AFTER PHYSICS

David Z Albert

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To my son, in whom I am well pleased

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Preface

This book consists of eight essays about the foundations of physics. They have various different sorts of connections to one another, and they have been designed to be read in the order in which they are printed, but they aren't meant to add up to anything like a single, sustained, cumulative, *argument*.

The first consists of some general, introductory, panoramic observations about the structure of the modern scientific account of the world as a whole, with particular attention to the role of chance, and to questions of the relationship between physics and the special sciences. The second (which depends on the first) is about the direction of time—and (more particularly) about the sense of *passing,* and the asymmetry of *influence,* and the fundamental physical underpinnings of what used to be called *being-towards-the-future*. The third (which also takes off from the first, but eventually moves into questions about the foundations of quantum mechanics) is about the business of deriving principled limits on our epistemic access to the world from our fundamental physical theories. The fourth (which depends on the third) is about the more subtle and more general business of deriving principled limits on what we can intentionally *do,* or *control,* or *bring about,* from our fundamental physical theories.

And each of the remaining essays can stand (I guess) more or less on its own. The fifth is about the relationship between quantum-mechanical nonseparability, and the special theory of relativity, and the principled possibility of saying everything there is to say about the world in the form of a story, and the sixth is about how to think of the particles and the fields and even the very *space* of the standard scientific conception of the world as *emergent*, and the seventh (which is a sort of companion piece to the sixth) is about why it might be *necessary* to think of them that way—why there might not be any sensible *alternative* to thinking of them that way—and the eighth is about the meaning of probability in many-worlds interpretations of quantum mechanics.

The first two essays should be accessible to readers without any specialized knowledge of physics—but readers who want to know more about the scientific and philosophical *background* of the discussions in those essays might like to have a look at an earlier book of mine called *Time and Chance* (Harvard University Press, 2000). The rest of the book (on the other hand) assumes a preliminary acquaintance with the foundations of quantum mechanics (the basic quantum-mechanical formalism of wave functions and state vectors and Hermitian operators, nonlocality, the measurement problem, the Ghirardi–Rimini–Weber [GRW] theory, Bohmian mechanics, the many-worlds interpretation of quantum mechanics, and so on)—the sort of material that can be found (for example) in another book of mine called *Quantum Mechanics and Experience* (Harvard University Press, 1992). And here and there—particularly in Chapter 5—I will take it for granted that the reader has a working knowledge of the special theory of relativity.

There are, as always, many people to thank. First and foremost is Barry Loewer, who has been willing to teach me, and to hear me out, and to correct my mistakes, and to show me what it was that I actually wanted to say, and to promise me that everything was going to be fine, and all with such unwavering patience and gentleness and encouragement and steadfast devotion to the truth, for something on the order of thirty years now. *Mannes hed ymagynen ne kan, n'entendement considere, ne tounge tell,* what I owe to him. Tim Maudlin is not as gentle as Barry—but Tim, in his way, and after his fashion, has been spectacularly generous, decade after decade, with his time and his energy and his attention—and much of this book, and great swaths of my whole imaginative life, for whatever any of that may be worth, have been roughly yanked into being by his fierce and brilliant and relentless critique. And I am thankful to Sean Caroll for a careful and detailed and helpful reader's report for Harvard University Press. And I am lucky, and I am grateful, for innumerable and interminable conversations about these matters with the likes of Hilary Putnam and Shelly Goldstein and Nino Zhangi and Detlef Durr and Roddy Tumulka and David Wallace and Simon Saunders and Giancarlo Ghirardi and Yakir Aharonov and Lev Viadman and Ned Hall and Sidney Felder and Alison Fernendes and Mattias Frisch and Brian Greene and Jennan Ismael and Eric Winsburg and Brad Westlake and veritable armies of still others.

