# CRITICAL NOTICE:
# JOHN EARMAN'S *A PRIMER ON DETERMINISM**

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> Your story is there waiting for you, it has been waiting for you there a hundred years, long before you were born and you cannot change a comma of it. Everything you do you have to do. You are the twig, and the water you float on swept you here. You are the leaf, and the breeze you were borne on blew you here. This is your story and you cannot escape it.
>
> —Cornell Woolrich, *I Married a Dead Man*

## I

Determinism as a physical thesis was articulated by fatalists long before the doctrine stood a chance of being clearly formulated, let alone proved—at least, by the lights of standard philosophy. Woolrichean sentiments were common centuries before Descartes and Newton put theory in a sufficiently definite form that the doctrine could be tested. Early faith in determinism rested instead upon analogies, for example, the presumption that the universe as a whole "works like" some sufficiently simple, obviously deterministic system, like the hydro- and aerodynamic events Woolrich mentions or, more commonly, collisions on a billiard table or the workings of machines like clocks or locomotives. It was taken for granted that these "simple" systems would prove deterministic in their own right.

John Earman's invigorating new book prompts the reflection that *none* of these traditional paragons of determinism indisputably fulfill the bill—that the mathematical frameworks normally employed to handle these systems allow breakdowns which seemingly permit undetermined behavior on the part of each of these favorite exemplars of fatalist visions. Since each of these cases falls within the province of classical physics, we seem led to Earman's tentative conclusion that classical mechanics does not,

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contrary to expectation, provide a "hospitable home for determinism".

Considered solely in its own right, the contention that classical mechanics is "really indeterministic" would be merely a novelty, albeit a startling one—like the discovery that Stegosaurus had multiple rows of armor. The real interest of any new finding in classical determinism must lie instead in the light it casts on other matters. Part of Earman's original motive in writing this book stems, I believe, from the nagging sense that the facts of modern classical physics do not straightforwardly accommodate themselves to the usual demands of popular philosophical accounts of law, theory and possibility. He correctly notes that

> determinism wins our unceasing admiration in forcing to the surface many of the more important and intriguing issues in the length and breadth of the philosophy of science. (p. 21)[1]

The suggestion that determinism might serve as a useful *probe* for uncovering largely unformulated, false presumptions in philosophy of science is one of the most intriguing and original aspects of this book. Nonetheless, as matters stand, this deeper aspect to the book may be missed by many of its readers, partially because of the way it is written. On the one hand, many of the most interesting examples are described in technical terms beyond the reach of the average philosophical reader—indeed, often only within the grasp of someone already well informed about the phenomenon in question. This technical compression is a great shame for one of the true beauties of this book is its rich appreciation that philosophy can learn much from the nitty-gritty of real-life science, matters generally passed over by the "Theory T" crowd. One of the objectives of this paper will be to give the reader a sense of what is involved in Earman's examples using complementary and, I hope, easier cases.

A second barrier to understanding lies in the particular architectonic, involving background space-time structure, in which Earman has enveloped his discussion. To me, this emphasis merely obscures the more salient points. In my discussion, therefore, I'll simply ignore this aspect of Earman's approach, save for a few brief remarks at the end of the paper.

This essay began life as a simple book review, but the likelihood that vital aspects of this very interesting book would be overlooked prompted me to a different treatment. Too many readers sifting through Earman's mounds of beautiful cases will respond with a simple "Here John Earman, through a lot of difficult examples, demonstrates that classical mechanics is not as deterministic as I presumed. An interesting curiosity, to be sure, but I never supposed that science in general had any vital connection with

[1]Page references are to *A Primer on Determinism* (1986), unless otherwise marked.

determinism." But, as I have already remarked, Earman's discussion pertains to more general issues in philosophy and it would be a shame if this fact was overlooked by readers who fancy themselves uninterested in determinism. As it happens, my diagnosis of the morals to be drawn is somewhat at variance with Earman's, but this disagreement, I hope, will only highlight the interest and difficulty of the issues. Indeed, I probably would have never considered such matters at all except for the impetus of *A Primer on Determinism*. So, on a reviewish note, I will simply state that this is the best book I have read in philosophy of science in a good number of years and merits philosophy's closest scrutiny. The book, moreover, contains intriguing discussions of a vast range of related issues I have left no room to discuss. Indeed, the topics I emphasize here represent a comparatively small part of the Earman whole, but they will perhaps prove most useful to the would-be reader, the best priming for approaching this "Primer".

I might also add that, as befits a book intended only as a survey of relevant concerns, Earman's own evaluation of the issues he raises is sometimes less than clear. Often the *attitude* is conveyed only by the *tone* and, since the latter is frequently ironic, the former may be as well. Hence the "John Earman" of this essay may entertain considerably less nuanced opinions than the real-life author. But I find, on the basis of an informal poll, that most readers will understand the argument in the way I suggest.

The organization of the present essay will be as follows: in section II, I will sketch a general picture of scientific theory that seems to lie behind most philosophical programs for investigating determinism, although it is invariably left largely tacit. In sections III to V, I will discuss several concrete examples from the point of view of section II. Since the conclusions reached in this fashion eventually prove unacceptable, sections VI and VII will backtrack to see what might have gone wrong in section II. The remainder of the essay will collect assorted loose ends connected with Earman's discussion.

## II

How, prima facie, might we expect the "determinism" of a theory to be settled? There seem to be certain widely held expectations about how the investigation should proceed, expectations that Earman seems to share. Unfortunately, the presumptions needed to render this program plausible are usually left unstated in literature. In this section, I'll try to fill in the rough outlines of what is needed.

In particular, it is commonly presumed that a theory $T$ is constructed by first delineating a firm set of basic parameters or "fundamental magnitudes" (for example, mass, charge, etc.) which $T$ plans to employ in the delineation of physical systems. *Laws* are then articulated to dictate how these parameters will influence the behavior of objects possessing them. Finally, in a third stage of *modeling,* concrete differential equations are fitted to hypothetical assemblages of objects "realizing" the basic laws. For example, in celestial mechanics we would combine Newton's Second Law, in the form "$F = ma$", with the Law of Universal Gravitation, to reach the standard formulas for $n$ point masses moving solely under mutual gravitational attraction:

$$m_i \cdot d^2\boldsymbol{x}_i/dt^2 = \sum_{i \neq j} G \cdot m_i \cdot m_j \cdot \boldsymbol{x}_i - \boldsymbol{x}_j/(|\boldsymbol{x}_i - \boldsymbol{x}_j|)^3$$

Each set of "modeling" equations admits in turn of a range of "solutions"—particular histories of systems conforming to the modeling. Thus, in the gravitational case the values of $n$ and $m_i$ which (approximately) model the solar system permit all sorts of solutions, most of which assign the planets quite otherworldly configurations and motions.

This setup suggests that the "possible systems" accepted by our theory $T$ should be relatively easy to delineate. It might be expected, for example, that a "$T$-possible physical situation" should correspond to *any* distribution of $T$'s parameter values to objects consistent with the prescribed laws, no matter how unlikely it would be for such systems to be realized in practice. Here it might be argued that one should always be able to assemble such a system by brute force, pushing objects with the right parameters into the right initial conditions.[2]

These apparently unexceptionable claims about the structure of theory naturally suggest a concrete format for deciding whether a theory is "deterministic" or not. In particular, the expectation is that "determinism" should turn out to be a species of *metatheorem* about the given theory's behavior. That is, $T$ will count as deterministic only if one can show as a provable, metamathematical fact about the laws of the theory that $T$ will not tolerate distinct physical possibilities which behave exactly alike up to a specified time to, but diverge in behavior thereafter. According to this "metatheoretical" stance, $T$ has no business *postulating* of itself that its potential systems evolve deterministically. The axioms of $T$ instead should restrict their attention to the articulation of substantive principles of system development—principles that delineate the "$T$-possibilities" crisply and cleanly. If these principles are sufficiently strong, they will assign $T$'s permissible initial histories unique continuations and thus render $T$ deterministic. The goal of an investigation of $T$'s determinism is to establish this kind of metatheoretical fact.

[2]This "assemblage argument" is plausible, at best, for physics governed by ordinary differential equations; it is obviously unsuited to partial differential equations.

