“Three Papers on How Physics Bears on Philosophy, and How Philosophy Bears on Physics”
A *Précis* of the Doctoral Dissertation

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To make a beginning
    out of particulars
To roll up the sum by defective means. . .

Rigor of beauty is the quest
But how will you find it when it is locked away in the mind
    beyond all remonstrance?

William Carlos Williams
Paterson

> Ahhh, it’s so hard, ya’ know, it’s so hard to believe in anything anymore, ya’ know what I mean? It’s like religion, you can’t really take it seriously, because it seems so mythological and it seems so arbitrary, and then on the other hand, science is just pure empiricism, and by virtue of its method it excludes metaphysics. And I guess I wouldn’t believe in anything if it weren’t for my Lucky Astrology Mood-Watch.
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> Steve Martin, “A Wild and Crazy Guy”

Contents

1 The Spirit of the Thing

The dissertation consists of three separate papers. The three could not be more different from each other, as determined using a legion of different measures. The most important is this: the earliest one, on physicalist interpretations of causality, though I stand by the substance of its claims, is jejune in style and presentation; the later two, on, respectively, singular structure in relativistic spacetimes and the regime of applicability of theories of mathematical physics, are relatively more mature. If I had had the opportunity, I would have liked to have substituted another paper for the first, or to have rewritten it; but time did not allow this.

The three do possess two related features of great importance, gestured at by the title of the dissertation. On the one hand, all three could be said to manifest a spirit of mitigated skepticism with regard to the interest and indeed the coherence of many questions dealt with traditionally by philosophers, both in their framing and in their content. On the other hand, they also all manifest a spirit that may be thought at first contrary to the first, though it is not, one of guarded optimism about the possibility for the advancement of real understanding and comprehension of the issues these traditionally philosophical questions have purported to address, albeit an optimism qualified by a demand intimated by the title of the dissertation: that many questions traditionally dealt with by philosophers require knowledge both detailed and comprehensive of our best physical theories, and that of both their formal and empirical content, if one is to make any substantive progress on them.

The time is long since past when anyone, philosopher or physicist, can hope to address in any serious way questions deeply rooted in the physical states of affairs of the world by sequestering himself in his study and requiring of the physical world that it conform, in nature and substance, to the limitations of his imagination and his "powers" of pure reasoning.[1] This is perhaps recognized by most professional philosophers practicing today, and indeed many workers in many fields at least pay lip-service to the idea that, in investigating certain issues, account must be taken of what physics has to say on the matter, even if only to dismiss it in the end as not relevant (which I will be the first to admit is frequently). I applaud this trend—or perhaps merely what I hope is a trend. We'll see.

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[1] I cannot resist quoting one of Stephen Crane's poems here:

> A man said to the universe:
> "Sir, I exist!"
> "However," replied the universe,
> "The fact has not created in me
> A sense of obligation."

In any event, I have qualms about how these good intentions are often translated into practice. Many times, when a physical theory or principle is introduced in the context of a philosophical discussion, even when the introductor earnestly and attentively tries to do it justice, the treatment of the theory or principle degenerates into the manipulation of simplistic, purely formal toy models, without any serious attempt to understand the implications of the deeper formal structures of the theory, and usually with no attempt at all to consider how the theory finds its actual application in experimental practice. Too often, the lessons we want to draw from superficial treatment of physical theories founder on one of these two rocks (or on both of them at once, when the ship is big enough). Close examination of the formal structure of a physical theory often leads one to conclude that notions and ideas seemingly so fundamental and natural as to be impervious to empirical falsification are, in fact, not only not fundamental and natural, but are not even coherently formulable in the terms of the theory. Examples abound, such as special relativity's dismissal of an absolute temporal structure, and general relativity's refusal to countenance anything like the classical principle of the conservation of energy. While this lesson has been learnt to some degree by some philosophers, I think the great majority of them, even among those who try scrupulously to address physical theory in their investigations, do not give the second factor—consideration of the empirical applicability and standing of the theory—a second thought. This practice is common not only among philosophers. It is common among mathematical physicists as well.[2]

When one considers the fundamentals in the schooling our intuitions have received in our contemplation of well worked out examples of physical theories, which by and large tend to include mathematical structures that strike us as 'simple' and 'natural', as applied to the modeling of physical systems whose sole virtue is their analytical tractability, this ought not escape our notice: most such examples of physical theories brandished by philosophers are demonstrably false (Newtonian mechanics and classical Maxwell theory) or have at the moment insuperable problems of interpretation (quantum mechanics) or experimental accessibility (general relativity). We should beware of relying too much on intutions trained in such schools—especially when one also recalls how much of our contemplation and employment of those theories involves models of systems with physically unrealistic perfect symmetries and vaguely jusified approximations, simplifications and idealizations. My earnest attempt in all three of these papers has been to leaven the loaf somewhat.