Earlier versions of some of the essays in this book have previously appeared elsewhere. Chapter 1 (for example) is a substantially reworked version of an essay called “Physics and Chance,” from *Probability in Physics*, ed. Yemina Ben-Menachem and Meir Hemmo (Berlin: Springer, 2012), pp. 17–40, © Springer-Verlag Berlin Heidelberg 2012; Chapter 2 contains a good deal of material from “The Sharpness of the Distinction between the Past and the Future,” from *Chance and Temporal Asymmetry*, ed. Alistair Wilson (Oxford: Oxford University Press, 2015), by permission of Oxford University Press; Chapter 5 is a somewhat less confused version of “Physics and Narrative,” from *Reading Putnam*, ed. Maria Baghramian (London: Routledge, 2013); and Chapter 8 is pretty much the same as “Probability in the Everett Picture,” from *Many Worlds? Everett, Quantum Theory, and Reality*, ed. Simon Saunders, Jonathan Barrett, Adrian Kent, and David Wallace (Oxford: Oxford University Press, 2010), by permission of Oxford University Press. I am thankful to the publishers for permission to reuse some of that material here.

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Physics and Chance

1. Chance

Suppose that the world consisted entirely of point masses, moving in perfect accord with the Newtonian law of motion, under the influence of some particular collection of interparticle forces. And imagine that that particular law, in combination with those particular forces, allowed for the existence of relatively stable, extended, rigid, macroscopic *arrangements* of those point masses—chairs (say) and tables and rocks and trees and all of the rest of the furniture of our everyday macroscopic experience.[1] And consider a rock, traveling at constant velocity, through an otherwise empty infinite space, in a world like that. And note that nothing whatsoever in the Newtonian law of motion, together with the laws of the interparticle forces, together with a stipulation to the effect that those interparticle forces are all the forces there are, is going to stand in the way of that rock's suddenly ejecting one of its trillions of elementary particulate constituents at enormous speed and careening off in an altogether different direction, or (for that matter) spontaneously disassembling itself into statuettes of the British royal family, or (come to think of it) reciting the Gettysburg Address.

It goes without saying that none of these is in fact a serious possibility. And so the business of producing a scientific account of anything at all of what we actually *know* of the behaviors of rocks, or (for that matter) of planets or pendula or tops or levers or any of the traditional staples of

1. And this, of course, is not true. And it is precisely because Newtonian mechanics appears *not* to allow for the existence of these sorts of things, or even for the stability of the very atoms that make them up, that it is no longer entertained as a candidate for the fundamental theory of the world. But put all that aside for the moment.

Newtonian mechanics, is going to call for something *over and above* the deterministic law of motion, and the laws of the interparticle forces, and a stipulation to the effect that those interparticle forces are all the forces there are—something along the lines of a *probability distribution* over microconditions, something that will entail, in *conjunction* with the law of motion and the laws of the interparticle forces and a stipulation to the effect that those forces are all the forces there are, that the preposterous scenarios mentioned above—although they are not *impossible*—are nonetheless immensely *unlikely*.

And there is a much more general point here, a point which has nothing much to do with the ontological commitments or dynamical peculiarities or empirical inadequacies of the mechanics of Newtonian point masses, which goes more or less like this: Take *any* fundamental physical account of the world on which a rock is to be understood as an arrangement, or as an excitation, or as some more general collective upshot of the behaviors of an enormous number of elementary microscopic physical degrees of freedom. And suppose that there is some convex and continuously infinite set of distinct exact possible microconditions of the world—call that set $\{R\}$—each of which is compatible with the macrodescription "a rock of such and such a mass and such and such a shape is traveling at such and such a velocity through an otherwise empty infinite space." And suppose that the fundamental law of the evolutions of those exact microconditions in time is completely deterministic. And suppose that the fundamental law of the evolutions of those exact microconditions in time entails that for any two times $t_1 < t_2$, the values of all of the fundamental physical degrees of freedom at t_2 are invariably some continuous function of the values of those degrees of freedom at t_1. If all that is the case, then it gets hard to imagine how $\{R\}$ could possibly fail to include a continuous infinity of distinct conditions in which the values of the elementary microscopic degrees of freedom happen to be lined up with one another in precisely such a way as to produce more or less any preposterous behavior you like—so long as the behavior in question is in accord with the basic ontology of the world, and with the conservation laws, and with the continuity of the final conditions as a function of the initial ones, and so on. And so the business of discounting such behaviors as implausible—the business (that is) of underwriting the most basic and general and indispensable convictions with which you and I make our way about in the world—is again going to call for something over and above the fundamental deterministic law of motion, something along the lines, again, of a probability distribution over microconditions.

