Anti Anti-Determinism:
Or What Happens When Schrödinger's Cat and Lorenz's Butterfly Meet Laplace's Demon in the Study of Political and Economic Development

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Given for one instant an intelligence which could comprehend all the forces by which nature is animated and the respective situation of the beings who compose it—an intelligence sufficiently vast to submit these data to analysis—it would embrace in the same formula the movements of the greatest bodies in the universe and those of the lightest atom: for it, nothing would be uncertain and the future, as the past, would be present to its eyes.[1]

The central insight of chaos theory—that systems governed by mathematically simple equations can exhibit elaborately complex, indeed unpredictable, behavior—is rightly seen as new and important. But sometimes there may be a temptation for researchers to hype their results, to make chaos theory sound too interesting, as if it will revolutionize our thinking not just about the physical world but about art and economics and religion as well. Considering that quantum mechanics was at least as revolutionary more than sixty years ago and that its cultural effects are still quite difficult to pin down, any grand claims about the implications of chaos theory may be setting us up for disappointment.[2]

The Copenhagen [interpretation of quantum mechanics], while necessary for the atomic world, does not have to be applied to the world of everyday objects. Those who do apply it to the macroworld do so gratuitously.[3]

1. Introduction

The first of the three epigraphs heading this essay is taken to be the classic statement of determinism. Cited in virtually every philosophical discussion of determinism, Laplace's depiction of a "demon" whose powers of data collection and computation allowed him to know all past and future states of the universe with absolute certainty powerfully evoked the confident tenor of its time. This vision of a deterministic universe represented by classical physics fared poorly in the twentieth century. Although Newton's laws of motion are still extremely valuable and can be used for many purposes, they do not

[1] Pierre-Simon Laplace, as quoted in John Earman, "Determinism in the Physical Sciences," in Wesley Salmon et. al., *Introduction to the Philosophy of Science* (Indianapolis: Hackett Publishing Company, 1992), 241

[2] Stephen H. Kellert, *In the Wake of Chaos* (Chicago: University of Chicago Press, 1993), ix-x.

[3] Heinz Pagels, *The Cosmic Code: Quantum Physics as the Language of Nature* (London: Penguin Books, 1982), 140.

exhaust the physical description or explanation of the mechanics of motion. At speeds approaching the velocity of light, for example, the special theory of relativity must be employed. But Einstein's reconceptualization of how space and time interact still rests comfortably within the classical framework: Einstein was the last classical physicist, one who famously resisted the probabilistic vision of reality depicted by quantum mechanics. Subsequent developments have more seriously undermined the classical vision of reality. The equations of quantum mechanics describe reality at the atomic level: those equations are irreducibly indeterministic. Even in the world of everyday objects moving at everyday velocities, research into chaotic phenomena provides a powerful demonstration of the limits of Laplacean determinism, because errors in measurement, however miniscule, are both inevitable and yield impressively large consequences: at the level of ordinary experience, small and unpredictable events have large and unpredictable consequences. While special relativity has been personified by a single iconic figure, quantum mechanics and chaos theory are represented less anthromorphically but no less poetically: Laplace's omniscient demon has been defeated by the twinned assault of a cat and a butterfly.

We should expect that the collapse of Newtonian physics would ramify widely in the social sciences. Newton's *Principia Mathematica* was the pinnacle of the Scientific Revolution, whose cultural, social, and political influences on modern political thought and practice is abundantly documented. Closer to our ivory tower homes, James Murphy has brilliantly demonstrated how rational choice models in political science have imported from microeconomics not only its formal definitions and methodologies, but also its tacit understandings of social processes that are deeply informed by classical mechanics. The effect of this borrowing, Murphy argues, is that "ultimately, rational choice theory is based on an analogy between the issue-space of political deliberation and the vector-space of classical mechanics, between the trajectory of agenda-setting and the trajectory of particles."[4] Not surprisingly, as the central pillars of classical mechanics have been removed, it has been rational-choice theorists who have played a correspondingly large role in introducing ideas of indeterminism and contingency into the qualitative study of politics. If the arguments of these and other scholars, all discussed extensively below, are correct, then the social sciences should join the physical sciences and banish determinism.

The second and third of the three epigraphs heading this essay, however, might give us some reason to pause before confidently dismissing determinism in the social sciences. Kellert's work on chaos theory and Pagels' survey of quantum mechanics both take great pains to delineate the boundary conditions governing the application of the two frameworks. Chaos theory provides general mathematical ideas for the study of highly idealized models of physical processes evincing specific features that are simply not relevant to social and political processes. At most, chaos theory can inspire a search for sources of unpredictability in political life: it can neither identify those sources of unpredictability nor provide tools for analyzing them. Quantum mechanics refers to the even more tightly bounded realm of atomic and subatomic particles. Although it is tempting to think that all macrolevel phenomena can be ultimately reduced to microlevel properties and processes so that microlevel probability translates into macrolevel probability, that intuition is simply false. Quantitative considerations of size and

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[4] James Bernard Murphy, "Rational Choice Theory as Social Physics," in Jeffrey Friedman, ed., *The Rational Choice Controversy* (New Haven: Yale University Press, 1995), 157.

qualitative considerations of the direction of time both point unambiguously to the same conclusion: microlevel indeterminism is consistent with macrolevel determinism. A large portion of this paper is dedicated to explicating the limitations of chaos theory and quantum mechanics as guides to the study of political science.

But the Roman poet Lucretius did not need quantum mechanics to argue that the spontaneity that exists in the atomic world holds determinism at bay and permits free will[5]; nor did Machiavelli need chaos theory to hold as true "that Fortune is the arbiter of one half of our actions, but that she still leaves the control of the other half, or almost that, to us."[6] By the same token, contemporary scholars can argue that small and contingent events have huge and enduring consequences without reference to the mathematical properties of nonlinear dynamical systems or that individual action is underdetermined by environmental conditions and is thus irreducibly probabilistic without reference to indeterminate states and the collapse of wave functions. A heterodox collection of arguments, in other words, can take seriously the specific boundary conditions defining political science and yet still argue that chance and probabilistic processes play a paramount role.