Nestled intimately within all this is some hard to formulate presumption to the effect that *concrete* evolution laws should carry much of the real burden of physics. By "concrete", I intend laws that dictate the behavior of material parameters, such as the gravitational law or Coulomb's law for charges. It has been frequently observed that Newtonian mass-point mechanics, as it is customarily presented, suffers a dearth of such laws, for, in its usual formulations, the theory proves maddeningly unspecific about which kinds of forces can be evoked in practice. Because of this indefiniteness about force, writers like Montague (1974) have complained that any metatheorem to the effect that Newtonian mechanics is deterministic will be impossible to establish. The customary philosophical evaluation of this lapse, prompted by the view of theory sketched above, is to see this failure simply as an artifact of the "incompleteness" of mass-point physics. The theory would prove fully deterministic, it is claimed, if it were properly decked out in a firmly delineated regalia of force laws—perhaps Universal Gravitation plus some form of electromagnetic equations. Since quantum mechanics seems to have been brave enough to settle upon a definite list of four (or less) basic forces, it is presumed that classical physics should eventually follow a similar pattern.

We might call this a "*bottom up*" view of theory, for it views a theory as "complete" only if a sufficient array of concrete laws stand ready to ground it. The more abstract laws emphasized in textbook presentations of mechanics—for example, Newton's Three Laws—require such a supplementary "grounding" before the theory can be viewed as fully satisfactory.

It would be hard to formulate precisely any of the presumptions I have sketched here. Nonetheless, their handiwork is betrayed by the fact that many widely accepted doctrines in contemporary philosophy of science and metaphysics seem to require their truth. As a fairly direct illustration, McKinsey, Sugar and Suppes, in their well-known article on mechanics (1953), criticize George Hamel for adopting a form of determinism as a postulate: "One does not see how this axiom could possibly intervene in the proofs of theorems, or in the solution of problems". Their reasoning, insofar as I can reconstruct it, seems to be: "The concrete laws of a theory, once they are fully filled in, will settle whether the systems of the theory evolve deterministically. A brute axiom of determinism would prove either inconsistent or redundant when this completion is achieved. On the other hand, premature postulation of determinism will be of no scientific value, because the postulate is airily abstract in a situation where concrete laws are wanted." Earman, too, often writes as if any presumption of determinism in the absence of a firm "concrete" backing counts as craven: "As always, fiat stands ready to establish determinism where honest toil does not suffice" (p. 185).

We shall see later that this point of view is untenable (we already have circumstantial evidence to that effect displayed by the fact that the redoubtable Hamel is under attack). It will turn out that there are circumstances where it is quite appropriate to adopt determinism as an axiom. In itself, this is merely a curiosity of axiomatics, but it provides indirect indication that something is amiss in the usual "bottom up" presumptions about theory. Insofar as I can determine, the basic facts about modern mechanics connected with this situation are largely unrecognized in contemporary philosophy. I cite as evidence the large variety of popular philosophical projects which will work only if the "bottom up" view of law is correct (some of my own endeavors suffered in this regard). As a popular case in point, many writers now hold that the field of "physical properties" is to be built from "fundamental magnitudes" of physical theory using logical constructions and, perhaps, "supervenience", to reach higher levels. This edifice then paves a royal road towards materialism (or, perhaps, antimaterialism—there is disagreement about the eventual moral). But, unless the laws of the science are suitably concrete, none of this will work, for the "laws" will neither list all relevant parameters nor determine how the further properties generated will behave, in the sense of "determine" that "supervenience" requires. Likewise, if the "bottom up" picture of theory is mistaken, we have much less reason to presume that the "physical possibilities" belonging to a theory can be easily delineated—say, by identifying these "possibilities", modulo some minor quibbles, with what logicians call the "models" of $T$. Insofar as I am aware, only Aldo Bressan (1980) has noted, through considerations related to those developed here, that this view of "scientific possibility" is implausible. Hence, it may be no exaggeration to claim that tacit "bottom up" presumptions lie behind virtually every contemporary philosophical project that relies heavily upon the notion of "law" for its success.

As I remarked, Earman's own investigation of determinism proceeds generally in accordance with the "metatheoretical" plan just sketched. The difference is that Earman actually stoops to *examples,* and this ipso facto allows his discussion to reach a much higher level of achievement than prior efforts. Indeed, it is only through pressing "the probe of determinism" sufficiently far that we can begin to see how a debate over the "metatheoretic" approach might begin. In the next two sections, I'll discuss several examples, first from Earman's perspective and then from my own. I have purposely selected these cases from among the fatalist warhorses listed in my opening paragraph.

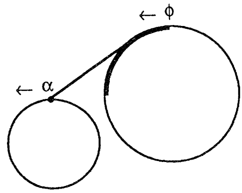
In passing, I shall note that the suggested analogy between classical physics and the "four forces" of quantum theory is potentially misleading. It is simply not reasonable to expect a similar structure within classical mechanics, at least if one wishes to permit that science most of its explanatory successes. Although most macroscopic interactions probably represent manifestations of *quantum* gravitation and electromagnetism, we cannot credit these effects solely to *classical* point-mass electromagnetism and gravitation. Classical approximations of force begin their validity essentially at the molecular level, where a large variety of distinct, effective potentials manifest themselves. Deriving these multifarious force functions from the electromagnetic and gravitational properties of electrons, protons, etc., involves many inherently quantum mechanical traits, which classical mechanisms cannot reproduce. Any viable foundation for classical mechanics[3] must admit a very broad range of primitive force types—a range it may be impossible to classify except in quantum mechanical terms. Hence the presumption that the "fundamental parameters" pertinent to the forces of classical physics will be small in number is motivated more by fond philosophical hopes than consideration of the organization nature permits. However, nothing in the discussion to follow will hinge upon this observation.

## III

Let us now examine the basic behavior behind many of Earman's examples within a very simple context—the theory of ordinary machines. As we have noted, fatalists generally believe that watches and locomotives offer admirable confirmations of their doctrines. If predicting the motion of a machine is one's sole ambition (as opposed to worrying when its parts will fail), a very simple, pre-Newtonian theory will suffice. Looking into a standard treatment of so-called "machine kinematics", as formulated, say, in Wilson, Sadler and Michaels (1983), one discovers that the "theory" involved is hardly more than analytic geometry and, as such, would have been available to Descartes (which in no way belittles the difficulty of the mathematics involved!). Suppose we have two wheels linked via a connecting rod as shown in Figure 1.

If we turn the small "crank" wheel counterclockwise through an angle $\alpha$, the larger wheel will oscillate with a corresponding angular displacement $\phi$. Clearly, determinism with respect to machines will involve this kind of functional dependence between input ($\alpha$) and output ($\phi$) variables. Will our theory of machines ratify our expectatons of determinism? In most cases, yes. We first need to formulate the algebraic equations which

[3]That is, for a "classical mechanics" based upon point particles. It is important to recognize that there is no unique entity called "classical physics"—there are many pretenders to the throne, which may behave quite differently in respect to "determinism". In this paper, we shall be largely concerned with *point particle mechanics,* formulated in ordinary differential equations and governed by "Newton's laws", and *continuum mechanics,* formulated in partial differential (or integral) equations and governed by the Euler-Cauchy laws.




(Heavy line marks range of large wheel movement.)

Figure 1.

describe the geometrical inter-relationships of the machine parts (for example, $(x_2 - x_1)^2 + (y_2 - y_1)^2 = C^2$, where the $(x_i, y_i)$'s represent the Cartesian coordinates of the pin positions on the small and large wheels and $C$ is the length of the connecting rod). Barring a certain technical complication (namely, the Jacobian of the set of equations vanishes), the implicit function theorem of analysis establishes without further inquiry that $\phi$ will be uniquely determined by the crank angle $\alpha$.

The rub comes at the exceptional points when the "technical condition" fails. This can happen in the very special situation where the distance between wheel centers happens to exactly equal the length of the connecting rod added to the radius of the large wheel but minus that of the small wheel. Here a "critical point" corresponds to wheel configuration (a) in Figure 2.

