ordered set is a regime of propriety for the theory, to the articulation of which I now turn.

I feel I need to make one last remark before proceeding, however. One may be tempted to think that a “fundamental” physical theory, such as the quantum field theory of the Standard Model, ought not require specification of a regime for its applicability. This is not the case. Quantum field theory can not solve in closed form the dynamical equations representing the evolution of arguably even the simplest micro-system, the isolated Hydrogen atom. It rather relies on perturbative expansions, and thus requires the system to be not too far from equilibrium of one sort or another. Quantum field theory in general, moreover, can not handle phenomena occurring in regions of spacetime in which the curvature is too large. The Standard Model breaks down in regimes far above the Planck scale. Not even quantum field theory formulated on curved-spacetime backgrounds can deal rigorously with phenomena under such conditions.²¹ Indeed, it appears that possession of a fairly well articulated regime of propriety, as we will characterize it, or something nearly like it, is necessary for a theory’s being viable as a theory of *physics*, as opposed to merely a chapter of pure mathematics.²² Bondi, in a paper on gravitational energy, puts his finger on the heart of the issue: “*Good physics is potential engineering.*”²³

3.2 Constraints on the Measure of Spatiotemporal Intervals

The idea of a regime, at bottom, rests on twin pillars: the idea that certain types of operations associated with the theory make sense (in some fashion or other) only when carried out over spatiotemporal regions whose dimensions satisfy certain constraints and in which some appropriate measure of the intensity of the metric curvature does not become too great; and that certain types of operations associated with the theory make sense (in some fashion or other) only when the values

²¹I know of no theory of quantum gravity mature enough for it even to attempt the claim that it could do so. Even if one could, and even were we able to observe and measure the peculiar quantities modeled by the theory, we presumably would measure them using technological apparatus of some stripe, which, again presumably, would be limited in its precision and its accuracy.

As an aside, I remark that I may appear to be leaving myself open to the charge of conflating two different ways in which inaccuracy can accrue to measurements and predictions, one based on the nature of the quantities (as with the statistical character of temperature, *e.g.*, or as in the constraints imposed by the Heisenberg principle on quantum phenomena), and the other based on *de facto* limitations due to the current stage of development of our technological prowess. I should rather say that part of the point of this paper is to show that this distinction may not be so sharp and clean as it appears at first glance. [*** discuss Newton’s Third Rule of Reasoning from *Principia*, though not in this footnote. ***].

²²The mathematician will balk at this *merely*, but she is not my primarily intended audience. Still, it would please me were she able to read this paper with some profit, so I hope such rhetorical flourishes do not put her off too much.

²³Bondi (1962, p. 132). Italics are Bondi’s.

of some set of quantities relevant to systems treated by the theory satisfy certain constraints. We begin with a few considerations about how one may constrain the measure of spatiotemporal intervals, which will culminate in a few quasi-technical definitions and results needed for the quasi-formal analysis of the idea of a regime I intend to give.

Real initial data for real physical problems are not specified with arbitrary accuracy over an arbitrary region of a spacelike hypersurface in a relativistic spacetime. It is less of an idealization to model initial-data as occupying a compact, connected region of spacetime, of non-zero metrical volume, determined by the spatial extent of the system in conjunction with the temporal interval during which the measurement or preparation of the initial-data takes place. As we have seen in the discussion of the dispute over the hyperbolic extensions to the Navier-Stokes system, moreover, the determinations of the values of real physical quantities appropriate for use in initial-data for a given system will always be coarse-grained in the sense that they themselves are in some sense more properly modeled as pertaining to compact, connected sets of non-zero metrical volume, satisfying certain collateral metrical conditions, contained in the region occupied by the physical system, rather than as pertaining to individual spacetime points contained in the region occupied by the physical system. Those sets, moreover, should be as small as possible in order to maximize the accuracy of the modeling of the experimental apparatus, while still being large enough to satisfy the constraints the theory places on the definition and measurement of its quantities and on the satisfaction of its equations using the chosen methods of observation under the specified environmental conditions.

To study some physical phenomena modeled by a particular theory, then, we first need a compact, connected region of spacetime of non-zero metrical volume, which for the purposes of this discussion we may without a great loss of generality assume to have properties as nice as we choose (we may demand, *e.g.*, that it be the closure of an open, convex, normal set), as the stage on which the phenomena will unfold and the experiment be played out. The theory may impose further requirements on the region; it may demand, *e.g.*, that its spatial and temporal dimensions (as determined in a specified manner) satisfy a set of algebraic constraints, or that the curvature in the region satisfy a set of differential and algebraic conditions. Once so much is settled, the difficulty lies in partitioning our region into components appropriate for the fixation of the values of the quantities modeled by the theory. Again, those components need to satisfy whatever constraints the nature of the quantities demand. It makes no sense in general to attempt to determine the temperature of a system, *e.g.*, on scales smaller than the mean free-path and the mean free-time of flight of the system's dynamically relevant constituents. For a sample of nitrogen gas under "normal" conditions on the surface of the Earth, for example, this would include the relevant measurements of the nitrogen molecules, not of their electrons and nucleons, as calculated in a frame co-moving with the surface of the Earth and not in one spinning wildly and moving at half the speed of light with respect to it. We therefore require that the individual regions to which values of temperature are to be ascribed be larger than, in an appropriate sense, those characterized by the theory's break-down scales. Similarly, if we are to try to model a sample of nitrogen gas using the Navier-Stokes equations, for instance, then we must ensure that the dynamical evolution of the system is such that the gradient of its temperature on those scales not be too great just off points of equilibrium (as it

settles down to equilibrium, *e.g.*, during preparation).

A Maxwell-Boltzmann sort of partitioning of phase-space, and *eo ipso* of the spatiotemporal region occupied by the system itself, into scraps of roughly equivalent volume and shape offers the most obvious way forward at this point.²⁴ I do not find this solution satisfactory, however—or, rather, I find it satisfactory for the particular treatment of the statistical mechanics of a more or less ideal gas, but I do not find it satisfactory for the generic treatment of the modeling of constraints on the determination of the values of quantities for many other kinds of physical theory. Although thermodynamics *cum* statistical mechanics provides the easiest and most straightforward examples of the kinds of constraints that interest us, I would argue that such constraints form an integral part of the nitty-gritty of *every* physical theory, no matter how seemingly “fundamental”, as I gestured at above.

Let us try to sketch a construction of a different sort of partitioning of a spatiotemporal region.²⁵ Fix a compact, connected subset C of spacetime, of non-zero metrical volume, representing the spatiotemporal region in which the physical phenomena we would model play out. We demand that such regions satisfy a few basic, generic, topological and metrical conditions, mostly along the lines of guaranteeing that the region is not “too small along either its spacelike or its timelike dimensions”, that its boundary is well-behaved, and so on. We will call such a region a *canvas*. More precisely, a canvas is a convex, normal, compact, connected, 4-dimensional, embedded submanifold of \mathcal{M} .²⁶ We will use canvases to model the spatiotemporal regions physical systems occupy in which a specified family of observations and measurements occurs, as well as to model the elements into which such regions will be carved for the purpose of serving as “points” of the system to which values of its associated quantities may be meaningfully ascribed (as opposed to ascribing the values of the quantities to points of spacetime itself).

To give a flavor of the sorts of algebraic conditions one may demand of the elements of such a partition, we first require terms in which to express the conditions. There is an endless supply of theoretical terms one could employ to do so. I offer here only a sampling, by way of example. I do not think that these have a preferred status over others one could propose. I offer them because they seem to me to be reasonably clear, to be easy to visualize and to have straightforward, meaningful physical content. Other sets of terms could well serve better the purposes of a particular investigation. Such choices are, I think, fundamentally of a pragmatic and æsthetical character. Choose, then, an element O of the proposed partition of C and a point q on the boundary of O , and consider the family of all spacelike geodesics whose intersection with $\overset{\circ}{O}$ (the interior of O) consists of a connected arc one of whose points of intersection with the boundary of O is q . Calculate the supremum of the absolute values of the proper affine length of all these arcs. Finally take the infimum of all these

²⁴Synge (1957) has worked out such a device in illuminating detail for the statistical-mechanical treatment of ideal, relativistic gases.

²⁵Again, for the rigorous details, see Curiel (2010b).

²⁶The full definition (see Curiel 2010b) includes the proviso that ∂C not be a null 3-space with respect to the spacetime metric. The exclusion of null hypersurfaces ensures that certain integrations and operations on the boundary are always well defined. Since light never travels, so far as we know, in a true vacuum in any real physical situation, this is a negligible exclusion for the goal of modeling real, inaccurate data over finite spatiotemporal regions.

suprema for every point q on the boundary. This is the *infimal spacelike diameter* of O . The *infimal timelike diameter* is calculated in the analogous way, using timelike rather than spacelike arcs. We take the infimum of the suprema, as the simple infimum of the lengths for a Lorentzian metric would in general be zero as the arcs may approximate as closely as one wishes to a null arc. Note that the spacelike or the timelike infimal diameter of a connected set with non-zero metrical volume will always be greater than zero (so long as the metric is “well behaved”, which we henceforth assume). It thus follows directly from the definition of a canvas that its infimal spacelike and timelike diameters are always both greater than zero. Also, any 4-dimensional set with non-zero infimal spacelike and timelike diameters has non-zero volume with respect to the spacetime’s volume element—we deal only with measurable sets in this paper—as one can always fit a non-trivial open set inside it (*e.g.*, a small tubular neighborhood of a geodesic, spacelike arc whose length is within some $\epsilon > 0$ of the infimal spacelike diameter). Thus it also follows that a canvas has non-zero measure with respect to the volume element of spacetime. We will use these sorts of properties of canvases, especially those relating to their infimal diameters, to articulate the first kind of constraints a regime imposes on a theory, those directly addressing characteristic spatial and temporal measures of spatiotemporal regions appropriate for the application of the theory. [*** briefly sketch one possible way to think of the physical content of these diameters—that the “longest” way across the region for a particle or rod crossing near the center of the region will never be smaller than this amount ***]

It is not so easy to articulate terms in which the second half of the possible constraints on the character of spatiotemporal regions appropriate for the definition of physical quantities, those pertaining to the general behavior of the metric in the region, may be formulated. For instance, one can impose constraints on the intensity of the curvature in a region in any of a number of ways, from, say, fixing an upper bound on the total integral of any scalar curvature-invariant over the region to fixing an upper bound on the average of such a scalar or an upper bound on the value of that scalar at any given point in the region; one may as well, for example, fix an upper bound on the integrated components of the Riemann tensor as measured with respect to a parallel-propagated frame-field along any timelike geodesic arc contained in the region; and so on. There are more general sorts of considerations one may bring into play as well, including the imposition of some kind of causality conditions (*e.g.*, that the region contain no almost closed, timelike curves), an exclusion of certain kinds of singular structure (*e.g.*, that the region contain no incomplete timelike geodesics), a restriction on the topology of the spacetime manifold (*e.g.*, that its second Stiefel-Whitney class vanish, the necessary and sufficient condition for a spacetime manifold to admit a globally defined, unambiguous spinor-structure—see Geroch 1969 and Geroch 1970b), and other general, metrical considerations (*e.g.*, that the spacetime be asymptotically flat). I will not attempt to characterize with any formality these sorts of metrical constraints, restricting myself mostly to speaking only of constraints on spatial and temporal measures, primarily because I see no way of doing so for the former in light of their amorphous nature, not because I think they are unimportant or not worth considering. On the contrary, I think it would be of enormous interest to construct a formalism for studying those sorts of constraints. In any event, the reader should bear in mind that, from hereon, when I speak of constraints on spatial and temporal measure I do not mean to exclude the other

sort from consideration. [*** remark—for reasons like those sketched in Curiel (1999)—that it is far more difficult to lend clear, unambiguous physical content to constraints on the behavior of the curvature and metric ***]

A set of algebraic constraints on the measure of temporal and spatial intervals, then, is a formal system of equations and inequalities with some number (greater than zero!) of unknown terms, each term representing a characteristic temporal or spatial scale associated with the quantities modeled by the theory. For the sake of simplicity, we will assume that, for any set of algebraic constraints on temporal and spatial measures associated with the regime of a theory, there are only two unknowns used in all the expressions in the set, which we will interpret respectively as the spacelike and timelike infimal diameter of any region that is a candidate for having the values of the theory’s quantities legitimately determined on it. A canvas *satisfies* such a set if its two diameters jointly satisfy the elements of the set. In the case of a relativistic Navier-Stokes fluid, for instance, we know that, for any element of a partition of the region it occupies, the infimal spacelike diameter ought to be strictly greater than c times the infimal timelike diameter (see equation (2.4.2)). We also know that the two infimal diameters ought to be, respectively, at least of the order of the mean length of the free path and the mean time of free flight of the fluid’s molecules, as determined in a “reasonable” frame.²⁷

3.3 Infimal Decoupages

I shall now sketch the proposed manner of generically partitioning a canvas into elements to which we may apply our algebraic constraints. Fix a set of algebraic constraints on spatial and temporal measures and a canvas C satisfying the chosen constraints in such a way that the canvas contains as proper subsets other canvases also satisfying the constraints. A *scrap* S of the canvas is itself a canvas such that

1. it is a proper subset of C
2. it satisfies the constraints
3. its interior is topologically \mathbb{R}^4

The *decoupage* of a canvas is the family of all its scraps. A rich family of mathematical structures accrues to the decoupage in a natural way. It has, for instance, a natural topology under which it is Hausdorff, connected, and compact, if C itself is so. This topology can be extended to a σ -ring on which a Lebesgue measure, and thus integration of scalar fields, can be defined, grounded on the natural Lebesgue measure associated with the spacetime metric’s volume element. (See Curiel 2010b for details.) The decoupage thus characterized has the structure of an infinite-dimensional space.

²⁷With a little more effort, one can state this last condition in a relativistically invariant way, by stating it in terms of the measure of intervals along and separations between timelike geodesics contained in the canvas such that two otherwise free particles instantiating two given timelike geodesic arcs contained in the canvas will, with a given probability, collide with a certain number of other particles traversing timelike geodesic arcs contained in the canvas closer than the given distance—get it? Probably not. Simplify this mess.

I found it convenient for technical reasons in Curiel (2010b) to construct by the use of equivalence classes a finite-dimensional space capturing in approximate form all the essential structure of the decoupage, and then to use this in place of the full decoupage. The construction of that derived space raises several interesting questions about the nature of the sorts of approximations one deals with in physics, which we will not be able to address in this paper. In any event, from hereon, the term ‘decoupage’ will refer to that finite, approximative space rather than to the full, infinite-dimensional space; nothing in the paper’s arguments turn on the fact.

We will attempt to capture the idea of “spatiotemporal regions whose dimensions satisfy certain constraints”, the ones appropriate for taking as the elements of the partition of the region in which the phenomena occur, by using decoupages. There are, again, several ways one may go about it. I will sketch only one. The following consideration will be our primary guide. On the one hand, the details of the physical state of the system on regions smaller than the break-down scale are, if not irrelevant, then at least *ex hypothesi* not sensibly representable in the theory at issue or do not yield results consonant with the solutions of the equations, whereas, on the other, those regions significantly larger than the break-down scale are not so fine-grained as one can in principle make them for the purpose of maximizing the accuracy of observation and measurement. Given a theory with its attendant set of algebraic constraints on spatial and temporal measure, we require a way of specifying a family of subsets of a region that are in some sense or other as small as possible while still conforming to the theory’s constraints. In general, neither the set of spacetime points constituting the region itself nor the whole decoupage itself of the region will serve the purpose.

Fix, then, a canvas $C \subset \mathcal{M}$ and a set \mathfrak{m} of algebraic constraints on spatial and temporal intervals. The *infimal decoupage* of C , \mathcal{C}^{inf} , consists of all the scraps of C whose volumes are, in a certain precise sense, as small as possible while still being consistent with \mathfrak{m} . An *infimal scrap* is a member of an infimal decoupage. Alternative definitions of an infimal decoupage could minimize, for instance, the volumes of the boundaries of the scraps, or a weighted average of the lengths of all the spacelike and timelike arcs contained in each scrap, or the average scalar curvature of each scrap, or some combination of these, and so on. I choose the definition based on volume not because I think it is *a priori* superior to the alternatives, but rather because it is simple, intuitively clear, and suggestive of the usual Maxwell-Boltzmann partition of phase space in statistical mechanics. One of the alternatives could well fit the purposes of some particular analysis or investigation more closely.

It makes no sense to talk about the temperature, *e.g.*, in regions on a scale finer than that characteristic of the break-down of the modeling of the given system, nor indeed to speak of possible solutions to the partial-differential equations of a theory on a finer scale, for the equations are no longer satisfied to the desired degree of accuracy in that regime, if they are well posed at all. This is why, in a substantive sense relevant to our purposes, a real-valued function whose domain is an infimal decoupage more appropriately models the details of the physical state associated with the fluid’s temperature, *e.g.*, than does a scalar field on a subset of spacetime: it forces one to focus attention on those details and only those details both relevant to the experimental problem at hand and sensical with respect to the formal structures of the theory being applied. For a Navier-Stokes fluid contained in a spacetime region, for example, the break-down scale, as discussed

in §2.4, defines part of the set of constraints on the spatial and temporal intervals over which the fluid's quantities are well defined and over which the solutions to the equations themselves model the fluid's actual dynamical evolution to the desired degree of accuracy, and so fixes the infimal decoupage over the scraps of which the quantities associated with the fluid should be considered fields.

Still, this all may sound more than superficially similar to the standard Maxwell-Boltzmann sort of partitioning. It differs from that device in important ways, however. Primary among the differences are two. First, in this scheme the scraps overlap in a densely promiscuous fashion. Thus, even though one can speak of, *e.g.*, the temperature only on finite scraps rather than as associated with individual spacetime points, one can still speak of the temperature on such scraps arbitrarily "close" to each other in a topological sense. This may seem a slight advantage at best, but, as is shown in Curiel (2010b), exactly this aspect of the machinery developed here allows one, in complete contradistinction to the ordinary Maxwell-Boltzmann partition of phase space, to bring to bear with complete rigor the full battery of mathematical structures one most often employs in attacking both theoretical and practical problems in physics, including topology, measure theory, differential topology, differential geometry, the theory of distributions and the theory of partial-differential equations on finite-dimensional manifolds. One can then use these structure to articulate and prove results of some interest (*e.g.*, theorem 6.4.1 below) illuminating the relations among ordinary scalar fields as employed in theoretical physics and fields defined on these decoupages that, I argue, more appropriately model the data gathered during the course of and used for the modeling of actual experiments. Second, and at least as important, I do not see any other way of attempting to define such a partition in a relativistically meaningful and useful way. The standard Maxwell-Boltzmann device fixes the partition of the observatory once and for all into a finite lattice of scraps. This partition may provide excellent service for one observer but be next to useless, or worse, for another. The idea of the infimal decoupage allows one to take account of all such partitions all at once, as it were, in an invariant manner.

3.4 The Kinematical Regime

Recall that the first type of failure of a theory's applicability to a given system, which we discussed in §2.4, stemmed from the ceasing to be well defined for one reason or another of the quantities the theory attributes to the system. The International Practical Temperature Scale of 1927, as revised in 1948, 1955 and 1960, provides an excellent, concrete example of this phenomenon.²⁸ For example, the thermodynamical temperature scale between the primary fixed point 0.01° Celsius (the triple point of water at one standard atmosphere) and the secondary fixed point 630.5° Celsius (the equilibrium point between liquid and solid antimony at one standard atmosphere) is defined by interpolation, using the variation in resistance of a standard platinum wire according to the equation

²⁸*Cf.*, respectively, Burgess (1928), Stimson (1949), Hall (1955) and Stimson (1961).

Theory and Experiment

of Callendar (1887):²⁹

$$t = 100 \left(\frac{R_t - R_0}{R_{100} - R_0} \right) + \delta \left(\frac{t}{100} - 1 \right) \left(\frac{t}{100} \right)$$

where

- R_0 is the resistance of platinum as measured with the thermometer immersed in an air-saturated ice-water mixture at equilibrium, at which point the ice-point temperature is unaffected (to an accuracy of $\pm 0.001^\circ$ Celsius) by barometric pressure variations from 28.50 inches to 31.00 inches of mercury, and the resistance of the wire is independent of the static water pressure up to an immersion-depth of 6 inches at sea-level
- R_{100} is the resistance of platinum as measured with the thermometer immersed in saturated steam at equilibrium under atmospheric pressure (as determined using a hypsometer), though corrections must be carefully made in this determination, the steam-point temperature being greatly affected by variations in barometric pressure (for which, standard tables may be consulted)
- R_t is the resistance of platinum at temperature t (the temperature being measured), *i.e.*, R_t is itself the quantity being measured that allows the calculation therefrom of the environment's temperature
- δ is a characteristic constant of the particular type of thermometer employed, defined at the primary fixed point 444.6° Celsius (the equilibrium point between liquid and solid sulphur at one standard atmosphere)

Below 0.01° Celsius and above 630.5° Celsius, the Callendar equation quickly diverges from the thermodynamic scale. From 0.01° Celsius down to the primary fixed point -182.97° Celsius (the equilibrium point between liquid oxygen and its vapor at one standard atmosphere), the temperature is also based on the resistance of a standard platinum wire, the interpolation being defined by an emendation of Callendar's equation (transforming it from one quadratic to one cubic in the unknown temperature), known as van Dusen's equation; above 630.5° Celsius up to the primary fixed point 1063.0° Celsius (the equilibrium point between liquid and solid gold at one standard atmosphere), the temperature is based on the electromotive force generated by a 90%-platinum/10%-rhodium versus 100% platinum thermocouple, the interpolation being defined by the so-called parabolic equation of thermocoupling; above 1063.0° Celsius, the temperature is based on the measurement of the spectrum of radiation by an optical, narrow-band pyrometer, the interpolation being defined by Planck's radiation formula.³⁰ In all these cases, moreover, it is clear that one cannot speak of the temperature's being measured on a spatial scale more finely grained than that corresponding to the physical dimensions of the thermometric device employed, or on a temporal one more finely grained

²⁹See, *e.g.*, Benedict (1969, §§4.1–4.4, pp. 24–9). This reference is not the most up-to-date with regard to the international agreement on defining the standard, practical methods for the determination of temperature, but I have found no better reference for the nuts and bolts of thermometry.

³⁰See, *e.g.*, Benedict (1969, pp. 27).

than that of the time it takes the state of the device to equilibrate when placed in proper thermal contact with the system under study, under the influence of fluctuations in the temperature of the system itself and its environment, under the given conditions.

As this example illustrates, the constraints on the definability and measurability of a quantity in a given theory must be variously given with regard to the parameters of particular types of systems under certain kinds of conditions, not generically once and for all in an attempt to constrain the definability and measurability of that quantity *simpliciter*. It is in part this very variability in the specification of a quantity’s definition—that it is possible to make in a variety of ways—that leads us to think that we have cottoned on to a “real” quantity, and not one artifactual of this particular experimental arrangement under those particular conditions.³¹ This example makes clear, moreover, that in modeling different ranges of values of a given quantity different theories must be used. If one treats phenomena in which temperatures rise above 1063° Celsius, for instance, one’s theory must include, or have the capacity to call upon the resources of, at least that part of quantum field theory required for a Planckian treatment of electromagnetic radiation.³² We will therefore assume, at a minimum, that the range of admissible values for any quantity modeled by a theory is bounded both from below and from above. In technical terms, this means that the family of scalar fields admissible for representing the distribution of the values of a quantity for any spatiotemporally extended system treated by the theory is itself uniformly bounded from below and from above. In a similar vein, we assume, roughly speaking, that the first several derivatives (the exact number being idiosyncratic to each theory) of all the scalar fields are uniformly bounded from above and below—there is no sense, for example, in using scalar fields that oscillate wildly in regions smaller than the breakdown scale when trying to represent a quantity.³³

To make these ideas precise, fix a physical theory comprising a system of partial-differential equations.

Definition 3.4.1 *A kinematical regime of propriety of a theory (or a kinematical regime, for short) is an ordered quintuplet $\mathfrak{K} \equiv (\mathfrak{c}, \mathcal{E}, \mathfrak{k}, \mathfrak{m}_k, \mathcal{K})$, where*

1. \mathfrak{c} is the set of variables and constants the partial-differential equations of the theory are formulated in terms of
2. \mathcal{E} is a finite set of variables and constants none of which appear in \mathfrak{c} , and thus not in any of the theory’s equations

³¹See Newton’s Third Rule of Natural Philosophy, at the beginning of Book III of the *Principia*, for a remarkably concise and incisive discussion of a few facets of this issue, with particular emphasis on the nomination of certain properties of a system as being *simple* or *fundamental* with regard to a theory.

³²This remark suggests that my use of ‘theory’ does not entirely harmonize with standard usages in physics and in philosophy. I will discuss this point below in §5.5.

³³We will characterize more precisely this property, of a field’s derivative being uniformly bounded in a kinematically relevant way, below in §5.2. We will there, moreover, be able to extend these ideas to tensorial quantities by imposing a more or less natural, kinematical norm on such quantities, the one developed in purely formal terms in §4.5, and then demanding that this norm, and the norm of some appropriately derived set of tensorial quantities, be uniformly bounded.

Theory and Experiment

3. \mathfrak{k} is a set of differential and algebraic conditions on the values of the elements of $\mathfrak{e} \cup \mathcal{E}$, including an upper and a lower uniform bound on values of the family of fields admissible for modeling values of the elements of \mathfrak{e} , as well as uniform upper and lower bounds on some fixed number of the derivatives (appropriately defined) of all fields admissible for modeling values of the rates of change of fields modeling the values of the elements of \mathfrak{e} ³⁴
4. \mathfrak{m}_k is a set of algebraic conditions, possibly involving elements of $\mathfrak{e} \cup \mathcal{E}$, on the measure of spatial and temporal intervals
5. \mathcal{K} is a set of particular types of interactions with the environment using particular kinds of experimental apparatus, in conjunction with methods for calculating the intervals of possible inaccuracy in preparing or measuring the quantities of the theory by dint, respectively, of those interactions, within given levels of confidence, under any particular set of circumstances conforming to the requirements imposed jointly by \mathfrak{k} and \mathfrak{m}_k

\mathfrak{e} represents the quantities directly modeled by the theory through its partial-differential equations. \mathcal{E} is to represent a set of environmental quantities the values of which play a role in the determination of the propriety of the theory but which are not themselves explicitly treated by the theory. The elements of \mathfrak{k} constrain the values of those environmental variables, in addition to constraining the values of the quantities directly treated by the theory. \mathfrak{k} attempts to capture the fact that the theory's quantities will remain well defined only under certain environmental conditions, and only while the quantities the theory treats do not exhibit behavior pathological with regard to other quantities treated by the theory. \mathfrak{k} contains constraints on both collateral environmental quantities and the theory's own quantities because the two often are not extricable from each other. In the case of the relativistic Navier-Stokes fluid, for example, the ambient Maxwell field ought not be so intense as to ionize the fluid, but the value at which the Maxwell field ionizes the fluid will itself in general depend on the temperature of the fluid; the temperature of the fluid, likewise, should not be so high as to denature the molecules constituting the fluid. In either of those two cases, for example, the definition of the fluid's shear-stress would become ambiguous, dependent on how one accounted for the contributions to it of the various particles as they ionize, denature and recombine.³⁵ The conditions contained in \mathfrak{m}_k delimit the spatiotemporal ranges over which the quantities represented

³⁴We will work out in some detail what this may mean in a specific example, §4.8 below

³⁵I have particular qualms about the idealizations involved in positing a fixed set of kinematic constraints, to wit, a fixed, probably infinite conjunction of (at least) second-order quantified statements. It seems likely to me that, in practice, nothing remotely approximating such a fixed set of conditions exists, even *could* exist, covering all possible experimental circumstances as modeled by a particular theory. The case rather seems to me to be more along the lines of the conclusion of the analysis of Anscombe (1971) of the conditions under which ordinarily caused events, *e.g.*, the lighting of a match, are taken to occur and not to occur. She argues that rather than stipulating a fixed list once and for all—in this case, perhaps to include the statements that the match not be wet, that the temperature not be too low, *etc.*—it is more appropriate to stipulate that, when the expected event does not occur, it behooves one to look for a contravening cause as best one may, without recourse to such a list. Though she fails to remark on this, one ought to note that this analysis, spot-on in many ways as it may be, raises the question—when ought one *expect* a given outcome, without something very like a tentative list of necessary conditions? Needless to say, this issue is too vexed to address here—or, really, I fear, anywhere.

by terms in ϵ are well defined. As we have seen in the case of the relativistic Navier-Stokes system, these constraints on spatial and temporal measures may employ terms in $\epsilon \cup \mathcal{E}$. A strong Maxwell field, *e.g.*, could affect the hydrodynamic scale of a gas by affecting the value of the mean free-path of the gas's molecules.³⁶ Finally, the interactions and the associated measurement techniques and methods of calculation contained in \mathcal{K} allow one, at least in principle, to calculate the range of possible, inevitable inaccuracy in a given experimental determination of the value of a quantity under particular conditions.³⁷ Confidence in these techniques and methods will itself, presumably, depend in large measure on the results of *other* theories, those treating the measuring instruments and the relevant environmental factors. How (or whether!) the theoretical dependencies sort themselves out in the end in a more or less consistent fashion is a fascinating question, but one well beyond the scope of this paper. I take it for granted, for the sake of my argument, that the details of this sorting out are irrelevant here.

The kinematical regime of a theory allows one to characterize those spacetime regions that may serve as appropriate arenas of observation and measurement of the quantities of a theory, irrespective of whether or not the dynamical evolution of those quantities in that region match the predictions of the theory to any desired degree of accuracy.

Definition 3.4.2 *Given a theory and its kinematical regime \mathfrak{K} , a \mathfrak{K} -appropriate observatory (or observatory for short) is a canvas O such that*

1. *the values of $\epsilon \cup \mathcal{E}$ in O satisfy \mathfrak{k}*
2. *O satisfies \mathfrak{m}_k*
3. *one can consistently define the infimal decoupage of O*

Observatories are where good experiments relating to the theory may be performed. It is worth remarking that, in certain spacetimes and for certain sets of conditions \mathfrak{k} and \mathfrak{m}_k , a theory may have no observatories at all, or may have no observatories in large swaths of the spacetime.

I believe it is acceptable to restrict observatories to compact subsets, even though this prevents us from specifying initial data on an entire Cauchy surface of a globally hyperbolic spacetime with a non-compact Cauchy surface, such as Schwarzschild spacetime. Only the relations between solutions to

³⁶Note that the constraints represented by the elements of this set do not depend on the types of measurements and interactions considered—they are, as it were, absolute constraints—the character of a particular experiment, depending, *e.g.*, on the sorts of apparatus used for measuring a quantity, may place coarser constraints on the measure of spatiotemporal intervals than those imposed by \mathfrak{m} , but we will not consider this complication at the moment; we assume, at any rate, that no experiment appropriately modeled by the theory ever places finer constraints than those in \mathfrak{m} .

³⁷This set is the cause of yet another in a growing list of qualms I have about the idealizations and simplifications with regard to my characterization of a regime. I doubt seriously that anything even vaguely approximating a complete set of this sort concerning the actual practice of physicists could be compiled, even for a fixed, single moment of time with its fixed state of technical competency for the field as a whole. This state of competency changes, advances and regresses continually, in all sorts of ways. Again, I seek solace in the fact that the task I have set myself consists only in this, a demonstration that such a thing as a regime can in principle be characterized so as plausibly to represent *in specie* the way the practices of the theoretician and of the experimentalist dovetail, if they indeed do.

the partial-differential equations of mathematical physics on the one hand and actual data specified and collected in actual experiments concerns us here. No matter how much we may wish to (or be glad we think we cannot) have the capacity to perform experiments unbounded in spatial and temporal extent, we in fact cannot, given the current state of our theoretical knowledge, technical prowess and organismal construction.

It is worth remarking that, even at this early stage of the game, the idea of a regime makes itself useful: it shortcuts the problem of truncation discussed at the end of §2.4. To ensure the propriety of one of the Navier-Stokes hyperbolizations, for example, one need only demand that the only higher-order terms of a size to manifest effects at the considered scales be those involved in the explicitly introduced novel terms in the equations (assuming that one has laid down an interpretation of those terms by reference to physical quantities amenable to physical probing). It is also important to remark, however, that this is a purely formal solution to that particular ill of the Navier-Stokes hyperbolizations. This may not be a satisfactory physical solution, for it is not obvious at all that it is physically plausible to demand of a Navier-Stokes fluid, when considered at scales comparable to any of its breakdown-scales, that only some small subset of the envisioned transient fluxes be large relative to the others.

4 Physical Fields

Crudely speaking, a *physical theory* is one possessing a fixed kinematical regime. In §5 we will be more precise and propose a somewhat formal definition of a representation of the type of theory in appropriate possession of all the features we have been discussing. In order to get there, we must first complete the work begun in §3.3 above, by making precise the sorts of mathematical objects to be used in the modeling of physical quantities in conformance with a regime. Ordinary scalar fields on spacetime will not serve the purpose, for their range, ordinary scalars, does not account for the inevitable inaccuracy in the determination of the values of physical quantities, as articulated in the kinematical regime of a theory; and such fields do not have the proper domain of definition, which should be the infimal decoupage of a canvas rather than (some subset of) spacetime. The first order of business, then, is to define a space to serve as the appropriate range for our fields to have and to characterize the structure of this space, before using it to define fields over infimal decoupages, which will constitute the desired representation of fields of physical quantities as modeled by a theory with its regime.

To construct an appropriate representation of such quantities, we must first delineate the roles these quantities will be expected to play, which is to say, the sorts of properties they ought (and ought not) to have, and the sorts of operations on them we require they make available to us. We begin by taking up this issue in §4.1, before moving on, in §4.2, to propose a way to define a space of scalar objects suitable to play the delineated role, and in §4.3 to endow this space with algebraic operations in conformance with the results of the reflections in §4.1. In preparation for treating fields of such objects on infimal decoupages, in §4.8, we first consider, in §4.4–§4.7, these scalar fields on

ordinary manifolds, extensions of these fields to the analogues of tensorial fields, and the analogue of linear operators on them, such as derivations and integrals of these.

4.1 Algebraic Operations on the Values of Quantities Treated by a Physical Theory

Since our proximate goal is to define operations akin to integration and derivation on the fields we will construct to represent physical quantities in a way conformable to the requirements of a regime, it would be pleasant to have something akin to a linear, normed structure on a space comprising them, to mimic as closely as possible the behavior of the space of scalars \mathbb{R} and the space of fields Σ composed of these scalars as used in theoretical physics. Before attempting to define and impose such structures, however, we must pause to consider the intended physical meaning of such operations and mappings, what it may mean in the context of physical theory to add together several values of a quantity associated with a physical system, or to multiply such a value by a scalar, and so on.

That discussion, while interesting in itself, may not seem required here, but I think it is, as a simple example suggests. Say we are considering the subtraction, one from another, of two values of a physical quantity, along with their respective, associated inaccuracies. Say that the modeling of a physical interaction requires that we subtract one of the magnitudes of the determined values of the quantities from the other. This seems straightforward enough—one applies the standard, additive group-operation on \mathbb{R} . How ought we combine the inaccuracies, though? One cannot apply the same additive operation, as this may yield a negative value, which makes no sense for the inaccuracy in measurement of a physical quantity (assuming, as we will, that the possible inaccuracy measures the absolute length of the interval within which the determined magnitude of the value may fall). How one does it in practice would seem to depend on circumstances such as the nature of the physical quantity, the nature of the experimental apparatus and techniques employed, *etc.* On the other hand, if one is trying to strike an average over time of the inaccuracies or measure their deviance from some fixed value over time, or some operation of this sort, it may make perfect sense to have a negative value for the inaccuracy. It seems, then, that how one handles the inaccuracy depends at least in part on the sort of operation one wants to apply to the values of the physical quantities. Indeed, I will argue that the signification of standard algebraic operations as applied in physics is not unambiguous in and of itself. We must, therefore, get clear on the different senses they may have, so that, when defining operations on our constructed space including those on the inaccuracies, we may fix the intended sense our operations are meant to schematize, and thus have a partial guide in constructing the operations.

In physics, the application of the same algebraic operation in form and appearance can have one of at least three distinct kinds of signification.³⁸ Consider addition.

1. One can add, at the same point in spacetime, the Faraday tensors representing the Maxwell

³⁸I do not mean to suggest either that the classification I propose includes all possible significations of algebraic operations as applied in physical theory, or that it exemplifies the only method of classification of such operations. This is merely the one I require for the task at hand.

fields associated with two separate charge-distributions, in computing the total Maxwell field at that point, in virtue of the linearity of Maxwell's equation.

2. One can add the vectors representing the velocities of two different bodies with respect to a third, in computing the velocity of one of the two in a new frame of reference, in virtue of the linearity of the Galileian transformations.
3. One can add the values of the gradient of the temperature of a body at two of its separate points, or at two different times, in striking an average, in virtue of the linear, additive group-operation available on the vector-space \mathbb{R}^3 .

The first exemplifies an operation that represents an aspect of the true, physical character, as it were, of a state or process associated with a given type of physical system, in this case the superposability of Maxwell fields. The third exemplifies an operation with no true physical signification whatsoever (this bald statement will be explained in a moment), the computation of the spatially or temporally distributed average of the temperature of a body, but rather one whose employment we find handy for a variety of practical reasons, some of them tending to the furtherance of physical investigation and others to the furtherance of more pedestrian concerns. The second occupies a funny no-man's land: on the one hand, it embodies nothing more than the preferences we often have for the particular form in which we represent to ourselves the states and processes of physical systems and signifies nothing about the true, physical character of the system under study; on the other hand, the nature of mathematical representations of physical theory often, if not always, demands that we muster such a preference even when we would rather not, demands that we choose one from among a fixed class of such superficially different yet physically equivalent forms on the basis of nothing more than our preferences, if theory is to find application in the quantitative modeling of physical phenomena. (This sort of operation has the same character as the fixing of a gauge in a theory with what is known as gauge-freedom, such as classical Maxwell theory, in the sense often discussed in the contemporary philosophical literature. Not all possible such operations as I consider, however, are thought of in the literature as manifestations of a gauge-freedom, so I will not use the terminology of gauges to describe them.)

To keep these three straight, I will nominate them as follows. I will call operations as used in the first context, those appropriate to the representation of a system's behavior having intrinsic physical significance, *physical*; those as used in the second, reflecting our preferences in choosing the representation of a system's quantities, *psychological*; and those in the third, bearing on the practical use we put our representations to, *pragmatic*. I will extend this usage promiscuously, for the qualification of the names of scalars, structures, *etc.* We are not used to distinguishing among these three, I think, because the peculiarly simple properties of the mathematical structures standardly used in theoretical physics allow the use of formally identical algebraic operations to represent all three, and so mask the differences in signification. (This fact already gives one the beginnings of a sense in which the articulation of a regime of propriety grounds the semantical content of a theory.)