A second portion of this paper addresses the shortcomings of these arguments without concluding that they are irremediable. There is surprisingly little discussion of the methodological dimensions of attributing large outcomes to small effects. All too often, the existence of contingent causes is taken for granted and the identification of

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[5] The great work of the Epicurean poet Lucretius is *On the Nature of the Universe*, translated by Sir Ronald Melville (Oxford: Clarendon Press, 1997). The key section on the random combination and recombination of atoms in constituting our world is in Book One at 1021.

[6] Machiavelli, *The Prince*, in Peter Bondanella and Mark Musa, eds., *The Portable Machiavelli* (Hammondsworth, England: Penguin Books, 1979), 159. Alisdair MacIntyre expresses a strong preference for a Machiavellian-inspired political science over a Hobbesian mechanistic model in his essay "Is a Theory of Comparative Politics Possible," in Alan Ryan, ed., *The Philosophy of Social Explanation* (Oxford: Oxford University Press, 1973), 171-88.

specific contingent causes is taken as straightforward and unproblematic. I argue that four generic issues must be confronted before arguments linking small causes to large effects can be granted validity: these are the problems of causal transitivity, causal confirmation, causal depth, and causal substitution.

Arguments on behalf of the probabilistic nature of social and political processes, in contrast, are well developed and defended. I have two responses to these arguments, one epistemological and one ontological. First, in contrast to many existing arguments about causal mechanisms, I argue that shifting our explanatory standards from laws to causal mechanisms contains a great deal of microlevel probability to the microlevel. Second, drawing on the field of statistical mechanics, I demonstrate that the degree of consistency of between microlevel probability and macrolevel determinism is a function of our correspondence rules for linking micro- to macrostates. These correspondence rules, in turn, are highly sensitive to the level of description we apply to our objects of explanation. Drawing on Alan Garfinkel's idea of an equivalence class, of essentially similar instantiations of a general phenomenon, I claim that determinism is more plausible when the equivalence class is constructed with relatively permissive boundaries so that particularizing detail is washed out. Taken together, and only when taken together, the epistemological and the ontological argument prompt the conclusion that demonstrating and theorizing randomness at the microlevel is a necessary but not a sufficient condition for demonstrating probability at the macrolevel. Determinism at the macrolevel is not necessarily destroyed by arguments about indeterminism at the microlevel.

The two major portions of the paper summarized briefly above seek to deliver on the promise of the title: they support an anti anti-determinist position. They do not reject anti-determinism whole cloth, but rather suggest some ways that anti-deterministic arguments could even be strengthened. What they reject is an a priori and totalistic rejection of determinism.

The third and final major portion of the paper goes somewhat further, making positive arguments supporting a deterministic perspective. Let me hasten to state that this portion of the paper does not argue for universal determinism but rather for what I call below segmented determinism: under some specified conditions, we can make arguments about the necessary and sufficient causes of outcomes so that our explanations will be complete without reference to chance mechanisms or contingent events. I advance these arguments in the spirit of defending macrostructural analyses. Macrostructural scholarship, as I conceive it in this paper, builds on Marx's famous dictum that "Men make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly found, given, and transmitted from the past."[7] Like Marx, macrostructural scholars build their explanations on large-scale and relatively permanent conditions of social action that are resistant to short-term change by intentional actors. These structures both constrain actors, eliminating certain options, but they also act as generative mechanisms that prompt microlevel behavior and thus produce observed associations between macrolevel phenomena.[8] The relevant set of structural conditions may be heterogeneous, embracing

[7] Karl Marx, "The Eighteenth Brumaire of Louis Bonaparte," in Robert Tucker, ed., *The Marx-Engels Reader* 2nd ed. (New York: W.W. Norton & Company, 1978), 595.
[8] I thus follow Ira Katznelson in distinguishing between macrostructural scholarship and the recent literature on historical institutionalism. See his "Structure and Configuration in Comparative Politics," in

structures of economic domination, coercion, and administration, but macrostructural scholars coalesce around the position that these structures (usually) do (almost) all of the explaining at a particular level of description.[9] Arguments from chaos theory and quantum mechanics—from small contingent causes and irreducible indeterminism—have correctly been perceived to be a necessary and welcome antidote to macrostructural scholarship in either its class-analytic or its more heterodox variants. The defense of determinism detailed below is what I believe to be a necessary defense of macrostructural scholarship.

The arguments I make on behalf of the possibility of determinism are in part inspired by a subfield of statistical mechanics called equilibrium statistical mechanics. While statistical mechanics renders compatible microlevel heterogeneity and macrolevel determinism, it does so by identifying microlevel processes that constitute the macrolevel. Equilibrium statistical mechanics, as exemplified by the seemingly simple study of boiling or freezing water, on the other hand, considers how a global condition—a restriction on the macrolevel state of a system—may uniquely determine the microlevel structure without eliminating its probabilistic features. The probabilistic configurations of the microlevel then correspond to a macroscopic state. Lowering the temperature of water does not determine the exact configuration of each and every molecule, but it does

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Mark Irving Lichbach and Alan S. Zuckerman, eds., *Comparative Politics: Rationality, Culture, and Structure* (Cambridge: Cambridge University Press, 1997), 81-112. Katznelson believes that the way to revive macroanalytic scholarship is to discard determinism and move to more configurative approaches. My comments below suggest both points of overlap but I believe more fundamentally points of divergence. For my defense of macrostructuralism and critique of historical institutionalism, see "From Intra-Type Variation to the Origins of Types: Recovering the Macroanalytics of State Formation," unpublished paper, University of Virginia, April 2002. Note that in this essay I use the terms macroanalytic and macrostructural synonymously.

[9] The parenthetical reservations are meant only to reinforce the point that I defend segmented, not universal, determinism. A second disclaimer is that my arguments for determinism are not necessarily transferable; I do not claim, that is to say, that the processes underlying deterministic conceptions of causality that I identify here will necessarily be those identified by other structuralists.

determine that those molecules will combine in the crystalline structure we call ice. Whereas statistical mechanics begins from microlevel probability and moves to macrolevel determinism, in other words, equilibrium statistical mechanics starts from the macrolevel and moves via the microlevel back to the macrolevel, all the while maintaining the compatibility of unit-level probability with system-level determinism. In other words, probabilistic features at the microlevel are not necessarily autonomous of global conditions; and they may uniquely determine macrolevel states of the system.