Notice that two distinct modes of motion are now available to the large wheel, as shown in the (b) and (c) configurations: it may continue oscillating within its original range or instead "cross-over" to move in a new range with a completely opposite sense. Nothing in the geometrical theory of machines *determines* how the mechanism will move through the critical point; the large wheel seems free to choose how it will respond to each successive round of crank rotation. In short, extremely simple machines seem subject to indeterministic breakdowns, fatalist expectations to the contrary. A little reflection should convince the reader that kindred indeterminism will be found quite commonly among machines.

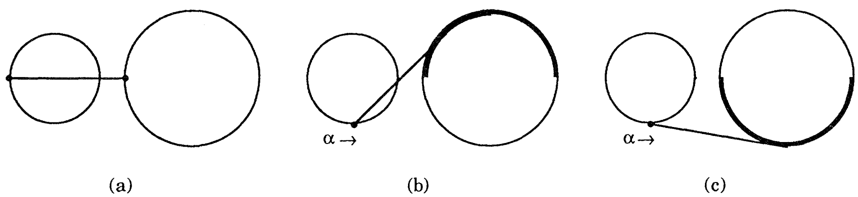



Figure 2.

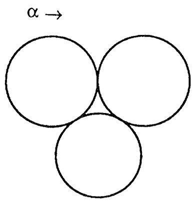


Figure 3.

Mathematicians say that the wheel equations *underconstrain* the solution at the critical point and, as a result, the assigned "side conditions" (= specification of how the crank wheel is turned) suffer a *failure of uniqueness,* that is, they are compatible with several distinct patterns of machine movement.

Machines can also fall prey to an allied danger—the device may move into a configuration where the various connections of its parts place incompatible demands upon further movement. This happen in cases of binding, say, when a set of gears assumes the configuration shown in Figure 3.

Turning the crank wheel through an angle $\alpha$ supplies two incompatible predictions for the displacement of the lower gear: (1) it will move counterclockwise, through $\alpha$'s direct influence; (2) it will move clockwise, because of $\alpha$'s indirect effect on the auxiliary gear. Mathematically, binding means that the relevant equations are *overconstrained* at the critical juncture. Here the theory of machines fails to be "deterministic", not by providing too many solutions to a prediction problem, as happens with non-uniqueness, but through permitting no consistent answer at all. In the jargon, the equations' solution runs into a *failure of existence* beyond the critical point.

The cases Earman discusses in his book are generally more complicated than this, involving breakdowns in the behavior described by ordinary or partial differential equations, rather than our simple system of algebraic formulas. This provides an opportunity for a few other varieties of breakdown to creep in.[4] Nonetheless, our two simple machine failures provide a tolerable first picture of how "determinism" goes awry in many of Earman's examples. Indeed, many of these difficulties trace to the same mathematical source—an inability to extract a needed "implicit function" at a critical juncture.

In our first example, the reader has no doubt noted that the angular

[4]In particular, differential equations can force some of their variables to become infinite within a finite interval of time, a phenomenon known as *blowup* (we'll see several examples below).

inertia of the large wheel should force the system to jump from pattern (b) to pattern (c) as it passes through the enigmatic wheel configuration. This consideration would block certain failures of determinism, yet at the cost of interjecting the apparently extraneous Newtonian principle of *momentum conservation* into the pristine, purely geometrical setting we originally gave our problem. In short, we can save determinism only by *extending* our original theory of machines to incorporate conservation of momentum. It is worth noting that in the theory of machines we need to appeal to momentum conservation only at very isolated junctures, whereas in a more orthodox Newtonian setting the principle is continuously active in dictating the evolution of its systems. Clifford Truesdell has dubbed such singular extrapolation rules "extension principles" and has supplied a nice variety of simple, complementary examples in some recent popular lectures (Truesdell 1987).

In our second, overconstrained case, repairs can't be effected so cheaply; the binding of machine parts can be dealt with only by turning to a comprehensive treatment of the forces and stresses within the machine parts, the friction between them, how the members distort under stress—in short, the vast range of topics engineers treat under the heading "dynamics of machines". Following the program of section II, it seems natural to classify "machine kinematics" (= the geometric theory we began with) as indeterministic in its own right, although possibly embeddable within a richer, genuinely deterministic theory. So far, then, our discussion has conformed nicely to the basic "metatheoretic" expectations as to how determinism should be investigated.

Before leaving the machine case, we should note, for later reference, that augmentation by familiar conservation principles will not eliminate *all* sources of indeterminism in the underconstrained case. In particular, these principles will not determine the direction the large wheel will strike when the crank wheel has been started from *rest* (this problem is a source of real-life headache in mechanical engineering). In fact, the strong symmetry of this start-up problem discourages expectations that any obvious "sufficient reason" can be found to induce the wheel to turn in a unique direction, even if we appeal to the full arsenal of Newtonian weapons (for example, consideration of the frictional forces in the pin joints; flexure under strain, etc.). In fact, the only plausible *deterministic* prediction is that, no matter how much force we apply to the crank wheel, neither wheel will in fact budge until some extraneous outside force—the breeze stirred by a distant butterfly, perhaps—breaks the symmetry and supplies a preferred direction to our wheels' motion. This, in fact, would be the orthodox "Newtonian" prediction. Of course, we never expect to see a real-life system frozen in such a preposterous state—technically called "a position of unstable equilibrium"—but we must assume such states' theoretical existence to preserve determinism, even at the cost of allowing the setup to store absurd amounts of internal energy.

## IV

Before taking up our next example, a few issues should be clarified. Inevitably, the "laws" in a modeling must be supplemented by various forms of "side condition" data before there is any reason to expect that the motion of the system is thereby uniquely determined. Thus, in the wheel case, as "side condition" we annexed the movement ($\alpha$) of the crank wheel to the algebraic "laws" of motion governing the wheels. Following standard practice, I plan to allow both initial and boundary data to be used as side conditions in forcing deterministic solutions. Somewhat idiosyncratically, Earman urges that we should tolerate only *pure* initial condition data in studies of determinism, where "initial condition data" are restricted to facts pertaining to a single time slice of the universe.[5] But most partial differential equations—for example, the theory of elasticity we will examine in the next section—are commonly studied in the light of so-called "mixed data", including *boundary condition* data *not* restricted to a single time slice. Thus, in the standard textbook treatment of a plucked violin string, one assumes as a boundary condition that the end points of the string will remain fixed throughout the string's motion with zero velocity. This side condition amounts to the claim that one already knows how the space-time "worms" corresponding to the violin peg and bridge will behave at future times. One cannot readily dispense with conditions of this sort, for the behavior of the string will be quite different if the peg and bridge act some other way in the future—for example, if the bridge were instead to fluctuate rhythmically back and forth. With virtually no argument, Earman dismisses the use of boundary conditions on the grounds that it represents "a departure from pure Laplacian determinism by requiring a specification of future data" (p. 33).

This dismissal is too brisk and inappropriate to our subject. The ends of the string are not governed by the same differential equation as the string itself—after all, strong forces of unspecified origin must be responsible for keeping the ends pinned.[6] We did not include the presence of these forces in our differential equation "modeling" of the string. Accordingly, the string equation loses its validity at the end points, and questions of how the end points of the string will behave requires some

[5]As Earman recognizes, many physical theories—for example, elasticity with memory—require a larger section of space-time than this. If we are considering distributional solutions, the notion of "values on a surface" doesn't make literal sense and "initial conditions" must instead be explained in terms of "traces".

[6]One shouldn't be misled by the accidental fact that in the present instance the end conditions happen to satisfy the wave equation.

further physics relevant to pins and bridges. It seems no violation of the spirit of determinism to assume that we may take such answers as given when we investigate the determinism of the string equation proper. Earman's point of view would apparently require that the string equation be somehow welded to the equations for peg and bridge, and, after that, to the formulas for the face plate and ever onward, in the fashion of "These Bones Gwine Rise Again", out to spatial infinity. The resulting set of differential equations, could they be constructed, would be truly formidable. In limited and idealized contexts, such a project might be viable, but it seems inappropriate to burden our investigations with this demand.