If the fundamental microscopic dynamical laws *themselves* have chances in them, then (of course) all bets are off. But there are going to be chances—or that (at any rate) is what the above considerations suggest—at one point or another. Chances are apparently not to be avoided. An empirically adequate account of a world even remotely like ours in which nothing along the lines of a fundamental probability ever makes an appearance is apparently out of the question. And questions of precisely *where* and precisely *how* and in precisely *what form* such probabilities enter into nature are apparently going to need to be reckoned with in any serviceable account of the fundamental structure of the world.

2. The Case of Thermodynamics

Let's see what there is to work with.

The one relatively clear and concrete and systematic example we have of a fundamental probability distribution over microconditions being put to useful scientific work is the one that comes up in the statistical-mechanical account of the laws of thermodynamics.

One of the monumental achievements of the physics of the nineteenth century was the discovery of a simple and beautiful and breathtakingly concise summary of the behaviors of the temperatures and pressures and volumes and densities of macroscopic material systems. The name of that summary is thermodynamics—and thermodynamics consists, in its entirety, of two simple laws. The first of those laws is a relatively straightforward translation into thermodynamic language of the conservation of energy. And the second one, the famous one, is a stipulation to the effect that a certain definite function of the temperatures and pressures and volumes and densities of macroscopic material systems—something called the *entropy*—can never decrease as time goes forward. And it turns out that this second law in and of itself amounts to a complete account of the inexhaustible infinity of superficially distinct time asymmetries of what you might call ordinary macroscopic physical processes. It turns out—and this is something genuinely astonishing—that this second law in and of itself entails that smoke spontaneously spreads out from and never spontaneously collects into cigarettes, and that ice spontaneously melts and never spontaneously freezes in warm rooms, and that soup spontaneously cools and never spontaneously heats up in a cool room, and that chairs spontaneously slow down but never spontaneously speed up when they are sliding along floors, and that eggs can hit a rock and break but never jump off the rock and reassemble themselves, and so on, without end.

In the latter part of the nineteenth century, physicists like Ludwig Boltzmann in Vienna and John Willard Gibbs in New Haven began to think about the relationship between thermodynamics and the underlying complete microscopic science of elementary constituents of the entirety of the world—which was presumed (at the time) to be Newtonian mechanics. And the upshot of those investigations was a beautiful new science called *statistical mechanics*.

Statistical mechanics begins with a postulate to the effect that a certain very natural-looking measure on the set of possible exact microconditions of any classical-mechanical system is to be *treated* or *regarded* or *understood* or *put to work*—of this hesitation more later—as a *probability distribution* over those microconditions. The measure in question here is (as a matter of fact) the simplest *imaginable* measure on the set of possible exact microconditions of whatever system it is one happens to be dealing with, the standard *Lebeguse* measure on the *phase space* of the possible exact positions and momenta of the Newtonian particles that make that system up. And the thrust of all of the beautiful and ingenious arguments of Boltzmann and Gibbs, and of their various followers and collaborators, was to make it plausible that the following is true:

> Consider a true thermodynamical law, *any* true thermodynamical law, to the effect that macrocondition *A* evolves—under such-and-such external circumstances and over such-and-such a temporal interval—into macrocondition *B*. Whenever such a law holds, the overwhelming majority of the volume of the region of phase space associated with macrocondition *A*—on the above measure, the *simple* measure, the *standard* measure, of volume in phase space—is taken up by microconditions which are sitting on deterministic Newtonian trajectories which pass, under the allotted circumstances, at the end of the allotted interval, through the region of the phase space associated with the macrocondition *B*.