Neither statistical mechanics nor equilibrium statistical mechanics provide a model or an analogy for social processes: the types of processes and inter-level linkages they posit simply make no sense for thinking about politics. Their role in the argument is to demonstrate the plausibility, in at least some domains, of reconciling determinism and probability. What I offer in defense of determinism in macrostructural analysis is a discussion of what conditions a deterministic argument must fulfill to be considered valid, and a picture of what it might look like to meet those requirements. These conditions are:

> 1. That equivalence classes be permissively construed, so that objects of explanation are considered "essentially similar," a great deal of particularizing detail is excluded, and the set of macrostates M_1 to M_n is limited, with a binomial opposition as the limiting case.

Condition 1 is a purely pragmatic consideration. Scholars are free to define their objects of explanation with whatever level of specificity they choose. But it must be clear that at least some of the debate about determinism is really a debate about the pragmatics of constructing objects of explanation and is thus not a debate about underlying social ontology.

> 2. That these macrostates are linked to microstates m_i to m_j by a set of correspondence rules. In the limiting case, these rules specify that the vast majority of these microstates are contained within a subset, m_k that corresponds to one and only one of the macrostates, M_1.

The first sentence of Condition 2 is a requirement for statements of the micro-macro link, such as aggregation rules in rational choice theory. The second sentence of Condition 2 states the essence of statistical mechanics. If you pour a quart of warm water on top of a quart of cold water, the two will always mix, producing lukewarm water. This outcome is, for all intents and purposes, predetermined even thought there is tremendous probability at the microlevel of individual molecules of water, which can be configured in an astonishingly large number of ways. Almost all of those configurations of individual molecules yield lukewarm water. In principle, the initial state of warm water resting atop cold water can recur; the odds against this are staggering. Note that in this example, the possible macrostates are limited to two: the complete separation of uniformly hot and uniformly cold water, and a lukewarm mixture.

Condition 2 is a shortcut to a deterministic argument: if it is fulfilled, the argument is valid and deterministic. Although it is possible to think of social processes that meet conditions 1 and 2, it is hard to do so. Making this project relevant for social processes requires two additional conditions. The first of these conditions accepts the argument about microlevel heterogeneity:

> 3. That there is a global condition—a macrostructure, S_1—whose effects are not uniform; S_1 is sufficient to generate, within that heterogeneous set of effects, a subset of the total set of microstates, m_{k^*}.

Condition 3 fully accepts the core of the anti-structural ontological argument: any given structure will generate diverse behavior among those agents influenced by it. Microlevel heterogeneity may occur for reasons that are purely random; or, more likely, that heterogeneity will occur because agents are exposed to multiple influences, some of which cannot be detected or measured. Condition 3 accepts, in other words, Mark Granovetter's critique of "under- and over-socialized" views of agency. Agents are embedded in multiple networks and are exposed to multiple structures; their reaction to singular structural effects such as changes in relative prices is therefore not uniform and predictable.[10] Condition 3 simply wagers the bet that among that set of heterogeneous modes of behavior will be microstate m_{k*}. Condition 3 sounds like a metaphysical assumption, but in fact it is a substantive and fully falsifiable proposition.

Condition 3 links an initial structural condition to a microstate; in order to explain the phenomenon of interest, that microstate must in turn be linked to the object of explanation, macrostate M_1. In order for that final linkage to be one of necessity so that the entire argument is deterministic, condition 4 must be fulfilled:

> 4. That within the total set of microstates m_i to m_j, there is a "causally privileged subclass," m_{k*}, which is sufficient to generate macrostate M_1, despite the presence of other microstates corresponding to other macrostates.

Condition 4 is a formal requirement: it defines the possibility of macrolevel determinism in a social world characterized by microlevel indeterminism. If and only if m_{k*} exists can we argue that macrolevel determinism is possible. Condition 4 is a metatheoretical

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[10] Mark Granovetter, "Economic Action and Social Structure: The Problem of Embeddedness," *American Journal of Sociology* 91 (November 1985): 481-510. Richard Snyder and James Mahoney base a significant chunk of their critique of structruralist approaches to regime change on Granovetter's analysis. See their "Rethinking Agency and Structure in the Study of Regime Change," *Studies in Comparative International Development* 34 (Summer 1999): 3-32.

hypothesis with observable and falsifiable consequences: whether such a sub-set of microstates exists cannot be treated solely as a metatheoretical issue, but rather must be considered in terms of theory, research design, and evidence.

To summarize, and close this introduction, condition 1 is purely pragmatic; condition 2 restates statistical mechanics and thus merely restates the possibility of rendering microlevel heterogeneity and probability with microlevel determinism. These conditions should thus be acceptable, although their relevance is not yet clear. Condition 3 states that while we cannot know how everyone will behave, we can know that under some conditions, some people will behave in specified ways. This is not at all a controversial claim; it is, after all, the basis of statistical reasoning. In his famous study of the social determinants of suicide, Durkheim demonstrated regular associations between social conditions and rates of suicide. Durkheim was fully aware of heterogeneity at the individual level; he wrote of the force external to individuals that it "does not determine one individual rather than another. It exacts a definite number of certain kinds of actions, but not that they should be performed by this or that person. It may be granted that some people resist the force and that it has its way with others."[11] Durkheim is saying, in our language, that S_1 has heterogeneous effects, but that among those effects it is almost guaranteed that m_{k*}--a defined rate of suicide—exists.

All of the action takes place in condition 4. Condition 4 essentially takes a statistical regularity and transforms it into a cause sufficient for a macrolevel outcome. It is as if Durkheim wrote a second book which argued that suicide rates of a defined level were sufficient to cause regime type or level of economic development or some other macrolevel variable. Conditions 1 through 3 are easy to fulfill; adding condition 4 to the

[11] Emile Durkheim, *Suicide: A Study in Sociology* (New York: Free Press, 1951), 325 at n. 20.

mix generates a valid deterministic argument, one that recognizes and builds upon the inescapable fact of probabilistic heterogeneity at the microlevel.