Let me try to clarify what I mean with an example illustrating the difference between operations of the first and the second kind. One is reading in a text-book on Newtonian mechanics a description of the modeling of a bicyclist who has been trundling along at 8 mph; the book proceeds to claim that, at a certain time, one ought to multiply 8 by 2 to represent the bicyclist's current speed. Though we do not often think so, there is a possible ambiguity in what the writer is claiming (though, I must emphasize, the ambiguity is almost never a problem for the reader's grasping of the sense of the writer, as context tends to disambiguate it—indeed, context tends to disambiguate it with such an immediacy, clarity and finality as to make us almost never aware of the possible ambiguity in the first place). She may be saying that the bicyclist is now traveling twice as fast as before. She may rather be saying, however, that, for whatever reason, we are changing our units of measurement from miles per hour to half-miles per hour. Likewise, if she says that one ought to add 2 to 8 to represent the current speed of the bicyclist, she may be saying either that the bicyclist is now traveling with a speed of 10 mph, or else that, for whatever reason, we are now changing to a system of units the zero-point of which is what we would have referred to as '-2 mph' in the original one (say, the "laboratory frame", of which text-book writers are so fond).³⁹ Such operations do not manifest themselves in physics only in the choosing of scales and zero-points for units of measure. The inevitable arbitrariness inherent in formulating a Lagrangian representation of a system provides another example. In the case of Lagrangian mechanics, for instance, the presentation of the space of states is, up to trivial isomorphisms, fixed once and for all; the Lagrangian function itself, however, is wildly indeterminate, in the sense that one can, without changing the solution to the equation, add to the Lagrangian any function that will not contribute to its total variation over any path.⁴⁰ The adding of such a function represents only a preference we may have for the representation of the system at hand, and nothing of intrinsic physical significance *vis-à-vis* the system.

To illustrate the differences between the first and the third types of operation, the physical and the pragmatic, consider, again, the operation of addition. Naively, that the values of all physical quantities are represented by real numbers suggests that these values may always be added, and thus that any quantity represented by such a structure satisfies a principle of linear superposition, such as a Maxwell field does. Otherwise, what sense can there be in adding and subtracting the values representing the quantity, as seems to be done when, say, striking averages, as may be done with the values of any physical quantity? In fact, however, this need not be the case. This addition does not, in general, represent the physical superposition of two manifestations of the quantity; rather,

³⁹I make this example intentionally out of the ordinary, with respect to the modification of units, to emphasize the point that the manipulation of the values of quantities treated by every branch of physics displays this ambiguity. The reader may find the point easier to swallow by reframing the example with the use of temperature, in terms of the Fahrenheit and Celsius scales.

⁴⁰To be more precise, the first statement means that, if one is given a manifold on which one can formulate and solve the Euler-Lagrange equation, then it follows that the manifold is the tangent bundle of some manifold, and, moreover, the structure of the space of solutions of the Euler-Lagrange equation suffices for the complete determination of the structure of the original manifold as a tangent bundle over a determinate configuration space. If one then fixes a Lagrangian on this tangent bundle and adds to it a scalar field defined by a 1-form on configuration space, the modified Lagrangian will determine the same dynamical vector-field on the tangent bundle as the original one. This is the content of the second statement. See Curiel (2010a) for details.

it represents a purely formal operation we perform to compute the value of a factitious quantity, such as the average or a certain approximation of the gradient. It makes no physical sense as the proper representation of a real physical operation to add, *e.g.*, the values of the mass-density or the temperature of two perfect fluids mixed together, because those two physical quantities do not satisfy a principle of linear superposition, but it does make sense to ask for the average of those densities and temperatures (a pragmatic operation), just as it makes sense to calculate the resultant density and temperature of the mixture by the addition of weighted terms and to calculate the spatial variation of the values of these quantities (physical operations), even when no physical significance attaches to the adding to or subtracting from the value at one point of that at another.

The real differences among the three can, I believe, be summed up in the following observations. Assume we are treating a physical system with 6 degrees of freedom.⁴¹ Then the physical operations apply to those quantities (the *physical quantities*) of which 6 taken together are necessary and sufficient for the complete determination of the state of the system at a given moment; these operations, furthermore, are such that their employment either signifies some actual modification or qualification to the physical state or dynamical evolution of the system (*e.g.*, by modeling an interaction of the once isolated system with its environment, such as the addition of a non-constant scalar field to the Hamiltonian), or else signifies the calculation of a physical quantity from some already known (or in principle knowable) other physical quantity (*e.g.*, the calculation of the gradient of the temperature of a body from knowledge of its temperature). More precisely (but not rigorously by any means), an operation is physical just in case, given a representation of the space of states of a system accomodating the operation, the operation acts either: 1) as a non-trivial mapping, to itself, of the class of vector-fields representing solutions to the partial-differential equations comprised by the theory treating the system; or 2) as a non-trivial mapping taking (in our example) a set of 6 physical quantities as represented by scalar fields, to a different set of 6 physical quantities as represented by 6 scalar fields, the values of which at a point represent the same state as those of the first set at that point.⁴²

The pragmatic operations apply to quantities (the *pragmatic quantities*) that are such that, though calculable from physical quantities (and indeed calculable *only* from physical ones, perhaps mediately by the use of other pragmatic quantities that are themselves calculated from physical ones), no number of them taken together determine the state of the system at any moment. This statement will perhaps clear up a misunderstanding that may have been engendered by my use of examples. So far I have spoken blithely of averages as essentially unphysical. This is certainly not true in every theory. In statistical mechanics, for instance, the temperature of a body is, roughly speaking, defined as the average of the kinetic energies of the fundamental constituents of a body (fundamental, that is, with respect to the theory employed), which surely is a physical quantity in my sense of the term. It indeed is, in a theory essentially expressive of statistical mechanics, and

⁴¹My use of of the term *degrees of freedom* is perhaps somewhat unusual—I mean by it the dimension of the reduced phase space, not of the reduced configuration space (assuming that any constraints on the system are integrable).

⁴²Note that this characterization holds for quantum systems as well. It can, as well, with some care, be extended to cover systems with an infinite number of degrees of freedom, such as Maxwell fields.

it can in fact serve as one component in a determination of the state of a system as represented by the theory. In thermodynamics, on the contrary, temperature is not an average of anything; it is, if you will, a brute quantity. The point is that a quantity's counting as "physical" or as "pragmatic" depends on the nature of the theory at issue purporting to represent it—it makes no sense to declare a quantity or operation to be physical or pragmatic, in my senses of the terms, absent the context of any theory representing it or within which it finds application.

The *psychological operations* are such as to apply to the same quantities as the physical operations, but only in a Pickwickian sense: their use does not signify any modification or qualification of the physical state or dynamical evolution of the system. More precisely, but again not rigorously, an operation is psychological just in case, given a representation of the space of states of a system accomodating the operation, the operation, up to appropriate isomorphism, commutes with the action of the operator representing the partial-differential equations comprised by the theory. In other words, speaking loosely, solving the equations for a given set of initial data and then executing the psychological operation on the resultant dynamical vector-field yields the same vector-field as does first executing the psychological operation and then solving the equations. It follows that, among many other things, a theory ought to specify what counts as an "appropriate isomorphism" (*e.g.*, a symplectomorphism in Hamiltonian mechanics). In fact, as we have seen, there are (at least) two distinct sub-types of psychological operations, those having to do with the defining of units of measure for physical quantities and those commonly thought of as gauge-transformations.⁴³

It follows from these observations that, whereas the pragmatic operations available to us in the manipulation of the values of physical quantities are fixed once and for all, irrespective of the theory at issue—in physics as commonly practiced, comprising all the richness accruing to the space of real numbers in all its many *personæ* (as an additive group, multiplicative group, field, affine space, vector-space, Hilbert space, topological space, smooth manifold, Lie group, measure space, *et al.*)—, the physical and the psychological operations available to us are dictated by the character of the theory at issue. The spaces representing physical scalars in general are real, one-dimensional, differential manifolds (the minimum structure we demand), as, for example, those representing temperature and mass-density.⁴⁴ The structure of a differential manifold neatly and precisely captures all the fundamental properties required of such scalars—that, *e.g.*, integrals and

⁴³Since I do not think that diffeomorphisms in general relativity are properly thought of as gauge-transformations, for reasons too involved to enter into here, I must bracket their status *vis-à-vis* this classification. I will say only that their character seems to me closer to that of a change in the definition of units of measure than that of a gauge-transformation.

It perhaps points up a weakness in the paper as a whole that general relativity time and again offers up a structure not easily amenable to treatment by my definitions and arguments. Why it is always general relativity that seems to cause trouble in this discussion, as opposed to classical mechanics or quantum mechanics, is, I suspect, a question worth thinking about. Perhaps it has something to do with the inextricability, in the theory, of kinematics from dynamics.

⁴⁴In the case of quantum mechanics, such spaces are sometimes discrete, composed of the eigenvalues of a bounded, self-adjoint operator on the Hilbert space representing the states of the system at issue. I believe that all my definitions and arguments carry over essentially intact to this case, with only cosmetic alteration, though I will not go into any details on the matter.

derivatives make sense and have true physical significance even when addition and subtraction do not—in so far as they represent the values of such quantities. They do not have in general any further structure.

In special cases, such as with the space representing the values of the electric potential in electrostatics, one can impose further, richer structures on the space, such as that of a real affine space. The space of objects representing electric charge (non-quantized) has the further structure of a full vector-space. None of this can be assumed, however; it depends entirely on the nature of the physical quantity under study. In the case of the electrostatic potential, for example, the affine structure represents the fact that, while such potentials satisfy a principle of linear superposition, they have no natural zero-point; in the case of electric charge, we have both a linear operation and a natural zero-point, so we use a vector-space.⁴⁵ The mass-density of a system composed of two fluids that may be mixed in different proportions provides perhaps a more interesting example. In this case, for two masses, we know how to add and to multiply them, we know how to take their ratio, and we know, up to a point, how to subtract them from each other. In the absence of negative mass, however, we do not have a fully linear structure. This space has two natural structures accruing to it, which are isomorphic in a certain sense, though not naturally so. The first is a modular structure, over the associative, commutative ring whose fundamental group is the non-negative, real numbers under multiplication (and so the non-group operation in this case is addition, which implies that the ring has zero-divisors and so is not an integral domain). This represents the fact that, in general, the mass-density of the body consisting of the mixture of the two fluids will be a linear, strictly non-negative combination of the mass-densities of the component fluids. This structure suffices for the defining of operators whose actions correspond to those of integrals and derivatives respectively. The second structure accruing to it is that of a real measure space with a natural Lebesgue measure, which makes available exactly the same set of operations, so long as the restriction on the subtraction of one mass from another is adhered to.

Finally, in virtue of the fact that the space of any physical quantity has, at a minimum, the structure of a differential manifold, we are now in a position to see the proper interpretation of psychological scalars: those associated with a change in the definition of units are the components of particular coordinate presentations of (subsets of) such manifolds; those associated with gauge-transformations, on the other hand, live in a fiber bundle associated with the space of states of the system bearing the quantity, in the sense that the elements of the associated bundle have a natural action on the kinematically and dynamically relevant geometrical structure of that space.⁴⁶ Pragmatic quantities do not, so far as I can see, “live” anywhere. They are simply abstract, mathematical structures, such as the vector space of real numbers.

⁴⁵We will not concern ourselves, for the sake of this example, with such high-falutin’ stuff as magnetic charge and dual rotations of the Maxwell field.

⁴⁶Specifying what counts as “kinematically” or “dynamically” relevant geometrical structure, and correlatively specifying conditions such an action must satisfy to be considered gauge, are issues beyond the scope of this discussion. I know of no single work in which all these questions are discussed taken as a whole. For discussions of various combinations of subsets of them, and related matters, see, *e.g.*, Geroch (1996) and Trautman (1962, 1970a, 1970b, 1980).

4.2 Inexact Scalars

With these considerations in mind, we turn now to the definition of our proposed space of scalars (we need not specify whether we are dealing with physical, psychological or pragmatic scalars until we attempt to introduce operations on the space). It will be convenient to define the following abbreviations. ‘ \mathbb{R}^+ ’ denotes the set $(0, \infty)$ of all strictly positive, real numbers, ‘ \mathbb{R}^\uparrow ’ the set $[0, \infty)$ of all strictly non-negative, real numbers. For $\gamma > 0$, ‘ \mathbb{R}_γ^+ ’ denotes the set (γ, ∞) of all real numbers greater than γ , and ‘ $\mathbb{R}_\gamma^\uparrow$ ’ the set $[\gamma, \infty)$ of all real numbers greater than or equal to γ . For $\omega > 0$, ‘ $\mathbb{R}_{<\omega}$ ’ denotes the set $(-\omega, \omega)$ of all real numbers with absolute value less than ω , and ‘ $\mathbb{R}_{\leq\omega}$ ’ the set of those with absolute values less than or equal to ω . For any two real numbers γ^- and γ^+ , ‘ $\mathbb{R}_{\gamma^-, \gamma^+}$ ’ denotes (γ^-, γ^+) , the space of all real numbers greater than γ^- and less than γ^+ , and ‘ $\mathbb{R}_{[\gamma^-, \gamma^+]}$ ’ denotes $[\gamma^-, \gamma^+]$, the space of all real numbers greater than or equal to γ^- and less than or equal to γ^+ .

Now, let \mathfrak{R} be the space of compact, connected, real intervals of non-zero length. For example, $[0, 1]$ is an element of this space, but $[0, 1)$ is not, nor is $[0, 1] \cup [2, \pi]$, nor $[\pi, \infty)$. Call it the *space of real intervals*. Because we are dealing only with compact, connected, real intervals of non-zero length, the standard Hausdorff metric on a space of sets is in this case a true distance function (*i.e.*, two intervals are at a separation of zero from each other if and only if they are identical).⁴⁷ \mathfrak{R} is a two-dimensional Hausdorff topological space under the topology induced by this metric. I will refer to the greater value of an interval as its *top* and the lesser as its *bottom*. In this parametrization of the space, we will denote the element representing, *e.g.*, the interval $[0, 1]$ by ‘ $(0, 1)$ ’; note that this denotes the ordered pair whose first element is the real number 0 and whose second is 1, and not the open, real interval from 0 to 1. Context should make clear which is meant.

In the event, however, the true Hausdorff metric is not the most useful for our purposes, as it has little physical relevance under the interpretation we will impose below on \mathfrak{R} . We will rather use the following variant of the Hausdorff metric, $\Delta : \mathfrak{R} \times \mathfrak{R} \rightarrow \mathbb{R}^\uparrow$,

$$\Delta((a, b), (y, z)) \equiv |y - a| + |z - b|$$

This is easily shown to be a Euclidean metric. In particular, for all $(a, b), (y, z), (m, n) \in \mathfrak{R}$,

1. $\Delta((a, b), (y, z)) \geq 0$
2. $\Delta((a, b), (y, z)) = \Delta((y, z), (a, b))$
3. $\Delta((a, b), (y, z)) + \Delta((y, z), (m, n)) \geq \Delta((a, b), (m, n))$

as easily verified. It is also easily shown that the topology induced by this metric is the same as that induced by the Hausdorff metric.

Now, one may think of \mathfrak{R} as follows. Let the x -coordinate of the Cartesian plane represent the bottom, and the y -coordinate the top. Because we deal only with intervals of non-zero length, points on the line $y = x$ do not represent elements of the space, nor, by the nature of our chosen

⁴⁷See, *e.g.*, Kolmogorov and Fomin (1970) for the definition of the Hausdorff metric on a set of sets of real quantities.

Theory and Experiment

representation, do points below this line, at which the value of x is greater than that of y . This mapping of \mathfrak{R} into the open half of the plane above the line $y = x$ is one-to-one and onto as well, and so is a point-wise isomorphism. Because, moreover, a continuous curve in the top half of the plane represents the shrinking and expanding in a continuous fashion of an interval on the real line (*i.e.*, the top and the bottom each trace out a continuous curve on the real line), it is natural to endow \mathfrak{R} with the topology induced by this isomorphism, so that it is homeomorphic to \mathbb{R}^2 . This topology, therefore, has all the nice properties one could wish for it, and so we will employ it in what follows. This construction does not essentially depend on the fact that we consider intervals of length greater than zero. For $\gamma^-, \gamma^+ > 0$, let $\mathfrak{R}_{\gamma^-, \gamma^+}$ be the space of intervals of length strictly greater than γ^- and strictly less than γ^+ . By the same argument, using this time the open strip⁴⁸ between the lines $y = x + \gamma^-$ and $y = x + \gamma^+$ rather than the half-plane above $y = x$, it follows that \mathfrak{R}_γ is naturally homeomorphic to \mathbb{R}^2 as well.

The parametrization of \mathfrak{R} by top and bottom is not, in the event, the most useful for our purposes. Because we are interpreting the elements of \mathfrak{R} as ranges of possible inaccuracy, it seems not unreasonable to treat them as though the idealized, determined values about which they are ranges is the mid-point of the interval, which we will take as the first component of a representation of an element of \mathfrak{R} in our new system of coordinates; we take the length of the interval as the second. In this scheme, the interval, say, $[1, 2]$, would represent a determined value of 1.5 with a range of inaccuracy of ± 0.5 , and so would be represented in our new system of coordinates by $(1.5, 1)$. From hereon, unless specifically stated otherwise, \mathfrak{R} will be assumed to be parametrized with respect to these coordinates. For ease of expression, we will sometimes refer to the first component in this parametrization as the *magnitude*, and to the second as the *inaccuracy*. The idea is to have \mathfrak{R} , or some modification of it, serve as the appropriate range of values of fields modeling physical quantities in so far as they conform to the regime of a theory: the interval represents all the values a physical quantity may take, within the range of its possible inaccuracy in measurement and preparation. I will refer to \mathfrak{R} in this guise as the *space of inexact scalars*.

In so far as the second component represents the possible or allowed inaccuracy of the magnitude of a quantity according to the regime of a given theory, it would seem that we ought to work exclusively with the space $\mathfrak{R}_{\gamma^-, \gamma^+}$, for some $0 < \gamma^- < \gamma^+$, or some modification of it, depending on the particular theory at issue. The thought is this. In any experimental arrangement, a non-zero inaccuracy inevitably accrues to the measurement or preparation of initial data. The nature of physical quantities, moreover, as characterized in this paper, strongly suggests that this inaccuracy is in principle strictly bounded from below, away from zero, and strictly bounded from above by some finite value, for every physical quantity treated by a theory with a non-trivial regime. It makes no sense, for instance, to conclude that the inaccuracy in a determination of the time-of-arrival of a particle at a sensor is greater than the known age of the universe, nor does it make any sense to conclude that the inaccuracy is less than 1 over the tetration of 10 by itself (*i.e.*, 10 raised to the power of itself 10 times, $10^{10^{10^{\dots}}}$).

⁴⁸I am tempted to call this a *Las Vegas strip*—always open.

Theory and Experiment

In practice, applying these structures to the modeling of a particular physical theory with its associated regime, one would sometimes want to work with only a single such pair of infimal and supremal inaccuracies for all quantities by the theory. In this case, one may take γ^- to be the supremum of the set of infimal inaccuracies accruing respectively to each of the quantities treated by the theory, and γ^+ to be the infimum of the set of supremal inaccuracies accruing respectively to each of the quantities treated by the theory,⁴⁹ so we will sometimes refer to γ^- in what follows as the *sup-inf inaccuracy*, and to γ^+ as the *inf-sup inaccuracy*. Recall from the discussion just before definition 3.4.1, moreover, that we demand as well that the absolute value of the magnitude of a given quantity, in so far as it is amenable to modeling by the theory, have a supremum, say $\omega > 0$ (for tensorial quantities, the magnitude will have to be expressed in terms of some more or less natural norm imposed on the values of the quantities—see §4.5 below). The magnitudes of our scalars, then, in so far as they are to model only systems amenable to treatment by our theory, will take their values in $\mathbb{R}_{<\omega}$, for some $\omega > 0$, the *space of scalars of kinematically bounded absolute value* (or *kinematically bounded scalars*, for short). We thus really want our scalars to take their values in $\mathfrak{R}_{\omega,\gamma^\pm}$, the space of real, connected, compact intervals of length at least $\gamma > 0$, the supremum of the absolute values of the tops of which is strictly less than ω . Our chosen coordinates, then, take their values in $\mathfrak{R}_{\omega,\gamma^\pm} = \mathbb{R}_{<\omega} \times \mathbb{R}_{\gamma^-, \gamma^+}$. These considerations notwithstanding, we will not bother to keep explicit track of the value of the sup-inf and inf-sup inaccuracies in play. Neither will we bother to keep track of all the suprema of the kinematically bounded values of the magnitudes of all the quantities. Keeping track of either of these two numbers for the (more or less) strictly formal purposes of this section would complicate the exposition without a real gain in perspicacity. Except in a few places where it will be convenient or of interest to re-introduce γ^- , γ^+ or ω explicitly, we will use \mathfrak{R} .⁵⁰ All arguments and results in §4 can be modified so as to be stated in the terms of and hold for $\mathfrak{R}_{\omega,\gamma^\pm}$.

In fact, one can go farther than treating \mathfrak{R} as merely a topological space. One can show that \mathfrak{R} naturally has the structure of something akin to a 2-dimensional smooth manifold “almost” diffeomorphic to \mathbb{R}^2 (in its guise as a two-dimensional manifold). The ‘something akin’ and the ‘almost’ come from the peculiar nature of the intended interpretation of \mathfrak{R} , which requires a few modifications in how we treat its differential structure. When raising issues bearing on or relying on the differential structure, we will treat as admissible only charts that respect the difference, as it were, between the components of \mathfrak{R} . We demand that a chart mapping a subset of \mathfrak{R} to \mathbb{R}^2 never “mix” the two components and, moreover, that the part of the chart mapping the second component restrict its range to \mathbb{R}^+ , in order to comply with the kinematical constraints that led to our construction of \mathfrak{R} in the first place.⁵¹ In effect, we are treating \mathfrak{R} as a two-dimensional space that locally has the structure of $\mathbb{R} \times \mathbb{R}^+$ rather than that of \mathbb{R}^2 . In order to state this a little more

⁴⁹This assumes that every theory treats only a finite number of quantities, or, at least, that the set of infimal inaccuracies of all the quantities is bounded from above, but this does not seem to me an onerous assumption.

⁵⁰“Pay no attention to the man behind the curtain!”

⁵¹If we were keeping explicit track of ω and γ , we would demand that the part of the chart mapping the first component restrict its range to $\mathbb{R}_{<\omega}$, and that mapping the second restrict the range to $\mathbb{R}_\gamma^\uparrow$.

Theory and Experiment

precisely, define the projection operators $\pi_1 : \mathfrak{R} \rightarrow \mathbb{R}$ and $\pi_2 : \mathfrak{R} \rightarrow \mathbb{R}^+$ to be, respectively, projection on the first and second components of elements of \mathfrak{R} : for $(a, \chi) \in \mathfrak{R}$, $\pi_1(a, \chi) = a$ and $\pi_2(a, \chi) = \chi$. An admissible chart $\phi : \mathfrak{R} \rightarrow \mathbb{R} \times \mathbb{R}^+$, then, is one that can be expressed as a pair of diffeomorphisms $\phi_1 : \mathbb{R} \rightarrow \mathbb{R}$ and $\phi_2 : \mathbb{R}^+ \rightarrow \mathbb{R}^+$, in the sense that

$$\pi_1(\phi(a, \chi)) = \phi_1(\pi_1(a, \chi))$$

and

$$\pi_2(\phi(a, \chi)) = \phi_2(\pi_2(a, \chi))$$

The meaning of fixing such a chart is a strictly psychological one, having to do with how one ought to change a given system of units for geometric quantities into another in such a way that respects the relation between the expression of the magnitude in the units of each and the expression of the inaccuracy in each of them. We will not consider it in any detail here, satisfying ourselves with the following observations. One can, in two different ways, decompose \mathfrak{R} into a family of equivalence classes with a group-operation by \mathbb{R}^+ imposed on it, though we will not use these presentations in what follows after this discussion. For the first, consider, for some fixed $\gamma > 0$, the equivalence class of all elements of \mathfrak{R} under the relation “being of the same length”. For example, the intervals $[0, \gamma]$ and $[29, 29 + \gamma]$ are in the same equivalence class, denoted (suggestively) by ‘ $\bar{\mathfrak{R}}_\gamma$ ’. The group-action of $r \in \mathbb{R}^+$ is a multiplicative one, mapping, for example, the equivalence class $\bar{\mathfrak{R}}_\gamma$ to $\bar{\mathfrak{R}}_{r\gamma}$. The space of all such equivalence classes inherits from \mathfrak{R} the structure of a 1-dimensional manifold. For the second, consider the space of equivalence classes of all elements of \mathfrak{R} under the relation “equal up to a multiplicative constant r ”. Thus, for example, the intervals $[1, \pi]$ and $[2, 2\pi]$ are in the same equivalence class. Denote the equivalence class by the mid-point of the unique interval in the equivalence class of length 1. In our example, the equivalence class would be written ‘ $\ll \frac{\pi + 1}{2\pi - 2} \gg$ ’. This is a real, 1-dimensional, affine space, where the affinity is given by the additive group-operation: the addition of a real number $r \in \mathbb{R}^+$ to an element of the space maps, for example, $\ll \frac{\pi + 1}{2\pi - 2} \gg$ to $\ll \frac{\pi + 1}{2\pi - 2} + r \gg$. These two spaces of equivalence classes and their group-actions have clear significance: they are both psychological. Multiplication of an element of the first by a strictly positive real number represents a re-scaling of one’s units of measurement in \mathfrak{R} by that factor. Addition of a scalar to an element of the second represents the choice of a new zero-point, a distance away from the old equal to the magnitude of the added scalar, for one’s unit of measurement in \mathfrak{R} .⁵²

⁵²There seems to be something about the choosing of the scale of units that is richer in physical content than is the choosing of a zero-point for one’s units. For instance, in general relativity, how a geometric object scales when the metric is multiplied by a constant, strictly positive number encodes a lot of information about that object, in particular about the so-called “dimension” of the object, whether it has, *e.g.*, the dimensions of stress-energy or of some other type of physical quantity. It is not difficult to show, for example, that the Riemann tensor, and so the Einstein tensor as well, does not rescale when the metric is multiplied by a constant (which shows, incidentally, that the scalar curvature must rescale as the inverse of the constant). It follows that, since the gravitational constant is dimensionless, a proper stress-energy tensor ought not rescale either, if it is to be a viable candidate for constituting the righthand-side of the Einstein field equation (see Curiel (2000) for details). Indeed, the fact that multiplying the

Finally, before moving on, I must pause long enough to escort one issue high up into the nose-bleed seats of the bleachers, the very back of the gymnasium. Our chosen parametrization of $\mathfrak{R}_{\gamma,\omega}$, and our nomination of its elements as ‘magnitude’ and ‘inaccuracy’, suggests that there may be such a thing as “the actual magnitude of the quantity at issue, which our measurement approximates to, with, we hope, ever smaller error”, in the sense of some variant or other of philosophical realism—*e.g.*, that there may be a single number that represents the actual, determinate value of the pressure in Torricellis inside this yet corked bottle of Taittinger, Brut, 1975, considered as a *Ding an sich*—which we could determine, if only we could make our probes sensitive and accurate enough. Well, there may be and there may not be. I take no stand on the issue in this paper. I do not feel I need to. Nothing in this paper hinges on it. I will sometimes use such words and speak in a manner that may suggest I have firm positions on these matters, but all such talky-talk should be taken with a large pebble of salt. I engage in it for the sake of brevity and ease of expression. The reader may supply such sense as he or she will (or won’t) for the words during such periods of play. Having disposed now of the unruly spectator, expect to hear no more from him.⁵³

4.3 Algebraic Operations on Inexact Scalars

Let us now try to use the considerations of the previous two sections to guide the attempt to impose various sorts of operations on \mathfrak{R} . We begin by dealing with pragmatic operations, as they are easier to manage, being fixed once and for all for all theories. On the face of it, the definition of the pragmatic operations are trivial. We are, after all, defining operations on the magnitudes and the inaccuracies of (potential) measurements of physical quantities the results of which do not purport themselves to be such magnitudes or inaccuracies. The average of a set of temperatures of a body over time is not itself the temperature of anything, and is, indeed, not a physical scalar at all. It’s just a number. There is a serious worry, though: in striking averages, normalizing data-sets, computing standard deviations, and so on, how one ought treat the inaccuracy? In particular, ought one treat the magnitude in isolation from the inaccuracy, so that, *e.g.*, in adding two elements of \mathfrak{R} , the sum of the first components of each would give the first component of the result, irrespective of how the inaccuracies are dealt with? We will hold the second question in abeyance for the moment, assuming its answer to be ‘yes’, though we will return to consider it in our treatment of the physical

metric by a constant does not alter the Riemann or the Einstein tensor shows it is a physically well defined operation; were it to have led to a different Riemann tensor, it would have altered the fundamental physics of the spacetime—it would in effect have defined a different spacetime. No similar proposition holds, so far as I know, with respect to the choosing of a zero-point for one’s units. In fact, one cannot even in general do this in general relativity, in so far as the concept of adding a “constant”, symmetric, two-index, covariant tensor to the metric is not defined in general, and, even when it is (say, in the vector-space \mathbb{R}^4 considered as a manifold), would yield an entirely different Riemann tensor than the first. Why is this?

⁵³For what it’s worth, it is difficult for me to imagine that the question depends on anything more than the kind of semantical analysis one prefers for such words as “real” and “actual” and “empirical”. I suppose if I were pressed on the issue, I would claim to be an atheist 6 days a week, declaring all such questions to be *Scheineprobleme*, but backslide on the Sabbath and come over all religious, declaring myself a knight of faith, and make the leap of the absurd into the waiting embrace of some variant of Peirce’s convergent pragmatism. That, at least, is how I strike myself today.

scalars. Indeed, assuming the answer ‘yes’ makes the definition of pragmatic operations trivial. We simply treat the separate components as the real numbers they are, and pay no heed to any possible relation they may have. It does not matter, moreover, that, *e.g.*, the difference of two inaccuracies may turn out to be negative, for we are not, as already stressed, computing an inaccuracy with these operations, rather only numbers that purport to give us useful information about the inaccuracies. Thus, we need not worry about whether or not these operations respect the restrictions placed on the values of the inaccuracy in \mathfrak{R} . The pragmatic, algebraic operations on \mathfrak{R} , then, are the ordinary, algebraic operations of \mathbb{R} applied to elements of \mathfrak{R} component by component.

Matters are far more difficult when we turn to physical operations, as we now do (we will not treat the psychological—they are beyond the scope of this paper). We will spend some time working through some ultimately unsuccessful attempts, before settling on one that seems to me acceptable, as the failures will be edifying. It will be convenient, for the moment, to re-introduce an explicit value γ for the sup-inf inaccuracy. Let us try first making the simplest choices in defining operations on these spaces, to see how far *naiveté* will carry us. We begin with an additive, a subtractive, a multiplicative and a divisive operation defined, respectively, as follows:

$$\begin{aligned}
 (a, \chi) +_{\phi} (b, \psi) &= (a + b, \chi + \psi) \\
 (a, \chi) -_{\phi} (b, \psi) &= \left(a - b, \gamma_{\phi}^{-}(\chi, \psi) \right) \\
 (a, \chi) *_{\phi} (b, \psi) &= (ab, \gamma_{\phi}^{*}(\chi, \psi)) \\
 (a, \chi) /_{\phi} (b, \psi) &= \left(a/b, \gamma_{\phi}^{\prime}(\chi, \psi) \right)
 \end{aligned} \tag{4.3.1}$$

where

$$\begin{aligned}
 \gamma_{-\phi} &= \begin{cases} |\chi - \psi| & \text{if } |\chi - \psi| > \gamma \\ \gamma & \text{otherwise} \end{cases} \\
 \gamma_{\phi}^{*} &= \begin{cases} \chi\psi & \text{if } \chi\psi > \gamma \\ \gamma & \text{otherwise} \end{cases} \\
 \gamma_{\phi}^{\prime} &= \begin{cases} \chi/\psi & \text{if } \chi/\psi > \gamma \\ \gamma & \text{otherwise} \end{cases}
 \end{aligned} \tag{4.3.2}$$

The ‘ ϕ ’ subscripted to ‘+’, ‘-’, ‘*’ and ‘/’ signifies that these are physical operations. Note that the operations on the right-hand sides of the equals-signs in equations (4.3.1) and (4.3.2) represent physical operations on \mathbb{R} , which is to say, the familiar algebraic operations on real numbers. These all define closed operations, albeit ones with no additive, subtractive, multiplicative or divisive identity in general. The divisive operation is not, in general, commutative, though the other three are. More problematic is the fact that the last three operations are not associative. Assume, for instance, that $0.1 < \gamma < 1$; then $\gamma_{-\phi}(5, \gamma_{-\phi}(1.1, 1)) = 5 - \gamma$, whereas $\gamma_{-\phi}(\gamma_{-\phi}(5, 1.1), 1) = 2.9$. It is difficult to know how to proceed in the definition of other structures such as derivations and integrals without associativity. *Naiveté* has been suggestive, but has not taken us far.

The difficulties involved appear to be twofold. First, while \mathbb{R}^{+} has a multiplicative group structure, it lacks the vector-space structure \mathbb{R} we ordinarily rely on in performing these operations. Even

were \mathbb{R}^+ to have had this, however, it would have been by no means clear that the correct way to have dealt with the inaccuracies associated with two magnitudes of a quantity, in subtracting them, one from another, for example, would have been by subtracting the inaccuracies as well—this could yield a value of zero or even a negative value for the inaccuracy, which is strictly *verboten*, in so far as, in this case, the number is meant to represent the inaccuracy in our knowledge of the magnitude of a physical quantity. The straightforward, unsubtle attack on the problem, in the persons of equations (4.3.1) and (4.3.2), ran squarely into this problem and failed to get past it.

It does not seem far-fetched, moreover, to imagine that, contrary to our assumption in the pragmatic case, in adding two elements of \mathfrak{R} , *e.g.*, the sum of the first components, the magnitudes, will have a non-trivial dependence both on the possible inaccuracies themselves, and on the fine details of how those inaccuracies may combine. This brings us to the second difficulty. In so far as the goal of this paper is to construct a generic model of the joint practice of the theoretician and the experimentalist, we want to define generic operations, once and for all, so as to be applicable to the magnitude and inaccuracy of any quantity, in any physical theory, without any notice taken of any idiosyncratic character of the quantity and the theory, much as the operation of the striking of an average of the value of a quantity in theoretical physics is defined once and for all, and applied promiscuously to all comers, irrespective of the character of the quantity or of its associated theory. I see no way, however, of answering such questions once and for all, with any fineness of grain, in a way applicable to the interplay between real physical data garnered from experiment and the descriptions and predictions offered by theory. The answer, for any particular case, will surely depend on (at least) the nature of the quantity, the nature of the interactions of the system being modeled, the nature of the experimental arrangements employed for observing the evolution of the system during its interactions, and, *a fortiori*, on the nature of the theory and its regime as well. Indeed, I wager there is no way to take account of all these factors even were one to attempt to construct, with even a moderate fineness of grain, for only a particular theory, and for only a restricted class of systems and experimental arrangements treated by the theory, a model of the transformation of the magnitudes of quantities and their associated inaccuracies during the dynamical evolution of such systems by the use of algebraic operations. The way, in particular, that the inaccuracies may combine seems to me to have an irremediably *ad hoc* character, albeit one governed by over-arching, generic, if highly abstract, principles (*e.g.*, that, in the long run, we expect the inaccuracies in determinations of a quantity to decrease, as more and more measurements are taken), just as the rules of hide-and-seek will be freely adapted by children to suit the particular characters of the field of play, the age and condition of the players, temporal constraints on the length of the game, and so on, while still remaining true to the core tenets of the game (for instance, that most of the children will hide and one, or at most a few of the rest, will try to find them). It is lucky for us that we do not require our model to have a fine grain. The nature of the project of this paper demands only that we construct *some* plausible model of the common playground and game of the theoretician and the experimentalist, one that, as it were, “has some seeming to it”, not that we construct one that is natural in some sense, or that is the most accurate (such a thing as which I doubt the existence of, in any sense of the term ‘accurate’, even for a single theory).

Theory and Experiment

To address the issue, we need some at least heuristic considerations to guide us. Consider what is known around the physics department at the University of Chicago as a ‘Fermi problem’. Two of my favorite examples are “How far can a duck fly?” and “How many piano-tuners are there in Chicago?”. The idea is to take a seemingly unanswerable question (in the absence of empirical investigation) and break it down into as many simple components as possible, the measure of simplicity for the components in this case being susceptibility to somewhat accurate, back-of-the-envelope estimation. The hope, then, is that, when one combines all the estimated answers to the simple components to compute the answer for the original question, the errors will tend to cancel each other out and the final result will be reasonably accurate. The name of such problems comes, passed down by word of mouth, from Fermi’s almost preternatural ability to pose and solve them. To give an example, probably the most famous: at the detonation of the first nuclear device, during the Manhattan project in the deserts of Nevada, just before the explosion occurred, Fermi licked his index finger and reached it out just beyond the protective, concrete shield the observers stood behind; at the moment the explosion occurred he reached his clenched fist out just beyond the shield and released a flurry of shredded paper; after the shock-wave passed (about 40 seconds after the explosion), Fermi walked over whither the shock-wave had pushed the shreds, studied them for a moment, turned around and, to what, I am sure, must have been the utter bewilderment of his colleagues (Oppenheimer, Von Neumann, Bethe, Feynman, *et al.*), declared that the explosion had released an amount of energy equivalent to the explosion of 10 kilotons of TNT. 8 weeks later, when the Los Alamos computers, churning away day and night, had finished calculating the energy released on the basis of data collected from the most sophisticated instruments of the day, the result, 18.6 kilotons, differed from Fermi’s estimate by only about 80%, well within an order of magnitude. Fermi already knew (roughly) or had good guesses at data such as: the distance of the shelter from the epicenter of the detonation; the density of the ambient air; the viscosity of the ambient air; the atmospheric pressure; the velocity of the ambient air just before the shock-wave passed; and so on. Based on these data, and on estimates he made on the spot, such as for the volume of the body of air the shreds of paper encompassed, the distance the shock-wave had pushed the shreds back, and how long it had taken it to do so, he computed the amount of energy that would have needed to have been released to have moved a body of air at the given distance from the epicenter, under the given conditions, the distance the air traveled in the time it took to travel that distance, as measured by the flight of the shreds of paper.⁵⁴

It’s easy enough to say that the errors tend to cancel out, but what does this really mean? In the example I gave of Fermi’s computation of the output of energy by the nuclear device, it means something like this. Let’s say that he overestimated the distance from the shield to the epicenter by 5%, underestimated the density of the ambient air by 3%, and so on. With enough such estimates in hand, the distribution of errors should begin to approximate a Gaussian curve centered on 0

⁵⁴After I first heard this story, a friend of mine and I were inspired to try it ourselves—we calculated the number of piano-tuners in Chicago. I came up with 100 and my friend came up with 32. There were something like 40 in the phone-book. Since my friend is an experimental neurophysiologist and I am a philosopher, I didn’t feel so bad. I urge the reader to try it. It’s fun.

