Philosophical Papers

PAUL HUMPHREYS

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To Rosemary, Norman, and Mark—sister, brother-in-law,
and brother extraordinaires

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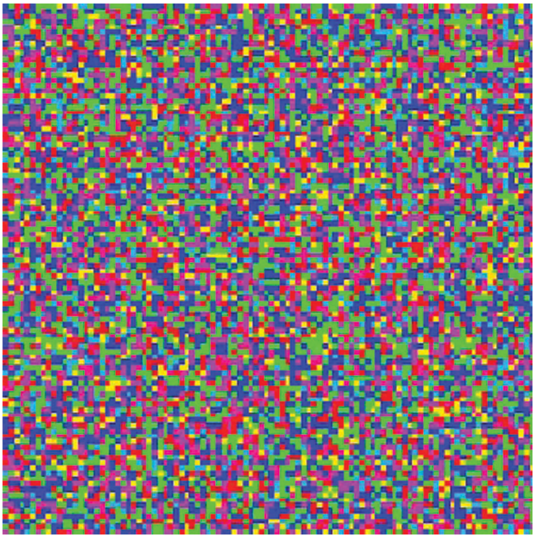

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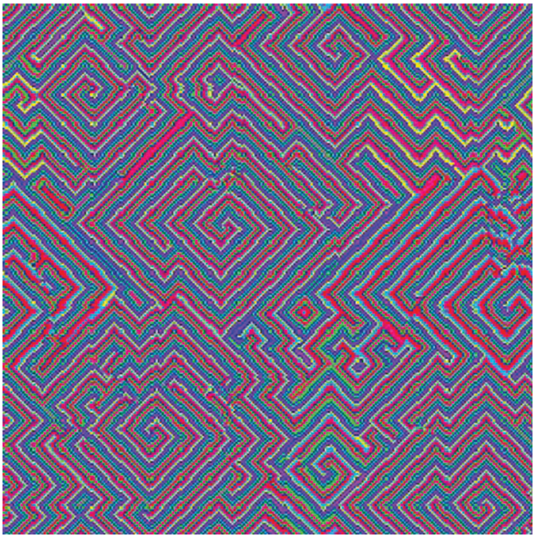

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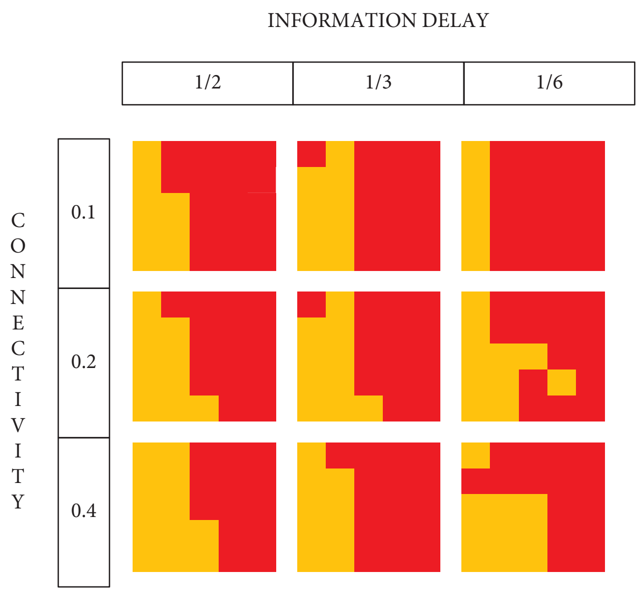

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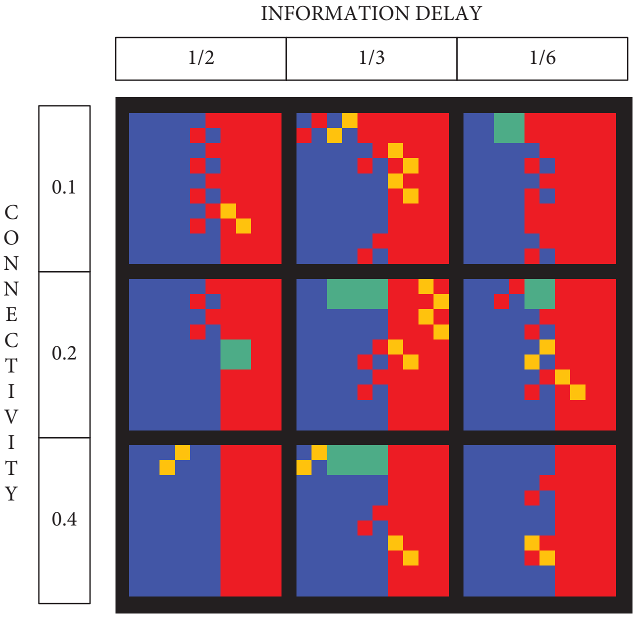