The paper is organized as follows. The next section briefly surveys some of the arguments against determinism in the social sciences. A third section answers the question "What is determinism" by making several distinctions that make determinism more intuitively plausible. A fourth section then explains why determinism is so critical to macrostructural analysis by considering three issues: methodology, ontology, and epistemology. The fifth section looks at chaos theory, arguing that its implications are more limited than is usually imagined and that they are strictly confined to domains that make them largely irrelevant to the concerns of social scientists. A sixth section looks at heterodox arguments for contingency: here I raise the issues of causal transitivity, causal confirmation, causal depth, and causal substitution. The seventh section looks at quantum mechanics as an argument for irreducible indeterminism and the eighth section explains why those arguments are irrelevant to the macrolevel. The ninth section introduces the core idea of equilibrium statistical mechanics and uses that idea to consider two powerful arguments for irreducible indeterminism that are relevant to the macrostructural arguments, those of Stanley Lieberson and Jon Elster. The tenth section attempts to respond to those arguments by elaborating conditions 1 through 4 above.

2. Determinism and Its Discontents

The two-pronged attack on determinism in the physical sciences has influenced, directly or indirectly, discussions of determinism in the social sciences. Over the past quarter-century, political scientists and sociologists have seized on the victory of post-classical physics to advocate programmatic changes in their discipline's foundational orientations. In 1977, Gabriel Almond and Stephen Genco called on their colleagues to reject the deterministic conceptualization of political reality as a clock (and also the fully indeterministic conceptualization of it as a cloud) in favor of a model of plastic controls, one that captures the creative and emergent properties of politics that follow from the fact that "actors in politics have memories: they learn from experience. They have goals, aspirations, calculative strategies. Memory, learning, goal seeking, and problem solving intervene between 'cause' and 'effect,' between independent and dependent variable."[12] A few years later, Stanley Lieberson drew on studies of "amplification effects" in the natural sciences, today popularized as chaos theory, as well as the irreducible indeterminism that is the hallmark of quantum mechanics, to question the validity of deterministic arguments that attribute inevitability to observed outcomes. Chance processes, he argues, are so widespread as to provoke deep suspicions about simple deterministic arguments.[13] More recently, James Fearon has fortified this position, arguing based on a loose analogy to cellular automata that when transition rules are

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[12] Gabriel A. Almond and Stephen J. Genco, "Clouds, Clocks, and the Study of Politics," *World Politics* 29 (July 1977), 492.

[13] Stanley Lieberson, *Making it Count: The Improvement of Social Research and Theory* (Berkeley: University of California Press, 1985), 225-27; "Small N's and Big Conclusions: An Examination of the Reasoning in Comparative Studies Based on a Small Number of Cases," in Charles C. Ragin and Howard S. Becker, eds., *What is a Case? Exploring the Foundations of Social Inquiry* (Cambridge: Cambridge University Press, 1992), 117-18; and "The Big Broad Issues in Society and Social History: Application of a Probabilistic Perspective," in Vaughn R. McKim and Stephen P. Turner, eds., *Causality in Crisis: Statistical Methods and the Search for Causal Knowledge in the Social Sciences* (Notre Dame: University of Notre Dame Press, 1997), 359-85.

stochastic, small and unpredictable events trigger large and equally unpredictable consequences. The unpredictability that is deeply inherent in causal processes suggests a research program for social scientists: "Discover and explain the mechanisms, or local transition rules, that make things somewhat predictable at 'local' levels. For more global or 'macro' levels, the analogy [to cellular automata] would suggest that all we can do is to describe or narrate how various essentially accidental conjunctions of mechanisms selected one historical trajectory from many other possible ones…"[14] Most radically, Alan Zuckerman argues that developments in quantum mechanics, chaos theory, and complexity theory compel a fundamental reorientation of our explanatory forays into social reality. The standard model of explanation asks the wrong sorts of questions and imposes inappropriate criteria for appraising the goodness of possible answers.

> Causal and nomological explanations…cannot easily serve as standards for the analysis of a world of nonlinear relationships among phenomena: no necessary parity between size and effect; sensitive dependence on initial conditions; the possibility of change at any point in time; open-ended processes; and the presence of chance as a substantive part of processes and their explanations—characteristics of an ontology associated with the chaos theory and other modes of analyzing complex dynamical systems.[15]

These programmatic injunctions have been replicated within particular subfields. The rejection of determinism is perhaps most visible in a group of studies of recent democratic transitions published between the mid-1980s and the early 1990s. Perhaps most famously, Guillermo O'Donnell and Philippe Schmitter inveighed against

[14] James Fearon, "Causes and Counterfactuals in Social Science: Exploring an Analogy between Cellular Automata and Historical Processes," in Philip E. Tetlock and Aaron Belkin, eds., *Counterfactual Thought Experiments in World Politics: Logical, Methodological, and Psychological Perspectives* (Princetone: Princeton University Press, 1996), 45-46. Fearon's essay draws this strong conclusion about the practice of social science while acknowledging that virtually no social processes can be modeled as a cellular automata. This raises the critical question of the legitimate domain of analogic reasoning between the physical and social sciences.

[15] Alan S. Zuckerman, "Reformulating Explanatory Standards and Advancing Theory in Comparative Politics," in Mark Irving Lichbach and Alan S. Zuckerman, eds., *Comparative Politics: Rationality, Culture, and Structure* (Cambridge: Cambridge University Press, 1997), 285.

deterministic conceptualizations of democratization, urging for their replacement by models that could capture "the high degree of indeterminacy embedded in situations where unexpected events (*fortuna*), insufficient information, hurried and audacious choices, confusion about motives and interests, plasticity, and even indefinition of political identities, as well as the talents of specific individuals (*virtù*), are frequently decisive in determining the outcomes."[16] In a vigorous endorsement of this claim, Giuseppe Di Palma explicitly eschewed claims of complete indeterminacy, but rejected on intellectual and normative grounds all efforts to attribute regime outcomes to any inventory of objective conditions. "Hard facts," as he referred to these conditions, "do not mean necessity. In political matters, particularly in matters of regime change, causal relations are only probable and outcomes uncertain."[17] In perhaps the most systematic formulation of this position, Youssef Cohen rejected structuralist accounts of democratic breakdowns because these accounts assumed a deterministic world in which actors were so tightly constrained as to have only one choice of action. Echoing a priori arguments made more generally by Jon Elster, and supporting his arguments with observations of tremendous variety of preferences and goals among members of the same class, Cohen claims, "In most situations, human beings have a choice." Consequently, our explanations must include actors' beliefs and intentions which are largely autonomous from structures, and so structural arguments will by necessity be either radically incomplete or seriously flawed.[18].