And if these arguments succeed, and if Newtonian mechanics is true, then the above-mentioned probability distribution over microconditions will underwrite great swaths of our empirical experience of the world: It will entail (for example) that a half-melted block of ice alone in the middle of a sealed average terrestrial room is overwhelmingly likely to be still more melted toward the future, and that a half-dispersed puff of smoke alone in a sealed average terrestrial room is overwhelmingly likely to be still more dispersed toward the future, and that a tepid bowl of soup alone in a sealed average terrestrial room is overwhelmingly likely to get still cooler toward the future, and that a slightly yellowed newspaper alone in a sealed average terrestrial room is overwhelmingly likely to get still more yellow toward the future, and uncountably infinite extensions and variations of these, and incomprehensibly more besides.

But there is a famous *trouble* with all this, which is that all of the abovementioned arguments work just as well in *reverse,* that all of the abovementioned arguments work just as well (that is) at making it plausible that (for example) the half-melted block of ice I just mentioned was more melted toward the *past as well.* And we are as sure as we are of anything that that's not right.

And the canonical method of *patching that trouble up* is to supplement the dynamical equations of motion and the statistical postulate with a new and explicitly *non*-time-reversal-symmetric fundamental law of nature, a so-called *past hypothesis,* to the effect that the universe had some particular, simple, compact, symmetric, cosmologically sensible, very low-entropy initial macrocondition. The patched-up picture, then, consists of the complete deterministic microdynamical laws and a postulate to the effect that the distribution of probabilities over all of the possible exact initial microconditions of the world is uniform, with respect to the Lebaguse measure, over those possible microconditions of the universe which are compatible with the initial macrocondition specified in the past hypothesis, and zero elsewhere. And with that amended picture in place, the arguments of Boltzmann and Gibbs will make it plausible not only that paper will be yellower and ice cubes more melted and people more aged and smoke more dispersed in the future, but that they were all *less so* (just as our experience tells us) in the past. With that additional stipulation in place (to put it another way) the arguments of Boltzmann and Gibbs will make it plausible that the second law of thermodynamics remains in force all the way from the end of the world back to its beginning.

What we have from Boltzmann and Gibbs, then, is a probability distribution over possible initial microconditions of the world which—when combined with the exact deterministic microscopic equations of motion—apparently makes good empirical predictions about the values of the thermodynamic parameters of macroscopic systems. And there is a question about what to *make* of that success: We might take that success merely as evidence of the *utility* of that probability distribution as an instrument for the particular purpose of predicting the values of those particular parameters, or we might take that success as evidence that the probability distribution in question is literally *true*.

And note—and this is something to pause over—that if the probability distribution in question were literally true, and if the exact deterministic microscopic equations of motion were literally true, then that probability distribution, combined with those equations of motion, would necessarily amount not merely to an account of the behaviors of the thermodynamic parameters macroscopic systems, but to *the complete scientific theory of the universe*—because the two of them together assign a unique and determinate probability value to every formulable proposition about the exact microscopic physical condition of whatever physical things there may happen to be. If the probability distribution and the equations of motion in question here are regarded not merely as instruments or inference tickets but as claims about the world, then there turns out not to be any physical question whatsoever on which they are jointly agnostic. If the probability distribution and the equations of motion in question here are regarded not merely as instruments or inference tickets but as claims about the world, then they are either false or they are in some sense (of which more in a minute) all the science there can ever be.

And precisely the same thing will manifestly apply to *any* probability distribution over the possible exact microscopic initial conditions of the world, combined with *any* complete set of laws of the time evolutions of those macroconditions.[2] And this will be worth making up a name for.