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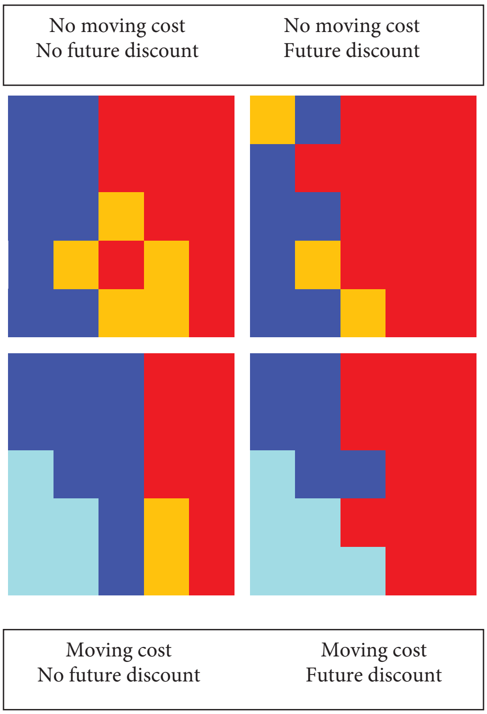

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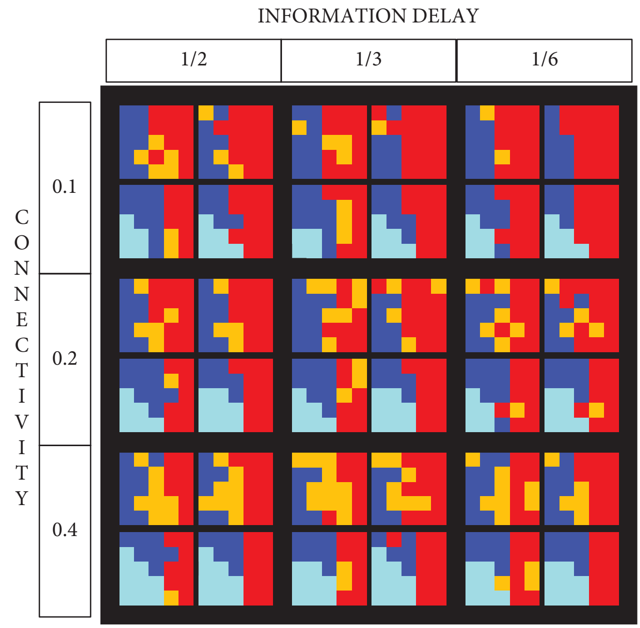

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Introduction

During a summer break in college, I set out to solo hike the Pennine Way, a 267-mile footpath snaking across the moors of northern England. One day, my first goal was Stoodley Pike, a hill topped by a 121-foot stone folly. That morning, the Pike was covered in a dense fog, with visibility down to about ten yards. I lost the trail, and for about an hour, I had no idea where I was or whether I was hiking toward or away from the monument. I seemed destined to spend the rest of my holiday on the Pike, but suddenly the mist parted and right in front of me was the folly. When doing philosophy I am often reminded of that morning.

Locating oneself geographically and philosophically involves local and nonlocal information. A benefit of rereading one's earlier work is the discovery of overarching themes that are not evident when individual articles are written. The fifteen papers in this collection are grouped in four general categories: the philosophy of computational science, emergence, the philosophy of probability, and scientific metaphysics and epistemology. On the surface, these themes appear to have little in common and indeed, to an extent, forcing connections between them would be unnatural. But all of these papers are motivated by positions in general philosophy of science, nailed to a specific area of scientific activity. Those positions are a scientific realism; the failure of empiricism as an adequate scientific epistemology and the concomitant need to relinquish an anthropocentric view of epistemology; the need to incorporate scientific and mathematical results into one's metaphysical conclusions; philosophy's susceptibility to being misled by ordinary language; and the exaggerations in claims to the effect that we are trapped within a linguistic framework.