In this connection, we should observe that machines, when they behave properly "deterministic", do not conform to Earman's "Laplacean" format either. Instead, well-behaved machines display only what might be called "here versus there" determinism, for example, at any moment, the local motion of the crank wheel fixes the behavior of rest of the device. Our appended "side conditions" are not initial conditions, but instead a complete, continuous *narrative* of how the crank is turned during the period of interest. Left to itself, without input to the crank, the device will fall idle. The Laplacean mold accordingly does not accommodate the determinism machines are sometimes heir to, but machine-style determinism is nonetheless a respectable way to prove deterministic.

The reader might be warned that mixed boundary condition problems are often reformulated within a "pure initial condition"-like format as a so-called "abstract Cauchy problem" (Walker 1980). Earman refers to some of this literature, but obviously cannot intend his restriction to "pure initial conditions" to be interpreted in such a liberalized sense.

## V

The kinds of mathematical breakdown we have examined are not special to the machine case—related snags impede the course of physics at every turning. In fact, matters seem only to get *worse* as physics grows more complicated—say, when it employs partial differential equations. These frequent occurrences of "breakdown" represent a basic fact of scientific life which philosophy has generally ignored. In my estimation, however, these further instances of breakdown do not usually prove as troublesome for classical determinism as the simple cases of machine breakdown we have already examined. These reservations apply in particular to the examples Earman presents in his book. The wheel case is distinguished by its special geometrical[7] symmetries, a feature absent from the cases Earman mentions.

[7]That is, the difficulties in preserving conservation principles, for example, energy or linear momentum, while simultaneously assigning bodies finite, nondistorting shapes. As

Nonetheless, the more sophisticated forms of breakdown found in Earman's examples are quite interesting in their own right, for they cast a novel perspective on the "bottom up" view of theories sketched in section II. One can see this easily if one looks into the mathematics of another paradigm of determinism—billiard ball collisions (it is worth observing that, although Earman selects his various examples of breakdown from disparate areas of physics, the relevant mathematical behavior can usually be illustrated without leaving the pool hall).

Certain common misapprehensions about this subject should be noted. Although "Newtonian physics" is popularly characterized as "billiard ball mechanics", in many ways this appellation is undeserved. Billiard balls in fact have always represented a rather sticky topic for classical mechanics. In particular, the standard approach to "billiard ball collisions" taught in freshman physics, under the supposed aegis of "Newton's Three Laws of Motion", is not only inadequate, but logically confused as well. Indeed, the alleged governing principle "$F = ma$" can't make sense for billiard ball collisions (because the acceleration $a$ will be infinite at the point of contact). The usual, misleading elementary textbook treatment appeals to momentum conservation, which is "derived" under implausible assumptions from Laws II and III. It doesn't seem proper procedure to let a *theorem* decide what happens when an *axiom* proves meaningless! In truth, this use of momentum conservation should be viewed as an extension principle for pushing the mathematics through a breakdown singularity. Presumably, unless the treatment of collision phenomena is altered, the axioms of "Newton's physics" must be reformulated so that momentum conservation (or something related[8]), rather than the original Newtonian laws, is chosen as a fundamental principle.

In fact, point-mass mechanics is a lousy format for investigating billiard ball behavior. The peculiar motions seen on a pool table reflect the *size* and *deformable nature* of the balls. The proper setting[9] for treating such problems is classical continuum mechanics, the study of how continuously distributed matter responds to imposed influences (Truesdell and Toupin 1980). In this framework, each ball should be assigned a so-called

we noted at the end of section III, it often seems preferable to admit indeterministic interactions rather than introducing the "invisible instabilities" required by determinism. This supports the suggestion of section VIII that "instabilities" will need to be an essential ingredient in a convincing argument for classical indeterminism.

Analytical mechanics, incidently, suffers related "geometrical" problems, especially within its non-holonomic fringes. See the contrasting treatments of a skate on an inclined plane in Arnold (1988).

[8]The standard repair is to reformulate the second law as an integral principle of momentum balance.

[9]Most predictive success, however, is achieved within the more limited framework of rigid body mechanics, which I'll not discuss here.

"constitutive equation" connecting strain in the material with mechanical stress (and possibly additional influences such as temperature). For concreteness, we might think here of Hooke's law, extended to three dimensions and relating strain linearly and isotropically to stress alone. Roughly speaking, such constitutive equations assume the role played by special force laws in point-mass mechanics, but we are left free to specify their nature. Most rigorous engineering practice today operates within this framework.

Although continuum mechanics represents a more promising framework for treating impacts, it can still only be said that, at the present time, no complete and fully rigorous treatment of billiard ball impact exists, despite a wide variety of promising, *partial* approaches to the problem. Let us survey a few of the difficulties:[10]

1. If one of the colliding objects has sharp corners (such as the end of a billiard cue), the stress it induces inside the ball will become infinite at certain points, no matter how feeble the pressure exerted. If Hooke's law is regarded as gospel, we would expect the relevant material to be stretched off to spatial infinity. The usual response to this problem, which is confirmed by experiment, is that, under great stress, the material will change its constitutive behavior to one of non-Hookean, plastic flow. Unfortunately, it is hard to track when the material will display these varying phases of response and this provides an immense obstacle to an existence proof. Worse yet, the equations governing the two phases of material have different mathematical forms requiring different types of side conditions. Picking side conditions suitable for one of the phases is likely to pose consistency problems with the other phase. Without knowing the answer to how the material will in fact evolve, it can prove very difficult to select "side condition" data appropriate to the situation in advance.
2. Even when the geometrical shape of the object does not lead to infinities, the material itself will spontaneously form so-called "shock waves", which, mathematically, represent areas where, following the theory, one will compute two or more incompatible values for the stress, the varying answers depending upon where inside the ball one starts the calculations. The classical approach to this problem of "shocks" is to assume that an internal surface forms within the material which manages to resolve the apparently incompatible demands placed upon it by the surrounding material. On these special "shock" surfaces, the ball's material is not ex-

[10]For a useful brief survey, see Kalker (1975).

pected to obey Hooke's law, but instead behaves in such a way to insure that the ball will not internally lose energy, momentum, etc. The familiar "shock waves" responsible for sonic booms are modeled in gas theory by singular surfaces of this sort. Accordingly, the basic differential equations for billiard balls with which we began—in particular, Hooke's law—break down on these internal surfaces and "extension principles" must be found to govern how the shock surfaces will move through the material. In the cases where this extension program has succeeded, the needed additional principles have been borrowed from thermodynamics.[11] However, to the best of my knowledge, the full three-dimensional theory has not yet been worked out.

3. These waves will reflect back from the boundaries of the ball, producing so-called "caustic" infinities potentially requiring further supplementary principles.
4. The boundaries of the two balls will move during the impact, unlike the *fixed* boundaries treated in textbook problems like a plucked string. The resulting nonlinearity makes existence proofs much harder to establish. Worse yet, we cannot readily specify data along this boundary, as we do in the string problem, simply because we don't know where the boundary will be until we have somehow solved the problem.
5. Balls can fracture or form internal cavities, whose surfaces store energy in a different manner from the rest of the material. One therefore needs principles of how these crack surfaces form and how they grow. In situations of great symmetry, it is unlikely that it is predictable whether a crack will happen to form in one direction rather than another.
6. Balls both stick and slide against each other. Most treatments of collision assume a nonsliding condition, which is obviously unrealistic. One needs to find plausible principles for how boundaries interact.

Certainly at present, we are nowhere near being able to offer a uniqueness and global existence proof for the collision of billiard balls—the metamathematical property that we have expected a genuinely deterministic theory to display. Insofar as such proofs have been given (for much simpler systems), they usually proceed by explicitly incorporating the breakdowns into the formulation of the problem.

[11]Apparently, some of Duhem's hostility to kinetic theory stemmed from this fact, for it means that the postulates of mechanics will not logically close without including entropy as a component.

## VI

What should we conclude about the determinism of billiard ball mechanics, given the litany of breakdown woes just cited? Several points of methodology should be noted. We have observed that, just as the string equation is not valid at its external boundary points, the basic differential equations for a billiard ball will *not* remain valid everywhere *inside* the ball, due to phase shifts, shock waves, cracks and kindred ills. Many of these breakdowns take place at an internal point or surface. A shock wave, for example, moves as a two-dimensional sheet through the medium. We saw that an account of how a tied string moves can't be completed until the differential equation for the string is supplemented by "boundary condition" principles governing points external to the string (for example, at its two tied ends). We now see that billiard ball behavior will also require "*internal* boundary conditions" for the singular surfaces where the basic differential equations for the ball break down. Suitable conditions can be quite hard to find.