2. Sheldon Goldstein and Detlef Dürr and Nino Zhangi and Tim Maudlin have worried, with formidable eloquence and incisiveness, that probability distributions over the initial conditions of the world might amount to vastly more information than we could ever imaginably have a legitimate epistemic right to. Once we have a dynamics (once again), a probability distribution over the possible exact initial conditions of the world will assign a perfectly definite probability to the proposition that I am sitting precisely here writing precisely this precisely now, and to the proposition that I am doing so not now but (instead) 78.2 seconds from now, and to the proposition that the Yankees will win the world series in 2097, and to the proposition that the Zodiac Killer was Mary Tyler Moore, and to every well-formed proposition whatsoever about the physical history of the world. And it will do so as a matter of fundamental physical law. And the worry is that it may be mad to think that there could be a fundamental physical law as specific as that, or that we could ever have good reason to *believe* anything as specific as that, or that we could ever have

Start (then) with the initial macrocondition of the universe. Find the probability distribution over all of the possible exact microconditions of the universe which is uniform, with respect to the standard statistical-mechanical measure, over the subset of those microconditions which is compatible with that initial macrocindition, and zero elsewhere. Evolve that distribution forward in time, by means of the exact microscopic dynamical equations of motion, so as to obtain a definite numerical assignment of probability to

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good reason to believe anything that logically *implies* anything as specific as that, even if the calculations involved in spelling such an implication out are prohibitively difficult.

Moreover, there are almost certainly an enormous number of very different probability distributions over the possible initial conditions of the world which are capable of underwriting the laws of thermodynamics more or less as well as the standard, uniform, Boltzmann–Gibbs distribution does. And the reasons for that will be worth rehearsing in some detail.

Call the initial macrocondition of the world M. And let R_M be that region of the exact microscopic phase space of the world which *corresponds* to M. And let ${}^aR_M$ be the subregion of R_M which is taken up with "abnormal" microconditions—microconditions (that is) that lead to anomalously widespread violations of the laws of thermodynamics. Now, what the arguments of Boltzmann and Gibbs suggest is (as a matter of fact) not only that the familiarly calculated volume of ${}^aR_M$ is overwhelmingly *small* compared with the familiarly calculated volume of R_M—which is what I have been at pains to emphasize so far—but also that ${}^aR_M$ is *scattered,* in unimaginably tiny clusters, more or less at random, all over R_M. And so the percentage of the familiarly calculated volume of any regularly shaped and *not* unimaginably tiny subregion of which is taken up with abnormal microconditions will be (to an extremely good approximation) the same as the percentage of the familiarly calculated volume of R_M as a whole which is taken up by ${}^aR_M$. And so any reasonably smooth probability distribution over the microconditions in R_M—any probability distribution over the microconditions in R_M (that is) that varies slowly over distances two or three orders of magnitude larger than the diameters of the unimaginably tiny clusters of which ${}^aR_M$ is composed—will yield (to an extremely good approximation) the same overall statistical propensity to thermodynamic behavior as does the standard uniform Boltzmann–Gibbs distribution over R_M as a whole. And exactly the same thing, or much the same thing, or something in the neighborhood of the same thing, is plausibly true of the behaviors of pinballs and adrenal glands and economic systems and everything else as well.

The suggestion (then) is that we proceed as follows: Consider the *complete set* of those probability distributions over the possible exact initial conditions of the world—call it $\{P_f\}$—which can be obtained from the uniform Boltzmann–Gibbs distribution over R_M by multiplication by any relatively smooth and well-behaved and appropriately normalized function f of position in phase space. And formulate your fundamental physical theory of the world in such a way as to commit it to the truth of all those propositions on which every single one of the probability distributions in $\{P_f\}$, combined with the dynamical laws, *agree*—and to leave it resolutely agnostic on everything else.

If everything works as planned, and if everything in the paragraph before last is true—a theory like that will entail that the probability of smoke spreading out in a room, at the usual rate, is very high, and it will entail that the probability of a fair and well-flipped coin's landing on heads is very nearly ½, and it will entail (more generally) that all of the stipulations of the special sciences are very nearly true. And yet (and this is what's different, and this is what's cool) it will almost every formulable proposition about the physical history of the world. And call that latter assignment of probabilities the *Mentaculus*.

I want to look into the possibility that the probability distribution we have from Boltzmann and Gibbs, or something like it, something more up-to-date, something adjusted to the ontology of quantum field theory or quantum string theory or quantum brane theory, is true.

And this is a large undertaking.

Let's start slow.

Here are three prosaic observations.