These positions are intimately connected with very general questions such as the relation between mathematics and science, what the different modes of understanding in science are, and why scientific methods are epistemically effective. These are all questions in general philosophy of science, an area that has become more difficult to pursue in recent decades because of increasing specialization and the entrenchment of subdisciplines that have their internal research agendas. But it is also better informed than it once was.

The choice of articles was dictated in some cases by citation levels, in others by what seems in retrospect to be novelty of ideas, and in still other cases by personal preference.

The articles are reprinted as they originally appeared except for omitting the occasional encomium, filling in incomplete reference information, making spelling and other points of style consistent, and correcting typographical errors. Each section in this volume is followed by an extended postscript. Rather than construct a separate postscript for each paper, it turned out to be more informative to combine the comments on the material with an eye to capturing the progress that has been made since their original publication.

I.1. Method

An approach to the philosophy of science that is too focused on case studies is philosophically uninformative. It leads to a kind of deforestation problem, with the philosophical trees mostly dead. Yet when we turn to investigations of how science is applied, highly abstract approaches are rarely instructive, omitting as they do most, if not all, of the intricate specifics that are required to bring scientific models into contact with data.[1] Balancing these considerations is not easy and I doubt if anything both true and general can be said about where the right balance lies. When the tension is cast in terms of an "in principle" versus "in practice" choice, we can say something informative. One of the themes running through Part I is the need for philosophy of science to explicitly incorporate the role of technology into its considerations of scientific method, especially in the area of theory and model application. This attention to the details of how science is applied requires a concession from the "in principle" approach that philosophy has often taken. There is a legitimate place for in-principle results when what is at stake is what cannot be known in principle, because what cannot be known in principle cannot be known in practice. But different considerations enter when we are interested in positive results.

Demonstrating that something can be known in principle legitimizes research in that area. But such a claim has a status similar to existence results in mathematics where it is known, for example, that a solution to a particular type of differential equation with generic boundary or initial conditions exists

[1] Suppes 1962 is an exception to the above claim, being sensitive to the complexities of bringing together abstract theory and data, although his central example is highly stylized.

but we have no idea what the specific form of that solution looks like. This is undoubtedly interesting but it is only a starting point for further investigation. Within philosophy, however, in-principle claims can spiral off into a nether region in which claims are made that God could have the in-principle knowledge, claims that are epistemologically dubious whatever one's deistic beliefs. Hypothetical omniscient beings would not become scientists; scientific methods would instead be exhibits in a Museum of Epistemic Inefficiency.

An investigation into the appropriate use of an in-principle epistemology would require, for a full treatment, a systematic investigation into what counts as a philosophical idealization. There is now a considerable body of literature in the philosophy of science on the criteria for what counts as a legitimate scientific idealization, but there is very little in the philosophical literature on what sorts of idealizations are appropriate in philosophy. Philosophy of science requires different kinds of idealization than do many other areas of philosophy. Unlike traditional epistemology, extreme skeptical objections about the reliability of evidence are off limits within the philosophy of science. No serious scientific activity can be expected to take place under conditions within which it is supposed that an evil demon could be systematically misleading us.[2] On the other hand, although there are parts of contemporary mathematical economics that are remote from economic reality, investigations into the behavior of ideally rational agents can be useful when the idealizations are a reasonable approximation to actual agents.

This tension between highly idealized scenarios and fully detailed actual examples raises questions about what should be the appropriate training for philosophers of science. Even if one acknowledges that investigating epistemological and ontological questions requires bringing to bear evidence from factual investigations, this is not by itself a reason to teach more concrete material in philosophy courses and seminars, if only because there is a strong relationship between the capacity for abstract thinking and the ability to do good philosophy of any kind.