The problem of formulating suitable boundary conditions now pertains to the ball's external boundaries as well, due to the fact that they *move*. Boundary conditions are easy to formulate in the simple string case, because we have convenient a priori knowledge that its end-point "boundaries" will "move"—namely, they stay still—and this simple fact enormously simplifies the problem. In particular, it allows us to *decouple* the problem of charting boundary behavior from the problem of predicting regular material behavior (= the part governed by the differential equations for the system). But for billiard ball impact, it is obviously impossible to characterize completely how the outside boundary of the ball—that is, its shape—will change without some simultaneous knowledge of what has happened inside the ball in its "regular material". So none of the various surfaces, internal and external, found in a billiard ball permit the convenient separation of problems that were permitted for a string. Mathematicians classify such invariably difficult problems as "moving boundary value problems". They cannot be treated until one establishes more delicate relationships between insides and boundaries—more delicate, that is, than a blunt "the string stays tied here". In the textbooks, typical choices for such "delicate" conditions on the external boundaries are that these surfaces are frictionless and that the balls collide along their line of centers. Such assumptions are obviously unrealistic and in need of further refinement. As we noted, the internal boundary conditions appropriate for shock waves turn out to involve considerations of entropy, a topic that a priori seems extraneous to the problem. We *hope* that, armed with a sufficient number of boundary principles, it will prove possible to chart the complete movement of billiard balls, by treating the boundaries in tandem with the regular material, but it is virtually certain that the boundary principles which have been formulated to date are not sufficient to track the movements of colliding billiard balls in a fully convincing manner. We certainly have no guarantee that Nature will eventually supply us with enough supplementary principles to complete the job.[12]

These difficulties, which are recurrent in many areas of mechanics, have led scientists to adopt a more modest approach to model building than suggested by the "bottom up" picture of section II. In superficial agreement with that picture, we may *start* the process of constructing a physical model by relying upon some plausible differential equation "law" for the material in question, as happens when Hooke's law is assigned to the material within our billiard balls. But we now recognize that these "laws" may not prove inviolable in the long run—they may need to be "relaxed" (= rendered false) in favor of supplemental "boundary" considerations, in order to accommodate the breakdowns that would otherwise arise. Most likely, until we've toyed with our initial model a bit, we will have little sense of where trouble is to be expected. We are therefore forced to follow a "bootstrapping" approach to the formulation of problems: begin with "laws" of a defeasible character and correct them later as the deficiencies of the original modeling become known.

The incursion of functional analysis techniques into applied mathematics has supplied this bootstrapping process with a recognizable format (Reed and Simon 1972; Richtmyer 1978). The differential equations with which we begin our modeling are initially treated as "purely formal". This means, surprisingly enough, that these equations are purposefully *not* assigned a precise mathematical *meaning*. Instead, following the lead of whatever physical clues are available, we search for special domains and special meanings for the mathematical operations in our "formal equations", so that, so interpreted, the precisified results would permit a uniqueness and existence proof. One then hopes that the special qualities assigned to the final construction will provide clues as to what sort of supplemental considerations need to be added to the physics of the problem. In short, we let the *inadequacy* of our original modeling direct us towards their means of correction. So, for example, Laurent Schwartz' famous generalization of differentiation permits easy existence proofs for

[12]Related difficulties attach also to the selection of "initial conditions". Contrary to expectation, these may not represent events where the equations in question are satisfied. The usual "initial conditions" assigned to the heat equation, for example, represent a state of the system *just before* the heat equation takes effect. Isolating a set of conditions that obtain on a time slice *governed* by the equation is as difficult a problem as obtaining its entire solution. Facts like this supplement the objections raised in section IV to Earman's "pure Laplacean" format; picking "initial conditions" suitable to a set of equations can prove a very subtle matter.

a wide range of equations if set in carefully chosen spaces of functions. If these techniques work for a given problem, the deviations of the resulting "weak solutions" from regular ones may provide a first clue of where the breakdowns will occur in, for example, the shock wave case.[13] One then tries to uncover the *physics* that correlates with these special structures. J. A. Walker explains the general philosophy this way:

> A given physical system can often be described by abstract evolution equations on a variety of spaces; if there is a "most appropriate" space (there may be several), the proper choice follows from the physics of the problem and not from any "formal" equation. In our search for an appropriate space, our primary clues are supplied by a firm belief that a physical problem has unique "motions" for all physically possible initial data, that physical motions exist [forward in time] if the physical system is not explosive, and that there is some sense in which physical motions depend continuously on time. (Walker 1980, p. 83)

Only when this process of adjustment is completed should we presume that we have finished our modeling of the original physical situation. In the final analysis, the various special spaces and reinterpretations of vocabulary lurking in the background are as important to the overall physics of the situation as the differential equations with which the process is started.[14]

These procedures seem subtly at variance with the expectations of the "bottom up" view of theories sketched in section II. In particular, the distinction between the "law" and "nonlaw" aspects of a physical problem seem rather more nebulous than the "bottom up" picture suggests. That account presumes that the "nomological content" of a theory can be localized in its "laws", expressed as differential equations, whereas the various initial and boundary conditions adjoined to a problem represent the physically "contingent aspects" of the situation. Accordingly, an "explanation" is held to trace how the "nomological content" of these differential equations pushes the system, at least with high probability, from given side conditions up to the event to be explained. Such a presumption clearly lies in back of Russell's proposal (1957) that the vague philosophical notion of "cause" is best jettisoned in favor of the precise "dif-

[13]Strictly speaking, the relation between distributional "weak solutions" and shock waves is a bit more complicated than this sketch suggests.

[14]The bootstrapping methodology is hardly special to classical mechanics. In quantum mechanics, the Schrödinger equation forms a skeleton on which a "formal" equation is built, but the problem remains of finding both a suitable Hilbert space and an extension of the Schrödinger operator well defined on that space. In fact, interpretational difficulties in naive quantum theory were a primary impetus behind the historical development of the modern "functional analysis philosophy" of differential equations evoked here.

ferential equations". This thesis is plausible only if the differential equations genuinely embody all of the principles that drive a physical system from initial conditions to other points in time. But in continuum mechanics and the many other branches of science based on partial differential equations,[15] "boundary conditions" do not play so passive a role as this picture suggests. Indeed, the consignment of physics to "equations" or "side conditions" can often prove rather arbitrary; in variational treatments, it is common to reassign portions of a problem's original "boundary conditions" to an "extended meaning" for the system's differential operators. Accordingly, the old identification of "nomological content" with "differential equation" calls for careful scrutiny. Personally, at least, I am much more suspicious of the notion of "nomological content" than I was formerly.

Insofar as metamathematical investigations of determinism go, these complications make it difficult to know whether an example displaying breakdown really should be treated as a "genuine physical possibility" for the theory. Although Earman is obviously well aware of this point, his *tone* often suggests otherwise. We have noted that the naive "bottom up" presumption that the "side conditions" needed for a theory will be easy to provide is mistaken. Nevertheless, Earman occasionally writes as if he believes a theorist is guilty of some quasi-Popperian form of methodological dishonesty if she does not specify in advance exactly which side conditions she intends to attach to her theory. Discussing shock wave breakdown, for example, he writes:

> [The believer in determinism sees] the apparent problem with determinism [here as] a welcome opportunity to investigate in detail the physics of the situation and to show that when that is done determinism works its way in a more subtle and wondrous form than we could have otherwise imagined. [A] skeptic will complain that the determinist should have been able to say in advance what all the constraints were and should not have been allowed to cut the cloth of physical possibility to suit the needs of determinism. (p. 51)

But the skeptic's demand is simply unreasonable; in problems at this level of mathematical difficulty, one cannot expect to foresee "in advance what all the constraints" or "all the possibilities" should be. In reality, there is little in the phenomena of shock waves to suggest indeterminism. In fact, of the various mathematical woes I listed in connection with billiard

[15]Historically, it is fairly clear that Laplace gained his famous picture of determinism from the behavior of *ordinary* differential equations. Although he certainly calculated with *partial* differential equations (for example, his well-known namesake), the subtle differences between these classes of equations were not well recognized at the time. The "bottom up" view of theory may represent a relic of this false assimilation.

waves, only crack production seems likely to represent an intrinsically indeterministic phenomena.