(counting underestimates as negative numbers and overestimates as positive). In the worst case, the errors will be concentrated on one side or the other, strongly skewing the total, resulting error; in the best case, one will get something like a perfect distribution and the total, resulting error will approach zero. In the long run, the total, resulting error will tend to oscillate around zero with an average, absolute value of a smaller order of magnitude than the smallest (absolute) error in the bunch, with a variance of an even smaller order of magnitude. No matter how one algebraically combines the magnitudes of the quantities, the same reasoning should apply, that the errors will, in the long run, tend to cancel each other out, whether one is “adding” or “multiplying” or “subtracting”, or what have you, the inaccuracies. We will adopt, therefore, only one template for physical operations on inaccuracies. To err on the simple side, let’s say, then, that, to represent the way the errors combine in such computations as we have just discussed, we require an operation taking two arguments that is associative, commutative, monotonically decreasing in each component separately, and that always yields a value somewhat smaller than the smallest of the two, but never zero. Denote the result of combining two inaccuracies χ_1 and χ_2 by $\alpha(\chi_1, \chi_2)$. Then something like the following suggests itself.⁵⁵

$$\alpha(\chi_1, \chi_2) \equiv \begin{cases} \exp(-1/(\chi_1 + \chi_2)) & \text{if } 0 < (\chi_1 + \chi_2) \leq 1 \\ \frac{1}{e} + \ln(\chi_1 + \chi_2) & \text{if } 1 < (\chi_1 + \chi_2) < \infty \end{cases} \quad (4.3.3)$$

While this proposal has much going for it, it has one marked demerit: it does not meet our requirements, for, while satisfying three of the conditions, it is still not associative.⁵⁶ For example, for the values of three inaccuracies χ_1, χ_2 and χ_3 for which $\chi_1 + \chi_2 + \chi_3 < 1$,

$$\exp\left(\frac{-1}{\chi_1 + \exp(-1/(\chi_2 + \chi_3))}\right) \neq \exp\left(\frac{-1}{\exp(-1/(\chi_1 + \chi_2)) + \chi_3}\right)$$

I believe that, as I have posed it, the problem has no solution. I have not found a proof of the following conjecture (albeit, I have not yet had much time to look for one), but I am reasonably confident it is true.

Conjecture 4.3.1 *There is no $\alpha : \mathbb{R}^+ \times \mathbb{R}^+ \rightarrow \mathbb{R}^+$ simultaneously satisfying these conditions:*

1. α is commutative: for every $r, s \in \mathbb{R}^+$, $\alpha(r, s) = \alpha(s, r)$
2. α is associative, in the sense that, for every $r, s, t \in \mathbb{R}^+$, $\alpha(r, \alpha(s, t)) = \alpha(\alpha(r, s), t)$
3. for every $r, s, s' \in \mathbb{R}^+$, if $\alpha(r, s) = \alpha(r, s')$, then $s = s'$

⁵⁵If we were keeping explicit track of the sup-inf inaccuracy γ , we would have to multiply the exponential by a normalizing factor to ensure it approaches γ rather than 0, and perhaps also add γ to the first and $\gamma - 1/e$ to the second of the constituent functions, if $\gamma > 1/e$.

⁵⁶I do not think the discontinuity in the derivative at $\chi_1 + \chi_2 = 1$ matters. In any event we can always multiply each constituent function by a smoothing factor in a small region encompassing the values for which $\chi_1 + \chi_2 = 1$, to make the entire function smooth.

Theory and Experiment

4. for every $r, s \in \mathbb{R}^+$ such that $r < s$, there exists a unique $t \in \mathbb{R}^+$ for which $\alpha(s, t) = r$
5. for every $r, s \in \mathbb{R}^+$, $\alpha(r, s) < \min\{r, s\}$
6. for every $r, s, t, u \in \mathbb{R}^+$ such that $r < t$ and $s \leq u$, $\alpha(r, s) < (t, u)$

I have a feeling a proof could run along these lines: show that it follows from the first three conditions that one can construct a homeomorphism $\phi : (1, \infty) \rightarrow (1, \infty)$ such that $\phi(r) < r$ for all $r \in (1, \infty)$ and, if one restricts the action of α to the open interval $(1, \infty)$, then $\alpha(\phi(r), \phi(s)) = rs$; it would follow that α could not satisfy the fourth condition (much more the fifth, which I include only because it seems to me a condition one wants to demand of such a function), since $\phi(r) < r < rs$ and $\phi(s) < s < rs$, and so $\alpha(\phi(r), \phi(s)) < rs$. This line of argument suggests itself by dint of the fact that, if one restricts the domain to the open interval $(0, 1)$, then ordinary multiplication satisfies all the conditions, as it does on the domain $(1, \infty)$ as well so long as one reverses all the less-than signs and changes ‘min’ to ‘max’. In any event, these seem to me the minimum conditions a generic, physical operation combining inaccuracies as we require should satisfy, and I can find no consistent way of defining a function that satisfies all the conditions (though, I emphasize, I also have not found a proof that none exists). If this conjecture is true, one could, if one liked, take it as one way to encapsulate precisely my earlier ruminations on the inexorably *ad hoc* and inexact character of such an operation, which will depend on the vagaries of the particular theory, system and experimental arrangement at issue.

We appear to have reached an impasse. All of our attempts to define a generic, physical operation on inaccuracies have come to naught. Indeed, I see only one way forward, and it requires yet another in an ever-growing list of approximations, fudges and hand-waving. Since I do not readily see how to pose the problem differently, we will have to do without a generic, physical operation on inaccuracies that is associative. I think the lack of associativity will turn out to be less of a problem than it may initially seem. I remarked earlier, after our first, naive attempt failed, that it is difficult to know how to proceed without associativity in the definition of other structures such as derivations and integrals. Difficult, yes, but I do not think impossible, at least not in practice. We want only a rule fine enough to guide us without ambiguity in our computations, which at the same time captures adequately the ideas drawn out in our discussion of Fermi problems. In this spirit, I offer the following proposal.

Definition 4.3.2 A compounding family \mathfrak{F} is a family of mappings $\{\alpha_i\}_{i \in \mathbb{I}_2^\uparrow}$, where $\mathbb{I}_2^\uparrow = \{2, 3, \dots\}$, such that, for each $n \in \mathbb{I}_2^\uparrow$,

1. $\alpha_n : \underbrace{\mathbb{R}^+ \times \dots \times \mathbb{R}^+}_n \rightarrow \mathbb{R}^+$ is continuous, surjective and totally symmetric
2. for every collection $r_1, \dots, r_{n-1}, s, s' \in \mathbb{R}^+$, if $\alpha_n(r_1, \dots, r_{n-1}, s) = \alpha_n(r_1, \dots, r_{n-1}, s')$, then $s = s'$
3. for every collection $r_1, \dots, r_{n-1}, s \in \mathbb{R}^+$ such that $s < r_1, \dots, s < r_{n-1}$, there exists a unique $t \in \mathbb{R}^+$ for which $\alpha_n(r_1, \dots, r_{n-1}, t) = s$

Theory and Experiment

4. for every collection $r_1, \dots, r_n \in \mathbb{R}^+$, $\alpha_n(r_1, \dots, r_n) < \min\{r_1, \dots, r_n\}$
5. for every collection $r_1, \dots, r_n, s_1, \dots, s_n \in \mathbb{R}^+$ such that $r_1 < s_1$ and $r_i \leq s_i$ for $i \in \{2, \dots, n\}$,
 $\alpha_n(r_1, r_2, \dots, r_n) < \alpha_n(s_1, s_2, \dots, s_n)$

We will refer to a member of such a family as a *compounder*, and, in particular, to a compounder taking n arguments as an *n -compounder*. To see how we would apply a compounding family in practice, take our earlier proposal, equation (4.3.3). For $(a_1, \chi_1), (a_2, \chi_2) \in \mathfrak{R}$, say for $(\chi_1 + \chi_2) < 1$, the combined inaccuracy is $\alpha_2(\chi_1, \chi_2) = \exp(-1/(\chi_1 + \chi_2))$ and, in general, the combined inaccuracies of n values, for $\sum_{i=1}^n \chi_i < 1$ will be $\alpha_n(\chi_1, \dots, \chi_n) = \exp\left(-1/\sum_{i=1}^n \chi_i\right)$. Although none of these functions is associative, the entire family does allow us to compute without ambiguity the result of any particular physical, inexact, algebraic operation; moreover, it will allow us to define the inexact analogue of partial derivatives and Lebesgue integrals on the inexact fields we will discuss in §4.4 below, analogously to the methods usually employed, using convergent approximations. In so far as we require only these operations, our definition will suffice. We therefore decree that the algebraic operation of combining inaccuracies is represented by a compounding family, the details of which will depend on the nature of the theory and the physical quantity at issue. We will, from hereon, not specify the exact form of the compounding family in play, using only the notation introduced in the definition when we need to refer to compounders in formulæ. We will use the following abbreviation for an exponentiated inaccuracy writing, *e.g.*, the “inexact square” of $\chi \in \mathbb{R}^+$, $\alpha_2(\chi, \chi)$, as ‘ χ^{α_2} ’. We will also allow ourselves the occasional abuse of notation by writing such things as ‘ $\alpha_2(\eta, \zeta)$ ’, for $\eta, \zeta \in \mathfrak{R}$.⁵⁷

I emphasize again that these seem to me only the most minimal conditions one would want to demand of such functions. I can easily imagine more that the character of a particular theory or quantity or experimental situation may require. For example, it seems to me almost certain that a theory would have not a single compounding family but rather a family of such families, one for each possible algebraic combination—physical coupling—of the different quantities among themselves. In the case, say, of the equation of state for an ideal gas, the compounder one would use in computing the inaccuracy accruing to the algebraic product of the magnitude of the pressure and that of the volume may well differ from the compounder used in computing the inaccuracy accruing to the algebraic division of the magnitude of the temperature by that of the volume. The choice of operation, in this case—multiplication or division—indicates the nature of the measurements taken, whether one jointly measures the pressure and the volume in order to calculate the temperature of an ideal gas at equilibrium, or whether one jointly measures the temperature and the volume in order to calculate the pressure. Speaking more generally, it is easy to imagine that the compounder one uses to calculate the resultant inaccuracy after a dynamic process mediated by two separate quantities of the same system will differ from the compounder one uses for a dynamic process mediated by

⁵⁷According to Pavlov, mixed conditioning, mostly positive with some negative thrown in at random, is the fastest type of habituation and leads to the most deeply ingrained habits. Perhaps the abuse’s being merely occasional in this case will serve us well in the end.

the self-interaction of one of those quantities (a non-linear process). In any event, since none of our arguments and results depends on the use of only a single compounding family for a given theory, we lose nothing by not taking account of such issues in what follows.

This leaves us still with the problem of combining the magnitudes of two inexact scalars by physical operations. Let us play for a moment with a toy model, to make the problem slightly more concrete. Fix a theory, a system modeled by that theory, and one of the system's quantities treated by the theory, without regard to the idiosyncratic character of any of the three. We want operations for algebraically combining and comparing the determined values of the magnitudes of the quantity, during the course both of the system's isolated dynamical evolution and of its interactions with other systems; we want these operations to be general enough, moreover, to apply when these magnitudes are determined with any of a number of different methods, perhaps depending on the application of different experimental techniques in different environmental circumstances, while the system is in markedly different states. Say we are to add the values $(100, 10e)$ and $(0.8, 0.1e)$, both in \mathfrak{R} , representing a physical magnitude of the given type of system, for which an additive operation seems required for the representation of an aspect of its physical character (perhaps this quantity satisfies a principle of linear superposition). According to our exemplary compounding family (per equation (4.3.3)), no matter what form we settle on in the end for the additive operation on the magnitudes, the value of the resultant inaccuracy will be 1, which is greater than the magnitude of the second value. Should the result of adding the two, then, be $(100 + .8, 1)$? $(100, 1)$? Or something entirely different? And is it, in the end, reasonable to demand that the answer to this question not depend in any way on the nature of the theory, the system, the quantity or the experimental circumstance modeled?

It is a remarkable fact that all known physical quantities, in so far as they are modeled by physical theories, can find their mathematical representation among a narrowly circumscribed set of mathematical entities and that, correlatively, all known physical interactions can find their mathematical representation among a narrowly circumscribed set of algebraic operations on those sorts of entities. At bottom, for instance, the physical mixing or combination of two physical systems, no matter how exotic are the systems and no matter how exotic are the forces mediating the process, can be represented by the operation of a group. I believe that we often lose sight of how remarkable this fact is, our vision obscured by the very familiarity and seeming "naturalness" of the group-operation.⁵⁸ I can see no reason, *a priori* or otherwise, why matters stand thus, much less why they *must* do so, if indeed they must, in some sense of the term. Every sort of physical operation could have found

⁵⁸Rilke puts it finely:

und Das und Den
die man schon nicht mehr sah
(so täglich waren sie und so bewöhnlich)
auf einmal anzuschauen: sanft, versöhnlich
und wie an einem Anfang und von nah. . .

– from "Der Auszug des Verlorenen Sohnes"

its natural representation in a structure not translatable into the terms of any other also modeling another physical operation. This would entail no logical inconsistency.⁵⁹ In any event, the actual state of affairs suggests that our search for a single operation or set of operations to represent the requisite form of the combination of magnitudes during the course of physical operations may not present so formidable a face as it first seemed to. If we can find a reasonable archetype for the elements of such a set of operations on the elements of \mathfrak{R} , enough to recapitulate, *mutatis mutandis*, the basic algebraic structures of \mathbb{R} used in physics as ordinarily performed, I believe we will have done enough.

Our analysis of errors arising from our discussion of Fermi problems provides once again a clue to a way forward. As we remarked there, we expect in the long run that the errors in the determination of a quantity will distribute themselves evenly around the mid-point of the interval of possible inaccuracy, which is to say, around the magnitude, approximating to a Gaussian. In particular, this means that we expect, again in the long run, for the actual value of the quantity to lie half the time above the magnitude (the mid-point of the interval of possible inaccuracy), and half the time below. If you squint your eyes just right, it sort of follows from these considerations, in conjunction with our definition of a compounding family, and helped along by our faithful crutch, the demands of the nature of this project, that it would not be unreasonable to compute the value of the resultant magnitude under a physical operation by use of the ordinary, corresponding algebraic operation that \mathbb{R} makes available to us, applied directly to the magnitudes of the elements of \mathfrak{R} at issue. At least, I believe this will suffice for a first-order approximation, as it were, and will not lead us too grossly astray. Our proposed set of basic, physical, algebraic operations on \mathfrak{R} , then, are as follows.

$$\begin{aligned}
 (a, \chi) +_{\phi} (b, \psi) &= (a + b, \alpha_2(\chi, \psi)) \\
 (a, \chi) -_{\phi} (b, \psi) &= (a - b, \alpha_2(\chi, \psi)) \\
 (a, \chi) *_{\phi} (b, \psi) &= (ab, \alpha_2(\chi, \psi)) \\
 (a, \chi) /_{\phi} (b, \psi) &= (a/b, \alpha_2(\chi, \psi))
 \end{aligned}
 \tag{4.3.4}$$

Note that the algebraic operations applied to the magnitudes on the righthand sides of equations (4.3.4) are the ordinary ones from \mathbb{R} . We will call such an algebra an *inexact (scalar) algebra*.

Whatever else may be the case about these operations, we demand at a minimum that they be “as linear as they can”. In this case, that means we require a group action on \mathfrak{R} that interacts with the operations in the appropriate way. There at least three ways one may impose such an action, the first two by the group \mathbb{R} and the third by the group \mathbb{R}^+ . For $r \in \mathbb{R}$

$$r *_{\phi} (a, \chi) = (ra, \chi) \tag{4.3.5}$$

and

$$r *_{\pi} (a, \chi) = (ra, r\chi) \tag{4.3.6}$$

⁵⁹And that is not the be-all, end-all of problems. But it’s one that we philosophers take seriously.

Theory and Experiment

and for $r > \mathbb{R}^+$

$$r *_{\psi} (a, \chi) = (ra, r\chi) \quad (4.3.7)$$

Equation (4.3.6) corresponds to the pragmatic operation of multiplying an element of \mathfrak{R} by a real number r , which we will make some use of below. It is linear over the pragmatic operations (*i.e.*, the ordinary algebraic operation on \mathbb{R} , applied component by component to elements of \mathfrak{R}) in the ordinary sense. (4.3.7) corresponds to the psychological operation of rescaling one's units, and we will not bother with it further. (4.3.5) is the physical one, used, for instance, when, in calculating the kinetic energy of a particle from its mass and velocity, one multiplies the product of the mass and the square of the velocity by 1/2: this represents the calculation of a physical quantity from one already given, and so represents a physical operation, as the notation suggests, but not one that increases or decreases the possible inaccuracy in any way, since the operation represents no process by which there could have been an increment or decrement in physical knowledge; thus this group-action does not affect the inaccuracy. it is "linear" in the sense that, for all $(a, \chi), (b, \psi)$ and $r \in \mathbb{R}$,

$$\begin{aligned} r *_{\phi} [(a, \chi) +_{\phi} (b, \psi)] &= (ra + b, \alpha_2(\chi, \psi)) \\ &= (ra + rb, \alpha_2(\chi, \psi)) \\ &= (ra, \chi) +_{\phi} (rb, \psi) \\ &= r *_{\phi} (a, \chi) +_{\phi} r *_{\phi} (b, \psi) \end{aligned} \quad (4.3.8)$$

I will call this property *inexact (physical) linearity*. Any space having the structure of an inexact algebra with such a group action satisfying inexact linearity defined on it is an *inexactly linear space*. From hereon, I will drop the subscripted ' ϕ ', *etc.*, from the signs denoting algebraic operations, as context should disambiguate the sort of operation meant.

Having a notion approximating to a linear group action by \mathbb{R} suggests the possibility of having a norm on \mathfrak{R} as well. Again, the discussion of inaccuracy in the light of Fermi problems points to a natural way of imposing one. We want all the values in the interval of possible inaccuracy to contribute in some way or other to the value of the norm, but not all equally, in so far as the values furthest from the mid-point are, we posit, the least likely to occur. In the long run we expect the errors, in the determination of the magnitude of a quantity, more or less to distribute themselves evenly around zero, approximating to a Gaussian. The notion that, in the long run, the actual magnitudes will tend correlatively to distribute themselves in a Gaussian around the mid-point of that interval suggests that, to compute the norm of an element of \mathfrak{R} , we integrate over the interval using a Gaussian-weighted measure. There are many ways of doing this. The one I propose seems to me simple, clear, and not devoid of physical content. Given $(a, \chi) \in \mathfrak{R}$, its norm will have a form something like

$$\|(a, \chi)\| \equiv \frac{1}{\nu(a, \chi)} \int_{(a - \frac{\chi}{2})}^{(a + \frac{\chi}{2})} \frac{-y^2(y - a)^2}{2(y - (a - \frac{\chi}{2}))(y - (a + \frac{\chi}{2}))} dy \quad (4.3.9)$$

where $\nu(a, \chi)$ is a normalizing factor that guarantees the value of the integral shrinks smoothly to a as the interval itself shrinks to zero (note that $\|(a, \chi)\| > |a|$), and the open parenthesis prepended to

the lower-limit of the integral and that appended to the upper limit jointly indicate that the integral is to be taken over the open interval rather than the closed one.⁶⁰ If one likes, this represents the “expectation value” of the quantity. It is straightforward to show that this mapping satisfies the definition of a norm (using the physical group-action posited above), *i.e.*, for all $(a, \chi), (b, \psi) \in \mathfrak{R}$ and $r \in \mathbb{R}$,

1. $\|(a, \chi)\| \geq 0$
2. $\|r(a, \chi)\| = |r|\|(a, \chi)\|$
3. $\|(a, \chi)\| + \|(b, \psi)\| \geq \|(a, \chi) + (b, \psi)\|$

This norm induces the same topology as does the metric Δ . (To see this, note that there is a homeomorphism h of \mathfrak{R} into itself such that $\Delta((a, \chi), (b, \psi)) \mapsto \|h((a, \chi) - (b, \psi))\|$.)

This mapping, strictly speaking, satisfies the letter of the definition of a norm, but does not seem to exemplify its spirit. It fails only in so far as there is no element in \mathfrak{R} whose norm is 0: for example, $\|(a, \chi) - (a, \chi)\| = \|(0, \chi^{\alpha_2})\| > 0$. This may seem problematic, but I think it makes physical sense. Let us say that the subtraction in this case represents the difference in values of a particular quantity associated with two numerically distinct but otherwise identical physical systems. This difference will be zero only inexactly, as it were, in so far as there is a non-zero inaccuracy accruing to the magnitude in the determination of each of the two values. This norm will indeed approach arbitrarily closely to zero in the limit as the inaccuracy shrinks to zero, but it will never make it there, as the inaccuracy will never itself be zero. We will work around this issue in the following way. Equation (4.3.9) will remain our “official” definition of the norm on \mathfrak{R} , the one we will refer to and exploit when we need explicit use of one; in secret, however, we will know that any quantity whose sup-inf inaccuracy is γ has as its “real” norm

$$\|(a, \chi)\|_{\gamma} \equiv \|(a, \chi)\| - \|(0, \gamma)\| \tag{4.3.10}$$

where the norm-signs on the righthand side of the formula refer to that defined by equation (4.3.9).

In the end, one ought to have no illusions about the adequacy of this treatment of the algebraic structure of inaccuracy and error as they appear in all their multifarious roles in physics; it is only a crude, and still very much naive, treatment, but one, I hope, that suffices for the aims of this paper.

4.4 Inexact Scalar Fields and Their Derivations

Let us call any mapping that has \mathfrak{R} as its range an *inexact field*, and in particular one whose domain is (a subset of) spacetime an *inexact scalar field*. We will sometimes refer to ordinary scalar fields on spacetime as *exact scalar fields*, to emphasize not only the difference, but the fact as well that inexact scalar fields constitute a certain sort of approximation to ordinary scalar fields, which, in the limit as the inaccuracy goes to zero, converges to an ordinary scalar field. The asymmetry in

⁶⁰More precisely, the integral should be taken over the closed interval $[a - \frac{\chi}{2} + \epsilon, a + \frac{\chi}{2} - \epsilon]$, where $\epsilon < \frac{\chi}{2}$, and then the limit of this integral taken as $\epsilon \rightarrow 0$.

Theory and Experiment

the naming reflects the fact that I do not want to seem to have a bias in favor of the theoretical structures by bestowing on them the honorific ‘accurate’.⁶¹

We want to define the analog of fields of compact support. Since the second component of $\mathfrak{R}, \mathbb{R}^+$, has no natural additive identity ε , we cannot define the support of an inexact field to be the set of points at which the value of the field equals $(0, \varepsilon)$, as we would otherwise naturally do. Let’s consider how such a thing as the idea of the support of an inexact field would be used in practice, then. For all intents and purposes, inexact scalar fields used to model the values of physical quantities in a \mathfrak{K} -appropriate observatory will have well defined values only in (some subset of) the spatiotemporal region representing the observatory. Outside that region, the values of the fields are assumed to be negligible with regard to the dynamic evolution of the fields in that region. As we remarked earlier, in practice every physical quantity, as modeled by a specific theory with a regime, will have associated with it a sup-inf inaccuracy $\gamma > 0$. We stipulate, then, that outside the region of the observatory, that physical quantity be represented by the value $(0, \gamma) \in \mathfrak{R}$. It is therefore natural to define the support of an inexact field ζ , $\text{Supp}[\zeta]$ to be the closure of the set of points at which it takes the value $(0, \gamma)$. We will also say that ζ is *inexactly zero* outside its support. Thus, we can restate the definition: the support of an inexact field is the closure of the complement of the set of points at which it is inexactly zero. More generally, we say that an inexact scalar field is *inexactly constant* if its value at every point is (k, γ) , for fixed $k \in \mathbf{R}$. It is worth keeping in mind that the notion of being inexactly zero, and so that of the support of an inexact scalar field, makes sense only in so far as one has fixed such a γ . We will not bother doing so explicitly in what follows, as it is easy enough to do so as the requirements of the case at issue warrant; indeed, as we have already remarked, doing so would complicate the exposition needlessly, with no corresponding gain in perspicuity.

Let, then, $\Sigma_{\mathfrak{R}}$ be the space of inexact scalar fields of compact support on \mathcal{M} . We need deal only with fields of compact support in virtue of definition 3.4.2, that of a \mathfrak{K} -appropriate observatory. In order to define the analogue of derivations on inexact fields, we need a class of operators on $\Sigma_{\mathfrak{R}}$, analogous to the linear ones on Σ , to consider. $\Sigma_{\mathfrak{R}}$ as a whole inherits the algebraic structure of an inexactly linear space from \mathfrak{R} , just as Σ inherits a linear structure from \mathbb{R} . By dint of the topology and differential structure of \mathfrak{R} , moreover, inexact scalar fields have natural notions of continuity, n -times differentiability, and smoothness accruing to them. Denote the subspaces of $\Sigma_{\mathfrak{R}}$ comprising only inexact scalar fields having those properties by ‘ $\Sigma_{\mathfrak{R}}^0$ ’, ‘ $\Sigma_{\mathfrak{R}}^n$ ’ and ‘ $\Sigma_{\mathfrak{R}}^\infty$ ’ respectively. Each of these is clearly an inexactly linear space as well. They have as well a natural notion of boundedness, in virtue of the norm on inexact scalars defined by equation (4.3.9). An inexact scalar field is bounded if and only if the supremum of the norms of the values it takes at all points of its domain is finite. This supremum in turn defines a norm on $\Sigma_{\mathfrak{R}}^b$, the space of bounded inexact scalar fields, the analogue of the sup-norm for exact scalar fields.

Let us say, then, that an operator $\Gamma : \Sigma_{\mathfrak{R}} \rightarrow \Sigma_{\mathfrak{R}}$ is *inexactly linear* if it respects the inexactly linear structure on $\Sigma_{\mathfrak{R}}$ respectively. In more detail, an operator Γ is inexactly linear if, for $\zeta, \eta \in \Sigma_{\mathfrak{R}}$

⁶¹It is well to keep in mind that ‘exact’ may be a term of derogation as well as of approbation. I invite the reader to recall the last sciolistic, pedantic lecture he or she has heard, in all its empty exactitude.

Theory and Experiment

and $r \in \mathbb{R}$,

$$\mathbf{\Gamma}(r\zeta + \eta) = r\mathbf{\Gamma}\zeta + \mathbf{\Gamma}\eta$$

Note that we use the physical group-operation for multiplication by r , that defined by equation (4.3.5). We impose an inexactly linear structure on the space of all inexactly linear operators in the standard way, by, *e.g.*, defining the addition of two of them, $\mathbf{\Gamma} + \mathbf{\Psi}$ by its action on inexact scalar fields: $(\mathbf{\Gamma} + \mathbf{\Psi})\zeta = \mathbf{\Gamma}\zeta + \mathbf{\Psi}\zeta$.

Turning now to differential operators in particular, the analogy with ordinary differential structure on real manifolds suggests that we define a smooth, inexact vector-field ξ^A on \mathcal{M} to be an inexactly linear operator on $\Sigma_{\mathbb{R}}^\infty$, satisfying a few collateral conditions. We continue to deal only with physical operations unless explicitly stated otherwise. In particular, the derivations we define are those appropriate for use in physical computations, not pragmatic. By parity of reasoning, the differential, pragmatic, inexact operations are as straightforward to define as were the algebraic ones. If we rely on the analogy with ordinary differential structure in defining derivations, we want the analogy to go as deep as it can, as it were. We would like, *inter alia*, to be able to associate with an inexact vector at a point a curve (with a fixed parameterization) passing through that point: the curve to which the vector is tangent. Standard treatments deal with this by fixing a chart around the point, pushing the field down to \mathbb{R}^n via the chart, finding the curve such that the derivative of the field with respect to its affine parameter equals the action on the field by the vector on the manifold, and pulling the curve back up to the manifold by the chart. The derivative of a field on \mathbb{R}^n with respect to the affine parameter of a curve is defined, when using charts, by one's taking the limit of the difference in values of the field at the point of the curve in question and at a neighboring point on the curve, dividing the difference by the affine distance between the two points, and taking the limit as the distance goes to zero.

We want to define the analogous operation on inexact scalar fields by use of a similar procedure. Say we are to compute the directional derivative of the inexact scalar field ζ along the curve η at the point q , where the affine parameter of η is s_q . Fix a chart (U, ψ) such that $q \in U$. What is the physical content of such an operation? Naively, one may picture it something like this. However we end up characterizing an inexact derivative, we expect it will consist of an ordered pair, the first component of which is something like an exact tangent vector, and the second component a representation of the inaccuracy accruing to the determination of that exact tangent vector. Computing the derivative of a quantity in the laboratory generally involves making (at least) two measurements of that quantity very close to each other in spacetime, taking the difference and dividing the magnitude by the separation of the events of measurement. The representation of physically combining these two inaccuracies comes precisely to computing the value of the two by the operation of a compounder. This suggests that we employ an operation as straightforwardly analogous to the ordinary directional derivative as possible, something like

$$\begin{aligned} \left. \frac{d(\zeta \circ \eta)(s)}{ds} \right|_{s=s_q} &\equiv \lim_{h \rightarrow 0} \frac{\zeta \circ \psi^{-1}(\psi \circ \eta(s_q + h)) - \zeta \circ \psi^{-1}(\psi \circ \eta(s_q))}{h} \\ &= \lim_{h \rightarrow 0} \frac{\zeta \circ \eta(s_q + h) - \zeta \circ \eta(s_q)}{h} \end{aligned} \tag{4.4.1}$$

Theory and Experiment

In this case, since the physical, subtractive operation acts on the inaccuracy by application of a compounder, that part of the difference in the numerator will not tend to zero but rather to

$$\lim_{h \rightarrow 0} \alpha_2(\pi_2 \circ \zeta \circ \eta(s_q + h), \pi_2 \circ \zeta \circ \eta(s_q)) = (\zeta \circ \eta(s_q))^{\alpha_2} \quad (4.4.2)$$

where, recall, ‘ $(\zeta \circ \eta(s_q))^{\alpha_2}$ ’ denotes the inexact square of $\pi_2 \circ \zeta \circ \eta(s_q)$. It follows that the division by h must use the physical operation of the group \mathbb{R} on \mathfrak{R} .

Several manifest difficulties attend on this way of doing it, in virtue of the fact that the inaccuracy at a point accruing to the inexact tangent vector, as defined by this method, is, in essence, a scalar element of \mathfrak{R} , for it is the same for each component of the directional derivative at that point, and, indeed, the same for the computation of any directional derivative of the inexact scalar field at that point. It follows that the space of inexact tangent vectors at a point, according to this method, is a 5-dimensional, inexactly linear space, each element of which consists in effect of an ordered pair the first component of which is an exact tangent vector and the second an element of \mathbf{R}^+ . This fact raises two puzzles. First, it seems as though the inaccuracy accruing to a determination of the magnitude of the directional derivative of a quantity may depend on the direction along which one makes the measurements. Second, and on a related note, what may it mean to represent the inaccuracy accruing to the determination of a vectorial quantity by a scalar? Of what exactly is it the inaccuracy in the measurement of?

Let us try a second proposal for the inexact directional derivative, to address these questions. The awkwardness in the first way arose almost entirely from the fact that the limit’s definition in equation (4.4.1) ensured that the value of the limit at a point for the inaccuracy depended on nothing else but the value of the inaccuracy at that point, and so the computation yielded a scalar to represent the inaccuracy of a vectorial quantity. The spirit rather than the letter of the ordinary, exact operation of taking a directional derivative points to perhaps the simplest way to avoid this problem,

$$\pi_1 \circ \frac{d(\zeta \circ \eta(s))}{ds} \Big|_{s=s_q} \equiv \lim_{h \rightarrow 0} \pi_1 \circ \frac{\zeta \circ \eta(s_q + h) - \zeta \circ \eta(s_q)}{h} \quad (4.4.3)$$

$$\pi_2 \circ \frac{d(\zeta \circ \eta(s))}{ds} \Big|_{s=s_q} \equiv \lim_{h \rightarrow 0} \pi_1 \circ \frac{\zeta \circ \eta(s_q + h) - \zeta \circ \eta(s_q)}{(\zeta \circ \eta(s_q))^{\alpha_2} + h} \quad (4.4.4)$$

The inaccuracy yielded by this computation manifestly depends on the direction along which the derivative is taken, and does so in a natural way. We thus obtain a vectorial kind of quantity for the total inaccuracy as determined using an orthonormal quadruplet of tangent vectors—the *vectorial inaccuracy*—, making the space of inexact tangent vectors at a point an 8-dimensional, inexactly linear space (four dimensions for the magnitude and four for the inaccuracy associated with this vectorial magnitude, one member of \mathbb{R}^+ for each component of the magnitude, the inaccuracy in the direction of the coordinate-axis the component of the magnitude was computed for). Thus, as with inexact scalar fields, inexact tangent vectors have no natural, additive identity.

This formulation of the directional derivative presents its own set of difficulties, primary among them the question whether or not it has physical content relevant to our project. Looking only at the

math suggests that to measure a vector is to fix a structure encoding a general rule for determining the rate of change of any given scalar quantity, no matter what that quantity may be, as measured along a particular spatiotemporal direction.⁶² From a physical point of view, such a goal is nonsense. When measuring vectorial quantities, as, for example, the electric 4-current in special relativity, one attempts to determine the particular rate of change of a particular scalar quantity—in this case, an electric charge-density—in a particular spatiotemporal direction. There is no sane way to derive from the numbers resulting from such a measurement, or from the methods employed in coming to them (the use of galvanometers, *etc.*), the sort of generic rule the math encodes.⁶³ The measurement of the 4-current, nonetheless, exemplifies the application of such a rule. Although the tools and techniques one may use to determine the spatiotemporal rate of change of any particular scalar quantity will depend on the nature of the quantity, the schema, as it were, of the determination remains the same: make as many measurements as one can, along the line that one wants to determine the spatiotemporal rate of change of the quantity, as close to the point of interest as possible, and grind through a computation of the standard form (equation (4.4.1)). Perhaps the most striking fact about the nature of the information one needs to give content to this schema is how basic it is—one need know only the differential structure of the spacetime manifold to compute the directional derivative of a scalar field at a particular point. Computing that value, however, is not the end-all, be-all of physics. One wants to compute the total rate of change of the quantity, itself a vectorial quantity, and one wants to be able to compare in a meaningful way the magnitude of this vectorial quantity with that of others of the same type. To perform these operations one needs, at a minimum, knowledge of the affine structure of spacetime, and, in general, that of the metric structure. We do not ordinarily need to invoke this knowledge explicitly, as we are almost never in a position requiring fine knowledge of the affine structure for the planning of a measurement—“flat” is almost always an excellent approximation—which, perhaps, is why we rarely realize the nature of the operation we are performing. It thus becomes clear that, in measuring a vectorial quantity such as the electric charge-current, we are not attempting to abduct from the results of the measurement the structure of a general rule; we are rather applying an already known general rule to a particular case. This constitutes the physical content in the application of equation (4.4.3).

The question about the physical content of equation (4.4.4) remains. To investigate it, let us try to refine somewhat our example of the measurement of a vectorial quantity in the laboratory.

⁶²We do not consider the measurement of vectorial amplitude-fields representing fundamental particles (*e.g.*, photons) in quantum field theory. It is not clear to me whether or not this treatment can be extended to treat that case. I suspect not.

⁶³*Cf.* Born (1943, p. 39), discussing the inadequacy of naive operationalism to define the quantities representing observables in quantum mechanics: “I cannot see what experimental ‘operation’ could be devised in order to define a mathematical operator [on a Hilbert space].” Compare also Eddington (1923, p. 120–1):

If we are to surround ourselves with a perceptual world at all, we must recognize as substance that which has some element of permanence. We may not be able to explain how the mind recognizes as substantial the world-tensor[, *i.e.*, the Einstein tensor, $R_{ab} - \frac{1}{2}g_{ab}R$], but we can see that it could not well recognize anything simpler. There are no doubt minds which have not this predisposition to regard as substantial the things which are permanent; but we shut them up in lunatic asylums.

Theory and Experiment

Let us say that, for whatever reason, we are attempting to measure the directional derivative of a scalar quantity along two different lines at the same point, on the first of which, as one moves away from the point of measurement, the inaccuracy sharply and monotonically increases, whereas on the second of which it sharply and monotonically decreases. The question whether or not to use equation (4.4.4) in calculating the inaccuracy of each directional derivative reduces in this case to the question whether the inaccuracies accruing to the measurement of both ought to differ from each other in accordance with our general rule, as the magnitudes do. It seems in fact they ought to. To measure the quantity along the line of increase, one will perform a sequence of ever more inexact [*** why “ever more”? ***] measurements as one moves farther from the point. *Prima facie*, a greater inaccuracy will accrue to the total, resulting determination of the quantity along this line than will accrue to that along the line of decreasing inaccuracy, and, indeed, the more quickly the inaccuracy increases or decreases along these lines, the greater the difference of the two should be. Considerations of these sort justify the use of equation (4.4.4) in determining the inaccuracy accruing to the measurement of the directional derivative of a scalar quantity.

There remains another problem concerning the physical cogency and possible significance of the idea of a vectorial inaccuracy itself. The inaccuracy of an inexact scalar serves to define a bounded region of the real line containing what we have referred to as the magnitude of the inexact scalar, that region in which we have reason to believe the “actual” value of the quantity being measured lies. In what way, if at all, may a vectorial inaccuracy define an analogous region around the exact vector constituting the magnitude of an inexact vector? The answer seems clear enough on the face of it. It seems that a vectorial inaccuracy may, in one sense, be considered nothing more than an exact vector with strictly positive components in all coordinate systems. This suggests that we take the convex hull in the exact tangent vector-space determined by the magnitude of an inexact vector and by its vectorial inaccuracy, considered, as suggested, as an ordinary exact vector. As inviting as this sounds, at this point the suggestion can not even be wrong. It does not make enough sense to be wrong. First of all, even if we could make sense of thinking of a vectorial inaccuracy as an exact vector, such a convex hull would be only two-dimensional. We would expect, however, that, in so far as the inaccuracy of a vectorial quantity includes uncertainty about its direction, the possible directions in which it may point subtend a non-trivial, three-dimensional solid angle in spacetime (as determined by the ambient spacetime conformal structure). If we also assume, as seems not unreasonable, that the inaccuracy in the determination of the direction of the vector cannot be so severe as to make it possible that the real value of the vector points in exactly the opposite direction from the determined magnitude, and, moreover, that it must be such as to permit the real value of the vector to lie only in that half-space of the whole tangent space bounded on one side by the three-dimensional hypersurface orthogonal to the determined magnitude and containing that magnitude, then it follows that the entire region in which we have reason to believe the actual value of the vector lies forms something like a four-dimensional cone, with its vertex consisting of the point lying in the orthogonal hypersurface at the foot of the vector representing the determined magnitude. I say “something like” a cone, because, if the possible inaccuracy does not permit the real value of the vector to be zero, then we will end up with a topological 4-sphere of exact vectors. In any event, we

Theory and Experiment

cannot make sense of thinking of a vectorial inaccuracy as an exact vector: it obeys utterly different transformation laws. We will not be able to address adequately the issue of the physical congenity and significance of a vectorial inaccuracy until §4.5.