I.2. Synopsis

Chapter 1 was originally presented at a Philosophy of Science Association symposium in 1990. It is, as far as I am aware, the first English language discussion of computer simulation methods in science by a philosopher, as contrasted with discussions about artificial intelligence. Although there is now a very large literature on the subject, the article still has some interest

[2] A brain in a vat could do science, even solipsistic science, but it is usually assumed in such scenarios that the brain's evidential inputs mimic those of standard science.

for its argument that scientific progress in many areas is tightly connected to progress in the application of tractable mathematics. This is one area in which detailed descriptive studies can be useful. Chapter 2 is a treatment of some of the broader intellectual consequences of computer simulations. It addresses some different senses in which the invention of computer simulations might be considered to count as a scientific revolution and discusses some ways in which science and technology interact. Chapter 3 was written as a response to claims that there is nothing philosophically new that is presented by the advent of simulation methods in science. In focusing on the principal methodological issues involved, this article is a useful corrective to some common misunderstandings about the differences between models and simulations in science. It also offers a pair of definitions for epistemic opacity, a concept that is central to many areas of computational science. Chapter 4 is the most technical of this group. Somewhat to my surprise, it has been cited many times. One reason perhaps is that it plays into the debates about the relation between simulations and experiments but in a somewhat perverse way—despite an explicit claim in the article that numerical experiments are different in important ways from material experiments, it has been cited as supporting the view that simulations are like material experiments. A final theme is that applied mathematics is not just pure mathematics applied, and that an improved understanding of how science is applied can be gained through examining these methods rather than through the philosophy of language.

Chapter 5 is the most widely cited paper in the collection. This article, which presents a way to escape the exclusion argument, became part of a longer-term program arguing the merits of diachronic approaches to emergence and exploring the possibilities of a nonatomistic ontology. The results of that program can be found in my *Emergence: A Philosophical Account* (Oxford University Press, 2016). Chapter 6, despite what I now think of as an excessively polemical tone, appears to have resonated with those who find the ambitions of metaphysical dependency relations to far exceed their usefulness. It also contains a set of criteria for what counts as emergent. Chapter 7 is an extended discussion of Mark Bedau's concept of weak emergence. It also contains an argument that this approach is, in a nontrivial way, essentially historical and cannot be accommodated within synchronic approaches to emergence. Chapter 8, which overlaps to some extent with chapter 7, lays out a taxonomy of types of emergence. It also contains the beginning of later investigations into conceptual emergence. There are other taxonomies, each with its own virtues but I have repeatedly found the one described in chapter 8 to be useful. The sixfold nature of the taxonomy reminds us of a moral that cannot be repeated often enough: there are multiple different types of emergence and no single account covers them all.

Chapters 9 and 10 form a pair centered around a result that has come to be known as "Humphreys' Paradox." The first article presents the paradox and explores a number of possible responses to it, while the second article demonstrates that the argument leads to a formal rather than an informal paradox, discusses in detail some published solutions to the paradox, and shows that none of them are successful. Although most readers have taken the arguments as a challenge to find a way out of the paradox, the principal point of the original article was to explore whether there are examples of propensities that are not correctly represented by standard probability theory and that propensities require their own theory. This theme is revisited in chapter 11, which appeared in a festschrift for a well-known statistician, and as such is probably unknown to most philosophers. The main thrust of this article is that, Quinean arguments notwithstanding, probability theory is a purely formal mathematical theory with empirical content injected via specific models that are separate from the theory.

Chapter 12 is included, although it is by now quite old, in part because it is an example of how an insight rather than pages of detailed argument can provide a solution to a philosophical problem. More important is that the position outlined here serves as a sharp contrast to those who hold that probabilistic explanation consists in the citation of probabilities and that there are no explanatory differences between contributing and counteracting causes. I have not published on explanation for many years, because I have come to believe that the fundamental philosophical concept is understanding rather than explanation. The former concept is far too grand for me to address here but chapter 13 provides a classification of explanatory schemes tuned to different modes of understanding. It also formulates a still open problem about causation regarding the point at which a causal factor can justifiably be said to have been identified. Chapter 14 is the most recent essay in the collection. It provides what I hope is a reasonably temperate and impersonal set of arguments against one way of pursuing metaphysics. It also makes a case for a certain kind of metaphysics as having a place in the philosophy of science, a view that runs counter to an antimetaphysical tradition in the area. A third theme fits with the quest to reconcile a scientifically informed realism with an equally scientifically informed empiricism. Chapter 15 is the only previously unpublished article in the collection. It describes the results from an agent-based model that uses a deformable, rather than a rigid, fitness landscape to explore how constrained maximization outperforms traditional unconstrained maximization in identifiable regions of endogenous uncertainty. One result among many is that the tragedy of the commons becomes just a special case of a much broader scenario to which specifiable solutions can be provided. I have included this paper because it shows how powerful but conceptually transparent simulations can illuminate issues in social philosophy and the philosophy of economics. More specifically, that when the assumptions underlying utility maximization fail, constraints on individualism can be beneficial. My *Extending Ourselves* book unfortunately gave short shrift to agent-based models and this article illustrates some novel features of such simulations.