I should add, in this connection, that mathematical breakdowns cannot be left unrepaired in a theory (although in practice they are often contained through discrete sweepings under the rug). From Earman's playful discussion, one might easily presume that any physicist who thinks, in the absence of a proper "concrete law" grounding, that classical mechanics should be deterministic must represent some kind of wild-eyed metaphysician; that a brute postulation of determinism represents but the idle whim of the dogmatist of determinism. But this conclusion would be erroneous. A mathematical modeling which suffers breakdowns in existence or uniqueness can easily prove practically worthless unless the singularities are corrected or otherwise controlled. Conventional methods of numerical approximation, for example, can easily become derailed by non-uniqueness—the calculations may jump from one solution branch to the other, producing as output a predicted pattern of behavior that resembles neither possible solution. Likewise, a failure of existence for given initial conditions—say, when our gravitational equation breaks down because of a collision or other singularity—can spell calculational disaster for other trajectories, unproblematic in themselves, located within a general neighborhood of the breakdown. Hence when a mathematician working in celestial mechanics rescales the time variable in a funny way so that the rewritten gravitational equations are free of breakdown, her purpose is to make accurate calculation of near-collision orbits possible, rather than salvaging determinism for the glory of metaphysics. To handle indeterminism constructively, the mathematical format of a theory usually calls for radical surgery, say, by using "deterministic laws" to plot the course of *probabilities* for the system. This is, of course, how quantum theory manages its indeterminism, but the ploy is widely utilized within purely "classical" contexts (for example, Langevin's equation; Wiener processes). Another common method for foregoing determinism is to describe the system's evolution only in discrete, sampled stages. So-called "quasi-static" treatments are of this nature; one tracks the system only as it relaxes from one equilibrium stage into another. The treatment becomes indeterministic if, as often happens, the system has several equilibrium positions open to it at subsequent stages of the process. Much "prediction" in classical physics—including that pertaining to billiard balls[16]—actually proceeds within this quasi-static format, rather than following the full dynamical evolution we have previously assumed in our discus-

[16]Practical calculations of billiard ball impacts usually follow some modification of the quasi-statical model developed by Heinrich Hertz. One can get "indeterminism" of the sort familiar from the "bifurcations" created as one applies an increasing force to an upright column.

sion. The orthodox parsing of these "nondeterministic" methods is that they represent mere approximations to the "true" deterministic classical physics, although there has been much speculation of late that these roles ought to be reversed (see below). My point is simply that accepting classical mechanics as indeterministic will require a considerable mathematical recasting of the entire edifice, not the mere acknowledgement of a breakdown.

The billiard ball case displays another unexpected dimension to the notion of "law", namely, the fact that many "laws" enjoy a less exalted status than suggested by the "bottom up" approach. In particular, "laws" now seem to come in at least two degrees of defeasibility. On the one hand, we have the more abstract principles of mechanics—for example, momentum balance, energy conservation, etc.—which we may still regard as immutable. Greater defeasibility attaches to what I called the "constitutive equations": the "laws" that govern the behavior of particular types of materials, for example, Hooke's law for a spring, the principles governing how fluids respond to applied stress, etc. As we saw in the billiard case, we must be prepared to overrule the contribution of a constitutive equation to the modeling, no matter how well established it might be (whereas we continue to use the "abstract" laws in discovering, for example, what traits shock surfaces will need to have). But such "constitutive equations" are exactly the "concrete laws" upon which the "bottom up" view of theory so heavily relies.

In section II, we noted that orthodox particle mechanics seemed "incomplete" in its usual formulations, due to its reluctance to specify fully many "force laws" beyond Universal Gravitation. Rather than representing a lazy incompleteness in mechanics, this omission now seems as if it might be both principled and necessary. Indeed, the "missing laws" of particle mechanics represent the analog of continuum mechanics' "constitutive principles", for they represent the concrete rules governing how particular classes of systems respond to forces, store energy, etc. In particle mechanics, too, practitioners simply *overrule* a "constitutive principle" if it leads to breakdown, even if that principle happens to possess the solid credentials of Universal Gravitation. On page 36, Earman ably explains how the $n$-body gravitational equation cited in section II is open to a variety of breakdowns. Earman cites this as evidence that perhaps gravitational particle mechanics shouldn't be viewed as deterministic. Yet, a more common reaction to this problem in the scientific literature (Galavotti 1983, pp. 27–28) is to conclude simply that one shouldn't allow, as a full-blown physical possibility, point-mass systems that interact through gravitation alone—even if one has no particular answer to what the supplementary forces will be. Indeed, this exclusionary attitude towards unwanted possibilities seems to me the only reasonable approach to the problems. If overly close encounters of point masses are allowed, the notion of potential energy threatens to become meaningless in a manner familiar from the "renormalization" difficulties of quantum theory. It is sometimes argued that these problems must be handled by quantum mechanics; a system falls within the gambit of "classical particle mechanics" only insofar as no complications attach to its potential energy. In other words, we should excuse classical mechanics from any obligation to deal seriously with the close encounters of point particles. Accordingly, we simply erase such systems from the purview of particle mechanics. On these grounds, even as venerable a "constitutive equation" as Universal Gravitation can be overruled.

In the abstract, we might agree with Earman that, prima facie, we should worry about the propriety of "cutting the cloth of physical possibility" to shore up a potentially problematic scientific theory. In this spirit, for example, Walter Thirring complains about post hoc attempts to render Dirac-style classical electrodynamics palatable simply by throwing out would-be crazy solution possibilities:

> Attempts have been made to separate sense from nonsense by imposing special initial conditions. It is to be hoped that some day the real solution of the problem of charge-field interaction will look different, and the equations describing nature will not be so highly unstable that the balancing act can only succeed by having the system correctly prepared ahead of time by a convenient coincidence. (Thirring 1978, p. 99)

But, in the case of classical mechanics at least, we must engage in a mild measure of this kind of repair if we expect to have a formalism that captures classical mechanics in any of its familiar forms. We wish to formulate the theory to accommodate the furthest reaches of its practical applicability and this seems possible only if we include prima facie physical possibilities that we later expunge by hand. But for any theory of this sort, we cannot expect a purely "metatheoretic" study of determinism to reveal conclusive results. Nor should we expect its set of "physical possibilities" to be easily delineated by the model theoretic techniques of logicians.

## VII

These considerations suggest that determinism might genuinely be permissible as a brute *postulate* in physics, the strictures of McKinsey et al. (1953) to the contrary. That is, once we decide that abstract physical principles can always override the possibilities that seem to flow from our "constitutive equations", there may be ample reason to formulate determinism as a fundamental axiom, especially if considerations of deterministic evolution play a major role in correcting constitutive equations (vide Walker quote above). Indeed, V. I. Arnold and his collaborators in their beautiful presentation of particle mechanics (1988) begin with exactly such a "principle of determinancy", serving, roughly speaking, in lieu of Newton's Second Law.[17] These authors are fully aware that Universal Gravitation suffers the breakdowns Earman cites—indeed, they include a nice discussion of the phenomenon. But this realization does not alter their postulation of determinism one whit—they simply conclude that gravitation must be utilized with some care in constructing "classical systems". Their postulation reflects a plausible assessment of which central *core* of material best deserves the moniker "classical particle mechanics". Bluntly assuming "determinism" justifies, inter alia, the painting of mechanical systems onto manifolds, the proper setting for the qualitative questions these authors wish to investigate. The purpose of this "postulation" is not to save work—the breakdown difficulties connected with the gravitational law need to be addressed in some format or other—but it isn't necessary to clutter up a statement of the theory's "fundamental principles" with them.[18]

An interesting aspect to all this is how *conventional* determinism's status as an axiom seems to be. In continuum mechanics, it is not customary to postulate determinism in Arnold's fashion, but this decision again reflects a judgment of which problems need to be emphasized in the field. Because of the great variability in the kinds of equation that can turn up in continuum mechanics, a blunt postulation of determinism here can serve only as a vague precursor to the more detailed process of selecting proper data and solution spaces needed in an acceptable modeling (compare comments in Truesdell and Toupin 1960, p. 701). One's goal is (usually) a deterministic model, but an axiom to that effect doesn't streamline exposition in the same convenient way as it does in particle mechanics (whereas the more specific requirement that strain must be locally determined by stress is useful in eliminating spurious possibilities—Hamel's despised axiom is of this type). Why include a postulate, no matter how true, if it plays little role in the formal developments of the field? McKinsey et al., of course, mean to raise exactly these considerations, but our discussion shows that one cannot read their moral in the categorical manner they presume.