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[16] Guillermo O'Donnell and Philippe C. Schmitter, *Transitions from Authoritarian Rule: Tentative Conclusions about Uncertain Democracies* (Baltimore: The Johns Hopkins University Press, 1986), 5.
[17] Giuseppe Di Palma, *To Craft Democracies: An Essay on Democratic Transitions* (Berkeley: University of California Press, 1990, 4-6.
[18] Youssef Cohen, *Radicals, Reformers, and Reactionaries: The Prisoner's Dilemma and the Collapse of Democracy in Latin America* (Chicago: The University of Chicago Press, 1994), quoted material from 123.

It should be clear from even this brief survey that these arguments are powerful, insightful, and devastating for determinism. They must be taken very seriously. One option is to maintain the basic premises of macrostructural analysis but to excise its deterministic elements. Section 4 below explains why this many not be a feasible project. Before entering into that material, section 3 immediately below first considers alternative conceptualizations of determinism. Determinism, I argue, is more defensible when construed as anchored in causal realism rather than in regular associations; as segmented rather than universal; and as possessing an epistemic component distinct from its ontological component.

3. What Is Determinism?

Much has been written on the subject of determinism: so little has been decided that John Earman refers to the “Tower of Babel character of the discussion.” This section tries to impose some order on the chaos of determinism by making three distinctions that I have found helpful for thinking about determinism in the social sciences: these distinctions are between a regularity and a causal mechanism understanding of determinism; between universal and segmented understandings of determinism; and between ontological and epistemic understandings of determinism. These distinctions are developed in a highly abbreviated fashion; my intent is not to impose one view but rather to consider how our views of determinism differ subsequent to choices made in how we configure out understanding of it out of these building blocks.

A *regularity* understanding of determinism builds on a Humean understanding of causality as the constant conjunction of events. In its best-known version, John Earman suggests the following thought experiment:

> Describe the state s_1 of the room in which you are now reading this book—give the location of the furniture, the lighting conditions, the temperature distribution, and so forth. Wait twenty-four hours and record your description of the new state s_2. Now imagine that sometime in the future you find that your description of the state s_3 matches the previous description for s_1. Again wait twenty-four hours and record your description of the new state s_4. Would you then expect your description of s_2 and s_4 to match?[19]

Notice how in this depiction of determinism there is no reference to the mechanisms linking subsequent states to their antecedents. Just as Hume identified as cause as "an object, followed by another, and where all the objects, similar to the first, are followed by objects similar to the second," Earman is identifying determinism as states of the world following regularly on one another's heels.[20] Earman thus reproduces Hume's epistemic notion of causality in which causal relations are what we come to *expect* based on prior observations of conjunctions. As we shall see, this notion of causality as what is to be expected also animates Carl Hempel's deductive-nomological model of explanation, a model that we must be prepared to discard as thoroughly inadequate.

A contrasting understanding of determinism is that of *scientific or causal realism* with its attendant notion of causality relying on a mechanistic approach. In this approach, we identify a causal relationship not by observing regular associations but by identifying a causal mechanism with the capacity to generate observed associations. Causal realism

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[19] John Earman, "Determinism and the Physical Sciences," in Wesley Salmon, et. al., *Introduction to the Philosophy of Science* (Indianapolis: Hackett Publishing Company, 1992), 232.

[20] Hume, *An Enquiry Concerning Human Understanding* section 7 part 2 at 28. As any number of commentators and readers have noticed, after citing the above definition of causality, Hume immediately adds "Or, in other words, where, if the first object had not been, the second never had existed." For discussion of the tension between these contrasting definitions, see Tom L. Beauchamp and Alexander Rosenberg., *Hume and the Problem of Causality* (Oxford: Oxford University Press, 1981).

thus explicitly rejects Hume's regularity theory of causation. In response, for example, to Hume's claim that the vibration of a string is the cause of a particular sound because this and all similar vibrations of the string have been followed by similar sounds, Rom Harré provides a definitive illustration of a causal realist account, one that stresses layers of mechanisms. Harré rejects the core Humean claim that we can conceive of no connection between the vibration of the string and the subsequent sound, arguing instead that

> the theory and experiments of sonic physics and neuro-physiology give us a very good idea of the connection between the vibration and the sound. We all know nowadays of the train of pressures in the air, the operation of the ear-drum, the cochlea, and so on, and we now know something of the train of electrochemical happenings between the inner ear and that part of the brain identified as the seat of audition. Furthermore, to explain what we mean by 'the vibration causes the sound' rather than something else, typically involves, I contend, reference to the intervening mechanism which links the vibration in the string to the sound we hear. The vibration of the string stimulates a mechanism which then acts in such a way that we are stimulated and hear a sound.[21]

This alternative view starts from the recognition that many events are constantly conjoined without their being a causal connection between them. Red shifts in the light spectra of more distant stars do not cause the expansion of the universe; they are constantly conjoined to it. We know that no such causal relationship exists because we know about the mechanisms linking the birth of the universe to its expansion; more generally, we use mechanisms to distinguish causes, effects, and symptoms. The physical sciences, likewise, are replete with examples of functionally interdependent relationships that manifest constant conjunction but do "not convey the activity and productivity inherent in causation."[22] Boyle's law governs the relations between the

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[21] Rom Harré, *The Principles of Scientific Thinking* (London: Macmillan, 1970), 105-06, as cited in Russell Keat and John Urry, *Social Theory as Science* 2nd ed (London: Routledge & Kegan Paul, 1982), 29.
[22] Mario Bunge, *Causality: The Place of the Causal Principle in Modern Science* (Cleveland, Meridan Books, 1959), 10.

pressure and temperature of contained gases; statistical mechanics provides the causal mechanisms. Causal mechanisms are not only independent of the events they generate, but also from our ideas about the association of those events. Causality, in this view, is ontological, not epistemological; and it is the operation of generative mechanisms that produces the stream of observations that are the empirical basis for laws.[23]

To explain why causal relations may not be manifest as regular associations, Bhaskar makes the critical distinction between closed or experimental systems and open systems where the operations of additional causal mechanisms cannot be controlled: constant conjunction can only be observed in the former, whereas in the latter, multiple causal streams produce highly variegated outcomes and thus preclude such regularity. Consequently, and this is the key point, we need not observe regularity to demonstrate either a causal relationship or a deterministic relationship: a constant conjunction of events is neither necessary nor sufficient for a causal relationship.[24] In committing to determinism, then, we are making an ontological commitment with epistemological consequences: our explanation will be based on identifying causal mechanisms sufficient, under the circumstances, to produce the outcome without explicit reference to mechanisms of chance.