This discussion also points to a subtle, at this point strictly mathematical problem with the characterization of the inaccuracy of tangent vector-field thus far, considered as an inexactly linear operator on $\Sigma_{\mathfrak{R}}^{\infty}$. It is clear how to deal with the compounding of these vectorial inaccuracies when considering the sum of two inexact tangent vector-fields: we side-step the issue, noting that the sum of two inexactly linear operators on $\Sigma_{\mathfrak{R}}$ is just that one defined by applying each summand separately to the argument of the sum and summing the two resultant inexact scalar fields. Because we already know how to compound inaccuracies for inexact scalar fields, this presents no problem. We define the compounded inaccuracy of the summed inexact tangent vector-fields to be the second component of that inexact tangent vector-field that acts in the way defined by the sum. It is not clear, however, how one is to compound the inaccuracies when one multiplies an inexact tangent vector by an inexact scalar, as we surely will want to do, for example, when calculating the static Coulomb force on a charged particle in a central field by multiplying the value of the charge by the value of the Coulomb field at its position. A compounding family as we have characterized it will not serve the purpose: in so far as it is not clear what one may mean by comparing a scalar inaccuracy to a vectorial inaccuracy using the “less-than” relation, as required by items 3, 4 and 5 of definition 4.3.2, we have no way of defining a compounder to meet this need. We will postpone this discussion as well, and its resolution, until §4.5 below.

We need now demonstrate only that our proposal satisfies a Leibniz rule in order to declare it an appropriate representation of the directional derivative, modulo the difficulties we have postponed. This is easily done, in the same way as in the exact case.

Definition 4.4.1 *A smooth, inexact, tangent vector-field on \mathcal{M} is an inexactly linear operator $\xi^A : \Sigma_{\mathfrak{R}}^{\infty} \rightarrow \Sigma_{\mathfrak{R}}^{\infty}$ satisfying the Leibniz rule: for $\phi, \chi \in \Sigma_{\mathfrak{R}}^{\infty}$*

$$\xi^A(\phi\chi) = \phi\xi^A(\chi) + \chi\xi^A(\phi)$$

In the same way as in the exact case, one can as well characterize these vectors by a slightly more general characteristic, that of being *inexactly affine*, in the sense that their action on $\Sigma_{\mathfrak{R}}^{\infty}$ is determined only up to the addition, to the operand, of a constant inexact scalar field.

As in the definition, we will indicate the indexical structure of these objects using the abstract-index notation of Penrose and Geroch (see, *e.g.*, Wald (1984) for an account of the notation), with majuscule indices. Exact tangent vectors and tangent vector-fields will be denoted as well using the abstract-index notation, with miniscule indices, *e.g.*, ‘ ξ^a ’. We extend to inexact tangent vector-fields (and, later, to higher-order tensorial and affine objects) the action of our projection operators π_1 and π_2 in the obvious way: for every $q \in \mathcal{M}$, $\pi_1 : T_{[q, \mathfrak{R}]} \mathcal{M} \rightarrow T\mathbb{R}^4$ and $\pi_2 : T_{[q, \mathfrak{R}]} \mathcal{M} \rightarrow T(\mathbb{R}^+)^4$ are, respectively, projection on the first and second components of elements of $T_{[q, \mathfrak{R}]} \mathcal{M}$. Note, finally, that there will be, in general, an endless family of inexact tangent vector-fields the actions of which on the same inexact scalar field all agree on the first component of their respective, resultant inexact scalar fields.

The smooth, inexact tangent bundle on \mathcal{M} , $T_{\mathbb{R}}\mathcal{M}$, is constructed in the usual way from these fields. Though this bundle is analogous in many ways to that of the ordinary, exact tangent bundle over a real manifold, there are important disanalogies as well. First of all, $T_{\mathbb{R}}\mathcal{M}$ is 12-dimensional rather than 8-dimensional, as its fibers themselves are 8-dimensional, diffeomorphic to $\mathbb{R}^4 \times (\mathbb{R}^+)^4$. We will write the fiber over the point $q \in C$ as $\mathcal{T}_{[q, \mathbb{R}]} \mathcal{M}$. As with $\Sigma_{\mathbb{R}}^{\infty}$, $T_{\mathbb{R}}\mathcal{M}$ has no distinguished zero cross-section. Still, there are important analogies. It is easy to see, for instance, that if the group-action on the fibers is topologically trivial then $T_{\mathbb{R}}\mathcal{M}$ is the trivial bundle, consisting of the topological product of the base space by the fiber.⁶⁴ Thus, non-trivial, global cross-sections do exist for such inexact tangent bundles. One can always impose an orientation on $T_{\mathbb{R}}\mathcal{M}$, moreover, in terms of the inexact structures, in the person of a non-zero, inexact 4-form, as in the ordinary, exact case (see §4.5 for a sketch of a characterization of inexact differential forms and tensors in general). Let $\mathcal{T}_{\mathbb{R}}^{1,0}$ be the space of smooth, global sections of $T_{\mathbb{R}}\mathcal{M}$ (*i.e.*, of smooth, inexact tangent vector-fields), and the space of sections of the exact tangent bundle $\mathcal{T}^{1,0}$.

4.5 Inexact Tensorial Fields and Their Derivations

We want to construct inexact tensorial spaces of all orders and indexical structures in, again, the usual way, by marching up the ranks of indices, as it were, starting with the definition of cotangent vector-fields as inexactly linear operators on inexact tangent vector-fields, and so on. We know the general form we want an inexact tensor of a given indexical structure to have: a first component consisting of an ordinary, exact tensor of the given indexical structure, and a second component consisting of the inaccuracy that, in some way or other, accrues to the measurement of this exact tensor. Recall the considerations that led us to take equations (4.3.4) as the definition of an inexact scalar algebra, in particular how we arrived at the form the operations should take when restricted to the first component, that in the long run the errors should more or less wash out and we should end up with the magnitude one would have gotten by applying the ordinary operations to the magnitudes in the first place. I believe these same considerations are as suitable (or not) here as there, and so we conclude that, when applying any sort of algebraic operation to an ordered set of inexact tensors (how ever we end up defining these things in full)—whether it be contravention on multiple indices of multiple inexact tensors, or multiplication of an inexact tensor by an inexact scalar, or contraction of indices on a single inexact tensor, or what have you—the calculation of the resultant first component, the magnitude of the resultant inexact tensor, will be independent of the calculation of the resultant second component, the inaccuracy, and will be, moreover, the result of applying the algebraic operation to the first components of the inexact tensors in the ordered set.

The only delicacy in the process lies in characterizing the way the inaccuracies combine under these algebraic operations, and characterizing, indeed, what form the inaccuracies should take in general for tensorial objects. We will take a cue from our treatment of inexact tangent vector-fields as inexact operators on $\Sigma_{\mathbb{R}}^{\infty}$, for one can demand that, as in the exact case, inexact cotangent vector-

⁶⁴More precisely, if the group-action on the fibers of the inexact tangent bundle arises from a group that is *solid* (for a precise characterization of which, see, *e.g.*, Steenrod (1951, §12.1)), then the bundle space is trivial.

fields ought to be inexactly linear operators on inexact tangent vector-fields, and inexact tensorial fields of arbitrary indexical structure ought to be inexactly linear operators on ordered sets of inexact tangent and cotangent vector-fields. We will not go deeply into the details here (I suspect you know by now where to find those), limiting ourselves rather to a brief sketch.

After the statement of definition 4.3.2 we remarked that different interactions mediated by different quantities, as treated by the same theory, likely would require the analogue of different compounding families for calculating the inaccuracies resulting from such different interactions. In so far as each index of a multi-index tensor potentially represents a physical interaction of a sort different from those represented by the other indices—or, if you like, represents “half” of such an interaction—the second component of an inexact tensor, that representing the inaccuracy accruing to the determination of the value of a quantity the tensor models, will in general have associated with it something like a family of compounding families, one family for each way of contravecting one of the tensor’s indices with the indices of other objects representing quantities the theory models. In particular, this shows that, as with inexact tangent vectors, the second component of an inexact tensor will in general be a tensor-like quantity—the *tensorial inaccuracy*, as we will call it—with the same indexical structure as the first component of the inexact tensor, its magnitude. Note that, as an ordinary, exact tensor-space is isomorphic as a vector-space to \mathbb{R}^n for some $n \in \mathbb{I} + \uparrow$ (though not naturally so), a space of tensorial inaccuracies, all of the same indexical structure, is diffeomorphic to $(\mathbb{R}^+)^n$ for some $n \in \mathbb{I} + \uparrow$ as well (though, again, not naturally so).

Now, in light of our considerations after equation (4.4.4) about the way to handle the mathematical issue of compounding the inaccuracies when one adds two inexact tangent vector-fields, we know already how to compound the inaccuracies when adding two inexact tensor-fields of arbitrary indexical structure, in so far as we consider those fields to be inexact operators over ordered sets of inexact tangent and cotangent vector-fields (over every such set, to be more precise, the contravecting of whose elements, term by term, with the indices of the given inexact tensor-field will saturate the indices of the tensor-field, yielding an inexact scalar field). In fact, we now know even a little more, for one can clearly use the same techniques to define the inaccuracy accruing to the result of a contraction of two indices on an inexact tensor. The problem we postponed in the same discussion, though, that of the proper way to compound the inaccuracies when multiplying an inexact tangent vector-field by an inexact scalar field, not only remains, but has become aggravated by the introduction of new algebraic operations we wish to impose on our inexact quantities, to wit, tensor-products and contravections. This observation suggests that the proper way to treat the compounding of inaccuracies for tensorial quantities is to extend definition 4.3.2. Items 3, 4 and 5, however, prevent us from performing such an extension in any more or less obvious, straightforward way, in so far as no obvious analogues of the “less-than” relation suggests itself for tensorial objects. We need a way to apply something like the “less-than” relation not only to ordered pairs consisting of a scalar and a vectorial inaccuracy, but as well to ordered pairs consisting of tensorial inaccuracies of arbitrary indexical structure. We will circumvent this problem by the introduction of a device that, at the moment, will likely appear purely formal and mostly *ad hoc*, but will prove itself, in §5.2 below, to have, under an appropriate interpretation, a use of real physical significance integral to

Theory and Experiment

the completion of this project. We will use this device here to extend the notion of a compounding family to cover the compounding of tensorial inaccuracies, which will complete our account of the fundamentals of inexact tensorial fields. The device consists of the imposition of a norm on tensorial inaccuracies, the values of which can be compared using the ordinary “less-than” relation.

We begin by imposing a norm on the space $\mathcal{T}_{\mathbb{R}}^{1,0}$ of inexact tangent vector-fields. We cannot define this norm with respect to the length of vectors as determined by a distinguished inexact semi-Riemannian metric, as, at this point, we have no such distinguished metric (indeed, we do not at this point, strictly speaking, know what such a thing is). Fix some $0 < k < \infty$, for $k \in \mathbb{R}^+$. Let $\Sigma_{\mathbb{R},k}^{\infty}$ be the subspace of the space $\Sigma_{\mathbb{R}}^{\infty}$ consisting of all smooth, inexact scalar fields uniformly bounded by k , *i.e.*, all those fields contained in the ball in $\Sigma_{\mathbb{R}}^{\infty}$ of radius k , as determined using the sup-norm. More precisely, let $\Sigma_{\mathbb{R},k}^{\infty}$ be the interior of this ball. In other words, no field in $\Sigma_{\mathbb{R},k}^{\infty}$ has a norm of k ; rather all are strictly less than k . Let us say, then, that an inexact tangent vector-field ξ^A of compact support is *k-bounded* if

$$\sup_{\zeta \in \Sigma_{\mathbb{R},k}^{\infty}} \{ \|\xi^A(\zeta)\| \} < \infty \quad (4.5.1)$$

(where $\|\xi^A(\zeta)\|$ is the sup-norm of the inexact scalar field resulting from the application of ξ^A to ζ). The value of this supremum for ξ^A is its k -norm, which we will write ‘ $\|\xi^A\|_k$ ’, to emphasize its dependence on k . Fix now $\mathcal{T}_{\mathbb{R},k}^{1,0}$, family of smooth, inexact tangent vector-fields uniformly (and strictly) bounded by k with respect to the k -norm. Again, this family constitutes the interior of the ball in $\mathcal{T}_{\mathbb{R}}^{1,0}$ of radius k with respect to the k -norm. By considering inexact cotangent vector-fields to be inexactly linear operators on $\mathcal{T}_{\mathbb{R}}^{1,0}$, we can now define the family of k -bounded inexact cotangent vector-fields by repeating essentially the same procedure, define its k -norm, and so define the subset $\mathcal{T}_{\mathbb{R},k}^{0,1}$ of $\widehat{\mathcal{T}}_{\mathbb{R}}^{0,1}$ consisting of the open ball of radius k as defined by its k -norm.⁶⁵ Marching up the ranks of indices in the usual way yields a k -norm on the space of inexact tensorial fields of any indexical structure (m, n) , defined as inexactly linear operators on ordered sets consisting of n inexact tangent and m inexact cotangent vector-fields. We will speak, therefore, of k -bounded inexact tensor-fields promiscuously, irrespective of their exact indexical structures.

Recall, moreover, that, as part of the definition of a kinematical regime, we demanded that the values of the fields and of some number of their “partial-derivatives” be uniformly bounded. k -norms provide the means for making this requirement precise. An inexact field ζ is *uniformly k-bounded to first-order* if $\zeta \in \Sigma_{\mathbb{R},k}^{\infty}$ and

$$\sup \left\{ \|\xi^N(\zeta)\| : \xi^A \in \mathcal{T}_{\mathbb{R},k}^{1,0} \right\} < \infty \quad (4.5.2)$$

Similarly, ζ is *uniformly k-bounded to second-order* if

$$\sup \left\{ \|\eta^M(\xi^N(\zeta))\| : \eta^A, \xi^A \in \mathcal{T}_{\mathbb{R},k}^{1,0} \right\} < \infty \quad (4.5.3)$$

⁶⁵Note that this account so far makes sense in the terms of our previous definitions and arguments, for we know how to characterize the compounding of inaccuracies for inexactly linear operators whose range is $\Sigma_{\mathbb{R}}$, even though we do not yet know how to do so for inexactly linear operators with ranges other than $\Sigma_{\mathbb{R}}$.

Theory and Experiment

and so on.⁶⁶ These suprema are the values to be used in determining whether or not the fields satisfy the regime’s constraints on the boundedness of “partial-derivatives” of admissible fields. Note that this is a different question from whether or not the field itself satisfies some particular differential constraint. This is rather an algebraic constraint on the derivatives of the field. We will see at the end of this section how to extend the notion of l -th order k -boundedness to inexact tensorial quantities.

This gets us closer to what we want, but we have not yet arrived. We require, at the moment, a norm on the tensorial inaccuracy, not on the inexact tensor-field as a whole. In fact, we can extract the appropriate norm on the inaccuracies by extending our projection operators π_1 and π_2 , in the obvious way, to inexact tensorial objects. To begin, we will use $\pi_2[\Sigma_{\mathfrak{R},k}^\infty]$ for this rather than $\Sigma_{\mathfrak{R},k}^\infty$, *i.e.*, we will use the space of exact scalar fields composed of the fields of scalar inaccuracies of those fields in $\Sigma_{\mathfrak{R},k}^\infty$. Given $\zeta \in \Sigma_{\mathfrak{R},k}^\infty$, for example, the corresponding field $\zeta_2 \equiv \pi_2(\zeta)$ in $\pi_2[\Sigma_{\mathfrak{R},k}^\infty]$ is that defined by assigning to the spatiotemporal point q in the domain of ζ the value $\zeta_2(q)$. We can use the ordinary sup-norm for this space. Consider now the space of vectorial inaccuracies $\pi_2[\mathcal{T}_{\mathfrak{R},k}^{1,0}]$. We define $\xi_2^A(\zeta_2)$, the derivation of ζ_2 by $\xi_2^A \in \pi_2[\mathcal{T}_{\mathfrak{R},k}^{1,0}]$, by $\pi_2(\xi^A(\zeta))$, where $\zeta \in \Sigma_{\mathfrak{R},k}^\infty$ and $\xi^A \in \mathcal{T}_{\mathfrak{R},k}^{1,0}$ are such that $\zeta_2 = \pi_2(\zeta)$ and $\xi_2^A = \pi_2(\xi^A)$. By construction, for any $\xi_2^A \in \pi_2[\mathcal{T}_{\mathfrak{R},k}^{1,0}]$,

$$\sup_{\zeta_2 \in \pi_2[\Sigma_{\mathfrak{R},k}^\infty]} \{ \|\xi_2^A(\zeta_2)\| \} < \infty$$

(where $\|\xi_2^A(\zeta_2)\|$ is the sup-norm of that exact scalar field). The value of this supremum for ξ_2^A is its k -norm, which we will, again, write ‘ $\|\xi_2^A\|_k$ ’, to emphasize its dependence on k . We extend this norm to tensorial inaccuracies of arbitrary indexical structure in exactly the same way as we did for the norms on inexact tensor-fields. This construction, in fact yields a family of norms on k -bounded, exact tensorial fields in general.

We are now in a position to characterize the generalization of compounding families to inexact tensors. Consider the uses a 2-compounder of tensorial inaccuracies may be put to. Since we need to know how to compound pairs of such inaccuracies with arbitrary combinations of indexical structures, we will need a separate 2-compounder for each possible combination—one, for example, for compounding a scalar and a vectorial inaccuracy, as well as one for compounding a (3, 4)-tensorial inaccuracy with a (4398, 9)-tensorial one. Only so much will still not suffice, for we will need separate ones for, *e.g.*, the contravention of a (3, 4)-tensorial inaccuracy with a (4398, 9)-tensorial one in all possible contraventional combinations, and a separate one for taking the tensor-product of them. To simplify matters a little, when considering all possible contraventional combinations of two types of tensorial inaccuracy, we will require separate compounders only for all possible resulting tensorial inaccuracies having numerically distinct indexical structures. In our example of contraventioning a (4398, 9)-tensor with (3, 4)-tensor, for instance, we would require only two separate compounders to deal with the cases where the contraventions yielded, say, a (4394, 6)-tensor and a (4399, 11)-tensor respectively. We will not require one for every possible way of contraventioning each index on the one with each index on the other so as to yield a tensor of the resultant indexical structure.

⁶⁶In fact, all these notions are most clearly, usefully and elegantly expressed in terms of jets and their inexact, mottled analogues, but we do not have the time or the space to rehearse such a discussion.

Theory and Experiment

This leaves us still with the need for an enumerably infinite number of 2-compounders: one for each possible ordered triplet of pairs of indices such that tensors of the indexical types represented by the first two pairs in the triplet can be contravected so as to yield one of a type represented by the third pair; and one for each possible ordered triplet of pairs of indices such that the tensor-product of tensors of the indexical types represented by the first two pairs in the triplet yields one of a type represented by the third pair. Let us call such a triplet of ordered pairs an *indexically possible triplet*. Note that, in attempting to describe all the possible combinations for an n -compounder, for $n > 2$, we would need to consider not indexically possible triplets but rather indexically possible $(n + 1)$ -uplets. A single definition of a compounding family that attempted to cover all this ground in one go—simultaneously defining n -compounders for all n , for all indexically possible $(n + 1)$ -uplets—would be all but incomprehensible. I will therefore offer a definition only for a 2-compounder of tensorial inaccuracies. The extension to compounders taking any number of arguments should then be clear, though tedious to construct explicitly.

Fix a complete family of k -bounded subsets of inexact tensorial fields of all ranks. Let $E \equiv \{((m_i, n_i), (p_i, q_i), (r_i, s_i))\}_{i \in \mathbb{I}^\uparrow}$ be an enumeration of indexically possible triplets. We will write, for example, the second ordered pair in the n^{th} item in the enumeration as ‘ $E(n, 2)$ ’, and a tensor space having this indexical structure as ‘ $\mathcal{T}^{E(n, 2)}$ ’.

Definition 4.5.1 *A k -bounded family of 2-compounders $\mathfrak{F}_{k, 2}$ is a family of mappings $\{\alpha_{2, i}\}_{i \in \mathbb{I}^\uparrow}$, such that, for each $n \in \mathbb{I}^\uparrow$,*

1. $\alpha_{2, n} : \mathcal{T}_{\mathfrak{R}, k}^{E(n, 1)} \times \mathcal{T}_{\mathfrak{R}, k}^{E(n, 2)} \rightarrow \mathcal{T}_{\mathfrak{R}, k}^{E(n, 3)}$ is continuous, surjective and totally symmetric
2. for every $\lambda \in \mathcal{T}_{\mathfrak{R}, k}^{E(n, 1)}$ and $\mu, \mu' \in \mathcal{T}_{\mathfrak{R}, k}^{E(n, 2)}$, if $\alpha_{2, n}(\lambda, \mu) = \alpha_{2, n}(\lambda, \mu')$, then $\mu = \mu'$
3. for every $\lambda \in \mathcal{T}_{\mathfrak{R}, k}^{E(n, 1)}$ and $\nu \in \mathcal{T}_{\mathfrak{R}, k}^{E(n, 3)}$ such that $\|\nu\|_k < \|\lambda\|_k$ there exists a unique $\mu \in \mathcal{T}_{\mathfrak{R}, k}^{E(n, 2)}$ for which $\alpha_{2, n}(\lambda, \mu) = \nu$
4. for every $\lambda \in \mathcal{T}_{\mathfrak{R}, k}^{E(n, 1)}$ and $\mu \in \mathcal{T}_{\mathfrak{R}, k}^{E(n, 2)}$, $\|\alpha_{2, n}(\lambda, \mu)\|_k < \min\{\|\lambda\|_k, \|\mu\|_k\}$
5. for every $\lambda, \lambda' \in \mathcal{T}_{\mathfrak{R}, k}^{E(n, 1)}$ and $\mu, \mu' \in \mathcal{T}_{\mathfrak{R}, k}^{E(n, 2)}$ such that $\|\lambda\|_k < \|\lambda'\|_k$ and $\|\mu\|_k \leq \|\mu'\|_k$, $\|\alpha_{2, n}(\lambda, \mu)\|_k < \|\alpha_{2, n}(\lambda', \mu')\|_k$

We will refer to a member of such a family as a *k -bounded, tensorial 2-compounder*. A collection of such families for all $n \in \mathbb{I}_2^\uparrow$ is a *k -bounded, tensorial compounding family*, \mathfrak{F}_k .

With this in hand, we now know how to characterize contravector and tensor-products for inexact tensor-fields, by analogy with equations (4.3.4). It is tempting straightaway to define a tensorial algebra on k -bounded, inexact tensorial fields, in the obvious way, but this will not quite work, for these spaces are not closed under the considered algebraic operations, as, for example, the sum of two k -bounded tangent vector-fields, say, is not itself necessarily k -bounded, nor is the tensor-product of two k -bounded tensors, or their contravector. The countably tensorial product of all these spaces is, however, convex, in the sense that, *e.g.*, for any two k -bounded vector-fields ξ^A and η^A , and any $r \in [0, 1]$,

$$r \xi^A + (1 - r) \eta^A$$

Theory and Experiment

is itself k -bounded, with the analogous statement holding for, respectively, multiplication by an inexact, k -bounded scalar field, contravention, contraction and the tensor-product on finite numbers of inexact tensors. For example, for any two k -bounded vector-fields ξ^A and η^A , and any $r \in [0, 1]$, the tensor-product

$$\frac{r}{k}\xi^A \otimes \frac{1}{k}\eta^A$$

is k -bounded. We will refer to operations of this form as k -convex. This suggests

Definition 4.5.2 *The convex algebra of k -bounded, inexact, tensorial fields over a differential manifold \mathcal{M} is an ordered pair $(\mathcal{T}_{\mathfrak{R},k}, \mathfrak{F}_k)$ consisting of the k -convex tensor-product of all k -bounded, inexact, tensorial spaces, with the algebraic structure imposed on it by the family of k -convex operations, and a k -bounded, tensorial compounding family.*

There are some similarities with the ordinary, exact tensorial algebras, such as the following

Proposition 4.5.3 *For every $m, n \in \mathbb{I}^\dagger$, an inexact tensor-field of indexical structure (m, n) is in $\mathcal{T}_{\mathfrak{R},k}^{m,n}$ if and only if it can be expressed as an inexactly linear sum of tensor-products of m inexact k -bounded tangent and n inexact k -bounded cotangent vector-fields.*

This follows from the compactness, connectedness and convexity of the space underlying the algebra.

We can recapitulate all these definitions and arguments to construct a true, inexactly linear algebra (*i.e.*, one closed under all algebraic operations), by restricting attention to uniformly bounded inexact scalar fields rather than restricting ourselves to k -bounded fields. We then characterize a set of inexact tangent vector-fields as those satisfying the analogue of equation 4.5.1 for uniformly bounded inexact scalar fields. Call the norm so defined the Σ -norm and the space of such inexact tangent vector-fields Σ -bounded, normed, inexact tangent vector-fields, $\mathcal{T}_{\mathfrak{R}}^{1,0}$. In order to define algebraic operations on this space, we generalize definition 4.5.1 in the obvious way to handle Σ -bounded rather than k -bounded entities. It is then easy to see that $\mathcal{T}_{\mathfrak{R}}^{1,0}$ is closed under addition, as well as under multiplication by uniformly bounded inexact scalar fields, and so is a true inexactly linear space. One now marches up the ranks of indices in the standard way, using our generalized family of tensorial compounding families, leading to the Σ -bounded, normed, inexactly linear tensor-algebra, $(\mathcal{T}_{\mathfrak{R}}, \mathfrak{F})$, an algebraically complete, inexactly linear tensor-algebra over the space of uniformly bounded inexact scalar fields.

These constructions allow us now to address the issues we raised in §4.4, following our proposal of equation (4.4.4), about the physical cogency and possible significance of tensorial inaccuracies. We want to know whether we can understand the tensorial inaccuracy of an inexact tensor as determining a topological 4-sphere within which lies not only the determined magnitude of the inexact tensor, but within which as well we have reason to believe the real value of the tensor lies. For the sake of simplicity, we will work with the Σ -bounded, inexact, tensor-algebra. Fix $\lambda \in \mathcal{T}_{\mathfrak{R}}^{m,n}$, with determined magnitude $\lambda_1 = \pi_1(\lambda)$ and tensorial inaccuracy $\lambda_2 = \pi_2(\lambda)$. Then the 4-sphere of possible values for the quantity represented by λ is defined as the ball of radius $\frac{1}{2}\|\lambda_2\|$ in $\mathcal{T}_{\mathfrak{R}}^{m,n}$ centered on λ . For small enough $\|\lambda_2\|$, where λ is, say, an inexact tangent vector, this may allow the real value of λ to

point in the opposite direction as λ_1 , but, this does not to be objectionable, in so far we this will, in general be possible only for very small vectors, where such a possibility does not seem far-fetched.

Before moving on, it will be instructive to consider in some detail the construction of the analog of the Lie derivative, as a derivation on the Σ -bounded algebra of inexact tensor-fields. Fix a smooth inexact vector-field ξ^A on C . We need first to characterize the analogue of integral curves for it. We will choose the simplest analogue to the exact case, declaring that the integral curve of ξ^A is the integral curve of the exact tangent vector-field associated with it, $\pi_1(\xi^A)$. As always, there are many ways one could do this, some more involved than others. This one, with its simplicity and physical content, suits our purposes. We can associate with ξ^A a family of diffeomorphisms $\{\xi_h\}_{h \in \mathbb{R}^+}$ of spacetime onto itself, the “flow” of the vector-field, in the standard way (we assume without further comment that ξ^A is complete, at least in the canvas C). Each of these diffeomorphisms define a new inexact tangent vector-field $\xi_h \circ \eta^A$ from a given one η^A by dragging its values along the integral curves of ξ^A a given distance with respect to the parametrization of the integral curves (in this case, a distance of h). Then it is easy to see that, for any smooth inexact tangent vector-field, the following limit is defined without ambiguity and exists, and so defines the first component of a new inexact tangent vector-field $\mathcal{L}_{[\xi, \mathfrak{R}]} \eta^A$,

$$\pi_1 \circ \mathcal{L}_{[\xi, \mathfrak{R}]} \eta^A \equiv \lim_{h \rightarrow 0} \frac{1}{h} \pi_1 (\eta^A - \xi_{-h} \circ \eta^A) \quad (4.5.4)$$

The same considerations as led us to choose equation (4.4.4) over equation (4.4.1) for the definition of the inexact directional derivative imply that we cannot define the second component of the inexact Lie derivative to be

$$\pi_2 \circ \mathcal{L}_{[\xi, \mathfrak{R}]} \eta^A \equiv \lim_{h \rightarrow 0} \frac{1}{h} \pi_2 (\eta^A - \xi_{-h} \circ \eta^A) \quad (4.5.5)$$

as it will not depend on ξ^A . The proper limit will not be so easy to define as was that of equation (4.4.4), in particular what the divisor of the difference should be. As in equation (4.4.4), it seems likely that it should be $h + N(\xi^A, \eta^A)$, where $N : T_{[q, \mathfrak{R}]} \mathcal{M} \times T_{[q, \mathfrak{R}]} \mathcal{M} \rightarrow \mathbb{R}$ is a linear, normalizing function. Presumably, it will depend on some general characteristic of the way that $\xi_{-h} \circ \eta^A$ approaches η^A as h goes to zero. It is beyond the scope of this paper to consider ways of making this idea precise. We will assume, therefore, that the second component of the inexact Lie derivative is given by

$$\pi_2 \circ \mathcal{L}_{[\xi, \mathfrak{R}]} \eta^A \equiv \lim_{h \rightarrow 0} \frac{1}{h + N(\xi^A, \eta^A)} \pi_2 (\eta^A - \xi_{-h} \circ \eta^A) \quad (4.5.6)$$

Note that, if one is keeping explicit track of the sup-inf inaccuracy, (4.5.6) would have to be modified as follows:

$$\pi_2 \circ \mathcal{L}_{[\xi, \mathfrak{R}]} \zeta \equiv \lim_{h \rightarrow 0} \frac{1}{h + N(\xi^A, \eta^A)} \pi_2 (e^\gamma (\eta^A - (\xi_{-h} \circ \eta^A))) \quad (4.5.7)$$

Again, one must keep in mind the primary difference between this inexact Lie derivative and the ordinary Lie derivative, to wit, that the inexact Lie derivative is only inexactly linear, not fully linear. As an example of the differences consider, for $\gamma > 0$, the inexact Lie derivative of a constant inexact scalar field, $\zeta \in \Sigma_{\mathfrak{R}}$. For any smooth, inexact vector-field $\xi^A \subset T\mathcal{M}_{\mathfrak{R}}$, then, $\mathcal{L}_{[\xi, \mathfrak{R}]} \zeta$ equals the constant inexact scalar field whose value at every point is $(0, \gamma)$, not $(0, 0)$, the latter not even

being an element of \mathfrak{R} . The two are very much analogous in other ways, however. For instance, for $\zeta \in \Sigma_{\mathfrak{R}}^{\infty}$, $\mathcal{L}_{[\xi, \mathfrak{R}]} \zeta = \xi^A(\zeta)$, and, for another $\omega \in \Sigma_{\mathfrak{R}}^{\infty}$, $\mathcal{L}_{[\xi, \mathfrak{R}]}(\zeta\omega) = \zeta\mathcal{L}_{[\xi, \mathfrak{R}]} \omega + \omega\mathcal{L}_{[\xi, \mathfrak{R}]} \zeta$. It defines a Lie algebra as well. Note that the inexact Lie derivative of a Σ -bounded, inexact tensor-field may not itself be Σ -bounded, so care must be taken when one attempts to algebraically combine the result of a Lie-derivation of an inexact tensor-field with a Σ -bounded, inexact tensor-field, as the operation may not be well defined.

We can use the inexact Lie-derivative to extend the idea of n^{th} -order k -boundedness from inexact scalars field to inexact tensorial quantities. An inexact tensorial quantity λ is *uniformly k -bounded to first-order* if $\lambda \in \mathcal{T}_{\mathfrak{R}, k}^{m, n}$ and

$$\sup \left\{ \|\mathcal{L}_{[\xi, \mathfrak{R}]} \lambda\| : \xi^A \in \mathcal{T}_{\mathfrak{R}, k}^{1, 0} \right\} < \infty \quad (4.5.8)$$

One then continues taking suprema of Lie-derivatives to define those quantities uniformly k -bounded to higher orders.

The inexact, covariant derivative operator can also be defined in close analogy with that in the exact case. It will be simplest to use a construction analogous to that of the Koszul connection in defining it.⁶⁷

Definition 4.5.4 *Let $\eta^A, \zeta^A, \xi^A \in \mathcal{T}_{\mathfrak{R}}^{1, 0}$ and $\zeta \in \Sigma_{\mathfrak{R}}^{\infty}$. An inexactly linear connection on \mathcal{M} is an inexactly linear operator $\nabla_A : \mathcal{T}_{\mathfrak{R}}^{1, 0} \times \mathcal{T}_{\mathfrak{R}}^{1, 0} \rightarrow \mathcal{T}_{\mathfrak{R}}^{1, 0}$ that satisfies*

1. $(\eta^N + \zeta^N)\nabla_N \xi^A = \eta^N + \nabla_N \xi^A + \zeta^N \nabla_N \xi^A$
2. $\eta^N \nabla_N (\zeta^A + \xi^A) = \eta^N \nabla_N (\zeta^A + \xi^A)$
3. $(\zeta \eta^N)\nabla_N \xi^A = \zeta(\eta^N \nabla_N \xi^A)$
4. $\eta^N \nabla_N (\zeta \xi^A) = \zeta \eta^N \nabla_N \xi^A + (\eta(\zeta))\xi^A$

where ‘ $\eta^N \nabla_N \xi^A$ ’ represents the value of the operation applied to $(\eta^N, \zeta^N) \in \mathcal{T}_{\mathfrak{R}}^{1, 0} \times \mathcal{T}_{\mathfrak{R}}^{1, 0}$.

One can demonstrate the existence of such an operator by, *e.g.*, fixing a chart, defining the analogue of an arbitrary collection of Christoffel symbols and defining ∇_A in their terms as usual. The action of this connection can be extended to inexact scalar fields in the usual way. This operator has the same sort of similarities and dissimilarities with the analogous, exact operator as does the inexact Lie derivative with its exact counterpart. Note, again, that the inexact covariant derivative of a Σ -bounded, inexact tensor-field may not itself be Σ -bounded, so care must be taken when one attempts to algebraically combine the result of such a derivative with a Σ -bounded, inexact tensor-field, as the operation may not be well defined.

4.6 Inexactly Linear Operators

We now generalize the results of §§4.4 and 4.5 by characterizing inexactly linear operators in the abstract, and a type of stability we may demand of them. We will proceed in the standard fashion.

⁶⁷See, *e.g.*, Spivak (1979, ch. 6)

Theory and Experiment

Let \mathbf{O} be an inexactly linear operator from any normed, inexactly linear space Λ_1 to another Λ_2 . We say \mathbf{O} is *bounded* just in case, over any set in Λ_1 bounded in norm, the supremum of the norms of its values is bounded: for every bounded set $L \subset \Lambda_1$ there is a c_k such that

$$\sup_{x \in L} \{ \|\mathbf{O}(x)\| \} < c_k$$

In virtue of the inexact linearity of the spaces, this is equivalent to: just in case the supremum of the norms of its values on the closed ball of radius 1 in the norm on Λ_1 is itself bounded. As in the exact case, an inexactly linear operator's being bounded implies that it is continuous with respect to the topologies induced on the domain and the range by their respective norms. Let B_1 be the ball of radius 1 (with respect to the norm) in Λ_1 . The *operator-norm* of a bounded operator \mathbf{O} is

$$\|\mathbf{O}\| \equiv \sup_{x \in B_1} \{ \|\mathbf{O}(x)\| \} \quad (4.6.9)$$

It is easy to see that, in virtue of the inexact linearity of the spaces, as in the exact case, this is equivalent to

$$\|\mathbf{O}\| \equiv \sup_{x \in \Lambda_1: \|x\| \neq 0} \left\{ \frac{\|\mathbf{O}(x)\|}{\|x\|} \right\} \quad (4.6.10)$$

We call the topology induced on the space of inexactly linear operators by this norm the *operator-topology*. We say a subset of this space is *uniformly bounded* if it consists only of bounded operators the supremum of the bounds of which is finite.

Definition 4.6.1 *An inexactly linear operator is stable if it has a non-trivial, uniformly bounded neighborhood, and it is ω -stable if it has a uniformly bounded neighborhood containing an open ball of radius ω in the operator-norm.*

Although we defined stability and ω -stability with respect to the operator-norm in particular, it is clear that we may define the same notions with respect to any norm we may impose on the space of bounded operators. As a related notion, we lay down

Definition 4.6.2 *A bounded perturbation of a stable operator is a second operator contained in the interior of a non-trivial neighborhood of compact closure of the first; an ω -bounded perturbation of an operator is a bounded perturbation of it contained in an open ball of radius ω in the operator-norm.*

There follows trivially

Proposition 4.6.3 *Every bounded (respectively: ω -bounded) operator has a bounded (respectively: ω -bounded) perturbation.*

It will be useful for future purposes, before leaving the topic, to record one more result. Impose the topology on $\mathcal{T}_{\mathfrak{R},k}^{0,1}$ induced by its norm. Because we have defined elements of this space as inexactly linear functionals on ordered sets consisting of n inexact tangent and m inexact cotangent vector-fields, we can apply to them the notions of stability and ω -stability as expressed in terms of the imposed norm and topology. There follows from the convexity of the space

Theorem 4.6.4 *Every element of $\mathcal{T}_{\mathfrak{R},k}^{m,n}$, for every $m, n \in \mathbb{R}^{\uparrow}$, an considered as inexactly linear functional on ordered sets consisting of n inexact tangent and m inexact cotangent vector-fields, is stable.*

4.7 Integrals and Topologies

We now turn to treat integrals on \mathfrak{R} . By pressing the same sort of analogy as we used in defining inexact derivations, we want to define inexact integrals as a species of inexactly linear functional, $\mathbf{T} : \Sigma_{\mathfrak{R}} \rightarrow \mathfrak{R}$, satisfying a few collateral conditions. As before, we continue to deal only with physical operations unless explicitly stated otherwise. In particular, the integrals we define are those appropriate for use in physical computations, not pragmatic. By parity of reasoning, the integral, pragmatic, inexact operations are as straightforward to define as were the algebraic ones.

Let us write, no matter how we end up defining it, the inexact integral of the inexact scalar field ζ over the canvas C as

$$\int_{[C, \mathfrak{R}]} \zeta \, d\hat{\mu}$$

where $d\hat{\mu}$ is whatever measure-like structure we end up using. By the same sort of reasoning that led to equation (4.4.3), we may conclude that the first component of the value of this operation ought to be the ordinary Lebesgue integral of the first component of ζ (the magnitudes of the values of the field) with respect to the ordinary Lebesgue measure defined by the volume-element ϵ_{abcd} associated with the spacetime's metric:

$$\pi_1 \circ \int_{[C, \mathfrak{R}]} \zeta \, d\hat{\mu} \equiv \int_C \pi_1 \circ \zeta \, \epsilon_{abcd}$$

This seems all right so far. As always, the trouble enters when trying to deal with the inaccuracy.

It will not do to define the second component of the integral as the Lebesgue integral of the second component of the field, the inaccuracy, considered as an exact scalar field in its own right. If this is to be a physical operation, then we expect the second component of the result to represent the inaccuracy associated with a measurement of the first component. The Lebesgue integral of the ordinary scalar field constituted by the values of the second component of the given inexact scalar field—the scalar field of inaccuracies, if you will—in so far as it combines the values in an alternating process of ordinary summations and limits, does not combine them in the proper way, which in this case must involve our inexact, physical, algebraic operation on inaccuracies, since we want to define a functional representing a physical operation. An obvious solution suggests itself: define a variant of the Lebesgue integral by using, rather than ordinary addition, the operations given by our compounding family to combine the inaccuracies associated with all the vanishingly small regions. This makes physical sense, as the act of combining all the inaccuracies as determined in “infinitesimal” cells throughout the region of integration, those accruing to the measurements of the values of a quantity in all those cells, comes to the application of our family of compounders on all these inaccuracies. This is, in essence, how we will proceed in the end, but getting there requires that we first deal with one somewhat delicate problem regarding the convergence of the values of our compounders applied to a sequence of sets of inaccuracies.