[17]As these authors point out, if one takes the implied regularity assumptions of "$F = ma$" seriously, Newton's Second Law is simply a restatement of determinism.

[18]I might remind the reader, to whom such a blunt postulation of determinism remains unpalatable, that the quantum mechanical postulate that observables are represented by self-adjoint operators has a similar mathematical status, namely, one has temporarily waved away in one's axiomatics the modeling difficulties that present themselves when one begins at the level of the naive Schrödinger equation.

The apparently "conventional" status that attaches to an axiom of determinism runs counter to most philosophical approaches to "law", where it is hoped that the realm of "nomological" is more absolutely fixed. Only those philosophers who see scientific laws as simply the principles optimally suited to the organization of data will be pleased by the conventionality of determinism. Earman happens to be partial to the organizational view and devotes an interesting chapter to the subject, although he does not bolster his case through the considerations cited here. Personally, I still lean towards absolutism, but know of no way, short of the usual dogmatic intransigence, to defend this preference.

I would like to stress once again that the status of classical determinism isn't desperately interesting in itself. Investigations like Earman's are valuable instead for the light they shed on sundry philosophical dogmas relating to law, possibility and theory. The realization that determinism can serve so effectively as a "probe" of this nature represents a genuinely novel contribution by Earman to philosophy. I hope that the dialectic in this essay has provided the reader with additional motivation to take up Earman's book with this emphasis in mind.

## VIII

Although my considerations have thus far spoken largely in favor of classical determinism, it must be said that there are serious, not directly philosophical, reasons for doubting that classical mechanics is best viewed in this vein. Indeed, many prominent scientists of late have denied as much (Weinberg, Prigogine, Szebehely, to name a few). Oddly, the intimations of indeterminism they sense are underreported in Earman's book. These authors are generally motivated by the escalating realization that many standard "classical" systems are greatly *unstable,* especially when one studies alterations in the system's structural parameters. What this means, crudely speaking, is that, given a particular classical setup, there will often exist cousin systems that will observationally resemble the original system at time $t_0$ in as much detail as one could possibly measure, but will nonetheless begin to act wildly different within a short period of time. In practical terms, this makes predictions about such setups "indeterministic", for one can only trust results obtained by averaging over the family of systems similar at $t_0$. The authors in question wonder whether we have not misread the mathematics of classical physics all along; physical systems should *not* be modeled by the single line trajectories (that is, integral curves) of standard approaches, but by some more abstract entity, such as a tube of trajectories, coupled, perhaps, to some intrinsic source of randomization (as in the Langevin equation). If the classical single line trajectory represents an overly idealized object, the same reservations should probably attach to many of the choices inherent in the basic modeling (for example, choice of boundary conditions, representation of forcing terms, the values of empirically derived parameters like mass, methods of containing singularities, etc. Accordingly, the selection of a suitable "tube" of trajectories is probably conditioned by the inevitably idealized set of decisions that characterize the setup of the original problem.

At the end of section III, I described the problem of getting our linked wheels to move if they are initially at rest in a critical position. We noted that determinism apparently requires an *outside influence* to drive the large wheel clockwise or counterclockwise; without such assistance, the apparatus will remain immobile, no matter how much force is applied to the crank. It is customary, on a standard interpretation of mechanics, to claim that the perfectly symmetrical, immobile condition of the highly stressed apparatus represents a "genuine possibility" for the system, but that we will never see such a possibility in real life due to its great instability. In other words, our alleged possibility is de facto "invisible" because (1) it is unlikely that a system could be prepared in such a perfectly symmetrical state and (2) no shielding can exist to wall off the apparatus from "symmetry breaking" outside influences. An unorthodox approach to mechanics hopes that some reinterpretation of mechanics can be found so that such "invisible possibilities" do not count as real possibilities at all. It might be held, for example, that a "real" description of our wheels in theory should always incorporate randomized "fluctuations". Normally, this random aspect to the wheels' movements will be swamped by its more deterministic aspects. Around the critical wheel configuration, however, the fluctuations become dominant and the system's behavior will prove straightforwardly "indeterministic".[19]

Our earlier discussion suggests some additional support for this perspective. Although I have indicated reasons why a worker in, for example, celestial mechanics might wish to excise singularities from her theory, there are also reasons why they should not be totally despised either. Indeed, the presence of singularities often provides a most helpful clue to discerning how a theory behaves. Except for the singularities and critical points like equilibria, the behavior of one physical system may look mathematically very much like another. The locations of the singularities serve as landmarks in understanding the system. Accordingly, we may want to *introduce* singularities artificially into an otherwise untroubled modeling simply to figure out what's going on. Perhaps the orthodox "invisible wheel possibility" should be viewed in this way; it plays

[19]This is an attempt to convey some of the hazily expressed philosophy of Serra et al. (1986). I should stress that I have provided here only the briefest hint of the incredibly diverse, and often contradictory, motivations that drive current interest in "classical indeterminism".

a central role in the mathematics of wheels, but we are not obligated to parse it as modeling a real possibility.

The motivation for such an unorthodox approach to "classical indeterminism" does not lie primarily in its novelty value. Instead, the hope is that the recasting will lead to a better understanding of how entropy and irreversibility enter mechanics in all areas of physics. This being granted, most attempts in the vein indicated do not seem to have advanced much beyond the stage of intriguing suggestion. The writings of, for example, Prigogine (1980) are frustrating conglomerates of promising insights and philosophical howlers. We need some level-headed Aeneas to guide us through these regions. *A Primer on Determinism* makes little attempt to provide such assistance, a surprising omission in light of current enthusiasm about such matters. An overly abbreviated chapter on "Determinism, Instability, and Apparent Randomness" is included, but the emphasis is largely on whether microscopic instabilities can produce the macroscopic randomness that statistical mechanics seems to need. Insofar as the philosophical program discussed in this section goes, Earman seems to regard any proposed link between "indeterminism" and "instability" as simply a variant of the familiar confusion between "indeterminism" and "unpredictability".[20] Since Earman casts his net widely enough to sample cheerfully some fairly dubious arguments for indeterminism, I find it surprising that he wanes so conservative in the face of indeterminism's current hot topics.

[20]Certainly, a balanced assessment of stability issues should begin by distinguishing sharply (as Earman does not) between behavioral divergence in the long term, as measured, say, by Lyapounov instability, and the immediate large differences in behavior seen in an ill-posed problem in the sense of Hadamard. In regard to the latter, I find Earman's commentary puzzling:

> [D]espite the seemingly straightforward appeal of the notion of continuous dependence of solutions on initial data, there are many inequivalent ways to gauge continuity, and especially in the case of partial differential equations, the verdict on stability can vary with choice of gauge. For particles as opposed to fields, the gauge is much less open to choice, but we have learned that in the case of particles Nature sets and solves many "non-correctly set" problems where determinism but not stability holds. (p. 154)

Through the use of the term "gauge" (as opposed to "norm"), there is a false, although possibly inadvertent, suggestion that this choice is entirely *conventional*. But the selection of norm is an integral part of the process of constructing the extended set of "solutions" that we will want our differential equations to admit. Accordingly, a choice of norm typically embodies some nontrivial additional physics.

It would have been also helpful if Earman had expanded upon his remark that "Nature" solves ill-posed problems (the mention of "particles" is especially cryptic to me, being unable to either trace a reference in the text or reconstruct how a set of ordinary differential equations might display behavior resembling Hadamard instability). Certainly, *we* are faced with many prima facie "ill-posed" problems in scientific life—that is, reconstituting the original of a television image—but I am unclear what bearing such cases have on Earman's issues.