Over the years, I have had the great good fortune of working with some remarkable people. There are too many to mention them all but three were of particular importance. Patrick Suppes, Wesley Salmon, and David Freedman, in very different ways, exemplified how deep ideas can be conveyed to the reader with great clarity. The essays reprinted here do not come close to their standards, but they would have been far worse without their example.

{ PART I }

Computational Science

{1}

Computer Simulations

1.1. Introduction

A great deal of attention has been paid by philosophers to the use of computers in the modeling of human cognitive capacities and in the construction of intelligent artifacts. This emphasis has tended to obscure the fact that most of the high-level computing power in science is deployed in what appears to be a much less exciting activity: solving equations. This apparently mundane set of applications reflects the historical origins of modern computing, in the sense that most of the early computers in Britain and the United States were devices built to numerically attack mathematical problems that were hard, if not impossible, to solve nonnumerically, especially in the areas of ballistics and fluid dynamics. The latter area was especially important for the development of atomic weapons at Los Alamos, and it is still true that a large portion of the supercomputing capacity of the United States is concentrated at weapons development laboratories such as Los Alamos and Lawrence Livermore.

Computer simulations now play a central role in the development of many physical sciences. In astronomy, in physics, in quantum chemistry, in meteorology, in geophysics, in oceanography, in crash analysis of automobiles, in the design of computer chips, in the planning of the next generation of supercomputers, in the discovery of synthetic pharmaceutical drugs, and in many other areas, simulations have become a standard part of scientific practice. My aim in the present paper is simply to provide a general picture of what computer simulations are, to explain why they have become an essential part of contemporary scientific methodology, and to argue that their use requires a new conception of the relation between theoretical models and their applications.[1]

[1] When examining this activity, we must be wary of one thing, which is that the field of computer simulation methods is relatively new and as such is rapidly evolving. Techniques that are widely used now may well be of minor interest twenty years hence, as developments in computer architecture,

Why should philosophers of science be interested in this new tool? Mostly, I think, because the way that simulations are developed and implemented forces us to re-examine a lot of what we tend to take as the right way to characterize parts of mathematically oriented methodology and theorizing. Where this re-examination takes us will become clear as we go along, but before I discuss computer simulations specifically, I want to make some general points about the role of mathematical models in physical science. Let's begin with a claim that ought to be uncontroversial, but is not given enough emphasis in philosophy of science. The claim is: *One of the primary features that drives scientific progress is the development of tractable mathematics*. Whenever you have a sudden increase in usable mathematics, there will be a concomitant sudden increase in scientific progress in the area affected. This should not really need to be pointed out, but so much emphasis is placed on conceptual changes in science that powerful instrumental changes tend to be downplayed. This kind of sudden increase in mathematical power happened with the invention of the differential and integral calculus in the middle of the seventeenth century; it happened with the sudden explosion of statistical methods at the end of the nineteenth century, and I claim that the ability to implement numerical methods on computers is, in the late twentieth century, as significant a development as those earlier inventions. But what kind of development is it? Has it introduced a distinctively different kind of method into science, as Rohrlich (1991), for example, claims, or is it simply a technologically enhanced extension of methods that have long existed? If computer simulation methods are simply numerical methods, but greatly broadened in scope by fast digital computation devices with large memory capacity, then the second "just much more of the same" view would be correct, and the situation would be similar to that in mathematics, where the introduction of computer-assisted proofs, such as were used to execute the massive combinatorial drudgery involved in the proof of the four-color theorem, is often regarded as not having changed the fundamental conception of what counts as a proof. My own view is that the situation is more complex than this simple dichotomy represents, because the introduction of computer simulation methods is not a single innovation but a multifaceted development. Let's begin with a couple of simple examples to show why mathematical intractability is an important constraint on scientific models.

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numerical methods, and software routines take place. The specific details of different kinds of simulation methods, such as finite-difference methods and Monte Carlo methods will be explored in a future paper, and some examples of currently used simulations are given in the following paper by Rohrlich (Rohrlich 1991).