Theory and Experiment

This is the problem. Let ζ be a simple, inexact field over C , *i.e.*, one taking on only a finite number of different values $(a_1, \chi_1), \dots, (a_n, \chi_n) \in \mathfrak{R}$. Let C_i be the subset of C on which the value is (a_i, χ_i) . Then the proposed analogue to the Lebesgue integral of ζ over C , using the ambient spacetime volume element, is

$$\sum_{i=1}^n (a_i, \chi_i) \int_{C_i} e_{abcd} = \sum_{i=1}^n \left(\int_{C_1} a_1 e_{abcd}, \int_{C_1} \chi_1 e_{abcd} \right)$$

where the multiplication of each element (a_i, χ_i) by $\int_{C_i} e_{abcd}$ uses the pragmatic group operation on \mathfrak{R} , and the summation over the inaccuracies uses our physical, algebraic operation. Thus, the value of the inaccuracy is

$$\alpha_n \left(\int_{C_1} \chi_1 e_{abcd}, \dots, \int_{C_n} \chi_n e_{abcd} \right)$$

This value has no ambiguity in its computation.

Consider the next step in defining the Lebesgue integral on more complex fields, extending this sum to countably simple fields, which take on only a countable number of different values, $\{(a_i, \chi_i)\}_{i \in \mathbb{I}^+}$. In this case, using the same notation, the value of the integral is

$$\lim_{n \rightarrow \infty} \sum_{i=1}^n (a_i, \chi_i) \int_{C_i} e_{abcd}$$

The value of the inaccuracy in this case is

$$\lim_{n \rightarrow \infty} \alpha_n \left(\int_{C_1} \chi_1 e_{abcd}, \dots, \int_{C_n} \chi_n e_{abcd} \right) \quad (4.7.1)$$

Our definition of a compounding family does not guarantee that this limit is unambiguously defined. Consider two different orderings of the countable number of values, $\{(a_i, \chi_i)\}_{i \in \mathbb{I}^+}$ and $\{(a'_i, \chi'_i)\}_{i \in \mathbb{I}^+}$. We have no way of knowing whether or not

$$\lim_{n \rightarrow \infty} |\alpha_n(\chi_1, \dots, \chi_n) - \alpha_n(\chi'_1, \dots, \chi'_n)| = 0$$

If this does not hold for every pair of enumerations of the countable family of values, however, then the limit of the countable sum has no unambiguous definition, since its value will depend on the order in which the inaccuracies are “fed into it”, as it were. We do know, however, that, for any particular ordering of our countable set of inaccuracies, the limit (4.7.1) does converge. Indeed, it is bounded from below by zero and from above by $\chi_{\text{inf}} \int_C \epsilon_{abcd}$, where χ_{inf} is the infimum of the set of inaccuracies. (The upper bound follows from item 4 in definition 4.3.2, and the fact that it converges and does not endlessly oscillate from item 5.) The general template of constructions in the theory of Lebesgue integration points to an appealing (though by no means the only) way forward from here. Fix a countably simple, inexact scalar field ζ over the canvas C , taking its values from $A = \{(a_i, \chi_i)\}_{i \in \mathbb{I}^+}$ on, respectively, the subsets $\{C_i \subset C\}_{i \in \mathbb{I}^+}$. Let \mathfrak{A} be the class of all enumerations of the elements in A , and $(\chi'_1, \dots, \chi'_n)$ be the ordered n -tuple consisting of the inaccuracies of the first n elements of the enumeration $A' \in \mathfrak{A}$. Then

$$\sup_{A' \in \mathfrak{A}} \left\{ \lim_{n \rightarrow \infty} \alpha_n \left(\int_{C'_1} \chi'_1 e_{abcd}, \dots, \int_{C'_n} \chi'_n e_{abcd} \right) \right\} \quad (4.7.2)$$

exists.

It is tempting to take this as the value of the second component of the integral of ζ , but I do not think it is yet quite right. Let's say, for example, that we are integrating a continuous distribution of electric charge over a region of spacetime, to compute the total charge contained therein. To accord with the principles laid down so far, we will represent the charge-density by an inexact scalar field, defining a variable density of inaccuracy, if you will, representing at a point the inaccuracy associated with the determination of the magnitude of the charge density in some vanishingly small subset of our region containing that point (perhaps the variability in the inaccuracy comes from the fact that the measurements become more inexact in proportion to the distance of the small region from the probe measuring the charge). Taking the integral of this charge-density corresponds, physically, to adding up the values of the determined magnitudes of the charge in as many vanishingly small subsets of the region as one can. To compute the integrated inaccuracy using the formula (4.7.2) would imply that the inaccuracy associated with measuring a charge-distribution increases in proportion to the volume the charge occupies, if the magnitudes and inaccuracies themselves remain unchanged as one proportionately increases the volume occupied by the charge-distribution. As we remarked earlier, however, we are defining a physical operation, and so we expect the second component of the result to represent the inaccuracy associated with a measurement of the first component. In this light, it seems to me rather that, if one applies the same techniques and instruments of measurement to two charge-distributions differing only in the volume each occupies, then the inaccuracy will be roughly the same for both, perhaps even a little less, in general, for the determination of the larger charge, in so far as one may be able to take more measurements, using the same techniques and instruments, in the larger volume than in the smaller one.

To account for this, I propose that, in defining our limits and their supremum, we take the volume-weighted average to compute the integrated inaccuracy. Denote the volume of the region C by ' $v[C]$ ', *i.e.*, $v[C] = \int_C \epsilon_{abcd}$.

Definition 4.7.1 *The inexact integral of the countably simple, inexact scalar field $\zeta \in \Sigma_{\mathfrak{R}}$ over the region C is fixed by the equations*

$$\begin{aligned} \pi_1 \circ \int_{[C, \mathfrak{R}]} \zeta d\hat{\mu} &= \int_C \pi_1(\zeta) \epsilon_{abcd} \\ \pi_2 \circ \int_{[C, \mathfrak{R}]} \zeta d\hat{\mu} &= \frac{1}{v[C]} \sup_{A' \in \mathfrak{A}} \left\{ \lim_{n \rightarrow \infty} \alpha_n \left(\int_{C'_1} \chi'_1 \epsilon_{abcd}, \dots, \int_{C'_n} \chi'_n \epsilon_{abcd} \right) \right\} \end{aligned} \quad (4.7.3)$$

One can now give a rigorous treatment of $d\hat{\mu}$ as a kind of Stieltjes measure, properly modified so as to accord with the structure of inexact fields, and so treat the integral itself as a modified type of Lebesgue-Stieltjes integral, in close analogy with the standard techniques, extending its action to all the inexact, integrable scalar fields in $\Sigma_{\mathfrak{R}}$ so constructed.⁶⁸ We will write the space of inexact, integrable scalar fields as ' $\mathcal{L}_1[C, \mathfrak{R}]$ ', the space of inexact, square-integrable scalar fields as ' $\mathcal{L}_2[C, \mathfrak{R}]$ ', *etc.*, in order to distinguish them from the analogous spaces of exact scalar fields, which we denote as usual by ' $\mathcal{L}_1[C]$ ', *etc.*

⁶⁸See Curiel (2010b) for the technical details.

Theory and Experiment

It will be convenient to impose a topology on $\mathcal{L}_1[C, \mathfrak{R}]$. There are several options to choose from, including the so-called strong and weak topologies (the analogues, for spaces of continuous operators, of the compact-open and the finite-open topologies for spaces of ordinary, continuous fields), that defined by uniform convergence (with respect to the norm on \mathfrak{R}),⁶⁹ and that defined by the natural \mathcal{L}_1 -norm:

$$\|\zeta\|_1 \equiv \int_{[C, \mathfrak{R}]} \|\zeta\| \, d\hat{\mu} \quad (4.7.4)$$

The topology defined by this last norm is coarser than that of uniform convergence (“more sets can be open when you account for the behavior of fields on sets of measure zero”), though if one considers the topologies induced by each on the space of \mathcal{L}_1 fields mod disagreement only on sets of measure zero, then they are the same. In fact, when employing the \mathcal{L}_1 -topology, we will always assume the space at issue to be composed of equivalence classes of fields agreeing up to sets of measure zero.⁷⁰ Both are coarser than the strong and weak topologies (“more sets can be open when you account for the behavior of fields on proper subsets of its domain”). One can as well impose the sup-norm on $\Sigma_{\mathfrak{R}}^b$, the space of inexact scalar fields bounded with respect to the norm on \mathfrak{R} . Since the topology of \mathfrak{R} satisfies the first axiom of countability, moreover, the sup-norm topology on $\Sigma_{\mathfrak{R}}^b$ is equivalent to the topology of uniform convergence (restricted to $\Sigma_{\mathfrak{R}}^b$). We can impose all these topologies on $\Sigma_{\mathfrak{R}}^b$, $\Sigma_{\mathfrak{R}}^0$, $\Sigma_{\mathfrak{R}}^n$, and $\Sigma_{\mathfrak{R}}^\infty$ in the usual way, by considering each as an open set in the respective larger family and using the restriction topology. We will require in this paper only the \mathcal{L}_1 -topology and the sup-norm topology (when we restrict attention to $\Sigma_{\mathfrak{R}}^b$ and its open subsets).⁷¹ It is useful to note that $\mathcal{L}_1[C, \mathfrak{R}]$ is a Banach space with respect to its norm, as is $\Sigma_{\mathfrak{R}}^b$, and that $\mathcal{L}_2[C, \mathfrak{R}]$ can be given, in the standard way, the structure of an inexact Hilbert space (*i.e.*, one that is in all ways like an ordinary Hilbert space, only having an inexactly linear rather than a linear structure on its elements), by defining the norm to be

$$\|\zeta\|_2 \equiv \left(\int_{[C, \mathfrak{R}]} \|\zeta\|^2 \, d\hat{\mu} \right)^{\frac{1}{2}}$$

and the inner product

$$\langle \zeta, \eta \rangle \equiv \int_{[C, \mathfrak{R}]} \zeta \eta \, d\hat{\mu}$$

Note that the inner-product, being a physical operation, takes its values in \mathfrak{R} rather than in \mathbb{R} .

We are now in a position to state the following proposition, which sums up almost all the properties of inexact scalar fields required for their easy application to the problems we will treat.

⁶⁹One ought to keep in mind that, given two sequences of inexact scalars, for the sequence formed by the norms of the differences of the scalars in the two sequences, taken in order, it must be the case that the magnitudes of the scalars in each sequence approach each other and that their inaccuracies separately approach the sup-inf inaccuracy, in order for the two sequences to converge to a common value.

⁷⁰Strictly speaking, equation (4.7.4) defines only a semi-norm, since it can happen that $\|\zeta\|_1 = 0$ even when $\zeta \neq 0$. With our present stipulation, that we deal only with equivalence classes of fields modulo disagreement on sets of measure zero, this becomes a true norm.

⁷¹The strong and weak topologies are used in Curiel (2010b) to construct inexact Sobolev spaces.

Proposition 4.7.2 $\zeta \in \Sigma_{\mathfrak{R}}$ is, respectively, \mathcal{L}_1 , \mathcal{L}_2 , bounded, continuous, n -times differentiable, or smooth if and only if each of its components, considered as an ordinary, exact scalar field in its own right, is, respectively, so with respect to the germane exact structure.

The proof is straightforward, so I skip it.

Finally, our norm on \mathfrak{R} can be used as well to define exact differential and integral operations on inexact scalar fields, which we will put to good use later. The definition of each such action follows the same template. To apply an exact operator to an inexact field, one takes the norm of the value of the field at each point to form an exact scalar field and then applies the operator to the constructed field. For example, the action of an ordinary, exact tangent vector-field ξ^a on an inexact scalar field ζ is given by

$$\xi^a(\|\zeta\|)$$

that of the exact integral by

$$\int_C \|\zeta\| \epsilon_{abcd}$$

and so on.

4.8 Motleys

We are now in a position to complete the definition of the kind of mathematical field required for use in modeling physical fields so as to conform to the requirements of a regime. Let \mathcal{C} be the decoupage of a canvas C .

Definition 4.8.1 An inexact motley on \mathcal{C} is a mapping $\theta : \mathcal{C} \rightarrow \mathfrak{R}$.

An inexact motley, then, is an inexact field with the decoupage as its domain.⁷² An *infimal, inexact motley* is one restricted to \mathcal{C}^{inf} , representing an instance of the finest possible specification of initial data conforming to the regime of a theory for the initial-value formulation of that theory. Similarly, an *exact motley* is a mapping from \mathcal{C} to \mathbb{R} (*i.e.*, an ordinary, exact scalar field on the manifold defined by \mathcal{C}). When we speak of a ‘motley’ without qualification, we should be understood to mean an inexact motley. Let $\Theta_{\mathfrak{R}}$ be the space of motleys, and Θ that of exact motleys. Because \mathcal{C} is compact and Hausdorff (by dint of the fact that C is compact and Hausdorff), all its subsets are of compact closure, and so all motleys on it have compact support. Let $\Theta_{\mathfrak{R}}^b$ be the space of bounded motleys (with respect to the norm on \mathfrak{R}), $\Theta_{\mathfrak{R}}^0$ that of continuous motleys (with respect to the topologies defined on \mathcal{C} and \mathfrak{R}), and $\Theta_{\mathfrak{R}}^\infty$ that of smooth motleys (with respect to the differential structures on the spaces).⁷³

⁷²In Curiel (2010b), for strictly technical reasons, a motley is defined only on subsets of \mathcal{C} such that the intersection of C with the spacetime region defined by the union of the scraps in that support (each scrap considered simply as a region of spacetime) is open and contains none of the boundary of C . We will not need to take account of this detail here. As always, for complete technical details, see Curiel (2010b).

⁷³Since motleys purport to represent actual, inexact data conforming to the regime of a theory, it could be objected that many real fields, such as Maxwell fields, are never confined to compact support. At first blush, this idealization seems no worse (and no better) than the ones of this sort standardly employed in physics. In fact, I think it is better, in so far as we demand only that these fields be inexactly zero, not exactly zero.

In virtue of the differential structure naturally accruing to \mathcal{C} as an 8-dimensional smooth manifold, inexactly linear tangent and co-tangent vectors, as well as higher-order inexactly linear tensors of arbitrary indexical structure, *etc.*, can be defined over motleys in the same way as was done for such structures over inexact scalar fields. To distinguish them from such structures over inexact scalar fields, we will use the adjective *mottled* to qualify them, speaking, *e.g.*, of *mottled, inexact tangent vector-fields*. We will represent these structures using, again, the Penrose-Geroch abstract-index notation, with upper-case Greek letters as indices, *e.g.*, ξ^Ω and ζ_Ω for a tangent and cotangent field respectively. Similarly, the *mottled, inexact covariant derivative* of the motley θ , for instance, will be denoted ‘ $\nabla_\Psi\theta$ ’. The similar tensorial and affine structures over exact motleys will be denoted using lower-case Greek letters as indices, *e.g.*, ξ^ω and ζ_ω for a mottled tangent and cotangent field respectively. The inexact integral of a motley as well is defined in the same way as that of an inexact scalar field, with the difference consisting only in the measure used to compute each. We will not go into the technical details of the construction of the measure on decoupages, for which see Curiel (2010b, appendix A). All the properties proved for the analogous structures on inexact scalar fields carry over intact to those on motleys. Thus, in the same vein as proposition 4.7.2, one has

Proposition 4.8.2 $\theta \in \Theta_{\mathfrak{R}}$ is, respectively, in $\mathcal{L}^1[\mathcal{C}, \mathfrak{R}]$, $\mathcal{L}^2[\mathcal{C}, \mathfrak{R}]$, Θ^b , et al., if and only if each of its components, considered as an exact motley in its own right, is, respectively, so.

We can immediately extend the definitions of all the various k -bounded and Σ -bounded norms and structures defined in §4.5 to the analogous mottled structures. We will, as usual, qualify these structures with ‘mottled’ when context will not suffice to disambiguate the sense.

One class of mottled, inexact tensorial and affine objects is of such importance and utility that we will explicate their construction. A smooth *mottled, inexact semi-Riemannian metric* of Lorentz signature, say, $(+, -, -, -)$, is a symmetric, indefinite, invertible tensor $g_{\Psi\Omega}$, such that there exists a tetrad $\{\xi^\Psi, \xi^\Psi, \xi^\Psi, \xi^\Psi\}$ orthonormal in the sense that

1. $\pi_1 \circ (g_{\Psi\Omega} \xi^\Psi \xi^\Omega) = 1$ (*inexactly timelike*)
2. $\pi_1 \circ (g_{\Psi\Omega} \xi^\rho \xi^\rho) = -1$, for $\rho \in \{1, 2, 3\}$ (*inexactly spacelike*)
3. $\pi_1 \circ (g_{\Psi\Omega} \xi^i \xi^j) = 0$, for $i, j \in \{0, 1, 2, 3\}$ and $i \neq j$
4. $\pi_2 \circ (g_{\Psi\Omega} \xi^i \xi^j) = \gamma$, for $i, j \in \{0, 1, 2, 3\}$

Note that the indefiniteness of the metric appertains only to the first component of mottled, inexact tangent vector-fields. If one wanted to have a more detailed and thorough treatment, one could demand, for example, that some smeared out average of $g_{\Psi\Omega} \xi^\Psi \xi^\Omega$ equal 1, but we do not need to go into such detail here; the reader is invited to supply such details on his or her own, following the schemata of methods employed earlier. One now defines the associated, inexact Levi-Cevita derivative operator, ∇_Ψ , in the usual way, as that unique torsion-free, affine connection with respect to which the metric is inexactly zero. The proof that such a connection exists follows exactly the

same line of reasoning as in the exact case (see, *e.g.*, the proof of theorem 2.2 in Kobayashi and Nomizu (1963, vol. 1, ch. 4)).

One oddity about such metrics must be pointed out: the act of lowering or raising an index of a tensor by its use tends to reduce the inaccuracy associated with the quantity represented by that tensor. I believe this makes some (perhaps not much, but some) physical sense. Say one lowers the index of an inexact tangent vector in order to contravect it with one of a given tensor, to represent the taking of the physical component of an index of that tensor at a point, in the spatiotemporal direction the inexact tangent vector determines. To do this, one must set up measuring devices along the line determined by the vector to take the directional derivative of the aspect of the quantity represented by the tensor's index, which process ought to reduce the uncertainty in the knowledge of the line along which the vector points.

It is interesting to note as an aside, moreover, that, although this treatment of mottled, inexact metrics looks at first glance as though it could be used to render an inexact treatment of the spacetime metric and affine structure, it in fact cannot, in so far as we had to assume the existence of the exact spacetime metric in all these arguments and constructions, *e.g.*, in defining infimal decoupages. Indeed, it does not seem possible to use this kind of scheme to attempt to reconcile observed gravitational measurements of metric structure and curvature, with their associated, inevitable inaccuracies, with rigorous solutions to the Einstein field equation itself, as the method outlined here requires, in general, a metric, at the least, for its employment—one needs already in place what one would be attempting to approximate—carving up spacetime to make it finite, in order to approximate spacetime itself. Indeed, this is why we have not attempted to take into account the inaccuracy of spatiotemporal determinations themselves in formulating the notion of a regime. The issues raised are too hard to be dealt with here. Of course, to a certain extent, one faces the same problem writ small in applying the method to any fields on a relativistic spacetime, as the fields one is attempting to approximate themselves form the flesh and bone of the spacetime's metric structure, in virtue of the Einstein field-equation. The methods work only in so far, then, as one ignores the contribution of the fields one is modeling to this metric structure. Consequently, this method itself has a limited regime of propriety, as it were: it cannot be applied to fields the intensity of which makes untenable the excluding of their contribution to the metric structure. The reasoning behind this conclusion, I believe, points to serious, generally ignored questions about the definability, in general relativity, of observable quantities in regions of intense curvature.

5 Physical Theories

[*** IT IS PRECISELY THE POSSESSION OF A REGIME THAT DIFFERENTIATES OTHERWISE FORMALLY IDENTICAL THEORIES—it is the representation of the fact that we distinguish among physical systems that are from a certain formal point of view dynamically identical. Otherwise, one could not tell what sort of physical system one was modeling. IT'S HOW A THEORY BECOMES A *PHYSICAL* THEORY, ABOUT SOME PARTICULAR PART OF THE PHYSICAL

WORLD. ***]

From §4, we now have (a sketch of) mathematical structures in hand in the terms of which we can model the behavior of physical systems in such a way as to incorporate directly into the model itself the sorts of constraints and conditions a regime may place on the application of a particular theory to that sort of modeling, and in the same terms of which we may articulate mathematically exact and rigorous theories. We will attempt, in this section of the paper, to use those structures to construct a single, unified model of the practices and of the subject-matters of the theoretician and the experimentalist. We must deal with several issues before we will be in a position to shoot directly for that goal. In particular, the discussions of sections 3 and 4 raise a poignant question: what becomes of the initial-value formulation of the partial-differential equations comprised by a theory with a regime? From the mathematical point of view, the partial-differential equations of the theory, in modeling a system in the sense of theoretical physics, are formulated in terms of quantities modeled by exact scalar and tensorial fields on spacetime—cross-sections of an exact linear bundle over the spacetime manifold itself—and not by fields of bounded, connected, compact intervals of \mathbb{R} over the closures of convex, normal, open regions of spacetime, in the terms of which, as I contend, the arguments and results of the experimentalist may be framed.

The penultimate goal of this section is to understand how one can construct a well set initial-value formulation for partial-differential equations over mottled, inexact fields in such a way as to take account of the demands possession of a regime imposes on the equations comprised by a theory.⁷⁴ In order to get there, we will need to clarify what these demands are, and how the structures developed in the previous section encode them. I begin by briefly sketching in §5.1 some of the details of the ways that the mathematical structures introduced in §4 can be used to construct physical theories whose components directly model the restrictions and conditions its regime, as spelled out in §3, may demand of it, as opposed to a theory employing the ordinary, exact structures of mathematical physics, which will necessarily have its regime appended as an entity external to the rest of the theory. Next, in §5.2, I study possible ways our inexact, mottled fields and the exact, spatiotemporal fields customarily employed in physics may relate to each other, with the aim of fixing a canonical, physically significant relation between the two. This will put us in a position to consider in §5.3, in a purely formal way, how to define an initial-value formulation for partial-differential equations on mottled, inexact fields, and what it may mean for one to be well set, using as our guides the analogous notions in the theory of exact partial-differential equations. We will also consider the possible relations, from a formal perspective, between these two sorts of equation and their respective sorts of solution. In §5.4, in the light of these discussions, we will re-work the purely formal notion of a well set initial-value formulation for partial-differential equations over mottled, inexact fields worked out in §5.3 in order to take account of the demands possession of a regime imposes on the equations comprised by a theory.

This analysis will lead us to the ultimate goal of this section, a more precise, partial characterization of a physical theory, given in §5.5, which will provide the terms in which we may at last, in §5.6,

⁷⁴Integral equations are beyond the scope of this paper.

articulate the primary contention of this paper, for which much of the paper up to that point may be considered a constructive proof, that of the consistency of the joint practice and subject-matter of the theoretician and the experimentalist.⁷⁵

5.1 Exact Theories with Regimes and Inexact, Mottled, Kinematically Constrained Theories

By the end of §3.4, we had more or less worked out what it meant to constrain by a kinematical regime the propriety of a theory of mathematical physics as ordinarily practised, *viz.*, a theory that represents the quantities it treats using exact fields on spacetime and that comprises only exact partial-differential equations over those fields. There is no clear way, using only that machinery, to represent in a single, unified structure the practice, on the one hand, of the theoretician in abducting exact, rigorous theories from the inexactly determined data provided by the experimentalist, about which she must judge whether or not they capture and express adequately the essential form of the patterns inherent in the data, and, on the other hand, that of the experimentalist in reckoning from the exact, rigorous theories provided by the theoretician models of actual experiments, about which he must judge whether or not they adequately model his experimental arrangements, and, if so, whether their predictions conform to the inexactly determined data he gathers from those experiments. The construction of such a representation, however, is the project we have set ourselves in this paper. In order to move towards its accomplishment, we will now explicate how the mathematical machinery developed in §4 provides a framework in the terms of which we can frame a unified, consistent representation of the practice of both.

Recall from definition 3.4.1 that a kinematical regime imposes the following kinds of conditions on the admissible exact values of the quantities modeled by a theory. First, there are algebraic and differential conditions that the values of the fields representing not only the quantities modeled by the theory but also those representing the relevant environmental quantities must satisfy. At a minimum these conditions include uniform upper and lower bounds on the values these fields can take, as well as on the values of their partial-derivatives up to some fixed order. For tensorial quantities, these bounds are imposed on the kinematical norms we constructed for the fields, and for their derivations, in §4.5. The regime dictates that the spatiotemporal region in which the measurements or observations take place conforms to the metrical conditions it imposes. It demands that the preparation and the measurement of these quantities proceed by way of one of a set of fixed interactions mediated by one of a set of experimental techniques, all of a type appropriate for the given scheme of conditions. Finally, it requires a family of algorithms for computing the intervals of inaccuracy of the determined values of the quantities based on the actual conditions of the experiment (the values of the environment's quantities, the exact spatiotemporal character of the laboratory, *etc.*).

To make all this a little more concrete, consider the following, highly schematic description of the way an experimentalist might go about observing the dynamical evolution of a known type of system

⁷⁵I will show you the bridge can be built by walking across it.

in order to compare the results of the observation with the predictions of a known physical theory that models that sort of system. He begins by sketching a crude schema of the type of experiment he wants to perform, containing just enough information for him to prepare a suitable laboratory and experimental arrangement of appropriate apparatuses in the laboratory for its performance;⁷⁶ he then prepares within this arrangement a token of the type of system he will observe, adjusting its initial state as nicely as the experiment requires and as available techniques and equipment and his knowledge allow, rendering it amenable to the observations he will make of it, in the context of the arrangement; he formulates a model of the arrangement of his proposed experiment, including the system, in the terms of the theory, representing all the germane quantities of the given, actual system using the set of exact fields on spacetime, perhaps some scalar, perhaps some tensorial, that the theory uses to represent those quantities; he then attempts to determine the values of quantities those fields model by employment of one of a variety of techniques and experimental arrangements suitable for the purpose, taking into account as best he could in this determination the family of ranges of possible inaccuracy in the determination of those values of that field as accomplished by the chosen method; after settling in some way or other on the set of exact magnitudes of the fields to be used in the model, he observes the dynamical evolution of the system, measuring the values of the various quantities along the way using some acceptable set of experimental methods, once again trying to take account of the possible inaccuracies while determining these magnitudes; at the same time, he constructs and solves, in the context of the model he has formulated of the arrangement, the initial-value formulation of the partial-differential equations comprised by the theory, using the initially determined values of the quantities as initial data; next, he compares, on the one hand, the final values of the quantities as determined by observation (account having been taken in this determination, as always, of the possible inaccuracies in measurement) with, on the other, the values predicted by the theory in the form of the solution to the equations' initial-value formulation; finally, he decides whether the inevitable deviance of the observed from the predicted values falls within the acceptable margin of error for such observations and calculations, as determined by some method appropriate for the task, in whatever way that propriety may be gauged; if they do, he can conclude that the outcome of the experiment accorded with the predictions of the theory; if they do not, then he must attempt to determine whether this discrepancy amounts to a contravention of the theory requiring its modification in some way or other, or whether the discrepancy can be accounted for by an inadequacy in the performance or in the modeling of the experiment, or in the calculation of the inaccuracies or in that of the acceptable deviances of predicted from observed values that could be rectified by a repeat performance.

To make these ideas vivid, imagine that the experimentalist has a set of thirteen canonical books containing all (and only) the information he needs to plan, model, perform and analyze experiments for all types of physical systems treated by a particular theory with its regime.⁷⁷ The volumes are

⁷⁶Because this is only a schematic description, one should not take literally the seemingly temporal verbs, conjunctions and adverbial phrases it employs, such as 'begin', 'then', 'at the same time', and so on; if one likes, they coordinate only the logical relations of the different bits of the description.

⁷⁷The descriptions of these volumes is, of necessity, schematic and sketchy in the extreme.

as follows.

[*** A better scheme? remove all purely non-syntactic elements from the first list; segregate all semantical elements into the 13th volume. Also, represent the volumes in symbols, as otherwise it's too difficult to keep in mind? Or, *à la* Peirce, leave it in words? ***]

1. The first book contains: an enumeration of physical quantities, both kinematic and dynamic; and an enumeration of types of physical system, the space of states of each of which can be parametrized by the (values of the) dynamic physical quantities in the first enumeration.
2. The second volume contains: an enumeration of a set of physical quantities, both kinematic and dynamic; and an enumeration of types of environment, each of which bears all the quantities in the first enumeration.
3. The third contains an enumeration of ordered pairs consisting of a type of system listed in the first volume and a type of environment listed in the second volume; this enumeration of ordered pairs, moreover, is such that each type of system in the first volume appears in at least one of the ordered pairs and each type of environment listed in the second volume appears as well in at least one of these pairs (*i.e.*, projection on each component of the ordered pairs is surjective).
4. The fourth contains a Σ -bounded, mottled, normed, inexactly linear tensor-algebra.
5. The fifth contains an enumeration of the same cardinality as the enumeration of quantities in the first volume, each enumerand of which is a tensorial subspace of the algebra in the fourth volume; each tensor-space, moreover, is of an indexical structure appropriate for the representation of the physical quantity at the same ordinal position in the enumeration of the first volume as this tensor-space occupies in the present enumeration.
6. The sixth contains an enumeration of the same cardinality as the enumeration of quantities in the second volume, each enumerand of which is a tensorial subspace of the algebra in the fourth volume; each tensor-space, moreover, is of an indexical structure appropriate for the representation of the physical quantity at the same ordinal position in the enumeration of the second volume as this tensor-space occupies in the present enumeration.
7. The seventh volume contains: an enumeration of physical quantities (partially) characterizing subsets of a spacetime; an enumeration of the same cardinality as this first enumeration, each enumerand of which is a tensorial subspace of the algebra in the fourth volume; each tensor-space, moreover, is of an indexical structure appropriate for the representation of the physical quantity at the same ordinal position in the first enumeration of this volume as this tensor-space occupies in the present enumeration.
8. The eighth contains an enumeration of ordered triplets, each consisting of a constant $k > 0$ (not necessarily the same for the first component of every enumerand), a k -bounded, tensorial

Theory and Experiment

compounding family defined over the elements of the algebra in the fourth volume, and an ordered pair from the enumeration in the third volume.

9. The ninth contains an enumeration of ordered pairs, the first component of which is a formal set of differential and algebraic conditions formulated in terms of the tensor-product of all the tensor-spaces listed in the fifth and sixth volumes (formal in the sense that we do not specify a particular k -bounded, tensorial compounding family for the algebraic and differential operations on the elements of the algebra), and the second component of which is an ordered pair from the enumeration in the third volume.
10. The tenth contains an enumeration of ordered pairs, the first component of which is a formal set of algebraic conditions formulated in terms of the tensor-product of all the tensor-spaces listed in the fifth, sixth, and seventh volumes (formal in the sense that we do not specify a particular k -bounded, tensorial compounding family for the algebraic and differential operations on the elements of the algebra), and the second component of which is an ordered pair from the enumeration in the third volume.
11. The eleventh contains an enumeration of ordered triplets, the first component of which is a type of experimental apparatus, the second a schematic technique for using that type of apparatus in experiments, and the third component of which is an ordered pair from the enumeration in the third volume.

The twelfth volume is more complex. It contains an enumeration of ordered octuplets, the components of which (in order) are

1. an ordered pair from the third volume
2. an ordered triplet from the listing in the eleventh volume, having as its third component the ordered pair in the first component of this octuplet
3. an ordered triplet from the listing in the eighth volume, having as its third component the ordered pair in the first component of this octuplet
4. the tensor-product of the k -bounded subspaces of all the tensor-spaces enumerated in the fifth volume, where k is given by the first component of the ordered triplet in the third component of this octuplet
5. the tensor-product of the k -bounded subspaces of all the tensor-spaces enumerated in the sixth volume, where k is given by the first component of the ordered triplet in the third component of this octuplet
6. the tensor-product of the k -bounded subspaces of all the tensor-spaces enumerated in the seventh volume, where k is given by the first component of the ordered triplet in the fourth component of this octuplet

Theory and Experiment

7. an ordered pair from the ninth volume, having as its third component the ordered pair in the first component of this octuplet
8. an ordered pair, the first component of which is an ordered pair from the tenth volume, having as its third component the ordered pair in the first component of this octuplet; the second component of the ordered pair consists of a particular relativistic spacetime, (\mathcal{M}, g_{ab}) .

The contents of these first twelve books provide a complete, (mostly) syntactic articulation of the regime of the theory the experimentalist employs in modeling those sorts of experiments.

The thirteenth book provides a semantical model of the syntactics of the first twelve volumes, as follows.⁷⁸

1. The first enumeration of the first volume lists the types of physical system modeled by the theory, and the second enumeration lists the types of physical quantities shared by those systems that are treated by the theory, in virtue of which the same theory can model all these system, though they (the physical systems) differ in ways significant enough for us to declare them of different types.⁷⁹
2. The first enumeration in the second volume lists the types of environment in which the systems enumerated in the first volume may manifest themselves in a form amenable to treatment by the theory, and the second enumeration lists those quantities borne in common by those environments the values of which are relevant in the determination of the propriety of the theory in modeling a system existing in one of these types of environments.
3. The ordered pairs of the third volume represent those combinations of particular types of systems and environments that do manifest themselves together and are in fact amenable to modeling by the theory.
4. The tensor-algebra of the fourth volume is the one used for all mathematical modeling of these types of systems and environments, and the relevant properties of regions of spacetime in which combinations of the two may manifest themselves.
5. The tensor-spaces of the fifth volume are the ones whose tensor-product is used to represent states of the types of physical systems listed in the first volume.

⁷⁸I suppose that we could have completely segregated the syntactic from the semantic elements in this mythology, by making no mention until the thirteenth volume of physical systems, physical quantities, environments, measuring apparatuses, experimental techniques, and so on, leaving only the formal, mathematical elements in the first twelve volumes. I don't see what would be gained, either with regard to physical comprehension or philosophical understanding, by such a maneuver, which strikes me as artificial and contrived.

⁷⁹I would love to know whether there is any basis over and above the preferences our psychological constitution more or less enforces on us for nominating physical systems to be of different "types" when, with respect to theory, they share the same dynamical structure. Think of the Newtonian equation representing the dynamical evolution of a simple harmonic oscillator, and then of how many systems of seemingly different "types" find their appropriate model in that equation. Think as well of the fact that, at the level of quantum field theory, the idea of the simple harmonic oscillator plays a fundamental role in several ways, if not most, of articulating the structure of the theory.

Theory and Experiment

6. The tensor-spaces of the sixth volume are the ones whose tensor-product is used to represent states of the types of environments listed in the second volume.
7. The first enumeration in the seventh volume lists those properties of regions of spacetime the values of which are relevant in the determination of the propriety of the theory in modeling a system existing in one of these types of environments in a spacetime region with given values for the listed properties; the second lists the tensor-spaces whose tensor-product is used to represent the properties of spacetime regions listed in the first enumeration of the volume.
8. The first component of a triplet in the eighth volume, k , represents the minimal bound on values of the scalar quantities (and on the kinematical norms defined on the tensorial quantities) demanded by the regime of a theory; the second component is a k -bounded, tensorial compounding families used to impose convex, k -bounded, inexact, tensorial, algebraic and differential structures on the germane subspaces of the algebra in the fourth volume, so as to represent the way that the inaccuracies of all the relevant quantities combine when experiments performed on the system *cum* environment indicated by the third component of the triplet are modeled using the theory.
9. A set of algebraic and differential formulæ constituting the first component of an ordered pair listed in the ninth volume defines the conditions that the values of the quantities of tokens of the type of physical system and tokens of the type of environment indicated in the ordered pair constituting the second component must jointly satisfy, when one of the systems manifests itself in one of the environments, in order to be amenable to modeling by the theory.
10. The algebraic formulæ constituting the first component of an ordered pair listed in the tenth volume defines the conditions that the values of the quantities of a region of spacetime, in conjunction with those of tokens of the type of physical system and tokens of the type of environment indicated in the ordered pair constituting the second component, must jointly satisfy, in order for those systems to be able to manifest themselves in those environments in such a region of spacetime, so as to be amenable to modeling by the theory.
11. The first two components of the ordered triplets in the eleventh volume consist of experimental apparatuses and methods for employing them that experimentalists can use to perform experiments on the given type of physical systems *cum* environment indicated in the ordered pair constituting the third component of the triplet, in a way amenable to modeling by the theory.

The twelfth volume, not surprisingly, requires a more involved semantical accounting. Each octuplet represents, broadly speaking, a family of systems amenable to modeling by the theory, in accordance with its regime, along with a schematic representation of the way the regime informs and constrains this modeling.

1. The first component of the ordered pair in the first component of an octuplet indicates the type of system at issue, and the second a particular type of environment in which a token of

that type of system may appear in such a way as to be amenable in principle to modeling by the theory in accordance with its regime.

2. The second component indicates a type of instrumentation the experimentalist may use, according to the associated techniques, to probe a token of that type of system in a token of that type of environment so that the entire experiment is amenable in principle to modeling by the theory in accordance with its regime.
3. Given an experiment of this type that an experimentalist proposes to perform, the third component of the octuplet fixes the mathematical structure—a convex, k -bounded, inexact linear, tensorial algebra—she will use to model the experiment, where k is chosen so as to enforce the minimal kinematical conditions on the values of the quantities for states of the combined system and environment—that they (and some subset of their partial-derivatives) be uniformly bounded from above and from below. This algebra enforces these minimal conditions by not allowing the representation of any state of the system *cum* environment that does not satisfy them; moreover, the k must be such that all states represented by elements of the algebra have values for their quantities well defined with respect to experimental probing by the chosen instrumentation, as applied using the given technique.
4. The fourth, fifth, and sixth components represent, respectively, the spaces of states of the system, the environment, and regions of spacetime in which the experiment may be performed.⁸⁰
5. The seventh component of the octuplet picks out a subspace of the combined space of states of the system and the environment, those the values of whose quantities jointly satisfy all the given algebraic and differential conditions; states represented by elements of this space satisfy the remainder of the conditions (besides the minimal one captured by the imposition of the k -bound) that the values of the system and the environment must jointly satisfy in order to conform to the regime of the theory, in so far as the system is probed using the experimental apparatus applied using the associated techniques given in the second component of the octuplet.
6. The spacetime indicated in the eighth component of the octuplet represents the world in which the envisioned type of experiment will occur. The algebraic conditions are such that only a canvas can satisfy them; they serve, moreover, to pick out a subspace of the space of canvases on the given spacetime, those in which it is both the case that a token of the type of system *cum* environment whose state can be modeled by an element of the subspace determined by

⁸⁰There is an awkwardness here in the formal presentation of these structures. We are demanding that the space representing the properties of regions of spacetime relevant to determining whether or not a given experiment conforms to the regime of a theory have the same k -bound as the spaces representing the states of, respectively, the environment and the system. It would be preferable to have the space representing these properties be only Σ -bounded, but to characterize the tensor-product of a Σ -bounded space and a k -bounded space would take us too far afield. The same, indeed, goes for the space of states of the environment, which one may expect to have a k -bound different from that of the space of states of the system. See Curiel (2010b) for complete details.

the elements of the seventh component of this octuplet manifests itself, and the case that the values of the relevant spatiotemporal properties of this canvas, along with the values of the quantities of the system *cum* environment manifested in the canvas, all jointly satisfy the algebraic conditions. These are the kinematically appropriate laboratories for these types of experiments according to the theory's regime, as spelled out by these thirteen volumes. This family of laboratories, finally, serves to refine further the subspace determined by the elements of the seventh component of this octuplet, to those systems *cum* environments appearing in one of these laboratories. The systems represented by elements of this subspace are precisely those, in the given spacetime, that do in fact conform to the regime of the given theory, and are such that experiments performed on them using the given experimental apparatus applied using the associated techniques can be consistently modeled by the theory in accordance with its regime.