These three papers also share, if one likes, a spirit of revolt against a naive and virulent form of neo-Aristotelianism persisting still among many philosophers of science and among many physicists today, exemplifed by the idea that there is such a thing as '*the* causal relation', or '*the* proper definition' of a singularity in general relativity, or '*the* relations' that theoretical and experimental knowledge have to each other in the practice of contemporary physics, and that, if physical theories are nagged and worried enough, they will yield up the οὐσία of the thing, without the investigator's ever having paused to consider whether what she asks of the theory is reasonable or even coherent,

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[2] I presented several years ago, at one of the biennial meetings of the Philosophy of Science Association, a paper entitled "Against the Excesses of Quantum Gravity: A Plea for Modesty", inveighing solely against mathematical physicists for perpetuating these same bad habits. (In the concluding remarks, I also upbraided philosophers for letting them get away with it uncritically, but that was not the thrust of the paper.)

when framed in its terms. This is like trying to define *the* way to cook sweet potatos at Thanksgiving, without even pausing to consider whether one is making the dinner for a bunch of yankees or for a bunch of confederates.

I contend rather that, when one plays with physical theory to see what it may or may not have to say on the issues one is interested in, one ought to do it with something like the tentative but eager stance of negotiation assumed by a group of children trying to work out the rules of hide-and-seek to apply to the game about to be played, freely adapting the general principles 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). If a kid approached the game in its progress, demanded to join, and further demanded that all must play by *the* rules of hide-and-seek, as he lays them down, without his having considered whether they are appropriate for the context, we all know what would (or should) happen to him. I regret that each physical theory cannot make these ungracious children, the ones who bully it and worry it and take no care to attend to its own peculiar character, eat dirt.

2 The Constraints General Relativity Places on Physicalist Accounts of Causality

The attempt in the 1960's to provide a physically based, causal theory of reference, grounded more or less on the idea of energy propagation and energy transference among physical systems, that would be suitable for use in analyzing fundamental physical theories, provides an excellent case-study of these issues. The founding ideas of the accounts—energy and its propagation—turn out not to be coherently definable in one fundamental theory (general relativity) and, in the fundamental theory in which they are more or less definable (quantum mechanics), turn out, by virtue of the sorts of idealizations and approximations we must exercise in order to employ these notions in actual, experimental proceedings, not to have the characteristics those theories of reference require of them, most notably lacking those of continuity, locality and identifiability. The first paper in the dissertation addresses these issues, examining the way that general relativity precludes us from formulating a rigorous, fundamental notion of energy, and of, indeed, any classically conserved dynamical quantity, in its terms, and so stands a hindrance in the attempt to construct accounts of causality based on the ideas of energetic quantities and phenomena.

By the claim that energy is not a fundamental quantity in general relativity, I mean that within the mathematical structure of the theory one cannot rigorously define a quantity that has any of the features one might take to be definitive of energy in classical physics. I should emphasize that I am not claiming that one cannot talk about energy at all within the theory. One can speak of it, but only in certain physically special situations, in which can one represent within the theory a quantity that is structurally similar to energy as it is manifested in classical physics and special relativity, and even then only by employing explicitly approximative and idealizing techniques that are not part of the theory *per se*. Consequently talk about energy reflects nothing fundamental about the theory itself.