## IX

As I stated earlier, Earman organizes his material in a different fashion than I have followed in this paper. In particular, he throughout emphasizes the bearing of the background space-time on questions of determinism. He writes:

> [S]pace-time is the canvas on which the possible worlds are painted. The details of the structure of this canvas will turn out to be as crucial to the success or failure as what is painted on it: *too little structure of the right kind or too much structure of the wrong kind and determinism will never succeed no matter how furiously or cleverly we paint*. (p. 24)

Indeed, Earman often writes as if this were the main or "overarching" moral of his discussion.

In this paper, I have instead emphasized the relevance of *breakdowns* for determinism, a feature found in most of Earman's best examples. From this point of view, the background space-time *is* less important to determinism than the paint we select. Indeed, the singularities in classical models have a nasty habit of simply reappearing within a relativistic setting. It is undeniable, of course, that the behavior of a given law can be profoundly influenced by special features of a space-time, for example, its dimensionality. Nonetheless, my subjective scorecard would not rate space-time choice as the major player in the struggle for determinism. To convince us otherwise, Earman has assembled a variety of considerations that I find misleading.

Earman first presents some rather abstract argumentation in favor of the thesis quoted above, namely, that certain space-times are incompatible with any form of determinism. Earman urges that we recast each of our theories in "local space-time form" consisting of tensor fields painted upon a space-time canvas. Often, as much of Earman's previous work has shown, this approach affords a very revealing point of comparison between theories. But when our interest turns to breakdowns related to, inter alia, geometrical constraints and boundary influences, we must beware that the important features will not be smoothed away or otherwise buried in the unstated technical details of an overly schematic presentation of the theory.[21]

Difficulties of formulation aside, let us briefly rehearse Earman's argument. His paradigm case of a space-time in which "determinism will never succeed no matter how furiously or cleverly we paint" is what he calls "Leibnizian space-time", namely, a structure admitting regular Eu-

[21]Many classical theories of materials—for example, plasticity—resist easy formulation in covariant form.

clidean metrics on the spacelike slices that can be cut from it (as well as a metric for a universal time), but supplying no interesting structure to tie the spacelike slices together (as inertia does in "neo-Newtonian space-time"). He then argues, in an extremely compressed passage, that "space-time symmetries and the symmetries of the laws of motion [should] coincide" (p. 26). Using this principle, it follows that if $a$ is a permissible solution to a set of laws in Leibnizian spacetime, then system $b$ must be as well, where $b$ resembles $a$ exactly up to time $t_0$ but thereafter heads off in a divergent spatial direction. System $b$ must be possible if $a$ is, simply because $b$ can be obtained from $a$ by a rigid, time-dependent rotation (a transformation belonging to the symmetry group of the space-time). But this pair of possibilities automatically makes the physics indeterministic, claims Earman, since all the facts at $t_0$ cannot tell us whether a given system will subsequently behave in the manner of $a$ or in the manner of $b$.

If Earman's "symmetries of laws = symmetries of space-time" principle were valid, a close link between space-time structure and determinism would be forged, my misgivings to the contrary. But Earman's principle cannot be straightforwardly correct. A law can simultaneously present space-time with *two* or more incompatible sets of symmetries to honor. In particular, Cauchy's law of motion—the equivalent of "$F = ma$" for a continuum—involves two kinds of forces, the local *contact* forces and the so-called "*body*" forces that operate from afar (like gravitation). These two kinds of forces behave differently with respect to symmetries. Crudely speaking, the former lack the compass of inertia that the latter have. One must then decide which preferred structure is to be accommodated within the fabric of space-time. In their well-known treatment of continuum mechanics, Walter Noll (1974) and Clifford Truesdell (1977) rule in favor of the contact forces. The inertial structure needed by the body forces is not inserted in space-time per se, but instead is attached to the ambient matter in neo-Machian fashion.[22] Jerrold Marsden and Thomas Hughes (1983), on the other hand, follow the more orthodox approach of including inertial structure. I see little a priori reason for preferring one scheme over the other, but neither satisfies Earman's expectations.

The Noll-Truesdell approach in fact selects Leibnizian spacetime as its background framework, but these authors have not thereby tumbled into an automatic indeterminism. The physical considerations about contact forces that motivate the use of Leibnizean space-time also suggest that

[22] This procedure allows the "material frame indifference" of the contact forces (which are the important ones for continuum mechanics) to come out a natural product of the container space.

the notion of a system's "motion" through the space-time be defined[23] in a slightly more abstract way than usual (namely, as an equivalence class of customary motions). This revised treatment of "motion" does not seem ad hoc to me, but the natural sidelight of the overall approach to mechanics. By this new definition's lights, *a* and *b* will have exactly the same "motion" and hence represent no obstacle to determinism.

In general, the moral is that a finely honed definition of "determinism" can't be reasonably drawn until one takes into account special features of the theory one intends to examine. In most cases, I believe it will be premature to attempt to adjudicate the "determinism" of a theory by inspecting its space-time framework alone.

In a less abstract vein, Earman traces many of classical mechanics' alleged difficulties with determinism to the fact that it tolerates infinitely rapid interactions between spatially separated subsystems, behavior that the various relativistic spacetimes (almost) prohibit. As cases in point, he cites universal gravitation and Fourier's heat equation. It is notorious that such equations (which, incidently, are not an obligatory feature of a classical landscape[24]) are hard to reconcile with an *isotropic* universe—that is, one where matter is more or less evenly distributed everywhere. In such universes, their local influence tends to pile up too greatly, in the manner of Olbers' paradox. Thus, if the temperature or the distribution of matter doesn't die away reasonably rapidly as one heads off towards spatial infinity (for example, the distributions belong to $L^2$), then the far-away stuff can be expected to wield an incalculable influence on local conditions. One can even construct cases where such influences seem to come from "beyond infinity"—that is, they don't show up asymptotically as one heads off to infinity (Earman calls these effects "space invaders").

Such lack of provision for isotropy is a genuine demerit of many classical schemes, but I find it vaguely unsportsperson-like to let much of the argument against classical determinism turn on this point. If standard classical physics were correct, we perforce would live in an island universe. The inability of classical doctrines to accommodate isotropic possibilities coherently shouldn't legislate against either their correctness or their determinism.

[23]In truth, neither author defines "motion" in exactly the way suggested, but the proposal conforms to the general tenor of their approach.

[24]In particular, continuum theories can easily escape this limitation. It is puzzling that the example (p. 60) Earman selects as a paradigm of "deterministic success" within Minkowski space-time—namely, the linear wave equation—displays exactly the same virtues within classical spacetime. The heat equation, on the other hand, is "classical" only in the sense of being old and comparatively easy to solve. An "axiom of equipresence" (Truesdell and Toupin 1960) would exclude this equation from fundamental physics on the grounds that it leaves the conduction of heat completely uncoupled to mechanical behavior.

For a nice account of historical attempts to supply particle interactions with a finite propagation velocity, see Roseveare (1982).

I should stress that Earman presents some very intriguing material in connection with all of these matters, and my apparent dismissal here is largely as a matter of emphasis. In my opinion, insofar as determinism per se goes, the most surprising facts about classical physics are (1) how frequently straightforward employment of its "laws" leads to breakdown requiring some sort of unanticipated correction or supplementation and (2) how *invisible* in practice are some of the structures that classical mechanics posits as a sidelight of its determinism. Accordingly, it is these aspects of Earman's discussion I have chosen to highlight, but most of the rest is interesting in its own right.

## X

A fundamental problem that implicitly dogs any discussion of classical determinism is the brute fact that classical mechanics, in any of its forms, is simply not true. At some ill-defined point, we lose Nature's guidance in deciding which doctrines properly belong to "classical physics" and which do not. The sad truth is that the thesis of determinism almost certainly falls within the mushy area. The surprising revelation of *A Primer on Determinism* is that orthodox philosophical views about theory, law and possibility do not accommodate such mush easily. If determinism is not cut and dried, then perhaps theories, laws and possibilities aren't either. The only way to assess the potential damage to standard philosophy is to examine each of the troublesome cases in their full particularity. The overriding purpose of Earman's book is to provide the philosophical community with a smorgasbord of potentially relevant examples. However the eventual fate of "determinism" is to be determined, Earman is to be thanked for an admirable presentation of the initial problematic.

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