1.2. Practical and Theoretical Unsolvability of Models

Take what is arguably the most famous law of all, Newton's Second Law. This can be stated in a variety of ways, but its standard characterization is that of a second-order ordinary differential equation:

$$\mathbf{F} = m d^2 y / dt^2 \tag{1}$$

To employ this we need to specify a particular force function. In the first instance, take

$$\mathbf{F} = GMm / R^2 \tag{2}$$

as the gravitational force acting on a body near the earth's surface (M is the mass of the earth, R its radius). Then

$$GMm / R^2 = m d^2 y / dt^2 \tag{3}$$

is easily solved. But the idealizations that underlie this simple mathematical model make it hopelessly unrealistic. So let's make it a little more realistic by representing the gravitational force as $GMm / (R + y)^2$, where y is the distance of the body from the earth's surface, and by introducing a velocity-dependent drag force due to air resistance. We obtain

$$GMm / (R + y)^2 - c\rho s (dy / dt)^2 = m d^2 y / dt^2 \tag{4}$$

Suppose we want to make a prediction of the position of this body at a given time, supposing zero initial velocity and initial position $y = y_0$. To get that prediction you have to solve (4). But (4) has no known analytic solution—the move from (3) to (4) has converted a second-order, linear, homogeneous ODE into a second-order, nonlinear, homogeneous ODE, and the move from linearity to nonlinearity turns simple mathematics into intractable mathematics. Exactly similar problems arise in quantum mechanics from the use of Schrödinger's equation, where different specifications for the Hamiltonian in the schema $\mathbf{H}\Psi = E\Psi$ lead to wide variations in the degree of solvability of the equation. For example, the calculations needed to make quantum mechanical, rather than classical, predictions in chemistry about even very simple reactions, such as the formation of hydrogen molecules when spin and vibration variables are included, are extremely difficult and have only recently been carried out. (An explicit discussion of the differences between ab initio and semiempirical methods in quantum chemistry is given below.)

You might say that this feature of unsolvability is a merely practical matter, and that as philosophers we should be concerned with what is possible in principle, not with what can be done in practice. But recent investigations into decision problems for differential equations have demonstrated that for many algebraic differential equations (ADEs) (i.e., those of the form

$$P\left(x, y_1, \ldots, y_n, y_1^{(1)}, \ldots, y_m^{(1)}, \ldots, y_1^{(n)}, \ldots, y_m^{(n)}\right) = 0$$

where P is a polynomial in all its variables with rational coefficients) it is undecidable whether they have solutions. For example, Jaśkowski (1954) showed that there is no algorithm for determining whether a system of ADEs in several dependent variables has a solution in [0,1]. Denef and Lipshitz (1984) show that it is undecidable whether there exist analytic solutions for such ADEs in several dependent variables around a local value of x. (Further results along these lines, with references, can be found in Denef and Lipshitz 1989). Obviously, we cannot take decidability as a necessary condition for a theory to count as scientifically useful, otherwise we would lose most of our useful fragments of mathematics, but these results do show that there are in-principle, as well as practical, restrictions on what we can know to be solvable in physical theories.[2]

There is a methodological point here that needs emphasis. While much of philosophy of science is concerned with what can be done in principle, for the issue of scientific progress what is important is what can be done in practice at any given stage of scientific development. That is, because scientific progress involves a temporally ordered sequence of stages, one of the things that influences that progress is that what is possible in practice at one stage was not possible in practice at an earlier stage. If one focuses on what is possible in principle (i.e., possible in principle according to some absolute standard, rather than relative to constraints that are themselves temporally dependent), this difference cannot be represented, because the possibility-in-principle exists at both stages of development. So although what is computable in principle is important for, say, the issue of whether computational theories of the mind are too limited a representation of mental processes, what is computable in practice is the principal feature of interest for the methodologies we are considering here.