We thus have two senses in which a physical theory may possess, and conform to, a regime. The theory and its regime may, on the one hand, be formulated in the terms of the ordinary, exact structures of mathematical physics and so have its regime appended as a separate entity that all the relevant, exact quantities must be made to conform to, as a separate mathematical condition on them. This is the mode of representation used in §3 when we first characterized a regime. We will call such a representation of a theory an *exact theory with regime*. On the other hand, the theory may be formulated from the start in terms of inexact, mottled structures that incorporate directly the strictures of its regime, in the way sketched just above. We will call such a theory an *inexact, mottled, kinematically restricted theory*, or just an *inexact theory*, for short. We will say that an inexact theory is *kinematically equivalent* to an exact theory with regime if all the kinematic constraints encoded in its inexact, mottled structures embody the regime of the exact theory. We will try to make these ideas precise in the next several sections.

Before moving on, I must record a qualm about this discussion—I hesitate to include the first two volumes (and so several of the others), those enumerating the physical systems and environments, in my proposed set of mythological volumes. They raise perhaps a larger number of difficult issues and questions they do not address than that of simple or difficult ones they do. What does a “type” of physical system come to? How are such types differentiated, if not by the very theories that successfully model them? Ought a type be a catalogue of all existing physical systems or only of possible ones, or only a listing of properties a system should have, necessary or sufficient, to constitute a token of the type? In any event, how ought one distinguish individual physical systems, one from another, for surely one can consider in a natural way the same sum total of fields of quantities in the same patch of spacetime as part of more than one “system”, depending on the joints one chooses to carve at? And so on. In the event, it is in large part on account of these very questions and others like them that I decided to include the two volumes. I see no way of satisfactorily answering any of them, and all others like them, once and for all. Indeed, I would argue that, viewed *sub specie æternitatis*, these questions have no sense. The only hope I see for their satisfactory address lies in formulating them with regard to a particular, larger set of issues one is trying to resolve, with

well defined goals and clearly delineated methods acceptable for the use in achieving them. I thus include them, in part, to underscore again the thoroughly pragmatic character of all attempts to understand and to employ scientific theories. Someone of a more pessimistic stripe could say with some justice that its inclusion underscores how little one would have accomplished in comprehending *physical* theory were one to have accomplished even to a high degree of success the project I have set myself in this paper.

5.2 Idealization and Approximation

In order to make contact with theoretical physics, we need a method for associating an exact scalar field with a motley by a relation substantive enough to use in comparing models of physical systems as represented by our constructions with those of physical systems as represented by the types of theories employed in physics as ordinarily practiced, along with the strictures of an externally imposed regime. We will associate the two by means of a construction—an algorithm that, given a motley as input, yields an exact scalar field that may be considered an idealized, exact model of the same quantity attached to the same physical system the motley inexactly models, satisfying the kinematical constraints encoded in the motley. This algorithm will of necessity have a bipartite character, for it will not only transform a field on an infimal decoupage of a canvas to a field on the spacetime points composing the canvas itself, it will also transform the field from one valued in \mathfrak{R} to one valued in \mathbb{R} —it will need to transform both the domain and the range of the field. There are several ways one may envisage implementing such a procedure. Purely for the sake of simplicity, we will construct a two-stage algorithm, transforming the domain and the range each in its own operation.

Before sketching the characterization of the operators we will use, it will be instructive to consider a possible method of constructing one for the domain, mapping $\Theta_{\mathfrak{R}}$ onto $\Sigma_{\mathfrak{R}}$, that will in the end not serve the purpose, though it will, in the event, shed useful light on the physical content of the machinery developed so far. Fix a canvas C along with its decoupage \mathcal{C} . Let us say that the inexact scalar field ζ is *in harmony with* (or *harmonious with*) the motley θ if, for all $S \in \mathcal{C}$,

$$\theta(S) = \int_{[S, \mathfrak{R}]} \zeta \, d\hat{\mu}$$

One might hope to associate a harmonious field with a given motley, if that motley satisfies certain conditions. In general, however, this will not be possible in a physically viable way. To see this, recall that a measurable, exact scalar field f is said to be *absolutely continuous* in a region C if, given any $\epsilon > 0$, there exists a $\delta > 0$ such that, given any finite set $\{S_i\}_{i=1, \dots, n}$ of mutually disjoint subsets of C for which

$$\sum_{i=1}^n \int_{S_i} \epsilon_{abcd} < \delta$$

then it is the case that

$$\sum_{i=1}^n \int_{S_i} f \epsilon_{abcd} < \epsilon$$

Analogously, an inexact set function θ , in this case a motley, is said to be *absolutely continuous* with respect to a σ -finite measure μ on the σ -ring \mathfrak{M} if, given any $\epsilon_1, \epsilon_2 > 0$, there exists $\delta > 0$ such that, if $\{S \in \mathfrak{M} : \mu(S) < \delta\}$, then $\pi_1 \circ \theta(S) < \epsilon_1$ and $\pi_2 \circ \theta(S) < \epsilon_2$. The Radon-Nikodým Theorem⁸¹ has as an immediate consequence the fact that a motley possesses an inexact scalar field in harmony with it if and only if that motley is absolutely continuous. Not every motley, however, not even every continuous or even smooth motley, need be absolutely continuous. Being absolutely continuous implies that the motley must, in general, take on smaller values for scraps of smaller volume, but nothing requires this of a motley.

The physical content of being absolutely continuous is easily illustrated. Let ζ be harmonious with the motley θ . Then for $S \in \mathcal{C}$, because ζ is continuous (since absolute continuity implies continuity), there is, by the mean-value theorem, a $q \in S$ such that

$$v[S]\pi_1(\zeta(q)) = \pi_1 \circ \int_{[S, \mathfrak{R}]} \zeta d\hat{\mu}$$

where we use the pragmatic, multiplicative group-operation, and so

$$\pi_1(\zeta(q)) = \frac{\pi_1(\theta(S))}{v[S]}$$

Let $T \in \mathcal{C}$ be another set such that, for the same q ,

$$v[T]\pi_1(\zeta(q)) = \pi_1 \circ \int_T \zeta \epsilon_{abcd}$$

(such a T can always be found), and so

$$\pi_1(\zeta(q)) = \frac{\pi_1(\theta(T))}{v[T]}$$

Thus, a constraint on the definition of θ is that, for all $S, T \in \mathcal{C}$ that share a mean-value point,

$$\frac{\pi_1(\theta(S))}{v[S]} = \frac{\pi_1(\theta(T))}{v[T]}$$

or, equivalently,

$$\frac{\pi_1(\theta(S))}{\pi_1(\theta(T))} = \frac{v[S]}{v[T]}$$

In effect, this says that absolutely continuous inexact scalar fields are inexact scalar densities: θ will scale in a way that approximates to being in exactly linear proportion to the volumes of the scraps it takes values in. It is difficult to see why such a severe constraint should be placed on a θ that is to represent possible initial data for a physical theory. Temperature, for instance, is a true scalar and does not satisfy this condition. All the same, it is useful to know that motleys representing scalar densities ought in fact to be absolutely continuous, at a minimum.

We turn now to characterize operators that will serve the purpose. Consider the physical content of transforming a motley into an exact scalar field. A single measurement made under actual,

⁸¹See, e.g., Halmos (1950, ch. VI, §31).

laboratorial conditions consists of necessity of a sort of smeared-out average of a multitude of more miniscule and varied interactions. A thermometer, for example, does not stay stationary with respect to the caloric mass being probed, certainly not, in any event, as measured in units of the order of magnitude of the mean free-path of the basic constituents of the caloric mass (that is to say, basic with respect to the theory at issue), which, by definition, will be of the same order of magnitude as the size of the infimal scraps determined by the kinematic regime of the theory at issue. Rather, it bobs and jiggles constantly, sampling, as it were, the temperature over most if not all the region immediately proximate to the point of interest during the time it takes for the system to equilibrate. On this picture, the value of the motley on “almost every” infimal scrap containing a given point $q \in C$ ought to contribute to the value of the constructed scalar field at q . Still, even though the thermometer in its jiggling samples “almost every” scrap containing q , one may expect that the largest contributions to the final reading will come from those scraps in which q lies closest to the spatiotemporal center, or at least furthest from the boundary, in some sense or other. Note that exactly these sorts of considerations as well illuminate some of the sources of the inevitable inaccuracy in measurements and observation in physics.

To render these considerations precisely, we begin by defining operators that perform the yeoman’s work, acting only on the domains and ranges of the fields at issue, which we will then employ to define the required mappings among the fields themselves. We require two kinds, one that maps scraps onto spacetime points, and so maps decoupages onto canvases, and another that maps \mathfrak{R} onto \mathbb{R} . With these operators in hand, we will have two obvious methods of constructing operators that map inexact motleys on a decoupage \mathcal{C} to exact scalar fields on its canvas C : we may define a pair of operators mapping, respectively, $\Theta_{\mathfrak{R}} \rightarrow \Sigma_{\mathfrak{R}}$ and $\Sigma_{\mathfrak{R}} \rightarrow \Sigma$, and then chain the two together; or we may define a pair mapping, respectively, $\Theta_{\mathfrak{R}} \rightarrow \Theta$ and $\Theta \rightarrow \Sigma$, and then chain the two together. The fact that two methods offer themselves will provide a clue as to natural conditions to impose on the operator we ultimately define to map $\Theta_{\mathfrak{R}} \rightarrow \Sigma$.

Definition 5.2.1 *A shrinker is a surjective, continuous, open and closed mapping \mathbf{S} of a decoupage \mathcal{C} onto its associated canvas C , such that, for every $S \in \mathcal{C}$, $\mathbf{S}(S) \in \check{S}$.*

For any scrap $S \in \mathcal{C}$, we will write ‘ p_s ’ for the value of \mathbf{S} at the scrap S , and we will refer to it as the scrap’s *center*.

Definition 5.2.2 *An exactor is a surjective, continuous, open and closed mapping \mathbf{E} of \mathfrak{R} onto \mathbb{R} , such that, for every $(a, \chi) \in \mathfrak{R}$,*

$$a - \frac{1}{2}\chi < \mathbf{E}(a, \chi) < a + \frac{1}{2}\chi$$

We will refer to the image of an inexact scalar (a, χ) under an exactor, written ‘ a_χ ’, as its *exactitude*. It immediately follows that smooth shrinkers and exactors are submersions.

To show that shrinkers exist, I explicitly constructed a useful one in Curiel (2010b). Though the fine details are too involved to go into in any depth here, the construction, roughly speaking, involves a method for explicitly shrinking the scraps of an infimal decoupage in a smoothly parametrized

way to points of spacetime. In order to reflect the considerations drawn out in our discussion just above of thermometry, it addressed one desideratum in particular: settling on a spacetime point to serve as p_s , its center as we have termed it, contained in a given scrap that lies, in some technical sense, furthest from its boundary and closest to its “true spatiotemporal center”. It relies on the approximation, mentioned above in §3.3, that we used in making the space of decoupages finite-dimensional from the full, infinite-dimensional decoupage. Using the parameters employed in that approximation, I defined a kind of “center of mass” of a scrap as that point that satisfied a set of conditions on the maximum and minimum values of those parameters. This mapping, moreover, smoothly varies in the spacetime points as one moves smoothly around the decoupage. I emphasize the point that, so far as I can see, there is no canonical or preferred way of constructing a shrinker. Indeed, the definition itself could be altered in any of a number of ways while remaining true to the spirit behind it.

Displaying examples of exactors is far easier. On the face of it, it would appear to be trivial, consisting of nothing more than the shrinking of the second component of an inexact scalar, *i.e.*, the extent of the inaccuracy itself, to zero, and, indeed, π_1 satisfies all the conditions and so is an exactor. Selecting the “proper” exactor, however, what ever criteria we plump for in coming to the judgment, is not so straightforward. Nothing requires that we hold the first component, the magnitude, fixed as we shrink the inaccuracy. We chose the magnitude to be the mid-point of the range of inaccuracy for the sake of convenience and simplicity, and because it satisfied a few physical, heuristic (*i.e.*, hand-waving) arguments. The idealized value of the quantity as represented by the exact scalar can in fact be any point in the entire interval of inaccuracy. One thus has a continuum of exactors, shrinking the same inexact scalar down to any of a continuously varying family of exact scalars.

We will use shrinkers and exactors to define the operators that map fields to fields. Because shrinkers are not injective and operate on the domains of the fields, a little footwork remains before we can define operators based on them, which we do first. The operators based on exactors, which we treat after the ones based on shrinkers, are easier to deal with; it will not matter that they are not injective.

To define a mapping from a space of fields on a decoupage to one on a canvas using a shrinker, one cannot simply declare that the value of the field on the canvas at a given point be the value of the scrap that gets mapped to that point, as there will be, in general, many scraps that get mapped to the same point. The discussion of the thermometer suggests that, given a shrinker, we require a way of distilling a value for a field at a spacetime point q in a canvas from the values of the motley at all the scraps in the pre-image of q under the shrinker. We also demand that the relations among the value of the field at q and those at the scraps in its pre-image reflect the fact that the value at q is supposed to be an idealization of some sort of the inexact values of the motley on the scraps in its pre-image. There are several ways one can try to make this idea precise. The one we work with has the virtues of simplicity and manifest physical content. Write the power-set of \mathcal{C} as ‘ $\mathfrak{P}[\mathcal{C}]$ ’. Given a shrinker \mathbf{S} , define $\mathfrak{P}_s[\mathcal{C}] \subset \mathfrak{P}[\mathcal{C}]$ to be the family of all and only those $\mathcal{S} \in \mathfrak{P}[\mathcal{C}]$ for which there is a $q \in C$ such that $\mathcal{S} = \mathbf{S}^{-1}[q]$. Roughly speaking, elements of \mathcal{S} are those maximal families of scraps

having a single point as their total intersection.

Definition 5.2.3 *Given a shrinker \mathbf{S} , an \mathbf{S} -distiller is a mapping $\mathbf{d}_s : \Theta_{\mathfrak{R}} \times \mathfrak{P}_s[\mathcal{C}] \rightarrow \mathfrak{R}$ such that, for $\theta \in \Theta_{\mathfrak{R}}$ and $q \in C$,*

$$\inf_{S \in \mathbf{S}^{-1}[q]} \{|\theta(S)|\} \leq \|\mathbf{d}_s(\theta, \mathbf{S}^{-1}[q])\| \leq \sup_{S \in \mathbf{S}^{-1}[q]} \{|\theta(S)|\}$$

Similarly,

Definition 5.2.4 *Given a shrinker \mathbf{S} , an exact \mathbf{S} -distiller is a mapping $\hat{\mathbf{d}}_s : \Theta \times \mathfrak{P}_s[\mathcal{C}] \rightarrow \mathbb{R}$ such that, for $\theta \in \Theta$ and $q \in C$,*

$$\inf_{S \in \mathbf{S}^{-1}[q]} \{|\theta(S)|\} \leq |\hat{\mathbf{d}}_s(\theta, \mathbf{S}^{-1}[q])| \leq \sup_{S \in \mathbf{S}^{-1}[q]} \{|\theta(S)|\}$$

Again, in order to show that distillers exist, I explicitly constructed one in Curiel (2010b), based on the shrinker I constructed (mentioned above), albeit the constructed distiller was defined only on $\mathcal{L}_1[\mathcal{C}, \mathfrak{R}]$ (more precisely, on the inexact Sobolev spaces based on it), not on all of $\Theta_{\mathfrak{R}}$, which, however, is all we require. The same construction serves to show that exact distillers exist as well. In brief, given an \mathcal{L}_1 -motley θ over a canvas, and a point q in that canvas, the value of the constructed distiller as applied to $(\theta, \mathbf{S}^{-1}[q])$ consists of the limit of the value of a kind of weighted average of the values of the motley over the scraps in $\mathbf{S}^{-1}[q]$ as the scraps shrink to q .⁸²

We have enough under our belt now to characterize the operators this section has worked towards. We first treat those that map the space of motleys into that of inexact scalar fields; we can then immediately extend the definition to operators mapping the space of exact motleys into that of exact scalar fields. The basis for these operators will be shrinkers and distillers. Given a shrinker \mathbf{S} and an \mathbf{S} -distiller, we will say that the field $\zeta \in \Sigma_{\mathfrak{R}}$ is the \mathbf{d}_s -distillate of $\theta \in \Theta_{\mathfrak{R}}$, if it is such that, for all $q \in C$, $\zeta(q) = \mathbf{d}_s(\theta, \mathbf{S}^{-1}[q])$. The *exact \mathbf{d}_s -distillate*, a field in Σ derived from a field in Θ , is defined in the obvious way, using definition 5.2.4 rather than 5.2.3. It would be convenient for these operators to have such nice properties as mapping $\mathcal{L}_1[\mathcal{C}, \mathfrak{R}]$ to $\mathcal{L}_1[C, \mathfrak{R}]$, $\Theta_{\mathfrak{R}}^b$ to $\Sigma_{\mathfrak{R}}^b$, and so on. It is indispensable that they “act as linearly as they can”, which in this case means that they ought to be, respectively, inexactly and exactly linear.

Definition 5.2.5 *A lens is an inexactly linear bjective $\mathbf{L} : \mathcal{L}_1[\mathcal{C}, \mathfrak{R}] \rightarrow \mathcal{L}_1[C, \mathfrak{R}]$ such that*

1. \mathbf{L} is bounded and stable in the operator-norm
2. there exists an ordered pair $(\mathbf{S}, \mathbf{d}_s)$ consisting of a shrinker \mathbf{S} and an \mathbf{S} -distiller \mathbf{d}_s such that, for all $\theta \in \mathcal{L}_1[\mathcal{C}, \mathfrak{R}]$, $\mathbf{L}[\theta]$ is the \mathbf{d}_s -distillate of θ

⁸²Strictly speaking, this description is misleading. A motley is not in general defined for decoupages containing arbitrarily small scraps, and so we cannot compute anything based on the values a motley assumes on a given scrap that shrinks to a point, once that scrap has become too small with respect to the metrical conditions imposed by the region at issue in its approach to the point. I circumvented this problem in Curiel (2010b) by defining a way of computing a number associated with an arbitrarily small scrap based on yet another weighted average of the values of the motley over all scraps that the motley assumes values for and that have non-null intersection with the given arbitrarily small scrap.

Theory and Experiment

3. the restriction of the action of \mathbf{L} to $\mathcal{L}_2[\mathcal{C}, \mathfrak{R}]$, $\Theta_{\mathfrak{R}}^b$, $\Theta_{\mathfrak{R}}^0$ and $\Theta_{\mathfrak{R}}^\infty$ is, respectively, an inexactly linear bijection into $\mathcal{L}_2[C, \mathfrak{R}]$, $\Sigma_{\mathfrak{R}}^b$, $\Sigma_{\mathfrak{R}}^0$ and $\Sigma_{\mathfrak{R}}^\infty$

Note that, in the last item, since we are dealing with the spaces $\mathcal{L}_2[C, \mathfrak{R}]$, $\Sigma_{\mathfrak{R}}^b$, *et al.*, defined as restrictions of $\mathcal{L}_2[C, \mathfrak{R}]$, we assume they have the \mathcal{L}_1 -topology. We will say that \mathbf{L} is *derived from* \mathbf{d}_s . Let us call the value of a motley under a lens its *focus*.

We can, almost without comment, modify this discussion to characterize a mapping from the space of exact motleys into that of exact scalar fields.

Definition 5.2.6 *An exact lens is a linear bijection $\widehat{\mathbf{L}}_s : \mathcal{L}_1[\mathcal{C}] \rightarrow \mathcal{L}_1[C]$ such that*

1. $\widehat{\mathbf{L}}$ is bounded and stable in the operator-norm
2. there exists an ordered pair $(\mathbf{S}, \mathbf{d}_s)$ consisting of a shrinker \mathbf{S} and an \mathbf{S} -distiller \mathbf{d}_s such that, for all $\theta \in \mathcal{L}_1[\mathcal{C}]$, $\widehat{\mathbf{L}}[\theta]$ is the exact \mathbf{d}_s -distillate of θ
3. the restriction of $\widehat{\mathbf{L}}$ to $\mathcal{L}_2[\mathcal{C}]$, Θ^b , Θ^0 and Θ^∞ is, respectively, a linear bijection into $\mathcal{L}_2[C]$, Σ^b , Σ^0 and Σ^∞

Let us call the value of an exact motley under the latter mapping its *exact focus*. One has available for exact lenses all the constructions used to extend lenses to higher-order structures.

I showed in Curiel (2010b) that lenses exist by proving that the constructed shrinker and its derived distiller defined one. The case of most importance for us will be that in which the original motley is defined on the infimal decoupage of a canvas that satisfies the requirements of the regime of a theory. All results proved in Curiel (2010b) involving lenses continue to hold when restricted to this case, since the use made of the decoupage in proving them depends only on the compactness of its elements (considered as subsets of spacetime) and on their sizes being (in a certain technical sense) uniformly bounded from below, both of which hold for infimal decoupages. I emphasize the point that, so far as I can see, there is no canonical or preferred way of defining such a mapping. Indeed, given a lens \mathbf{L} , one can always define a new one by multiplying \mathbf{L} by a smooth inexact scalar field bounded by some $\epsilon > 0$ that depends on the particular properties of \mathcal{C} and \mathbf{L} .

The construction of an exact scalar field from an inexact scalar field, and, correlatively, an exact motley from a motley, is somewhat simpler than the construction of an inexact scalar field from a motley, in that the domain of the field in each case remains the same. We will use the same considerations as guided our definition of a lens to define the process of deriving an exact scalar field from an inexact one (and so for that of deriving an exact motley from a motley). In this case we demand that the operator yield an exact scalar field respecting the kinematic constraints encoded in the inexact field, to wit, that the exact value of the quantity fall within the interval of possible inaccuracy.

Definition 5.2.7 *A polarizer is a linear bijection $\mathbf{P} : \mathcal{L}_1[C, \mathfrak{R}] \rightarrow \mathcal{L}_1[C]$ such that*

1. \mathbf{P} is bounded and stable in the operator-norm
2. there exists an exactor \mathbf{E} such that, for every $\zeta \in \mathcal{L}_1[C, \mathfrak{R}]$ and every $q \in C$, $\mathbf{P}[\zeta](q) = \mathbf{E}(\zeta(q))$

Theory and Experiment

3. the action of \mathbf{P} restricted to $\mathcal{L}_2[C, \mathfrak{R}]$, $\Sigma_{\mathfrak{R}}^b$, $\Sigma_{\mathfrak{R}}^0$ and $\Sigma_{\mathfrak{R}}^\infty$ is, respectively, a linear bijection into $\mathcal{L}_2[C]$, Σ^b , Σ^0 and Σ^∞

We will say that \mathbf{P} is *derived from \mathbf{E}* . Its being derived from an exactor ensures that the determined value lies within the interval of possible inaccuracy. Given an inexact scalar field ζ and a polarizer \mathbf{P} , we call the image of ζ under \mathbf{P} its *polarization*. Similarly,

Definition 5.2.8 A mottled polarizer is a linear bijection $\widehat{\mathbf{P}} : \mathcal{L}_1[\mathcal{C}, \mathfrak{R}] \rightarrow \mathcal{L}_1[\mathcal{C}]$ such that

1. $\widehat{\mathbf{P}}$ is bounded and stable in the operator-norm
2. there exists an exactor \mathbf{E} such that, for every $\theta \in \mathcal{L}_1[\mathcal{C}, \mathfrak{R}]$ and every $S \in \mathcal{C}$, $\mathbf{P}[\theta](S) = \mathbf{E}(\theta(S))$
3. the action of \mathbf{P} restricted to $\mathcal{L}_2[\mathcal{C}, \mathfrak{R}]$, $\Theta_{\mathfrak{R}}^b$, $\Theta_{\mathfrak{R}}^0$ and $\Theta_{\mathfrak{R}}^\infty$ is, respectively, a linear bijection into $\mathcal{L}_2[\mathcal{C}]$, Θ^b , Θ^0 and Θ^∞

Given a motley θ and a mottled polarizer $\widehat{\mathbf{P}}$, we call the image of θ under $\widehat{\mathbf{P}}$ its *mottled polarization*.

The simplest polarizer, \mathbf{P}_π , is given by direct application of π_1 : $\mathbf{P}_\pi[\theta](q) = \pi_1(\theta(q))$. To construct another, we need only, then, choose some map $\sigma : \Sigma_{\mathfrak{R}} \times C \rightarrow \mathbb{R}^+$ such that the mapping $\mathbf{P}_\sigma : \Sigma_{\mathfrak{R}} \rightarrow \Sigma$ defined by

$$\mathbf{P}_\sigma[\theta](q) = \sigma(\theta, q)\pi_1(\theta(q)) \quad (5.2.1)$$

satisfies definition 5.2.7. Indeed, since $\mathcal{L}_1[C, \mathfrak{R}]$ is a Banach space, the implicit function theorem guarantees that, for any polarizer \mathbf{P} , there exists a $\sigma : \mathcal{L}_1[C, \mathfrak{R}] \times C \rightarrow \mathbb{R}$ such that \mathbf{P} can be represented in the form given by equation (5.2.1). In this case, σ is the *implicit polarizer* of \mathbf{P} . The mapping from $\mathcal{L}_1[C, \mathfrak{R}]$ to $\mathcal{L}_1[C]$ with perhaps the clearest physical content is that defined by our norm on \mathfrak{R} , under which, for $\zeta \in \mathcal{L}_1[C, \mathfrak{R}]$, $\zeta(q) \mapsto \|\zeta(q)\|$. This, however, strictly speaking, is not a polarizer, since some of the values of a $\mathcal{L}_1[C, \mathfrak{R}]$ field may not have well defined norms; moreover, there is no guarantee that, when the norm is defined, its value lies within the interval of possible inaccuracy. If we want to avail ourselves of this mapping, we cannot address the issue by simply restricting our definition of a polarizer to $\Sigma_{\mathfrak{R}}^b$, for then we could not extend our constructions to the relevant, inexact Sobolev spaces. We therefore will employ a small cheat when we use this polarizer: we will “smooth out” each $\zeta \in \mathcal{L}_1[C, \mathfrak{R}]$ at just the points at which its values have no defined norm, by convolving it with a small, smooth normalizing factor in neighborhoods of the singular points smaller (in a technical sense) than the infimal scraps of the decoupage. Since the set of such singular points will always be of measure zero, this affects nothing of substance. We will, furthermore, apply a smooth normalizing factor (perhaps depending on the value it is applied to) to the mapping, to ensure that the resulting value always lies within the interval of possible inexactness.

Chaining a lens and polarizer together yields a mapping from \mathcal{L}_1 -motleys to exact \mathcal{L}_1 -scalar fields, $\mathbf{P} \circ \mathbf{L} : \mathcal{L}_1[\mathcal{C}, \mathfrak{R}] \rightarrow \mathcal{L}_1[C]$. Likewise, chaining a mottled polarizer and an exact lens together, $\widehat{\mathbf{L}} \circ \widehat{\mathbf{P}}$, yields a mapping from \mathcal{L}_1 -motleys to exact \mathcal{L}_1 -scalar fields. We now want criteria to impose on such possible mappings to pick out the physically relevant ones. The fact that I see no way of imposing a preference for one of these ways of chaining over the other suggests one such natural

Theory and Experiment

condition, I think the one of importance at a brute level, as it were; as we take account of the demands a regime places on these structures, more constraints will naturally suggest themselves. Let us say that \mathbf{L} and $\widehat{\mathbf{L}}$ are *co-focused* if they are derived from the same \mathbf{S} -distiller, and that \mathbf{P} and $\widehat{\mathbf{P}}$ are *co-polarized* if they are derived from the same exactor.

Definition 5.2.9 *An idealizer is a linear, bijective mapping $\mathbf{I} : \mathcal{L}_1[\mathcal{C}, \mathfrak{R}] \rightarrow \mathcal{L}_1[C]$ such that there exists an ordered quadruplet $(\mathbf{L}, \mathbf{P}, \widehat{\mathbf{L}}, \widehat{\mathbf{P}})$ consisting of a lens \mathbf{L} , a polarizer \mathbf{P} , an exact lens $\widehat{\mathbf{L}}$, and a mottled polarizer $\widehat{\mathbf{P}}$, for which*

1. \mathbf{L} and $\widehat{\mathbf{L}}$ are co-focused
2. \mathbf{P} and $\widehat{\mathbf{P}}$ are co-polarized
3. for $\theta \in \mathcal{L}_1[\mathcal{C}, \mathfrak{R}]$

$$\mathbf{I}[\theta] = \mathbf{P} \circ \mathbf{L}[\theta] = \widehat{\mathbf{L}} \circ \widehat{\mathbf{P}}[\theta]$$

There follows from definitions 5.2.5–5.2.9

Theorem 5.2.10 *For an idealization \mathbf{I} ,*

1. \mathbf{I} is bounded and stable in the operator norm
2. the action of \mathbf{I} restricted to $\mathcal{L}_2[\mathcal{C}, \mathfrak{R}]$, $\Theta_{\mathfrak{R}}^b$, $\Theta_{\mathfrak{R}}^0$ and $\Theta_{\mathfrak{R}}^\infty$ is, respectively, a linear bijective mapping into $\mathcal{L}_2[C]$, Θ^b , Θ^0 and Θ^∞

By dint of the fact that the lenses and polarizers I constructed in Curiel (2010b) (sketched above), when composed, form an idealizer, there follows

Theorem 5.2.11 *Idealizers exist.*

We will call the inverse of an idealizer an *approximator*. As it is easy to see, all the same properties hold for approximators as do for idealizers.

It is not difficult to extend these operators to ones on inexact tensorial and affine spaces. We have already done most of the heavy lifting, in §4.5. I will restrict myself to stating the definitions and perhaps making a remark or two about the details. I will do so only for lenses and polarizers; the treatment of exact lenses and mottled polarizers should then be clear. We will also state definitions only for the Σ -bounded, inexact, mottled tensorial algebra $\widehat{\mathcal{T}}_{\mathfrak{R}}$ rather than for a convex, k -bounded one $\widehat{\mathcal{T}}_{\mathfrak{R},k}$. The extension of the definitions to the convex, k -bounded case is largely straightforward; the only delicacy lies in keeping track of which combinations of operators and entities are and are not permitted, which must be done manually, as it were, since such spaces are not algebraically closed. Let us write ‘ $\mathcal{T}_{\mathfrak{R}}^{m,n}|_c$ ’ for the restriction to the canvas $C \in \mathcal{M}$ of the (non-mottled) inexact space of (m, n) -tensorial fields on \mathcal{M} , and ‘ $\mathcal{T}_{\mathfrak{R}}^{m,n}|_q$ ’ for the fiber of that space over $q \in C$, etc.

In so far as shrinkers do not care about the character of any space one may define over the scraps and points it deals with, we can use definition 5.2.2 in this case without alteration. Everything else, though, requires a bit of reworking.

Definition 5.2.12 A tensorial exactor is a surjective, continuous, open and closed mapping $\mathbf{E} : \mathcal{T}_{\mathbb{R}} \times \mathcal{M} \rightarrow \mathcal{T}$ (the space of exact tensor-fields on spacetime), such that, for every $(m, n) \in (\mathbb{I}^{\uparrow})^2$ and $q \in \mathcal{M}$,

1. $\mathbf{E}[\mathcal{T}_{\mathbb{R}}^{m,n} \times q] = \mathcal{T}^{m,n}|_q$
2. for $\lambda \in \mathcal{T}_{\mathbb{R}}^{m,n}|_q$, $\mathbf{E}(\lambda, q)$ is in the 4-sphere of possible values contained in $\mathcal{T}^{m,n}|_q$ defined by the magnitude $\pi_1(\lambda)$ of λ and the Σ -norm of its inaccuracy $\pi_2(\lambda)$

As with the definition of the scalar exactor, the second condition captures the requirement that the idealized exact value ought to lie within the bounds of possible inaccuracy that λ determines.

Definition 5.2.13 Given a shrinker \mathbf{S} , an (m, n) -tensorial \mathbf{S} -distiller is a mapping $\mathbf{d}_s : \widehat{\mathcal{T}}_{\mathbb{R}}^{m,n} \times \mathfrak{P}_s[\mathcal{C}] \times C \rightarrow \mathcal{T}_{\mathbb{R}}|_C$ such that, for $\lambda \in \widehat{\mathcal{T}}_{\mathbb{R}}^{m,n}$ and $q \in C$,

1. $\mathbf{d}_s(\lambda, \mathbf{S}^{-1}[q], q) \in \mathcal{T}_{\mathbb{R}}^{m,n}|_q$
- 2.

$$\inf_{S \in \mathbf{S}^{-1}[q]} \{ \|\lambda(S)\| \} \leq \| \mathbf{d}_s(\lambda, \mathbf{S}^{-1}[q], q) \| \leq \sup_{S \in \mathbf{S}^{-1}[q]} \{ \|\lambda(S)\| \}$$

Note that we use here the Σ -norm, $\|\lambda(S)\|$, for elements of $\widehat{\mathcal{T}}_{\mathbb{R}}^{m,n}$.⁸³ Note also that, in so far as there is no natural isomorphism between inexact tensor-spaces over different scraps of a decoupage, as there is none for exact tensor-spaces over different points of spacetime, we must include explicitly in the domain of the distiller the points of the canvas over which the fields are defined.

Definition 5.2.14 A tensorial lens is an inexactly linear bijection $\mathbf{L} : \widehat{\mathcal{T}}_{\mathbb{R}} \rightarrow \mathcal{T}_{\mathbb{R}}$ such that

1. \mathbf{L} is bounded and stable in the operator-norm
2. there exists an ordered pair $(\mathbf{S}, \mathbf{d}_s)$ consisting of a shrinker \mathbf{S} and an \mathbf{S} -distiller \mathbf{d}_s such that, for all $\lambda \in \widehat{\mathcal{T}}_{\mathbb{R}}$, $\mathbf{L}(\lambda)$ is the \mathbf{d}_s -distillate of λ
3. the restriction of the action of \mathbf{L} to a k -bounded, convex, tensorial sub-algebra of $\widehat{\mathcal{T}}_{\mathbb{R}}$ is an inexactly k -convex bijection into $\mathcal{T}_{\mathbb{R}}$

Similarly,

Definition 5.2.15 A tensorial polarizer is a linear bijection $\mathbf{P} : \mathcal{T}_{\mathbb{R}} \rightarrow \mathcal{T}$ such that

1. \mathbf{P} is bounded and stable in the operator-norm
2. there exists an exactor \mathbf{E} such that, for every $\lambda \in \mathcal{T}_{\mathbb{R}}$ and every $q \in C$, $\mathbf{P}[\lambda](q) = \mathbf{E}(\lambda(q))$
3. the restriction of the action of \mathbf{P} to a k -bounded, convex, tensorial sub-algebra of $\mathcal{T}_{\mathbb{R}}$ is an inexactly k -convex bijection into \mathcal{T}

⁸³To be more precise, we are using the Σ -norm defined on individual points of a tensorial field rather than that defined on sections of the field itself. The restriction of the latter to the former is straightforward.

Finally, one has

Definition 5.2.16 *A tensorial idealizer is a linear, bijective mapping $\mathbf{I} : \widehat{\mathcal{T}}_{\mathfrak{R}} \rightarrow \mathcal{T}$ such that there exists an ordered quadruplet $(\mathbf{L}, \mathbf{P}, \widehat{\mathbf{L}}, \widehat{\mathbf{P}})$ consisting of a tensorial lens \mathbf{L} , a tensorial polarizer \mathbf{P} , an exact tensorial lens $\widehat{\mathbf{L}}$, and a mottled tensorial polarizer $\widehat{\mathbf{P}}$, for which*

1. \mathbf{L} and $\widehat{\mathbf{L}}$ are co-focused
2. \mathbf{P} and $\widehat{\mathbf{P}}$ are co-polarized
3. for $\lambda \in \widehat{\mathcal{T}}_{\mathfrak{R}}$

$$\mathbf{I}[\lambda] = \mathbf{P} \circ \mathbf{L}[\lambda] = \widehat{\mathbf{L}} \circ \widehat{\mathbf{P}}[\lambda]$$

Not only can we construct in this way all smooth, exact scalar and tensorial fields out of motleys, but, because we can use the smooth motleys to approximate to analytic motleys, we can, in the limit, construct all tensorial and scalar fields ordinarily used in theoretical physics to model physical fields. It would have been awkward, indeed, if we could not have done so, as, rightly or wrongly,⁸⁴ analytic fields are the stock in trade of the physicist, both theoretically and experimentally, and it would have been a severe difficulty if they could not have been used. Even had we not been able to recover them, however, I do not think that would have shown the method I use here to be wholly unjustified. The game, after all, is not in the end to recapitulate the functions that are in fact used in the practice of physics; it would have sufficed for our purposes merely to have shown that one can construct *some* fields or other that would appear to be acceptable and sufficient for use in physical investigations, since, again, the aim of this paper is only to show that *some* such logically sound reconstruction is possible.

One may have hoped that imposing “natural” conditions on possible procedures of focusing and polarizing would have sufficed for picking out a unique idealizer, or even, conversely, that imposing “natural” conditions on possible procedures of idealization would have sufficed for picking out a unique set of lenses and polarizers. I see no way of doing this. The process of idealization and approximation in physical science seems to me to be irremediably a pragmatic work of art, guided by a pragmatic artist’s intuition, informed by his æsthetical predilections and constrained by the needs of particular investigations.

5.3 An Inexact, Well Set Initial-Value Formulation

Idealizers and approximators are the tools we will use to try to make sense of the relation of kinematical equivalence between an inexact and an exact theory. Before we can apply them directly,

⁸⁴My vote: wrongly. It’s not as though we can solve in closed form any mildly versimilitudinous equation of motion for any vaguely realistic model of a physical system anyway. We can’t even solve the Schrödinger equation for the isolated Hydrogen atom, but rather are forced to rely on perturbative expansions in a power-series of spherical harmonics and the like. I feel strongly that the focus, in teaching physics, on overly idealized, simplified equations with perspicuous, analytic solutions can easily lull people into a false sense of what it is and is not possible to accomplish with physical theory.

however, we need to clarify the nature of the relations that may hold, on the one hand, between partial-differential equations formulated over inexact, mottled fields and their solutions, to, on the other, partial-differential equations formulated over exact fields on spacetime and their solutions. It is not yet clear, for instance, what it may mean to require that the inexact, mottled quantities of the inexact theory satisfy the “same” differential and algebraic constraints on their values as do their counterparts in a kinematically equivalent exact theory.