The laws of thermodynamics are not quite true. If you look closely enough, you will find that the temperatures and pressures and volumes of macroscopic physical systems occasionally fluctuate away from their thermodynamically predicted values. And it turns out that precisely the same probability distribution over the possible microconditions of such a system that accounts so well for the overwhelming reliability of the laws of thermodynamics accounts for the relative frequencies of the various different possible transgressions *against* those laws *as well.* And it turns out that the particular *features* of that distribution that play a pivotal role in

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entirely abstain from the assignment of probabilities to universal initial conditions. It *will* entail—and it had *better* entail—that the probability that the initial condition of the universe was one of those that lead to anomalously widespread violations of the laws of thermodynamics, that the probability that the initial condition of the universe lies (that is) in ${}^{a}R_M$, is overwhelmingly small. But it is going to assign *no probabilities whatsoever* to any of the *smoothly bounded* or *regularly shaped* or *easily describable* proper subsets of the microconditions compatible with M.

Whether or not a theory like that is ever going to look as simple and as serviceable and as perspicuous as the picture we have from Boltzmann and Gibbs (on the other hand) is harder to say. And (anyway) I suspect that at the end of the day it is *not* going to spare us the awkwardness of assigning a definite probability, as a matter of fundamental physical law, to the proposition that the Zodiac Killer was Mary Tyler Moore. I suspect (that is) that *every single one* of the probability distributions over R_M that suffice to underwrite the special sciences are going end up assigning very much the *same* definite probability to the proposition that the Zodiac Killer was Mary Tyler Moore as the standard, uniform, Boltzmann–Gibbs distribution does. And if that's *true,* then a move like the one being contemplated here may end up buying us very little.

And beyond that, I'm not sure what to say. Insofar as I can tell, our present business is going proceed in very much the same way, and arrive at very much the same conclusions, whether it starts out with the standard, uniform, Boltzmann–Gibbs probability distribution over the microconditions in R_M, or with any other particular one of the probability distributions in $\{P_f\}$, or with $\{P_f\}$ as a whole. And the first of those seems by far the easiest and the most familiar and the most intuitive and the most explanatory and (I guess) the most advisable. Or it does at first glance. It does for the time being. It does unless, or until, we find it gets us into trouble.

accounting for the overwhelming reliability of the laws of thermodynamics are largely *distinct* from the particular features of that distribution that play a pivotal role in accounting for the relative frequencies of the various possible transgressions *against* those laws. It turns out (that is) that the relative frequencies of the transgressions give us information about a different *aspect* of the underlying microscopic probability distribution (if there is one) than the overwhelming reliability of the laws of thermodynamics does, and it turns out that both of them are separately *confirmatory* of the empirical rightness of the distribution as a whole.

And consider a speck of ordinary dust, large enough to be visible with the aid of a powerful magnifying glass. If you suspend a speck like that in the atmosphere, and you watch it closely, you can see it jerking very slightly, very erratically, from side to side, under the impact of collisions with individual molecules of air. And if you carefully keep tabs on a large number of such specks, you can put together a comprehensive statistical picture of the sorts of jerks they undergo—as a function (say) of the temperatures and pressures of the gasses in which they are suspended. And it turns out (again) that precisely the same probability distribution over the possible microconditions of such a system that accounts so well for the overwhelming reliability of the laws of thermodynamics accounts for the statistics of those jerks too. And it turns out (again) that the particular features of that distribution that play a pivotal role in accounting for the reliability of the laws of thermodynamics are largely distinct from the particular features of that distribution that play a pivotal role in accounting for the statistics of the jerks. And so the statistics of the jerks give us information about yet *another* aspect of the underlying microscopic probability distribution (if there is one), and that new information turns out to be confirmatory, yet again, of the empirical rightness of the distribution as a whole.

And very much the same is true of isolated pinballs balanced atop pins, or isolated pencils balanced on their points. The statistics of the directions in which such things eventually *fall* turn out to be very well described by precisely the same probability distribution over possible microconditions, and it turns out (once more) that the particular features of that distribution that play a pivotal role in accounting for the reliability of the laws of thermodynamics are distinct from the particular features of that distribution that play a pivotal role in accounting for the statistics those fallings.