It seems just a simple move from this definition of determinism to a Leibnizian principle of sufficient reason—"a thing cannot occur without a cause that produces it"—or to a Kantian law of universal causation—"everything that happens presupposes

[23] From a large literature, Roy Bhaskar's work is most central. See especially his *A Realist Theory of Science* (London: Verso, 1975). An excellent introduction is Keat & Urry, *Social Theory as Science*.
[24] Bhaskar, *Realist Theory of Science*, especially chapter 1.

something from which it follows according to a rule."[25] To avoid this move, let me introduce a second distinction between a *universal* and a *segmented* understanding of determinism. The former implies that if anything is determined, everything is determined: the chain of determination is unbroken. To show why this claim is ludicrous, Paul Humphreys asks us to imagine the poor person who, on a whim, takes a pleasant afternoon motorcycle ride, only to meet his fate when

> Descending a hill, a fly strikes him in the eye, causing him to lose control. He skids on a patch of loose gravel, is thrown from the machine, and is killed. This sad event, according to the universal determinism, was millions of years beforehand destined to occur at the exact time and place that it did. Billions of generations of flies and aeons of air currents have inexorably led to this fly's being there and then. The invention of motorcycles and gravel-covered tarmacadam, the rider's decision to go out for a spin, his failure to wear goggles, and he accumulation of stones in that exact spot—all these inescapably were to come to pass. This claim, when considered in an open-minded way, is incredible.[26]

In contrast to this implausible determinism, Humphreys proposes a large role for indeterminism, for things that "just happen." His examples include a paternal grandfather who won Britain's national lottery whose equipment was based on a physically indeterministic process; the same grandfather who, lighting a cigarette in a trench during World War I, narrowly missed being killed by a sniper's bullet; and his father who, trapped on the beach at Dunkirk, saw his unit split in two by a commanding officer. A simple flip of a coin evacuated his father's section; the second half all perished.[27]

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[25] These formulations are from Wesley Salmon, *Causality and Explanation* (New York: Oxford University Press, 1998), 34.
[26] Paul Humphreys, *The Chances of Explanation: Causal Explanation in the Social, Medical, and Physical Sciences* (Princeton: Princeton University Press, 1989), 17.
[27] *Chances of Explanation*, 18.

Segmented determinism, on the other hand, allows for at least *some* causes that are sufficient to produce their effects without reference to chance mechanisms.[28] Some aspects of some present states of the world are sufficient to produce future states of the world under the appropriate description. There are two distinct elements to this response. First, causes that are sufficient to produce subsequent states may not uniformly be preceded by analogously sufficient causes. Coincidences, or events "whose constituents are produced by independent causal processes,"[29] may be quite pervasive. It may be a coincidence, to return to Humphrey's first example, that the motorcyclist and the fly met precisely at a spot in the road where the gravel was loose; but once they did, the resulting accident might well be determined. This particular claim is rather trivial, but not all such claims will be. Second, microlevel probability is fully consistent with the *possibility* of macrolevel determinism. It is simply not the case, as Ernest Nagel argued over forty years ago, that every conclusion drawn from a statistical theory must also be statistical.[30] It is thus critical to specify the level of description involved in any deterministic claim. This point is one that will be returned to regularly in this paper.

The nature of causal relations in different parts of the world can thus be highly variable: causal orders in one domain need not imply anything about causal orders in others.[31] At one extreme lies universal determinism, a position that is increasingly

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[28] Note that when speaking of relations of sufficiency, mechanisms must still be invoked for a causal relationship to exist. My moving to the left of you is sufficient to put you to the right of me; but it does not cause you to move to the right of me.
[29] David Owens, *Causes and Coincidences* (Cambridge: Cambridge University Press, 1992), 7.
[30] Ernest Nagel, *The Structure of Science: Problems in the Logic of Scientific Explanation* (New York: Hracourt, Brace, & World, Inc., 1961), 312-16.
[31] I have been quite influenced by the excellent discussion in John Dupré, *The Disorder of Things: Metaphysical Foundations of the Disunity of Science* (Cambridge: Harvard University Press, 1993), chapter 8, from which this paragraph draws heavily. Dupré, I should add, does not allow for segmented determinism and in fact suggests some reasons why segmented determinism is impossible, a position

untenable; at the other extreme resides complete randomness: the probability of any event is completely unconnected to any antecedent state. In between lies segmented determinism along with two types of probabilism. According to probabilistic uniformitarianism, there are universal laws that that link antecedents to strictly defined ranges of events by way of precise and invariant probabilities. Alternatively, the determinants of probabilities might be so large in number, varied, and conjunctural that no stable probabilities link events to their antecedents; rather, unique constellations set probabilities unique to their circumstances so that, in effect, probability distributions exist but cannot be determined. To argue for determinism is only, then, to argue that *some* causal orders evince determinism at specified levels of description.

Finally, we can distinguish between an epistemological and an ontological understanding of determinism. Laplace, recall, posited a demon with perfect knowledge: in this understanding, determinism exists when we can predict, and determinism is as much about our knowledge as it is about the world. But there may be times when we have good reason to believe determinism is operating at the level of mechanisms but not at the level of events; therefore, we could have determinism and explanation without prediction.[32] This might be because of the complex interplay of causes in which we have no way of knowing a priori which cause will be vindicated in the final event. It might also be because we do not know all of the conditions that permit a cause to produce its effect. Most patients suffering from a streptococcus infection respond to penicillin and subsequently recover; but not all do. We could say that the causal effect of penicillin is

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seconded by Jesse Hobbs, "Chaos and Indeterminism," *Canadian Journal of Philosophy* 21 (June 1991): 141-64. The remainder of this paper should be seen as my response to that argument.