This inability to obtain specific predictions from mathematical models is a very common phenomenon, because most nonlinear ODEs and almost all PDEs have no known analytic solution. In population biology, for example, consider the Lotka-Volterra equations (first formulated in 1925)

$$dx/dt = ax + bxy$$

$$dy/dt = cy + dxy$$

[2] A further source of difficulty, at least in classical mechanics, involves the imposition of nonholomorphic constraints (i.e. constraints on the motion that cannot be represented in the form $f(r_1, \ldots, r_n, t) = 0$ where $\{r_i\}$ are the spatial coordinates of the particles comprising the system). For a discussion of these constraints, see Goldstein (1980), pp.11–14.

where x = population of prey, y = population of predators, $a(>0)$ is the difference between natural birth and death rates for the prey, $b(<0)$, $d(>0)$ are constants related to chance encounters between prey and predator, $c(<0)$ gives the natural decline in predators when no prey are available. With initial conditions $x(0)=e$, $y(0)=f$, there is no known analytic solution to the equation set.

These examples could be multiplied indefinitely, but I hope the point is clear: clean, abstract, presentations of theoretical schemas disguise the fact that the vast majority of those schemas are practically inapplicable in any direct way to even quite simple physical systems. This is not the point that models are never applicable to real systems: the point here is that even with radical idealizations, the problem of intractability is often inescapable, i.e., in order to arrive at an analytically treatable model of the system, the idealizations required would often destroy the structural features that make the model a model of that system type. This problem is widespread, and cuts across both sciences and subfields of those sciences, although it is more prevalent in some fields than in others.

These problems put severe limits on the applicability in practice of the standard, syntactically formulated method of hypothetico-deductivism, for most of the equations that represent the fundamental or derived theories of physics, chemistry, and so on cannot be used in practice to make precise deductive predictions from those representations together with the appropriate initial or boundary conditions. I should say here that I want to remain neutral as far as possible about the relative merits of the syntactic and semantic (or structuralist) reconstructions of theories. Although the semantic approach has definite advantages, both accounts are logical reconstructions of scientific practice. Because we are concerned here to stay as close as possible to considerations that present immediate problems to actual scientific practice, the debate over the merits of these reconstructions has only an indirect relevance to our interests. It is worth noting, however, that the issue of practical unsolvability means that the formulation of a theoretical model in some specific mathematical representation, rather than as a set of metamathematical structures, is an inescapable concern, and that whereas the semantic approach generally considers different linguistic formulations as mere linguistic variants of an underlying common structure, linguistic reformulations frequently have a direct impact on the ease of solvability of a mathematical representation, and hence this level cannot be ignored completely. In particular, I want to urge that what is of primary interest here is the mathematical form of equation types and not their logical form. To be specific: one could reformulate (1), (3), and (4) in a standard logical language by using variable-binding operators, thus forcing it into the standard quantified conditional form that serves as the representation of laws in the traditional syntactic approaches, but to do this would be to distort what is crucial to issues of solvability, which is the original mathematical form.

It is this predominance of mathematically intractable models that is the primary reason why computational physics (and similar methods in other sciences), which provides a practical means of implementing nonanalytic methods, constitutes a significant and, I think, a permanent addition to the mathematical methodology of science.

1.3. Definitions of Computer Simulation

Here, taken more or less at random, are some suggestions that have been made for characterizing computer simulations:

(1) "Simulation is the technique by which understanding the behaviour of a physical system is obtained by making measurements or observations of the behaviour of a model representing that system" (Ord-Smith 1975, 3).
(2) "This is what simulation is all about, i.e., experimenting with models" (Ord-Smith 1975, 3).
(3) "A precise definition of simulation is difficult to obtain . . . the term simulation will be used to describe the process of formulating a suitable mathematical model of a system, the development of a computer program to solve the equations of the model and operation of the computer to determine values for system variables" (Bennet 1974, 2).
(4) "The mathematical/logical models which are not easily amenable to conventional analytic or numeric solutions form a subset of models generally known as simulation models. A given problem defined by a mathematical/logical model can have a feasible solution, satisfactory solution, optimum solution or no solution at all. Computer modelling and simulation studies are primarily directed towards finding satisfactory solutions to practical problems" (Neelamkavil 1987, 1).
(5) "Simulation is a tool that is used to study the behaviour of complex systems which are mathematically intractable" (Reddy 1987, 162).

Because of the variety of uses to which the term "simulation" has been put, I am reluctant to try to formulate a general definition. It would be more profitable at this stage to simply explore the methods that are used under categories (1), (2), and (3) above. We can, however, formulate a working definition based on the last definition, which needs to be modified in three ways. First, simulation is a set of techniques, rather than a single tool. As the other quotations indicate, it would be hard to make a case for the view that there is an underlying unity to the set, at least at the present