And so the standard statistical posit of Boltzmann and Gibbs—when combined with the microscopic equations of motion—apparently has in it not only the *thermodynamical* science of *melting,* but also the *quasi*-thermodynamical science of chance fluctuations *away* from normal thermodynamic behavior, and (on top of that) the *quasi*-mechanical science of *unbalancing,* of *breaking the deadlock,* of *pulling infinitesimally harder this way or that.* And these sorts of things are manifestly going to have tens of thousands of other immediate applications. And it can now begin to seem plausible that this standard statistical posit might in fact have in it the entirety of what we mean when we speak of anything's happening *at random* or *just by coincidence* or *for no particular reason.*

3. The Special Sciences in General

The upshot of the previous section was a picture of the world on which the fundamental laws of physics amount, in some principled sense, to all the science there can ever be. And the literature of the philosophy of science is awash in famous objections to pictures like that. And the business of coming to terms with those objections, in their oceanic entirety, is altogether beyond the scope of an essay like this.

But maybe it will be worth at least gesturing in the direction of two or three of them.

i. Translation

According the picture sketched out in the previous section, the special sciences must all, in some principled sense, be *deducible* from the fundamental laws of physics. And it is an obvious condition of the possibility of even *imagining* a deduction like that that the languages of the special sciences can at least in some principled sense be *hooked up* with the language of the fundamental laws of physics. The business of reducing thermodynamics to Newtonian mechanics (for example) depends crucially on the fact that thermodynamic parameters like pressure and temperature and volume all have known and explicit and unambiguous Newtonian-mechanical correlates. And the worry is that that's not the case, and that perhaps it will *never* be the case, and that perhaps it *can* never be the case, even as a matter of principle, for (say) economics, or epidemiology, or semiotics.

And the cure for that worry, it seems to me, is merely to reflect on the fact that that there are such things in the world, that there are such *concrete embodied physical systems* in the world, as competent speakers of the various languages of economics, and epidemiology, and semiotics, and whatever other special sciences may happen to amount, at present, to going and viable concerns. There are physical systems in the world (that is) which are capable of distinguishing, in a more or less reliable way, under more or less normal circumstances, between those possible fundamental physical situations of the universe in which there is (say) a flu going around, and those in which there isn't. And so there *must* be a fully explicit and fully mechanical technique for coordinating epidemiological situations with their fundamental physical equivalents—or (at any rate) for doing so in a more or less reliable way, under more or less normal circumstances—because there are (after all) mechanical devices around, right now, that can actually, literally, *get it done.*

The thought (in slightly more detail) is this: Insofar as there is any such thing in the world as an actual, practicable, empirically confirmable, well-functioning science of epidemiology, it must be the case that there are actual, identifiable, physical systems—call them *E*-systems (epidemiologists, say, or teams of epidemiologists, or teams of epidemiologists with clipboards and thermometers, or something like that)—which are capable, under more or less normal circumstances, of more or less reliably bringing it about that there is an "*X*," at t_2, in the box marked "there was a flu going around at t_1," if and only if there *was,* in fact, a flu going around at t_1. And note that whether or not there *is* an "*X*" in some particular box at t_2 is the sort of thing that manifestly *can* be read off of the values of the *fundamental physical variables of the world* at t_2. And note that the sorts of fundamental dynamical laws that we have been thinking about here entail that the values of all of the fundamental physical variables of the world at t_2 are fully and completely and exclusively and exhaustively *determined* by the values of all of the fundamental physical variables of the world at t_1. And so—insofar as there *is* some region of the fundamental physical phase space of universe in which there are any such physically embodied things as *E*-systems—it *must* be the case, throughout that region, that the distinction between a flu going around and a flu *not* going around corresponds to some difference in the values of certain *fundamental physical variables.* And it follows that an ideal, scientifically impossible, infinitely fast, logically omniscient computer, equipped only with the fundamental laws of physics, and with a fundamental physical description of an *E*-system, will in principle be capable of determining, by pure calculation, precisely *what those correspondences are.* It follows (that is) that an ideal, scientifically impossible, infinitely fast, logically omniscient computer, equipped only with the fundamental laws of physics, and with a fundamental physical description of an *E*-system, will in principle be capable of producing, by pure calculation, a *manual of translation* from the language of fundamental physics to the language of epidemiology—a manual which is exactly as reliable, and which is reliable across exactly the same region of the fundamental physical phase space, as is the *E*-system in question itself.

ii. Explanation

There are other worries about reduction—worries of a different kind than the ones we have just been talking about—that have to do with questions of explanation.