[32] The tight coupling of explanation and prediction is one of the defining characteristics of positivism; their decoupling is a defining feature of scientific realism.

strictly statistical; but we could also say that we do not yet know which additional factors interact with penicillin to produce deterministically recovery or non-recovery. Salmon suggests that in this and many other (but surely not all) instances, "Scientific experience indicates that…further research will make possible more complete explanations."[33] Quantum mechanics, on the other hand, is by all of the best accounts, irreducibly indeterministic. The perhaps unfortunately named "uncertainty principle" (about which much more below) might seem to suggest an epistemic constraint on full knowledge, such that electrons have definite states but we cannot know them with certainty. On the contrary, the principle refers to ontology, for electrons and other particles simply do not have states with precise and determinate values.

A number of thoughtful scholars have suggested that determinism must be wrong because we have no validated deterministic theories. The distinctions raised above suggest some reasons why this intuition may be premature. If we think of determinism as regular associations, then by Bhaskar's reasoning we will surely not produce deterministic theories; if we think of determinism as universal, we will find countless exceptions; and these two considerations might lead us to consider indeterminism as a rock-bottom feature of reality. To these objections, I respond that a mechanism-based approach to causality does not demand regular associations; and that determinism might be a feature of only some causal orders but not others. Therefore, the present absence of robust deterministic theories might reflect (potentially remediable) epistemic constraints

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[33] Salmon, *Causality and Explanation*, 39-40. The acceptance of probabilistic explanations does not diminish the desire for more. After explaining how precisely 96% of photons striking a sheet of glass will pass through it, while four percent will reflect back, Richard Feynman expresses his sense of wonder and mystery that "Try as we might to invent a reasonable theory that can explain how a photon 'makes up its mind' whether to go through the glass or bounce back, it is impossible to predict which way a given photon will go." *QED: The Strange Theory of Light and Matter* (Princeton: Princeton University Press, 1985), 19.

and not represent any ontological status at all. These distinctions, in other words, establish *the possibility of determinism*: we can have valid explanations that contain only statements of necessary conditions and sufficient causes and that thus contain no chance mechanisms. The remainder of the paper tries to more fully elaborate that position.

4. Determinism and Macrostructural Analysis

Early works in macrostructural scholarship did not explicitly specify causal mechanisms; they were thus criticized for connecting macrostates to macrostates by way of teleological or functionalist assumptions.[34] Only recently has political science embraced an ontology of causal mechanisms; at the same time, however, some of the best work on causal mechanisms has argued that macrostates cannot be connected to macrostates by microlevel causal mechanisms because at the microlevel, probability and contingency are pervasive.[35] With a few important exceptions, scholarship is being split between an older macrostructural approach without *explicit* mechanisms and a newer,

[34] Jon Elster has made this critique most trenchantly of Marxist scholarship: see his *Making Sense of Marx* (Cambridge: Cambridge University Press, 1985); and *Explaining Technical Change* (Cambridge: Cambridge University Press, 1983). Youssef Cohen has adopted Elster's arguments *in toto* to critique structuralist arguments of regime change in his *Radicals, Reformers, and Reactionaries*. See also Edgar Kiser and Michael Hechter, "The Role of General Theory in Comparative-Historical Sociology," *American Journal of Sociology* 97 (July 1991): 1-30; and Michael Taylor, "Structure, Culture, and Action in the Explanation of Social Change," *Politics & Society* 17 (June 1989): 115-62. Of these, only Elster is reliable. Kiser and Hechter are simply confused about the relationship of Humean regularity to causal mechanisms. It may be correct to argue that Moore, O'Donnell, and Skocpol are less explicit in defining their causal mechanisms than are rational-choice theorists such as Cohen, Kiser and Hechter, and Taylor; but those mechanisms are surely present nonetheless. Taylor, for example, claims (115) that Skocpol's theory of revolutions permits no role for "intentional action or for individual goals, desires, and beliefs." This is simply incorrect, as should be fairly clear from a more fair-minded reading of a theory in which state elites seek military modernization and local elites seek to block projects stripping them of power and privilege. A far superior interpretation of structuralist approaches that clarifies their reliance on causal mechanisms is Daniel Little, *Varieties of Social Explanation: An Introduction to the Philosophy of Social Science* (Boulder: Westview Press, 1991).

[35] Jon Elster is largely responsible for both positions. See especially his *Nuts and Bolts for the Social Sciences* (Cambridge: Cambridge University Press, 1989). A second early advocate of a mechanism-based approach to causality and explanation is Daniel Little, *Varieties of Social Explanation*.

mechanism-based approach without macro-structures.[36] The aim of this paper is to attempt to bring these approaches together. Doing so requires defending determinism.

In the conclusion to his study of regime formation in inter-war Europe, Greg Luebbert provided a concise statement of the role of determinism in macrostructural analysis. Pre-existing structural conditions, such as pre-industrial cleavages dividing the bourgeoisie and the strength of the labor movement, induced coalitional strategies which in turn produced outcomes. From those large-scale and non-contingent conditions Luebbert deduces outcomes without reference to chance mechanisms. "One of the cardinal lessons I have told," he summarized his approach,

> is that leadership and meaningful choice played no role in the outcomes. To be sure, leaders did make choices that had implications for the regime outcomes; leadership choices were the mediating agent between inherited social and political conditions—"structures" in the argot of social science—and the eventual regime outcomes. For better or worse, however, those choices were always consistent with the short-term imperatives of the pursuit of power through the optimization of mass support.[37]

Luebbert's statement of structural determinism is exceptional only for its being concise and explicit. He makes the same point that Theda Skocpol was making when, in her study of social revolutions, she quoted approvingly Wendell Philips' declaration that "Revolutions are not made; they come."[38] And Thomas Ertman surely has something like this in mind when he pithily summarizes the causes of types of political regime and state apparatus resulting from European state formation as, respectively, "determined by

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[36] Two exemplary works that both specify causal mechanisms and neatly tie them to antecedent structural conditions are Thoms Ertman, *Birth of Leviathan: Building States and Regimes in Medieval and Early Modern Europe* (Cambridge: Cambridge University Press, 1997); and Dietrich Rueschemeyer, John Stephens, and Evelyne Huber Stephens, *Capitalist Development and Democracy* (Chicago: University of Chicago Press, 1991). My own *State Building and Late Development* (Ithaca: Cornell University Press, 1999), takes these two books as models of emulation.