In this section, therefore, we will treat the initial-value formulation of partial-differential equations on mottled, inexact fields. We will do so at first from a formal point of view, without worrying about the physical content of the constructions. We will then attempt to use the machinery developed in the previous section to explore possible ways of translating such equations into partial-differential equations on exact fields and of relating the solutions of the inexact equations to those of the constructed exact ones, and vice-versa. Defining an initial-value formulation for mottled, inexact equations and characterizing what it may mean to say that it is well set do not pose any serious problems. Exploring the relations of these equations and their solutions to those of the constructed ones, however, pose significantly more involved technical issues, which we will address in this paper with only the most minimal of sketches; they are addressed at length in Curiel (2010b).

First, recall the classical notion of a well set initial-value formulation for partial-differential equations over fields on a relativistic spacetime; while the following characterization does not exactly match any other I have seen in the literature, it articulates what we require in this paper while remaining true enough to standard accounts to come to very much the same thing.

Definition 5.3.1 *An exact initial-value formulation is an ordered quadruplet consisting of*

1. *a differentiable manifold*
2. *an exact partial-differential equation over a family of exact fields on the manifold*
3. *a non-trivial subset of an appropriate hypersurface in that spacetime (the domain of initial data)*
4. *a specification of values on the given portion of the hypersurface for each field in the given set and for the appropriate number of their derivatives if any (the initial data)*

Given an initial-value formulation, its *domain of dependence*, roughly speaking, is the maximal subset of the manifold on which the dynamical evolution of the system may be uniquely determined by the evolution, as modeled by a solution to the partial-differential equation, of initial data off the domain of initial data.⁸⁵

Definition 5.3.2 *An exact initial-value formulation is well set if*

1. *there exists a unique solution to the equation in the domain of dependence satisfying the initial data*

⁸⁵See Geroch (1970a) for a thorough discussion of this notion in general relativity.

2. *that solution is stable (in a certain technical sense) under small perturbations of the initial data*

We now turn to constructing the analogous definition for mottled, inexact fields. In order to consider partial-differential equations, we must have derivative operators to formulate them with. The constructions of §§4.4 and 4.6 provide us with these, and we may easily use them to write down inexact, mottled partial-differential equations willy-nilly. Next, in order to have access to classical results on the solutions to exact partial-differential equations in defining a well set initial-value formulation over inexact fields—or, more precisely, to be able to use all the classical results on exact structures for giving reasonably simple proofs of the analogous results for inexact structures—I defined in Curiel (2010b) distribution-like functionals on test-spaces of inexact fields, and showed they had all the required properties needed for use in constructing the analogue of Sobolev spaces of inexact fields. Members of such spaces are the natural candidates to serve as initial data for an initial-value formulation of inexact partial-differential equations, as well as the natural spaces in which solutions to the equations may be found. The definition of the proper domain for initial data requires construction of an appropriate region of a manifold to serve as the analogue of the domain of initial data, which I called the *convex hull of an infimal decoupage*.

Definition 5.3.3 *An inexact, mottled initial-value formulation \mathfrak{i} is an ordered quadruplet consisting of*

1. *a differentiable manifold whose elements are canvases of an ordinary manifold*
2. *an inexact partial-differential equation over a set of motleys on a decoupage of a canvas in that manifold*
3. *a convex hull of an infimal decoupage on a canvas in the manifold (the the domain of initial data)*
4. *a specification of values, on the given domain of initial data, for each motley in the given set and for the appropriate number of their derivatives, if any, as determined using the fixed, derivative operator (the initial data)*

One then defines the *domain of dependence* of the domain of initial data in a way naturally analogous to that in the theory of exact partial-differential equations, as the maximal decoupage on which the dynamical evolution of the system may be uniquely determined by an initial-value formulation. We now define a well set, inexact initial-value formulation in more or less the same way as was done in the exact case.

Definition 5.3.4 *An inexact initial-value formulation is well set if*

1. *there exists a unique solution to the equation in the domain of dependence satisfying the initial data*
2. *that solution is stable (in a certain technical sense) under small perturbations of the initial data*

It is clear how to apply the schema of these definitions to the case of an exact motley, and to that of an inexact field on an ordinary manifold, as opposed to a motley.

In the same way as in the ordinary case, one can classify these partial-differential equations (at least the quasi-linear ones, which are the only ones we consider in this paper) into hyperbolic, parabolic and elliptic, and show that the solutions to those of parabolic or elliptic type are analytic fields, whereas solutions to those of hyperbolic type possess the same properties as in the exact case, most importantly that they have well set initial-value formulations, that their characteristic wave-fronts propagate with finite speeds, and that discontinuities in initial data propagate into the solutions.⁸⁶ One has, for example,

Theorem 5.3.5 *Only inexact, mottled, hyperbolic partial-differential equations have well set, inexact initial-value formulations on decoupages in any given spacetime. No inexact, mottled parabolic or elliptic partial-differential equation has a well set, inexact initial-value formulation.*

This theorem does not rule out the possibility that inexact parabolic or elliptic partial-differential equations have well set, inexact boundary-value or mixed problems (analogous, *e.g.*, to the classical Neumann or Dirichlet problem). In this paper, however, we are interested only in the initial-value formulation of partial-differential equations. Note as well that even hyperbolic equations may have no well set initial-value formulations for initial data posed on certain regions of a spacetime. Possession of a well set inexact initial-value is relative to the spacetime and the particular regions of the spacetime on which the equations are formulated,

In order to state the sought-after results of this section, the primary remaining problem concerns the construction of exact equations from inexact ones and the relating of the solutions of the one to those of the other in a meaningful way. In a naive sense, we already know how to translate inexact, mottled partial-differential equations, posed in terms of an inexact, mottled, covariant derivative operator, into partial-differential equations on exact scalar fields in a more or less direct way, by fixing an idealizer and applying it individually to all the fields used in formulating the inexact partial-differential equation on motleys, simultaneously transforming the inexact covariant derivative operator into its idealized counter-part. So much is straightforward. On the face of it, however, nothing guarantees that the solution of the idealized partial-differential equation bears any substantial relation to that of the original, inexact one. Without a strong relation with clear physical significance between the two, the sort of analysis of a regime I advance would fall on its face. Indeed, we demand that the strongest relation possible hold between them: that the process of idealization commute with the solution of partial-differential equations. In other words, we demand that, if we first solve the inexact partial-differential equation and then idealize the solution, we end up with the same exact scalar field as we would have, had we first idealized the partial-differential equation itself and then solved that idealized equation.

⁸⁶It is this last property that plays a decisive role in our ability to individuate collections of otherwise undifferentiated and commingled physical fields into discrete, separate physical systems identifiable over extended periods of time—the boundary of a body may be thought of as a discontinuity propagating in the solution to the governing system of hyperbolic equations of dynamic evolution in the region.

To show that this is indeed the case, I constructed in Curiel (2010b) a method of interleaving among the different stages of idealization, treated as the composition of the actions of a lens and a polarizer, processes for approximating partial-differential equations and their solutions with sequences of convergent, finite processes, one such process for each of the possible species of partial-differential equation at issue, those on inexact and exact motleys and those on inexact and exact fields. For an inexact, mottled partial-differential equation, for example, the process consists of approximating the decoupage by a sequence of finite, ever-denser, lattice-like structures the elements of which are spacetime points, and approximating a motley on the decoupage by its restriction to the elements of the lattice-like structures. Given an inexact, partial-differential equation over a motley on the decoupage, one writes the analogous algebraic, finite-difference equation on each member of the sequence, and proves that it has a solution. The construction then ensures that, in the limit as the finite lattice-like structures converge, in a rigorous sense, to the infimal decoupage, the solutions to the algebraic, inexact, finite-difference equations converge to a solution of the original, inexact partial-differential equation on the decoupage (assuming it has at least one, which we do assume hereafter without further comment). I proceeded to show how to intercollate these approximative methods with the processes of focusing and polarization, from which procedure it followed that the space of inexact motleys, the equations over them and the solutions to those equations converge, respectively, to the space of exact fields, partial-differential equations over them and their solutions, in the joint limits as one takes the finite-difference equations into partial-differential equations, as one takes the decoupage into a canvas and as one takes the inexact quantities into exact ones; this convergence, moreover, takes place in such a way that, given an inexact partial-differential equation over motleys, a well set initial-value formulation of that equation and a solution for that fixed initial data, the solution to the idealized partial-differential equation, as derived from the approximative process, is in fact the idealized solution to the original inexact, mottled partial-differential equation.

I modeled the approach after that standardly employed in approximating exact partial-differential equations,⁸⁷ though the differences between inexact, mottled and exact structures demand not insignificant differences between the methods of approximation. In standard accounts, one starts with exact Banach spaces of fields over an ordinary, differential manifold (*e.g.*, Sobolev spaces of some appropriate order), and discretizes these spaces by restricting the values of the fields to finite lattices on the manifold. In other words, in a certain sense, the discretized spaces may be considered restrictions of regions of the manifold, or, if you like, the discretized spaces may be considered the images of a family of finite, injective mappings of a region of the manifold onto itself, such that in the limit as the lattices get bigger and bigger the spaces of discrete fields become dense (in a certain technical sense) in the exact Banach spaces. In this paper, on the other hand, we are starting with a manifold built up from *subsets* of an ordinary manifold (in this case, the decoupage of scraps built up from a canvas in the spacetime manifold), on which are defined Banach spaces of fields that may be considered generalizations of ordinary Banach spaces of fields. One then considers particular ways of “shrinking” the elements of these manifolds and restricting the generality of these fields via

⁸⁷See, *e.g.*, Włoka (1987).

exacting approximations in such a way that, in a certain limit, there results a collection of points dense in the original subset of the ordinary manifold, and a family of fields that may be considered a dense subset of the ordinary Banach space of fields over that subset. In the process, moreover, we must transform as well the range of the fields involved in the equations.

The results may be summed up in

Theorem 5.3.6 *Fix an idealizer and a hyperbolic, mottled, inexact partial-differential equation with a well set initial-value formulation on a decoupage over a canvas in a manifold. If one first solves the equation and then applies the idealizer to the inexact, mottled solution, one arrives at the same exact field on the canvas as if one had first applied the idealizer to the inexact, mottled partial-differential equation to derive an exact partial-differential equation on the canvas and then solved that idealized equation.*

This theorem does not assert that the idealized solution mentioned in the theorem is independent of the idealizer chosen. It asserts only that the exact solution, for a fixed idealizer, is unambiguously determined.

This method of addressing the problem, by intercollating finite, discrete, convergent approximations among the stages of idealization, has other virtues as well. The approximative methods developed allow one to state and prove in a natural way results on the stability of solutions to both inexact and exact partial-differential equations and the stability of relations among them. This expressive power will come in handy in §5.4 below, in formulating what it may mean for the initial-value formulation of the partial-differential equations comprised by a physical theory to be well set in a physically relevant sense.

5.4 A Physically Well Set Initial-Value Formulation

[*** Why it is important to build relativistic invariance in at the start when attempting to approximate the solutions to partial-differential equations on spacetime: when one works in particular coordinates, approximations always introduce non-Lorentz-invariant biasing (truncating the components to the coordinate x_1 but not those of x_2 , *e.g.*) ***].

In §5.3, we sketched the definition of a well set, inexact initial-value formulation from a strictly formal point of view, without relation to physical theory. In this section we will attempt to take account of the constraints a physical theory's possession of a regime places on the initial-value formulation of a partial-differential equation, in so far as the partial-differential equation is part of that physical theory and the initial-value formulation conforms to the regime.⁸⁸ Because we now are dealing, in contradistinction to the strictly formal case of §5.3, with partial-differential equations modeling the dynamical evolution of physical systems, we require a substantive relation between an inexact initial-value formulation's being well set and the physical well-setness, as it were, of exact

⁸⁸To do full justice to this subject, we would need also to consider ways of moving from finite sets of inexact data to inexact fields, as required for a more complete model of the interaction between the practice of the experimentalist and that of the theoretician. That, however, is beyond the scope of this paper. See Curiel (2010b) for a brief sketch of how such an account might go, were we to essay one.

initial-value formulations we construct from the inexact one. To work out an appropriate one, we will need to state with a sufficient amount of rigor and precision what it means for an exact theory and an inexact theory to be kinematically equivalent. With the results of §5.3 at our disposal, we are now in a position to do this. An inexact theory and an exact theory are kinematically equivalent if there exists an idealizer and its related approximator such that,

1. applying the idealizer to the partial-differential equations the inexact theory comprises yields the partial-differential equations comprised by the exact theory
2. applying the approximator to the algebraic and differential constraints on the values of the physical quantities treated by the system yields a system of inexact, mottled algebraic and differential constraints automatically satisfied by all the relevant entities in the inexact theory

It must also be the case that the metrical conditions encoded in the collection of infimal decoupages on which the fields of the inexact theory may be defined agree with those articulated in the exact theory. Roughly speaking, an inexact theory and an exact theory are kinematically equivalent if one can transform each into the other by applying an idealizer or approximator to everything in the theory the operator can act on. One has, as the summation of much of §4 and of this section up to here,

Theorem 5.4.1 *The relation of being kinematically equivalent is stable under “small” perturbations of the given idealizer.*

A complete argument for this claim is tedious and not worth the effort the details require, at least for the purposes of this paper.⁸⁹ It makes intuitive sense, though. An idealizer takes an inexact structure and renders from it an exact structure satisfying the kinematic constraints encoded in the inexact structure. There are, in the event, many exact structures that will satisfy these constraints, given the looseness of fit provided by the inaccuracies of the regime. Perturbing the given idealizer slightly enough will, because idealizers are ω -stable, yield one of these other exact theories.

Now, we turn to the characterization of a physically well set initial-value formulation for exact partial-differential equations comprised by an exact theory with regime, before moving on to the case of the inexact, mottled initial-value formulation of partial-differential equations in an inexact theory. Fix an exact physical theory with its kinematical regime $\mathfrak{K} = (\mathfrak{e}, \mathcal{E}, \mathfrak{k}, \mathfrak{m}_k, \mathcal{K})$, its exact partial-differential equations \mathfrak{C} and a well set initial-value formulation \mathfrak{i} for those equations.

Definition 5.4.2 \mathfrak{i} conforms to \mathfrak{K} if

1. the domain of initial data is a \mathfrak{k} -appropriate subset of a \mathfrak{k} -appropriate observatory
2. the values of $\mathfrak{e} \cup \mathcal{E}$ satisfy \mathfrak{k} in the domain of initial data
3. the preparation and measurement of the initial data occur in conformity with some subset of \mathcal{K}

⁸⁹As ever, see Curiel (2010b) for the details.

We will also say that the initial-value formulation *kinematically conforms* to the regime. Although this definition may appear (somewhat) innocuous, it imposes a severe restriction on the initial-value formulation of a physical theory, in so far as that initial-value formulation will conform to the kinematical regime of the theory, in an important way: the domain of initial data must be compact. The remarks in §3.4, after the definition of a kinematically admissible observatory, foreshadowed this fact.

Definition 5.4.3 *An initial-value formulation of an exact theory with a regime is physically well set if it is well set and it conforms to \mathfrak{K} .*

We can now offer the primary definition of this section, and state the primary results.

Definition 5.4.4 *A physically well set, inexact initial-value formulation of an inexact theory consists of a well set, inexact, mottled initial-value formulation and an idealizer, such that*

1. *the exact initial-value formulation yielded by applying the idealizer to the inexact one is physically well set in the exact theory with regime that is equivalent to the given inexact theory*
2. *the process of idealizing the equation is uniformly ω -stable, in the sense that for every $\omega > 0$ there exists a $\delta > 0$ such that the idealization of a δ -perturbation of a solution to the inexact equation yields an ω -bounded perturbation of a solution to the idealized equation*
3. *the process of idealizing the equation is stable, in the sense that for every $\omega > 0$ there exists a $\delta > 0$ such that using a δ -perturbation of the original idealizer to construct the exact equation and its solutions yields a process of idealizing that is uniformly ω -stable*

The requirement that the relations among data and theory not depend in an essential way on the choice of idealizer suggests the last two conditions. There follows from this definition, in particular from the compactness of the domain of initial data required for an initial-value formulation to be physically well set with regard to a theory, in conjunction with theorem 5.3.5,

Theorem 5.4.5 *Only inexact, mottled, hyperbolic partial-differential equations have physically well set, inexact initial-value formulations on decoupages over canvases in a given spacetime. In particular, no inexact parabolic or elliptic partial-differential equation has a physically well set, inexact initial-value formulation.*

This result says, in effect, that parabolic and elliptic partial-differential equations are acceptable for the modeling of physical systems *to the extent* that we are willing to accept the risk of almost-null thunderbolts disrupting our experiments, as it were. Since we in fact live our entire lives having tacitly, for the most part, accepted this risk (at least, those of us without psychoses or acute neuroses of some stripe), this seems on the face of it a reasonable assumption to make in the theater of physical science. This attitude, however, while surely good enough in the actual performance of experimental physics, cuts no ice in attempting to understand the relation of theoretical physics to that performance. According to theoretical physics, if the prediction of an initial-value formulation

of a parabolic or elliptic partial-differential equation does not hold good when the domain of initial data forms a proper subset of a spacelike hypersurface, correlations between the initial data given and the values of quantities far distant on a spacelike hypersurface containing the domain of initial data may bear the blame—if one had from the start enlarged the domain of initial data to have included that distant, in the event relevant data in the initial data, the prediction would have held good. This dilemma—bad equation or bad data?—does not arise, at least not in this form, when one uses hyperbolic partial-differential equations. If one has collected the data as carefully as possible and the prediction still does not hold good, then the problem lies with the partial-differential equation itself in the hyperbolic case. One never has this iron-clad (aluminum-foil clad?) guarantee with parabolic or elliptic partial-differential equations.

This theorem, with its suggestion that a partial-differential equation may have a physically well set initial-value formulation in one spacetime but not in another, raises an interesting question: what it may mean, in a physical sense, to speak of the “same” equation posed on different spacetimes. Take, for example, the relativistic Navier-Stokes equations. It is not clear to me, on its face, what one means in referring to *the* relativistic Navier-Stokes equations *simpliciter*. Such a system must be formulated on a particular manifold having a particular topology and differential structure, using a particular affine structure. Presumably, in speaking of the *same* equations on two different spacetimes—the Navier-Stokes equations on each—we often intend something like the following: we mean that system on each that has the syntactic form of the system (2.3.3)–(2.3.6), with the appropriate derivative operator used in each case. So much causes no trouble, as it poses only a problem of orthography. The problem arises in the attempt to understand each of these systems as equations of the same *semantic form*, as it were.

Let me try to clarify what I am gesturing at. Think of the difficulties early investigators in general relativity faced in trying to generalize a system of equations from Minkowski spacetime to a generic curved spacetime by way of an application of the principle of equivalence. How ought one introduce the “coupling of the curvature” to the terms of the equation, to get the proper form? One cannot rely on mere syntactic equivalence of the equations (substitution of ‘ ∇_a ’ for ‘ ∂_a ’), if one demands only that the equations in curved spacetime “reduce to” those in Minkowski spacetime “in the limit as curvature vanishes”, as all terms involving curvature will be identically zero in Minkowski spacetime. Equation (2.3.3), for instance, could be written either as

$$\nabla_m(\nu\xi^m) = 0$$

or as

$$\nabla_m(\nu\xi^m) + R_{abcd}R^{abcd} = 0$$

in a spacetime with non-trivial curvature, and still reduce to

$$\partial_m(\nu\xi^m) = 0$$

in Minkowski spacetime. Determining the correct form in spacetimes with non-trivial curvature depends essentially on a determination of the way the fluid’s particle-number density *physically* depends on the curvature.

In the same way, in trying to determine which system of equations on two different spacetimes model the same sorts of physical systems, it will not suffice to demand only that the equations have the same syntactic form in the absence of a physical investigation. For all we know, the sorts of systems that we want, for various reasons, to identify as being “the same” in two different spacetimes couple to the curvature in a way that does not manifest itself except in spacetimes whose Riemann tensors have some *outré* property, or the systems may depend on the topology of the spacetime manifold, in the sense that non-trivial terms depending on, *e.g.*, its Euler characteristic must be included in the equations of the system—indeed, the systems may exhibit behavior that is, for one reason or another, best modeled using the terms of almost any mathematical structure one can imagine accruing to a model of the system. It is only by observation that such issues can be settled.

How do Navier-Stokes fluids behave in areas of non-trivial curvature, and in spacetimes with non-trivial Euler characteristic?—which is to ask, what system of equations most accurately models their behavior, within the proper regime? The answer to that question arbitrates questions as to the proper form for the Navier-Stokes equations. This, then, raises the questions of how one identifies a “Navier-Stokes fluid”, if not by susceptibility of modeling by whatever it is we settle upon as the Navier-Stokes equations—and here, I think, is the place where causal just-so stories find their place, in a limited way, in physics. “We know it’s a Navier-Stokes fluid because it is the concomitant, in the expected place, at the expected time, with the expected result, of this sort of coupling with this experimental apparatus. . .”—and if it fails to obey either the “regular” or a recognizably altered version of the Navier-Stokes equations, then we’re off and running, back to the races. This is, in general, no mean feat, especially when one is attempting to account for the features of physical phenomena that may “depend”, in some sense or other, on global properties of a spacetime. One cannot move to a region of non-zero Euler characteristic in a spacetime that has a zero one!⁹⁰

This discussion goes some way, I hope, towards explaining why I felt it necessary to include in the eighth component of the octuplets in volume twelve of our mythologizing books described in §5.1, and in the definition of well set initial-value formulations in this and the previous section, explicit reference to the spacetimes on which all these structures are imposed. Some spacetimes will not admit \mathfrak{m}_k -appropriate observatories for some kinematical regimes, so it seems not out of place to specify from the start which spacetimes one has in mind with regard to the application of one’s theory. It would be interesting to formulate some precise questions along these lines, to attempt to determine, for example, what sorts of constraints common spacetime models (Schwarzschild, FRWL, *et al.*) impose on the admissibility of potential models of laboratories.

5.5 Maxwell-Boltzmann Theories

The word ‘theory’ in physics and in philosophy has various, sometimes partially overlapping, meanings. I will focus here on its usage in physics. In physics, for instance, one may speak of classical

⁹⁰I believe considerations of this sort show why David Lewis’s and Saul Kripke’s conceit of some sort of *a priori* identification of physical systems “across possible worlds” is fair nonsense—or, if you will, given the uncritical acceptance often accorded the idea, unfair nonsense.

Navier-Stokes theory, of the theory of stellar structure, of quantum field theory and of thermodynamical theory. Roughly speaking, these examples descend in order of clear delineation of subject matter, clear delineation of physical phenomena to which they are applicable, clear delineation of experimental techniques used to probe those phenomena, and clear delineation of generally accepted mathematical structures and illustrative techniques used in solving problems in their respective domains.⁹¹

My use of ‘theory’ in this paper does not exclude those traditional uses; rather, it generalizes them. For my purposes, a theory is any more or less formal structure that contains both a system of partial-differential equations and a dynamical regime for the application of those partial-differential equations in the quantitative modeling of physical phenomena. Such a structure may include bits and pieces of other structures more commonly conceived of as integral theories, mixing and matching as it chooses, as happens in the modeling of actual experiments, so long as the sum total has a single, consistent dynamical regime. An example would be the use, in the elements of the regime of a theory, of just so much of quantum field theory as required for a Planckian treatment of electromagnetic radiation when modeling the measurement of systems having temperatures above 1063° Celsius.⁹²

In my usage, the intended sense of ‘theory’ is in many ways an ambiguous, even a nebulous concept—it is not *a priori* clear, for instance, whether a theory that treats joint electromagnetic and thermodynamic phenomena for which the Callendar equation suffices ought to constitute a theory different from one modeling essentially the same phenomena at similar and at lower temperatures, requiring the use as well of the van Dusen equation. I think this ambiguity is, in any event, no worse than that accruing to the standard usages. More to the point, however, I think this ambiguity underscores in a salutary way an important fact about physical theory: how one delineates a particular set of structures for the modeling of a more or less well delineated family of phenomena—the only substantive issue, I think, one can dispute about concerning the application of the term ‘theory’—is a profoundly pragmatic procedure. There is nothing *a priori* about it. Whether, for instance, the two structures differing almost only by the inclusion of the van Dusen equation constitute different theories will depend on one’s purposes in using or analyzing the structures of the theories, and why one cares in the first place about distinguishing various structures, more or less formal, as different theories.

This point relates to the Carnapian one about the pragmatic character of the selection of a linguistic framework. It differs from it in that here, one may say, one is attempting to decide how to

⁹¹I would not want to cavil about my proposed ordering—I’d be more than happy to rearrange it in the face of even mildly convincing argument. I care only that the point I am trying to make is clear.

⁹²I am glossing over an important distinction, that between, on the one hand, what theoretical apparatus forms part of a theory *per se*, and, on the other, what forms part of the concomitant but tacit theoretical apparatus required for a theory of measurement of the quantities modeled by the theory in experiments testing or employing the theory at issue—the apparatus forming part of the theory *per accidens*, if you will—this is a complex question, which I will not be able to address here.

In any event, though it may appear superficially similar, this issue is not directly related to the distinguishing in definition 3.4.1 between ϵ , the quantities directly treated by the theory, and \mathcal{E} , the environmental factors relevant to the propriety of the theory.

differentiate linguistic frameworks, one from another, rather than to distinguish among for various purposes and select one from an already existing family of frameworks. I should emphasize that my use of ‘pragmatic’ in this paper is not technical in the slightest. It bears no particular relation to any of its variegated uses by philosophers from Peirce to James to Carnap to Goodman to Quine and beyond. I (am trying to) use it in its diurnal, pedestrian—its pragmatic, if you will—sense: that pertaining to the choosing of courses of action by the weighing of alternatives and striking what one hopes is a satisfactory balance among all the competing and confluent objectives.

To guard against a possible misconstrual of my arguments and conclusions, I want to take a brief pause in the flow of this section to make clear that I think these arguments and conclusions serve directly to controvert positions such as those advocated in, *e.g.*, Cartwright (1999); they in no way support them. In particular, I am talking about Cartwright’s arguments and conclusions to the effect that science—scientific theories—consist of *nothing more* than families of more or less disparate, unrelated schemata of ways of modeling particular kinds of experiments. For instance, there’s the schema using the quantum *S*-matrix formalism to model the scattering of fundamental particles, which bears (on her view) no particular relation to the schema using standard perturbative techniques (say, expansion in spherical harmonics) to model, at the quantum level, the dynamic evolution of a Hydrogen atom in a static electric field. On the contrary, I believe my arguments show the profound, inextricable connections among such different theoretical models—the idea of a regime of propriety gets off the ground in the first place only to the extent that one can bring to bear on each other superficially disparate theoretical structures, as in the application of the Planckian treatment of electromagnetic radiation to thermodynamical thermometry. One must have already in hand a well worked out theoretical apparatus with understood ramifications into other such structures going far beyond an enumeration of highly schematized mathematical models in order to ascertain with confidence the propriety of a given theoretical structure for the modeling of a particular experimental arrangement, just as one must have already in hand the practical experience of many performances of particular kinds of experiments in order to conclude that their outcomes accord with or contravene the predictions of those theories, and whether the fault, in the case of contravention, lies with the experimental arrangement or performance, or perhaps rather indicates the presence of some novel phenomena not accounted for by the given theory.⁹³ If anything, this paper serves as an argument for a sort of Carnapian pluralism—one chooses a “framework” (theory with a regime) based on *pragmatic* criteria—simplicity, ease of use, facility for physical insight, elegance, what have you. It is a striking, brute fact about physics, perhaps the most singular fact about physics as a human enterprise—a fact that deserves puzzling over—that, in almost all known cases, there is a *single* candidate that jointly satisfies all these interests to a greater degree, for almost all active investigators in the field, than any competitor.

To pick up the thread of the argument of this section, I contend that, in order to be viable as a *physical* theory, a theory must have, at least in principle, a regime of propriety (or something very

⁹³To guard against a further misunderstanding, let me emphasize as well that I am speaking here only of the case of the application of an already well entrenched theory, not the testing of a novel one, which is a far more difficult case to straighten out.

like it) allowing for physically well set initial-value formulations in some (model of) spacetime or other. Before trying for more precision in the definition, I want to point out that, even as it stands, it can make itself useful. Many comments and questions about physical theories themselves and about their inter-relations can be formulated naturally in its terms. Take, for example, the puzzling case of the Bernoulli Principle, which has as a consequence that a body of water moving with a uniform, constant velocity has a static, hydrodynamic pressure less than that of an otherwise identical body of water at rest. This principle manifestly controverts the more fundamental and dearly held Principle of Galileian invariance: consider two channels parallel to and at rest with respect to each other; one, *A*, contains water still in relation to its banks; the other, *B*, contains water moving uniformly in relation to its banks. Is it the case, then, that, the water in *A*, and thus the 2 channels, are at rest and the water in *B* is moving? That the water in *B* is actually still, but the water in *A* as well as the two channels are in motion? Or that all four are in motion, so arranged as to give the described relation among them? According to Galileian invariance, no experiment we could perform should differentiate these possibilities from each other. In fact, however, actual experiments *do* differentiate them—a shower curtain’s motion inward toward the stream of water when a shower is first turned on provides a simple example, as does the fact that a blocked garden-hose will burst whereas one whose water is under the same motive force but is flowing will not. In the terms of the idea of a regime of propriety, we would describe the situation by saying that, in the hydrodynamical regime (for gross measurements of fluid velocity), no well-defined quantity will manifest any behavior that is not Galileian invariant, excepting only this, the hydrostatic pressure under conditions in which Bernoulli’s Principle finds application. In another regime, a finer-grained one, we expect there will be defined only Galileian invariant quantities, in terms of which one can show why satisfaction of the Bernoulli Principle appears to be a violation of that invariance, but is in fact not.⁹⁴ This suggests that, no matter what else is the case, equality (in some sense) of regimes is a necessary condition for the identity of two seemingly different theories.⁹⁵

One may ask, “To which theory in particular does the Bernoulli Principle belong, if it contravenes Galileian invariance, one of the foundations of Newtonian mechanics?” I do not think the question is of interest and perhaps not even, as it stands, sensible. Again, the taxonomy of theories can be argued over as one likes, or even the referent of ‘theory’, without changing the fact that it makes sense to articulate a system of partial-differential equations and an interpretation of them (which includes a regime) such that the equations under that interpretation adequately model the physical systems whose behavior conforms to that described by the Bernoulli Principle—“model it” in the sense that the theory models not only the particular family of phenomena demarcated by the

⁹⁴One may, for instance, give an “explanation” of the phenomena as follows. At the hydrodynamic scale, that at which the Bernoulli principle finds application, each body of water moves uniformly. At molecular scales, *i.e.*, those scales at which the hydrodynamic regime breaks down, the vector-field representing the accelerations of the molecules in the one body differs dramatically in kind from that representing the accelerations of the molecules in the other, in such a way as to yield, when averaged out, a lower hydrostatic pressure in the one than in the other. Because acceleration is a Galileian invariant quantity, we thus recover our dearly held principle.

⁹⁵This example suggests another possible virtue of the idea of a regime, no matter how one fleshes it out: the idea of a reduction of one theory to another may naturally be framed in its terms.

Bernoulli Principle, but some non-negligible other class of families of hydrodynamic phenomena as well (*e.g.*, laminar flow).⁹⁶ This last caveat is an attempt to respect the cantilevered, intermixed, even polygamous character of physical theories (or, if you prefer, of the components of Physical Theory).

Accepting all these difficulties, ambiguities and caveats, I propose the following as a characterization of part of what it is to be a (highly idealized) representation of a physical theory.

Definition 5.5.1 *A Maxwell-Boltzmann theory \mathfrak{T}_{MB} is a physical theory that includes (at least) an ordered triplet $(\mathfrak{B}, \mathfrak{I}, \mathfrak{V})$ such that*

1. \mathfrak{B} is a set of thirteen canonical volumes, whose contents are as described in §5.1
2. \mathfrak{I} is a connected set of idealizers, bounded with respect to the operator norm
3. \mathfrak{V} is a family of initial-value formulations of the partial-differential equations the theory comprises, each one physically well set with regard to the exact initial-value formulation yielded by application of any of the idealizers in \mathfrak{I}

To be a physical theory, I claim, a theory must be capable in principle of being made a Maxwell-Boltzmann theory. I use the qualifier “Maxwell-Boltzmann” to gesture at the fact that many of the seeds of this notion are already contained in a Maxwell-Boltzmann statistical treatment of thermodynamical phenomena. Accepting, then, definition 5.5.1, at least provisionally, one has as its most obvious consequence

Theorem 5.5.2 *A Maxwell-Boltzmann theory whose family of initial-value formulations contains at least one member must comprise only hyperbolic partial-differential equations.*

This is one way to make precise the claim that, so far as physical theory goes, only hyperbolic partial-differential equations have well set initial-value formulations. It also explicates in a precise way at least part of the privilege of the role played in physics by hyperbolic partial-differential equations.

5.6 The Consistency of Theory and Experiment

We are finally in a position to articulate the primary claim of this paper: in so far as one accepts that the models I have proposed adequately represent logical forms, as it were, of the common playground of the theoretician and the experimentalist, of their toys and rides, and of the games they play with each other—or at least in so far as one accepts that my proposals show that it is possible that something much better in their spirit can be constructed to represent these things—, one may conclude that there is no inherent contradiction between the practice and the subject-matter of the theoretician on the one hand and the experimentalist on the other, in so far as the

⁹⁶The question of what makes phenomena “hydrodynamic” over and above susceptibility to satisfactory modeling by theories that we nominate ‘hydrodynamic’ is fascinating—or, I should rather say, the question whether one can ascribe any meaning to the term relevant to physics over and above this susceptibility. It is also too difficult to address here.

entire paper up to this point has served as a constructive proof of this claim. The proof of the claim, moreover, does not depend on the existence of any actual theory or any actual, extended interplay between theoreticians and experimentalists that my models adequately represent. It is enough that such things may be logically modeled within the same, consistent schema, in a manner close enough to the actual proposal of theories and the performance of experiments to have some seeming to it.

Before leaving this subject, I want to emphasize one last time the lack of pretense that any thorough rigor accrues to this claim and its proof. The goal will have been reached if we have achieved a modicum of rigor, in those parts of the subject that can bear it, and slightly more clarity than that in the whole.

6 The Soundness of Physical Theory

So far, we have treated theories only in so far as they may be kinematically appropriate for the modeling of physical phenomena, without regard to how well or how poorly their models fare in the fineness and accuracy of their predictions about those phenomena. We turn now to consider these latter issues. We begin in §6.1 by examining what it may mean to claim that a theoretical prediction made by a Maxwell-Boltzmann theory does or does not agree with the experimental determination of the value of a physical quantity. This discussion naturally leads to a characterization of the self-consistency of a Maxwell-Boltzmann theory, in a particular form related to the idea of being able to test a theory in an unambiguous way that conforms to its kinematical regime. This will allow us, in §6.3, to characterize what it may mean to say that such a theory is *sound*, in the sense of modeling to a desired degree of accuracy the phenomena it purports to treat. We will focus on characterizing the elements a theory must possess in order for one to be able to judge whether or not it is sound—its *regime of dynamical soundness*—and on the sorts of properties those elements must have for one to conclude that the theory is in fact sound. This will lead, in §6.4, to a discussion of a peculiar form of under-determination necessarily attendant on any sound physical theory, one which I will be able to summarize with a precise, formal statement.

We will not treat any of these matters with anything near the same degree of thoroughness that we have up till now attempted in investigating the requirements for the kinematical adequacy of a theory. It is, perhaps, a game for another time.

6.1 The Comparison of Predicted and Observed Values

In studying the interplay between theory and experiment with regard to the kinematical requirements on physical theory, the most important and difficult issues involved accommodating the inevitable inaccuracies attendant on the determination of the values of quantities in any physical investigation. Now, in studying the issues that must be grappled with in attempting to determine whether a theory is adequate for the modeling of a class of physical systems in the sense of yielding sufficiently accurate predictions about the dynamical behavior of those systems, we must try to accommodate the related inevitability of the deviance of predicted from observed values in the modeling of thoroughly

understood physical systems by application of even the most well founded of theories. It is difficult to imagine, for instance, a theory better comprehended and more well founded than Newton's theory of gravitation—it is what we use, after all, to calculate the trajectories of successfully executed manned flights to the moon. It's hard to get more successful than that in science. Even so, errors inexorably occurred in the calculation of those trajectories, for reasons of widely varying types, theoretical, empirical and pragmatic. Our goal at the moment is to characterize with only the broadest of brush-strokes how this inevitable deviance of predicted from observed values may be treated within the framework we have developed so far.

As with the inevitable ranges of inaccuracy accruing to the experimental determination of the value of any physical quantity, we want to demand that a theory itself, in conjunction with other theories—those treating the measuring instruments employed in a particular experiment, for example—provide means for calculating ranges of admissible deviance of, on the one hand, the predictions of the theory for a particular system from, on the other, the results of measurements made during the course of the actual dynamical evolution of that system. *Admissible* here means nothing less and nothing more than that any measurement not according with the prediction to within that range of deviance ought require that one re-calculate the prediction, attempting to include the influence of factors not yet accounted for, or else that one similarly re-calculate the range of admissible deviance, or else that one repeat the experiment with a finer grain of control over the experimental circumstances, until the difference between the measurement and the prediction does fall within with the range of admissible deviance, or else that one count the experimental evidence as a contravention of the theory. Determining which conclusion to draw, and so which course of action to attempt, in any given case is one of those peculiar games that often cannot be played by either the experimental or the theoretical physicist alone, but will require the active participation of both.⁹⁷

It is important to be clear on how this differs from the calculation of the inaccuracies in the determination of the values of those quantities. Calculating the inaccuracies of a measurement involves no normative judgment; its result is a description of a brute, factual matter. Judging whether or not the value determined by the experiment accords sufficiently well with the value predicted by the theory is a thoroughly normative affair; it is not a description of a brute, factual matter, is not, indeed, a description of a physical state of affairs at all, but rather an assessment of the soundness of our knowledge of the physical world. When I say the former involves “no normative judgement”, I do not mean to imply that the aesthetic and pragmatic considerations I have emphasized all along as being in play in the mutual application of theory and experiment are not normative. They are. I mean rather to say only that calculations of inaccuracy are not normative statements. They express no judgments about fineness or suitability or acceptability or what-have-you. Judging the admissibility of a certain deviation of predicted from observed values, however, is inescapably normative, in so far as the physical world does not provide for us criteria to

⁹⁷The discovery in the 1950s of the non-conservation of parity in processes mediated by the weak nuclear force provides a vivid example of the necessity of the participation of both sides. For a lively and gripping account of the episode, see Yang (1961).

judge the degree of soundness of our knowledge of it in any particular case. There are similarities between the two cases as well. Although judging the soundness of the prediction is a normative affair, determining the deviance itself of the predicted from the observed value is a brute, factual matter along the same lines as the computation of the inaccuracy accruing to the measurement. There are, therefore, two distinct steps in judging the soundness of a theory's predictions as compared to experimental observations. First, one must determine what the deviance is of the prediction from the observation. Next, one must judge whether or not it is admissible. In this section, we will consider only the former issue, postponing the latter one until §6.3.