Suppose (then) that we put aside the sorts of worries that were raised in (i). Suppose (that is) that we are willing to grant, at least for the sake of the present conversation, that every special-scientific term has, at least at the level of principle, some more or less explicit and unambiguous *translation* into the language of (say) Newtonian mechanics. Then it's going to follow—supposing (of course) that Newtonian mechanics is *true*—that the outcome of any particular special-scientific procedure or experiment or observation can in principle be *deduced* from the Newtonian-mechanical laws and initial conditions, and that any particular event, described in any special-scientific language you like, can in principle be given a complete Newtonian-mechanical *explanation,* and (most importantly) that every successful *special-scientific* explanation can in principle be *translated* into a *Newtonian-mechanical* one.

And there are a number of different ways of worrying that the resultant Newtonian-mechanical explanations are nevertheless somehow *missing* something, that the special-scientific explanations are somehow *better* or *deeper* or more *informative* than the Newtonian-mechanical ones, that the business of translating the special-scientific explanations into Newtonian-mechanical explanations invariably and ineluctably involves some kind of a *loss*.

One way of making a worry like that explicit—this is associated with figures like Hilary Putnam and Jerry Fodor—has to do with the so-called multiple realizability of the special sciences.[3] Here's the idea: There must

3. See, for example, J. Fodor, "Special Sciences," *Synthese*, Vol. 28, No. 2 (Oct. 1974), pp. 97–115, and H. Putnam, "Reductionism and the Nature of Psychology," *Cognition* 2 (1973), pp.131–146.

be many logically possible worlds, with many different fundamental microphysical laws, in which all of the terms in the vocabulary of epidemiology happen to have referents, and in which (moreover) all of the laws and principles of epidemiology happen to come out true. And it follows that epidemiological explanations of particular epidemiological phenomena, where both the explanation and the phenomenon to be explained are described in epidemiological *language,* are going to be exactly as successful in all of those *other* worlds as they are in the one that we actually happen to *live* in—whereas (of course) the Newtonian-mechanical *translations* of those explanations are only going to apply to our *own*. So (the argument goes) the genuinely epidemiological explanations tell us something much deeper and more general and more enlightening and more to the point about how it is that people get sick than their translations into Newtonian mechanics do.

But something's funny about all this.

Consider (for example) the laws of thermodynamics. The relationship of thermodynamics to Newtonian mechanics is generally held up as a paradigmatic example—or (rather) as *the* paradigmatic example—of a successful, straightforward, intertheoretic reduction. But there are obviously any number of possible worlds, with any number of different fundamental physical laws, in all of which the laws of thermodynamics come out true.

Or consider the conservation of energy. I take it that everybody is going to agree that there is no autonomous and independent and irreducible special science of energy. I take it (that is) that everybody is going to agree that the science to which the principle of the conservation of energy properly and unambiguously *belongs;* I take it that everybody is going to agree that the science from which our deepest and most illuminating and most satisfactory understanding of the truth of that principle properly and unambiguously *derives,* can be nothing other than fundamental physics. But the conservation of energy can obviously be *realized,* the conservation of energy can obviously be *underwritten,* by any number of distinct sets of fundamental laws of physics—laws which will in many cases be radically different, in any number of other respects, from our own!

Maybe this is worth belaboring a little further. Suppose that we that we would like to calculate the difference between the total energy of the world at t_1 and its total energy at some later time t_2. Here are two ways of doing that calculation: We could calculate the energy of the world at t_1 by plugging the values of the position and the momentum of each of the particles