I must emphasize that none of the arguments in the paper pretend to bear on accounts of causality that are not beholden in some way to fundamental physical theory. Some accounts of causality purport to treat only relations among middle-sized dry goods in everyday practical affairs; others take causality to be something akin to a logical category of thought that structures our knowledge of various matters; yet others take 'causality' merely to indicate that a special type of explanation is required or is in the offing; and yet others take it as a merely subjective, psychological phenomenon, the manifestation of a brute fact about the way we are constructed to view the world. The arguments of this dissertation do not pretend to bear on any such accounts. Certain sorts of accounts of causality, perhaps best exemplified by Russell in *The Analysis of Matter*, rest on the idea that causality is a *physical* relation holding among *physical* entities, and as such must accord with best going physical theory. It is only such accounts that concern me in the paper. I conclude that, in so far as such accounts pretend to be founded on fundamental physical theories and, moreover, pretend to characterize *the* idea of causality, they cannot do so in the terms they are usually formulated in, those of the propagation and transference of conserved, dynamical quantities.

3 The Analysis of Singular Spacetimes

In the second paper of the dissertation, the two issues I raised in the introduction to this summary—the need for both a formal and a practical sense of a physical theory one works with, if one is not to be led astray—are this time found to distribute themselves in problematic ways almost equally among both philosophers and physicists. The issue is that of *the* proper definition of 'spacetime singularity' in general relativity, and the question of whether the prediction of such entities (however exactly one characterizes them) leads to a crisis for the theory. The arguments I present are mostly long and mostly technical, not lending themselves easily to a brief, non-technical synopsis. I will remark here only that I draw two main conclusions in the paper. First, how one characterizes such entities is not something that can be settled in the abstract, once and for all, but will depend inextricably on such factors as the purposes and requirements of the investigation at hand, the aesthetic predilections of the investigator, and other such pragmatic considerations. Second, most, if not all, of the arguments claiming that much of the structure often referred to, in the context of general relativity, as 'singular' points to a deep, even a pathological, inadequacy in the structure of the theory itself turn, in the end, on nothing more than just these sorts of aesthetic and pragmatic considerations—what Bob Geroch evocatively calls 'psychology'—and that those who make such arguments, rather than addressing the possibility that the theory is trying to tell us something about novel phenomena that may manifest themselves in our universe and about how we perhaps could go about trying to observe them, rather demand that the theory must be bad, because they cannot conceive of the universe manifesting such phenomena.

4 On the Formal Consistency of Experiment and Theory in Physics

The third paper is the most difficult to summarize, as well as the most difficult to digest. In it, I 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. 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 modeling any particular experimental or observational arrangement, 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 deduce from the exact, rigorous structure of that theory, as provided by the theoretician, models of actual experiments so that he may judge whether or not the predictions of those models conform to the inaccurately determined data he gathers from those experiments. It is one of the games 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 under what conditions 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 given conditions. Jointly, the two try to find, in the physical world and in the realm of mathematics, 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 bedrock of science, as it were—it behooves us, at the least, to get clearer on what is being decided and articulated.

I do not examine the actual play of the theoretician and the experimentalist 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 do 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 the playground shared by the theoretician and the experimentalist, that having to do with the comparison of predicted and observed values of a system as it dynamically evolves. 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).

I examine in the 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 æternitatis*, 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 investigate only whether one can construct such a structure to represent some idealized form of these relations. I am not, in the 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 the joint practice of the theoretician and the experimentalist (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. 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, 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. 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 defense of its feasibility, I offer the paper.

Without a doubt, one can learn an extraordinary amount about a physical theory 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 argue, however, 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 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 is 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*, that of modeling and comprehending the physical world, in complementary, indeed mutually supportive, ways. The answer I propose is constructive—a proposal for a more or less formal, explicit method of representing the connection between the stocks in trade of the two that remains true to the character of these two stocks. 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 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 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.

I focus the discussion around the idea of the *regime of applicability* of a physical theory. 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, 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 not directly treated by the theory but whose values in a given neighborhood are relevant to the issue of the theory's applicability to 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 expressions may include as well terms from the set of variables representing relevant environmental quantities

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

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 ±1%, with a degree of confidence of 95%

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 do not pretend to offer in the paper a definitive analysis of the concept of a regime or indeed of any of its constituents. I 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 contructive proof of the formal consistency of the practice of the experimentalist and the practice of the theoretician in physics, at least in so far as one accepts the viability of the sort of formal model I construct, 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.

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

The entire dissertation, if you will, may be considered an exercise in approximation and idealization in the philosophy of physics.