On its face, the idea of *the* deviance of predicted from observed values is not a clear one. Consider trying to calculate such a thing for a scalar quantity. In predicting the value of that quantity for a particular system under particular circumstances, one will produce not an exact scalar but rather an inexact scalar, representing the spread of possible values for the scalar within the range of possible inaccuracy of the experiment being modeled. Measuring the quantity will also yield not an exact but an inexact scalar, and, inevitably, a different one. We can compute their algebraic difference readily enough, using any of our three types of operations—physical, psychological or pragmatic—but none of them seems quite right for the job. The pragmatic one will not do, since we are not treating these values as mere numbers, but rather as the representation of the value of a physical quantity. On the other hand, the physical operation will not do either, for these are not representations of physical quantities whose physical combination we are trying to represent by the use of an algebraic operation; these are rather different types of representations of the value of the same physical quantity. Recall that we selected the form for our physical operations based on an analysis of the way that inaccuracies combine and propagate in calculations involving the values of physical quantities, in so far as those combinations of values represent the kinematical and dynamical relations and interactions of the quantities; in particular, our analysis relied on the fact (or, if you like, the assumption) that these sorts of inaccuracies tend to cancel each other out and so decrease over time. In order for this argument to work, we must assume that, in a typical case, the inaccuracies of all the quantities distribute themselves in a more or less Gaussian form around, respectively, the more or less stable mean of each. It makes no sense to say that the two inaccuracies we are considering here will “tend to cancel each other out over time”, because the inaccuracy as determined by the theoretically predicted value does not arise from the physical interaction of actual physical systems, the very variability of which over time allows us to treat inaccuracies as we do. The predicted inaccuracy arises from something like a representation of the Platonic form of the observation—it never changes—not from the actual, physical circumstances of the experiment as it is being performed. We must, it seems, come up with some other way of comparing the predicted and the measured values.

[*** The point: the comparison of the two types of values is not an algebraic operation at all; it is wholly topological ***]

We want to define a way of comparing inexact fields, for use as a criterion in determining, without ambiguity, whether some set of measured values, with their associated inaccuracies, falls within the ranges of allowed deviances from the predicted values. Because an approach based on algebraic

operations faces formidable difficulties, we will attempt a route with a more topological bent. As with almost every structure proposed in the construction of our model, there is more than one way to do it, and some of the choice must be made on pragmatic grounds, influenced by the demands of the enterprise at hand and guided by taste and predilections. I choose one that seems to me to have clear physical significance in a wide variety of applications, and that is simple to comprehend and simple to apply.

Definition 6.1.1 *Two inexact scalar fields are consonant if*

1. *they share the same support*
2. *at every point of their support, the real intervals representing, respectively, the values of each have a non-trivial intersection*

Of two consonant, inexact scalar fields, the first dominates the second if, at every point of their support, the inaccuracy of the first is greater than that of the second, and their intersection includes the magnitude of (i.e., the midpoint of the interval representing) the first.

For many pairs of consonant, inexact scalar fields, neither will dominate the other. This definition can be extended directly to inexact tensor-fields.

Definition 6.1.2 *Two inexact tensorial fields are consonant if*

1. *they share the same support*
2. *at every point of their support, the 4-spheres of possible magnitudes representing, respectively, the values of each have a non-trivial intersection*

Of two consonant, inexact tensor-fields, the inaccuracies of the first dominate those of the second if, at every point of their support, the radius of the 4-sphere of possible values of the first, as determined by the Σ -norm (or k -norm, as applicable), is greater than that of the second, and their intersection includes the magnitude of (i.e., the center of the 4-sphere of) the first.

The relation of dominance gets us closer to what we want, but does not by itself suffice, as it provides no quantitative measure of the differences between the inexact values. We can, however, use it to state a plausible necessary condition for a predicted value to deviate admissibly from the observed value: the deviation is admissible only if the field of experimentally observed values dominates the field of predicted values.

There is, as always, a wide selection to choose from in imposing a quantitative measure on the relation of dominance, which can then be used to formulate sufficient conditions on the two fields for the deviance of the values of the one from those of the other to be admissible. We choose the following for all the usual reasons. Fix two inexact, mottled tensorial fields, κ and λ , such that κ dominates λ . At every scrap S of their shared support \mathcal{S} , let $\delta_s(\kappa(S), \lambda(S))$ be the absolute magnitude of the difference of the diameters of their respective 4-spheres of possible values.

Definition 6.1.3 *The dominance of κ over λ , $\Delta_\kappa(\lambda)$, is the volume-weighted average of the integral of δ_s over \mathcal{S} :*

$$\Delta_\kappa(\lambda) \equiv \frac{\int_{\mathcal{S}} \delta_s(\kappa(S), \lambda(S)) d\mu(S)}{\int_{\mathcal{S}} d\mu(S)}$$

$d\mu$ is the measure on decoupages we defined in Curiel (2010b) and referred to in §4.8 above. We can now characterize the deviance of predicted from observed values in terms of dominance. Given the predicted inaccuracy in the value of a field after dynamical evolution of a certain sort, and given the range of possible inaccuracy accruing to the determination of that value dictated by the kinematical regime under the experimental circumstances after the evolution, we say these two have a *deviance* equal to the dominance of one over the other, if one of them does in fact dominate the other. Clearly, then, the admissible deviances of a theory can be represented, at least in a purely formal fashion, by a family of dominances. One now can determine whether or not a given prediction accords with a given experimental result by comparing the actual deviances of the predicted from the observed values with the admissible deviances.

6.2 Consistent Maxwell-Boltzmann Theories

Before we discuss what is involved in laying down criteria for the admissibility of deviances, let's assume for the moment that we have a set of such criteria in hand, expressed as a family of dominances. There now arises the issue of the self-consistency, in a certain sense, of the physical theory. Let's say, for example, that an experimentalist is modeling a proposed experiment using a Maxwell-Boltzmann theory, including the family of dominances. She constructs and solves a physically well set initial-value formulation modeling the experiment, including the calculation of the inaccuracies accruing to the determination of the magnitudes at the end of the experiment, according to her solution. She then finds, to her surprise and discomfiture, that these calculated inaccuracies are all greater than the admissible deviances of predicted from observed values for that type of system under those experimental circumstances. It would seem, in such a case, that one could not determine whether an observation that seemingly conformed to the theory actually did so; the inaccuracy in the determination of the value is so great that the experimental agreement may be purely artifactual and not a true indicator of the soundness of the theory.

[*** clarify this muddle ***]

In more traditional terms, one might describe the problem as follows. Any two sets of exact initial data falling within the interval of possible inaccuracy for that system at the moment the experiment commences⁹⁸ have equal claim to represent the idealized, exact state of the system. Given two sets of initial data differing only slightly from each other, moreover, less than the possible inaccuracy, the two respective solutions to the set of exact partial-differential equations may, in general, evolve to be further and further apart, in a variety of technical senses, as time passes. Say, then, for a fixed observatory, we begin with two \mathfrak{R} -appropriate sets of exact initial data for the idealized partial-differential equations of a theory, each set representing the initial state of the same spatiotemporal

⁹⁸Where, recall, “moment” really means “a likely quite brief, yet finite, temporal interval”.

region of the observatory, the two differing from each other by no more than the theory's interval of possible inaccuracy. Say, moreover, that solving the equations for the evolution of the system for each of the two sets yields respective exact solutions that, after a finite period of time, differ from each other by more than the admissible range of deviance of observed values of the quantities from predicted ones. Under these circumstances, it would not seem to make sense to ask whether the outcome of an experiment beginning with exact initial data within the possible range of inaccuracy encompassing those two sets conformed to or controverted the predictions of the theory, in so far as one's choices among differing but equally acceptable sets of exact initial data yield respective exact results that cannot possibly all be consonant with the predictions of the theory.⁹⁹

In the terms of the machinery developed in this paper, we would say that, for the models of this experiment provided by the theory, the solutions to the physically well set initial-value formulations derived from two different idealizers will eventually be so different that at most one of them could be correct. This possibility arises from the fact that the solution to a well set initial-value formulation of an inexact partial-differential equation is itself an inexact field. It would seem that, if we are to have the capacity to judge whether or not the predictions of a theory soundly model physical phenomena, then we require that the inaccuracies of solutions to the theory's physically well set initial-value formulations dominate the ranges of possible inaccuracy accruing to the determinations of the values of those solution as dictated by the kinematical regime, and does so, moreover, by at least the given dominance. In the absence of any further requirements, nothing guarantees this.

[*** consistency can only be had for finite temporal intervals, since any solution will achieve arbitrarily large inaccuracies eventually, after a long enough period of evolution—or is that true? are there solutions that yield asymptotically bounded inaccuracies? I bet there are ***]

This possibility raises two questions. Given a Maxwell-Boltzmann theory and a set of criteria for determining the admissibility of observed deviances, are there any physically well set initial-value formulations that yield solutions of a kind as to be meaningfully compared with observational data? And is there any way to guarantee that a theory will possess such physically well set initial-value

⁹⁹I will not have room to treat another fascinating topic along these lines. So far, I have treated (albeit implicitly) “noise” as a source only of inaccuracy in the outcomes of experiments. Noise, however, can play other, indeed beneficial roles. In modeling and measuring the firing of action potentials in large populations of neurons, for instance, one *depends* on the presence of a small amount of background noise in order to potentiate the firings. More precisely, in the presence of subthreshold inputs, a small amount of noise (which doesn't have to be white—it can be colored, *i.e.*, temporally correlated, such as white noise passed through a first-order linear filter) causes threshold crossings and can reveal information about the structure of the otherwise subthreshold signal. Because the firing of a neuron is a time-delayed event occurring only after a discrete threshold in the potential is achieved, rather than a continuous and continuously responsive process, therefore, one can show that, under certain circumstances, the presence of noise markedly increases the probability of a firing-event during any given temporal interval. In so far as we believe these conditions to obtain *in vivo*, it is hypothesized that the character of the ambient noise in the brain plays an integral role in determining the firing patterns a neuronal population will exhibit under fixed stimulus. This is an example of a more general phenomenon known as ‘dithering’, familiar to the engineering community since the 1950s. See, for example, Knight (1972), Gammaitoni (1995), Wiesenfeld and Moss (1995), and Hunter, Milton, Thomas, and Cowan (1998).

I thank John Hunter for bringing to my attention these sorts of peculiar roles noise can play in experiments, as well as for directing me to the citations.

formulations, for instance by imposing requirements on the form of its comprised partial-differential equations? The latter question is beyond the scope of this paper. We can, however, give a simple way to characterize theories in answer to the former question. Let us denote the set of admissible deviances we assume the theory to possess by ‘ \mathcal{D} ’. A physically well set initial-value formulation i of a Maxwell-Boltzmann theory *respects* \mathcal{D} if the inaccuracies of the fields derived as its solution dominate the kinematical inaccuracies accruing to the determination of the values of those fields, with a dominance at least as great as that given by \mathcal{D} . This gives us the required condition for consistency.

Definition 6.2.1 *A Maxwell-Boltzmann theory is consistent with a family of dominances \mathcal{D} if every physically well set initial-value formulation in the theory respects \mathcal{D} .*

This suggests that, in order to be consistent, a Maxwell-Boltzmann theory must have its number of canonical volumes increased by two: a fourteenth volume consisting of an enumeration of ordered pairs, each consisting of a family of formal dominances and an octuplet from the twelfth volume, and a supplement to the thirteenth volume to render the semantical interpretation of these dominances as admissible deviances. We now turn, in the next section, to discussion of the sorts of content we may want in this semantical supplement.

6.3 The Dynamical Soundness of a Physical Theory

To begin to get a grip on these issues, consider again, for a given theory, the system of equations the theory comprises. Recall from the discussion of Navier-Stokes fluids in §2.4 that such a system may fail in so far as it is applied to regions in which it does not provide adequate predictions—in other words, the initial-value formulation of that system breaks down when formulated over such regions—even though the quantities treated by the theory are well defined over such regions. Now, what counts as an adequate prediction by a theory will vary from application to application in the following sense. Say we are to measure the temperature of a given type of physical system under two different sets of initial conditions, using a type of thermometer different in the one case from the other, and we then compare the results of the measurements with the predictions of our theory. Whereas we may find it admissible, say, 5% of the time, for the actual, measured value of the temperature to deviate 3% from the predicted value in the first case, we may find such a deviance to be wholly inadmissible with any non-negligible frequency in the second. In order to make these sorts of judgements, we require a set of methods for calculating, for particular types of systems under particular conditions, admissible deviances, from the values predicted by the theory, of the values of the quantities measured using particular types of probes, along with a statement of probability indicating one’s level of confidence that the actual error will lie within the range of deviance settled on.

The measurement of temperature once again provides an excellent, concrete example of this phenomenon. Consider a thermometer immersed in an environment the temperature of which one has reason to believe is increasing at a constant rate. Because the equilibration of the thermometer

Theory and Experiment

with its environment always takes a finite amount of time, at any given instant the thermometer's reading will be a measure not of the environment's temperature at that very instant, but rather at an instant in the more or less immediate past. One naturally wonders about the time of response of the thermometer to the change in temperature—how many seconds behind the actual environmental temperature is the thermometer's reading? In order to treat this question at a somewhat elementary level, let us make the following assumptions. All the heat transferred to the thermometer is to be by convection, and all this heat is then retained in the thermometer. Thus, the rate of the transfer of heat through the convective layer immediately surrounding the thermometer exactly equals the total rate at which the thermometer absorbs heat. The equation of the evolution of the thermometer's temperature can then be expressed by combining Newton's law of cooling with Black's equation of heat capacity:

$$\frac{d\theta}{dt} = \frac{\chi A}{wc}(\theta_e - \theta)$$

where θ is the thermometer's temperature at time t , θ_e is the environment's temperature at time t , χ is the coefficient of convective heat-transfer between the environment and the thermometer, A is the surface area of the thermometer through which heat is transferred, w is the weight of the thermometer and c is its specific heat capacity.¹⁰⁰ $\tau \equiv \left(\frac{\chi A}{wc}\right)^{-1}$ has the dimensions of time, and is known as the *characteristic time constant* of the system. Assuming the simple initial relationship $\theta = \theta_e - Rt$, where R is the rate of temperature increase, then, after some elementary manipulation and integration, one deduces that, in the limit $t \gg \tau$, the relationship settles down to $\theta = \theta_e - R\tau$.¹⁰¹ In other words, if the thermometer has been immersed in the environment for a long enough period of time, then the characteristic time constant is the length of time between the environment's being at a certain temperature and the thermometer's indication of that temperature.

In order to determine the characteristic time constant of the system, therefore, it suffices to immerse the thermometer, initially at a fixed uniform temperature, into an environment the rate of change of the temperature of which is constant and known. Two obvious problems now arise: in order to determine the rate of change of the environment's temperature, one must have a thermometer whose characteristic time constant for that environment is either known already or, at least, is known to be negligibly small; and to do this, the coefficient of convective heat-transfer must be known. This latter presents a particularly difficult challenge, for it follows from Nusselt's equation of heat transfer by forced convection that determination of this coefficient within reasonable bounds of uncertainty depends on somewhat detailed knowledge of the mass velocity of the environment relative to the thermometric surface, which is in general a complicated, 3-dimensional flow. Other factors, which must be taken into consideration when going beyond our elementary assumptions, are known to influence the characteristic time constant as well, including, *inter alia*, the Mach number of the environment, the size of the temperature change being considered, the rate of axial conduction of heat from the environment to the thermometer, the intensity and quality of the ambient radiation,

¹⁰⁰Of course, all these latter quantities, when measured to a sufficient accuracy, are themselves functions of the temperature, among other relevant quantities, but we will bracket this point for a moment, assuming that the coefficient of convective heat-transfer, the area, *etc.*, are constant.

¹⁰¹See, *e.g.*, Benedict (1969, §11.2, pp. 144–5).

and the turbulence in the environment. For certain kinds of systems, for example, a fourfold increase in the total temperature change can lead to a 25% variation in τ , and a 1.5% change in the intensity of turbulence can, by dint of influencing the coefficient of convective heat-transfer, change τ by up to 25% as well.¹⁰² As this example illustrates, the specification of admissible deviances must be made with regard, *e.g.*, to temperature measurements of particular types of systems under certain kinds of conditions, not generically for all thermodynamical temperature measurements *simpliciter*.

These sorts of consideration suggest the following way of making these ideas precise.

Definition 6.3.1 *A regime of dynamical consistency (or dynamic regime, for short) for a Maxwell-Boltzmann theory is an ordered pair $(\mathfrak{d}, \mathcal{D})$ such that*

1. \mathfrak{d} is a set of algebraic and differential constraints on the physical quantities of the systems cum environments treated by the theory
2. \mathcal{D} is a family of dominances defined in terms of those quantities consistent with the theory

This allows for, finally,

Definition 6.3.2 *A Maxwell-Boltzmann theory is dynamically sound if it has a regime of dynamic consistency that accords with experiment.*

6.4 Theoretical Under-Determination

[*** The fact that any given *measurement* is compatible with any of an infinite number of different theoretical propositions (*i.e.*, the ascription of an exact real number, or field of real numbers, to a point or region of spacetime) is well known (see, *e.g.*, Duhem, *The Aim and Structure of Physical Theory*, on the difference between what he calls practical facts and theoretical facts); the theorem I offer is more far-reaching: any given set of measurements, no matter the cardinality one allows for the set, is compatible with an infinite number of different dynamical structures, in a certain sense all continuous with each other—that is to say, with an infinite number of different mathematical theories of the same phenomena. ***]

[*** see intro to Geroch (1995) on describing the physical content of a Navier-Stokes and possible hyperbolizations: an example of highly non-trivial re-jiggerings that have all the same physical, semantical content ***]

Let us say that we have a dynamically sound theory in hand. We have used the machinery of canvases and decoupages, and of inexact fields and motleys, to represent the inaccuracy inevitably inhering in the modeling of experiments and the data they generate, as well as in defining the admissibility of deviances of predicted from observed values; nevertheless, it is still the case that, when we want to make contact with physics as practiced today, we must idealize. This raises the question: what idealizer will we choose? Even though the idealization of an inexact structure picks out a unique exact structure, there will in general, in virtue of the stability of idealizers, be many that yield exact structures so close to each other, in a certain technical sense, as to be indistinguishable

¹⁰²*Ad loc.*

with respect to the regime of the theory in play. In this section, we will make these considerations precise and draw out a few of their implications.

Consider again the proposed hyperbolic theories of relativistic, dissipative fluids discussed in §2, assuming for the sake of argument that they possess the structure of sound Maxwell-Boltzmann theories. Because the fineness of the observation and measurement of terms in the hyperbolic systems is circumscribed by the regime's possible inaccuracy, one will not be able to distinguish in a finite temporal interval any two solutions of the system differing from each other in an appropriate sense by no more than allowed by this inaccuracy during that interval. More to the point, one will not be able to distinguish a solution to one hyperbolic system from that of another, comprised by otherwise identical theories, so long as, again, those two solutions differ from each other by no more than the possible inaccuracy allowed by their shared kinematical regime. This fact naturally suggests the question, whether, given a hyperbolic system and a kinematical regime, there exists another hyperbolic system such that the set of solutions to the first system corresponding to any set of admissible initial-data continued for a finite temporal interval differs by no more than the possible inaccuracy allowed by that kinematical regime, for the same set of initial-data during the same temporal interval.

The perhaps surprising answer is that one can give an almost trivial proof to a mathematically precise statement of the question; the proof depends, however, on the hyperbolicity of the partial-differential equations at issue. As a consequence, given any sound two Maxwell-Boltzmann theories agreeing in their regimes and differing only with respect to the systems of partial-differential equations they comprise, both of which satisfy the conditions of the theorem, one will have no grounds for concluding on purely observational grounds that one of the theories is to be preferred over the other. In any event, one should again *not* take this as an argument for any sort of anti-realism or instrumentalism, *à la* Cartwright (1999).

Before stating the primary result of this section, the mentioned theorem, we need to lay down a few more definitions. First, the *supremal spacelike diameter* $\sigma_{\text{sup}}[O]$ of an open subset of spacetime of compact closure O is defined by

$$\sigma_{\text{sup}}[O] \equiv \sup \left\{ \int (|\gamma^m \gamma^n g_{mn}|)^{1/2} ds : \gamma \in \mathfrak{s}_O \quad \& \quad \gamma^a = \frac{d\gamma}{ds} \right\}$$

A second one depends on the fact that, as shown in Curiel (2010b, §5.5.2), given an inexact initial-value formulation, there is a more or less natural way to single out an ordinary spacelike hypersurface contained wholly within the spacelike, convex hull of an infimal decoupage on which the inexact initial data is fixed. Consider the family of all ordinary timelike geodesics orthogonal to this surface that intersect the future boundary of the ordinary domain of dependence of that hypersurface. The *maximal time of Cauchy development* for this inexact initial-value formulation, then, is the supremum of the intervals of proper time from the hypersurface to the boundary along the geodesics in the family.

There follows, as a direct consequence of theorems 5.3.6, 5.4.1, and 5.5.2,

Theorem 6.4.1 *Given any sound Maxwell-Boltzmann theory and any $\sigma, \tau > 0$, there exists a second sound Maxwell-Boltzmann theory distinct from the first having the same exact theory with regime as its idealization, if one restricts attention to physically well set initial-value formulations such that the supremal diameter of the domain of initial data is less than σ , and its maximal time of Cauchy development is less than τ .*

In effect, one has the freedom to change the system of partial-differential equations comprised by any Maxwell-Boltzmann theory, so long as one makes corresponding adjustments in one's choice of a family of idealizers, and produce a second, mathematically distinct theory that is observationally indistinguishable from the first: no possible set of observations could favor one over the other.

This theorem differs in significant ways from traditional results on the under-determination of theory by data. For starters, this result bears solely on the mathematical structure of the theory—its syntax, as it were—not on its interpretation, as in, say, disputes over the observational indistinguishability of that old chestnut, the Copernican and the Ptolemaic systems. It mandates, nevertheless, a far more pragmatic, almost inductive form of indistinguishability than the traditional one, in so far as it declares an indistinguishability tied to our actual practices of investigating nature by the vehicle of physical science—you tell me how long you want indistinguishability for, and I'll arrange it. Even when the issue is not one of strict observational indistinguishability, all such results I know of rely on the quantity of data being finite or even countable, for any rigorous conclusions to be drawn. No such restriction is placed here. No other result I know of, moreover, allows one to quantify the allowable deviances of different theories from each other depending on the spatiotemporal extent one wants to allow for observation and measurement. Finally, and perhaps most importantly, the whole tenor of this result differs from traditional ones, in so far as it allows one to have already in hand a theory whose predictive power is as accurate as allowed for by the nature of the quantities at issue and the character of the techniques available for their study—quite literally, as accurate as possible—and still declares that this does not suffice for the unique fixing of a mathematical structure for a theory, *not even up to isomorphism of any sort*. In traditional arguments about the observational indistinguishability of two theoretical structures, such as with the Copernican and the Ptolemaic systems, one can always show that the essential kernel of the respective mathematical structures are isomorphic in the relevant way. In fact, I would argue that any proof of traditional indistinguishability requires such an isomorphism. This conclusion, if you like, underscores the necessarily approximative character of the enterprise of physical science.

7 The Theory Is and Is Not the Equations

Hertz famously said, “Maxwell's theory is his equations.” Well, yes and no.

In one sense, the sense I think he meant, Hertz was assuredly correct. Maxwell's theory at the time stirred up a storm of controversy in large part because physicists of the day did not know how to think about the theory in the terms with which they were accustomed. The theory manifestly modeled a type of system of an oscillatory nature, and yet the theory said not a word about what

was “doing” the oscillating. Where there are waves, the thinking went, there is a physical medium waving (the “luminiferous æther”); any putative theory that predicts waves and yet does not identify the medium, does not tell us how to investigate the properties of the medium, does not tell us how to envision the medium, is no physical theory. The same sort of thinking lay behind much of the contemporaneous controversy surrounding Newton’s publication of his theory of gravity, and in particular his bold claim, “Hypotheses non fingo.” I think he was correct in that bold claim, in the same sense that Hertz was correct in his assessment of Maxwell’s theory. The theory in both cases, in the sense I am currently discussing, just *is* a more or less formal structure representing the patterns of behavior we have managed to extract from and impose on masses of observational data. The sterling virtue of this form of representation, moreover, is the capacity it lends us both to make predictions about the future behavior of physical systems of a certain sort, and to reason in a fairly precise way about quite general features of those sorts of physical systems. We do not require of this structure, in order to use it for these purposes, that it embody the tenets of any system of beliefs about the nature of the world that goes beyond the empirical, beyond, that is to say, what can be observed experimentally.

As I say, in that sense I think Hertz (and Newton) was correct. There is still a sense, however, in which I think the statement is not correct, though, I must stress, the sense is different enough that it has no bearing on the correctness of Hertz’s intent, as I see it, in stating it.¹⁰³ To explicate that sense, consider the recent controversy, misguided in my opinion, about the priority of the “discovery” of general relativity: Hilbert or Einstein? If one thinks the theory of general relativity “is the Einstein field-equation”, then I suppose there is a strong case for Hilbert. In a certain sense, I agree with this sentiment: knowing the Einstein field-equation, one, in a *recherché* sense to be sure, but still in a definite sense, knows how to model and comprehend all the phenomena putatively treated by the theory. What more could one ask of a theory?

As I have strenuously attempted to demonstrate in this paper, however, in a substantive and profound sense, the equation is most assuredly *not* the theory: one also needs all the collateral knowledge, both theoretical and practical, *not* contained in the equation, in order to apply the equation to the modeling and comprehension of all the phenomena putatively treated by the theory. To put the matter more vividly: the equation *as a result of* a (profound) investigation of the physical phenomena at issue, of all the empirical data and attempts to model that data heretofore, a teasing apart and characterization of the maximally common structure underlying the system of relations that obtains among them (supposing there is such a thing)—the sort of investigation that Newton and Maxwell and Einstein did and Hilbert did not accomplish—the equation as a result of that is the theory.¹⁰⁴

¹⁰³I suspect—or at least hope—based on my reading of Hertz that he would have agreed with what I am saying here.

¹⁰⁴Compare the remarks of Born (1943, p. 9), very much in harmony with the spirit of this discussion: “. . . none of the notions used by the mathematicians, such as potential, vector potential, field vectors, Lorentz transformations . . . are evident or given *a priori*. Even if an extremely gifted mathematician had constructed them to describe the properties of a possible world, neither he nor anybody else would have had the slightest idea how to apply them to the real world. The problem of physics is how the actual phenomena, as observed with the help of our sense organs aided by instruments, can be reduced to simple notions which are suited for precise measurement and used for the

There is no theory without experimental data to comprehend, in all its extravagant and fertile inaccuracy, without the capacity to get the laboratory into the theory. It was Einstein the physicist, not Hilbert the mathematician, who calculated the precession of Mercury's perihelion using a system of intuitively well founded approximations (Schwarzschild had not yet discovered his solution), who modeled a real physical system in the terms of his proposed theory and demonstrated that the theory had the (or: a) structure proper for the modeling of the phenomena. This is not the same thing as demonstrating that the theory is accurate in its predictions about the phenomenon; I think it is all too common, however, to conflate these two, by focusing on prediction as the be-all, end-all of physics. The two—modeling and prediction—do not come to the same thing, as the distinction between the kinematic regime and the domain of soundness shows.¹⁰⁵

Not only are theory and experiment consonant with each other, they are mutually inextricable—not, however, as equals. Theory plays Boswell to the subtle and tragic clown of experiment's Johnson.

References

- Anile, A., D. Pavón, and V. Romano (1998). The case for hyperbolic theories of dissipation in relativistic fluids. *arXiv:gr-qc/9810014v1* (<http://xxx.lanl.gov/abs/gr-qc/9810014>).
- Anscombe, G. E. M. (1971). Causality and determination. In *Metaphysics and the Philosophy of Mind*, Volume 2 of *The Collected Papers of G. E. M. Anscombe*, pp. 133–147. Minneapolis: University of Minnesota Press, 1981. Originally delivered as Anscombe's inaugural lecture for her professorship at Cambridge University in 1971.
- Benedict, R. (1969). *Fundamentals of Temperature, Pressure and Flow Measurements*. New York: John Wiley & Sons, Inc.
- Bondi, H. (1962). On the physical characteristics of gravitational waves. In A. Lichnerowicz and A. Tonnelat (Eds.), *Les Théories Relativistes de la Gravitation*, Number 91 in *Colloques Internationaux*, pp. 129–135. Paris: Centre National de la Recherche Scientifique.
- Born, M. (1943). *Experiment And Theory in Physics*. New York: Dover Publications, Inc. The work is a slightly expanded form of a lecture given to the Durham Philosophical Society and the Pure Science Society, King's College, Newcastle-upon-Tyne, May 21, 1943. This edition is a 1956 reprint of the original Cambridge University Press printing of 1943.
- Burgess, G. (1928, July–December). The international temperature scale. Technical Report RP 22(635), The Journal of Research of the National Bureau of Standards.
- Callendar, H. (1887). On the practical measurement of temperature. *Philosophical Transactions of the Royal Society (London)* 178, 160.

formulation of quantitative laws.”

¹⁰⁵My version of the mechanist philosophy? No explanations except relational ones?

- Cartwright, N. (1999). *The Dappled World: A Study of the Boundaries of Science*. Cambridge: Cambridge University Press.
- Curiel, E. (1999). The analysis of singular spacetimes. *Philosophy of Science* 66(Proceedings), 119–145.
- Curiel, E. (2000). On the non-existence of purely gravitational stress-energy tensor in general relativity. Unpublished manuscript.
- Curiel, E. (2005). *Three Papers on How Physics Bears on Philosophy, and How Philosophy Bears on Physics*. Ph. D. thesis, University of Chicago, Chicago.
- Curiel, E. (2010a). Classical mechanics is Lagrangian; it is not Hamiltonian. Unpublished manuscript submitted to [***], [***] 2010, for review.
- Curiel, E. (2010b). A formal model of the regime of a physical theory, with applications to problems in the initial-value formulation of the partial-differential equations of mathematical physics. Unpublished.
- Earman, J. (1978). Combining statistical-thermodynamics and relativity theory: Methodological and foundations problems. *Philosophy of Science* 2(Proceedings), 157–85.
- Eddington, A. (1923). *Mathematical Theory of Relativity* (second ed.). Cambridge: Cambridge University Press.
- Eu, B. (2002). *Generalized Thermodynamics: The Thermodynamics of Irreversible Processes and Generalized Hydrodynamics*. Dordrecht: Kluwer Academic Publishers.
- Fine, A. (1982). Some local models for correlation experiments. *Synthese* 50, 279–94.
- Gammaitoni, L. (1995). Stochastic resonance and the dithering effect in threshold physical systems. *PRE* 52(5), 4691–4698.
- Geroch, R. (1969). Spinor structure of space-times in general relativity I. *Journal of Mathematical Physics* 9, 1739–1744.
- Geroch, R. (1970a). Domain of dependence. *Journal of Mathematical Physics* 11(2), 437–449.
- Geroch, R. (1970b). Spinor structure of space-times in general relativity II. *Journal of Mathematical Physics* 11, 343–8.
- Geroch, R. (1995). Relativistic theories of dissipative fluids. *Journal of Mathematical Physics* 36, 4226.
- 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.
- Geroch, R. (2001). On hyperbolic “theories” of relativistic dissipative fluids. *arXiv:gr-qc/0103112v1* (<http://xxx.lanl.gov/abs/gr-qc/0103112>).

- Hall, J. (1955). The international temperature scale. In *Temperature*, Volume 2, pp. 116. New York: Reinhold.
- Halmos, P. (1950). *Measure Theory*. New York: Van Nostrand and Co.
- Herrera, L. and J. Martínez (1997). Dissipative fluids out of hydrostatic equilibrium. *arXiv:gr-qc/9710099v1* (<http://xxx.lanl.gov/abs/gr-qc/9710099>).
- Herrera, L. and J. Martínez (1998). Thermal conduction before relaxation in slowly rotating fluids. *arXiv:gr-qc/9804035v1* (<http://xxx.lanl.gov/abs/gr-qc/9804035>).
- Herrera, L. and D. Pavón (2001a). Hyperbolic theories of dissipation: Why and when do we need them? *arXiv:gr-qc/0111112v1* (<http://xxx.lanl.gov/abs/gr-qc/0111112>).
- Herrera, L. and D. Pavón (2001b). Why hyperbolic theories of dissipation cannot be ignored: Comment on a paper by Kostädt and Liu. *Physical Review D* 64(088503). Also available as *arXiv:gr-qc/0102026v1* (<http://xxx.lanl.gov/abs/gr-qc/0102026>).
- Herrera, L., A. D. Prisco, J. Martín, J. Ospino, N. Santos, and O. Troconis (2004). Spherically symmetric dissipative anisotropic fluids: A general study. *arXiv:gr-qc/0403006v1* (<http://xxx.lanl.gov/abs/gr-qc/0403006>).
- Herrera, L., A. D. Prisco, and J. Martínez (1998). Breakdown of the linear approximation in the perturbative analysis of heat conduction in relativistic systems. *arXiv:gr-qc/9803081v1* (<http://xxx.lanl.gov/abs/gr-qc/9803081>).
- Hiscock, W. and L. Lindblom (1985). Generic instabilities in first-order dissipative relativistic fluid theories. *Physical Review D* 31, 725.
- Hunter, J., J. Milton, P. Thomas, and J. Cowan (1998). Resonance effect for neural spike time reliability. *JNP* 80(3), 1427–38.
- Jou, D., J. Casas-Vázquez, and G. Lebon (2001). *Extended Irreversible Thermodynamics* (3rd ed.). Berlin: Springer-Verlag.
- Knight, B. (1972). Dynamics of encoding in a population of neurons. *JGP* 59, 734–766.
- Kobayashi, S. and K. Nomizu (1963). *Foundations of Differential Geometry*. Number 15 in Interscience Tracts in Pure and Applied Mathematics. New York: John Wiley & Sons. Volume 1.
- Kolmogorov, A. and S. Fomin (1970). *Introductory Real Analysis*. New York: Dover Publishing, Inc. Trans. R. Silverman. A 1975 re-print of the edition originally published in 1970 by Prentice-Hall, Inc.
- Kostädt, K. and M. Liu (2000). On the causality and stability of the relativistic diffusion equation. *Physical Review D* 62(023003). Also available as *arXiv:cond-mat/0010276v1* (<http://xxx.lanl.gov/abs/cond-mat/0010276>).
- Landau, L. and E. Lifschitz (1975). *Fluid Mechanics* (Second ed.), Volume 6. Oxford: Pergamon Press. An expanded, revised edition of the original 1959 edition. Translated from the Russian by J. Sykes and W. Reid.

- Leray, J. (1934). Essais sur la mouvement d'un liquide vis qu'enx emplissant l'espace. *Acta Mathematica* 63, 193–248.
- Müller, I. and T. Ruggeri (1993a). *Extended Thermodynamics*. Berlin: Springer-Verlag.
- Müller, I. and T. Ruggeri (1993b). *Rational Extended Thermodynamics* (2nd ed.). Berlin: Springer-Verlag. This is the second edition of *Extended Thermodynamics* by the same authors.
- Ruelle, D. (1981). Differential dynamical systems and the problem of turbulence. *Bulletin of the American Mathematical Society* 5, 29–42.
- Sommerfeld, A. (1964). *Partial Differential Equations in Physics*, Volume VI of *Lectures on Theoretical Physics*. New York: Academic Press. Trans. E. Straus.
- Spivak, M. (1979). *A Comprehensive Introduction to Differential Geometry* (second ed.), Volume 1. Houston: Publish or Perish Press. First edition published in 1970.
- Steenrod, N. (1951). *The Topology of Fibre Bundles*. Number 14 in Princeton Mathematical Series. Princeton, NJ: Princeton University Press.
- Stein, H. (1972). On the conceptual structure of quantum mechanics. In R. Colodny (Ed.), *Paradigms and Paradoxes: The Philosophical Challenge of the Quantum Domain*, Volume 5 of *University of Pittsburgh Series in the Philosophy of Science*, pp. 367–438. Pittsburgh, PA: University of Pittsburgh Press.
- Stein, H. (1994). Some reflections on the structure of our knowledge in physics. In D. Prawitz, B. Skyrms, and D. Westerståhl (Eds.), *Logic, Methodology 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.
- Stein, H. (2004). The enterprise of understanding and the enterprise of knowledge—for Isaac Levi's seventieth birthday. *Synthese* 140, 135–176. 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 65 pages long, and the published version about 41. Multiplying the page numbers I give by $\frac{41}{65}$ and adding the result to 135 (the number of the first page in the published version) should give approximately the page number in the published version.
- Stein, H. (unpub.). How does physics bear upon metaphysics; and why did Plato hold that philosophy cannot be written down? Typed manuscript not submitted for publication.
- Stimson, H. (1949, March). The international temperature scale of 1948. Technical Report RP 1926(209), The Journal of Research of the National Bureau of Standards.

- Stimson, H. (1961, September 8). The international temperature scale of 1948, text revision of 1960. Technical Report 37, National Bureau of Standards Monograph.
- Synge, J. (1957). *The Relativistic Gas*. Amsterdam: North-Holland Publishing Co.
- Temam, R. (1983). *Navier-Stokes Equations and Non-Linear Analysis*. Philadelphia: Society for Industrial and Applied Mathematics.
- Trautman, A. (1962). Conservation laws in general relativity. In L. Witten (Ed.), *Gravitation: An Introduction to Current Research*, pp. 169–198. New York: Wiley & Sons Press.
- Trautman, A. (1970a). Fibre bundles associated with space-time. *Reports on Mathematical Physics* 1, 29–62.
- Trautman, A. (1970b). Invariance of Lagrangian systems. In L. O’Raifeartaigh (Ed.), *General Relativity*, Chapter 5, pp. 85–99. Oxford: Clarendon Press.
- Trautman, A. (1980). Fiber bundles, gauge fields, and gravitation. In A. Held (Ed.), *General Relativity and Gravitation*, Volume 1, Chapter 9, pp. 287–308. New York: Plenum Press. 2 Volumes.
- Wald, R. (1984). *General Relativity*. Chicago: University of Chicago Press.
- Wiesenfeld, K. and F. Moss (1995). Stochastic resonance and the benefits of noise: From ice ages to crayfish and SQUIDS. *Nature* 373, 33–36.
- Wloka, J. (1987). *Partial Differential Equations*. Cambridge: Cambridge University Press. Originally published in German as *Partielle Differentialgleichungen*, Stuttgart: B. G. Teubner, 1982. Trans. C. B. and M. J. Thomas.
- Yang, C. (1961). *Elementary Particles: A Short History of Some Discoveries in Atomic Physics*. Princeton, NJ: Princeton University Press.
- Zimdahl, W., D. Pavón, and R. Maartens (1996). Reheating and causal thermodynamics. *arXiv:astro-ph/9611147v1* (<http://xxx.lanl.gov/abs/astro-ph/9611147>).