[37] *Liberalism, Fascism or Social Democracy: Social Classes and the Political Origins of Regimes in Interwar Europe* (New York: Oxford University Press, 1991), 306.

[38] Theda Skocpol, *States and Social Revolutions: A Comparative Analysis of France, Russia, and China* (Cambridge: Cambridge University Press, 1979), 17.

the ability of national representative assemblies to resist royal pressures for absolutism, which was in turn a function of the nature of local government," and "determined in the first instance by the conditions under which such apparatuses were first constructed."[39]

These statements are exemplary only in their explicit avowal of determinism. They represent core elements of macrostructural analysis in that they attribute "large" outcomes to "large" causes; they make no reference to chance mechanisms in their explanations which are instead conceived of as conjunctural packages of necessary and sufficient conditions; and they assign minimal role to contingency, which, if it plays a role at all, largely affects idiosyncratic features of the outcomes without deflecting the combined influence of initial conditions and causal mechanisms in determining outcomes.[40] This package of ontological and epistemological intuitions characterizes almost all of macrostructural analysis. If, as Gerardo Munck avers, "much qualitative work operates with a deterministic, as opposed to a probabilistic model of causation,"[41] then great care must be paid to formulating that position and defending it from the critiques surveyed briefly above in section 3.

Arguments about the intrinsically probabilistic nature of social reality, along with those counseling heightened sensitivity to the often large-scale consequences of contingent, small causes, pose acute challenges to macrostructural analysis. Shared

[39] Ertman, *The Birth of Leviathan*, 317 and 318. Ertman adds a third determining condition which appropriately complicates the argument without reducing its determinism.

[40] Of the works cited above, Ertman assigns the largest role to contingency in explaining why a small number of cases deviate from what would be predicted based on their immediate post-Roman Empire initial conditions. Denmark, for example, deviated from its expected outcome due to the large-scale influx of German elites. But this is hardly a contingent cause in the sense of a small and wholly unexpected event producing disproportionately large effects. Indeed, given the massive transformation of initial conditions, the Danish case then behaves as predicted. The lesson to be learned is that sometimes one large cause will outweigh another equally large cause: this has nothing to do with contingency or indeterminism. Saying that social reality is not monochromatic is not to say that it is irreducibly indeterministic or susceptible to ephemeral contingent events.

[41] Gerardo Munck, "Canons of Research Design in Qualitative Analysis," *Studies in Comparative International Development* 33 (Fall 1998), 33.

conceptions of the methodological, ontological, and methodological pillars of macrostructuralism rely to a great extent on deterministic understandings of causality. In one of his many important reflections on the (precarious) status of determinism, Stanley Lieberson has argued for the adoption of a probabilistic perspective, replacing "if X then Y," with "if X then Y with greater frequency." Lieberson's challenge is particularly acute as he proclaims his own preference for deterministic explanations which are "cleaner, simpler, and more easily disproved."[42] Lieberson may indeed be right: it is worth considering, however, just how deeply macrostructural analysis is implicated in conceptions of determinism. Moving to a probabilistic perspective would involve far more than altering the status of individual propositions, as the following subsections attest.

4A. Methodological Adequacy: Determinism and Fair, Causal Comparison

Debates about methodology often hinge on underlying conceptions of whether causal processes are probabilistic or deterministic. An important example of this tight connection between ontology and methodology is Stanley Lieberson's demonstration that Mill's Methods of Agreement and Difference yield fallacious results when used to evaluate probabilistic theories.[43] Much of the recent commentary critical of Mill's methods has scorned the Method of Agreement, basically a no-variance design in which

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[42] Stanley Lieberson, "The Big Broad Issues in Society and Social History: Application of a Probabilistic Perspective," in Vaughn R. McKim and Stephen P. Turner, eds., *Causality in Crisis? Statistical Methods and the Search for Causal Knowledge in the Social Sciences* (Notre Dame: University of Notre Dame Press, 1997), quoted material from 364.

[43] This critique first appeared in "Small N's and Big Conclusions: An Examination of the Reasoning in Comparative Studies Based on a Small Number of Cases," in Charles C. Ragin and Howard S. Becker, eds., *What is a Case: Exploring the Foundations of Social Inquiry* (Cambridge: Cambridge University Press, 1992), 105-118. Lieberson considers other problems with Mill's methods that are not considered here.

the dependent variable is held constant across the cases studied, in favor of the Method of Difference, in which the dependent variable is allowed to vary.[44] By properly attending to the ontological underpinnings, Lieberson has raised serious doubts about the validity of results obtained by either version of Mill's methods. Because the logical core of Mill's methods is to eliminate any variable that does not perfectly coincide with the dependent variable, the methods will improperly rule out probabilistic variables whose causal relevance is perfect obvious. In Lieberson's well-chosen example of the causes of car accidents, Mill's methods would eliminate potential causes such as running a red light, a variable which quite obviously is neither necessary nor sufficient for accidents but which is probabilistically related to them nonetheless.

By some accounts, macrostructural analysis has and would continue to flourish were we to collectively agree to eliminate Mill's methods from our repertoire. Charles Tilly and Jack Goldstone, two of the most prominent scholars in the macroanalytic tradition, agree that Mill's methods are entirely inappropriate for use in the social sciences. Their critique basically mirrors Mill's own warnings. Mill's methods require cases that are identical except for a single independent variable that covaries perfectly with the dependent variable. Achieving these conditions would require both a finite set of causes, thus assuaging completely fears of omitted variable bias, and complete control over interaction effects; virtually nobody believe in the possibility of meeting these requirements outside of laboratories. Tilly's solution is primarily ontological (urging the adoption of what he calls "relational realism") and methodological (substituting a research programme of identifying and verifying the impact of causal mechanisms

[44] See for example Gary King, Robert Keohane, and Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton: Princeton University Press, 1994), 129; Fearon, "Causes and Counterfactuals in Social Science," 48.

without concern for cases or variables): this project will not depend on Millian logic.[45] Goldstone's related solution is process-tracing, or "the decomposition of a complex narrative into stages, episodes, or events which can be connected by causal sequences that are simpler and easier to explain the narrative as a whole. The explanation of these particular stages, episodes, or events is mainly deductive, although some inductive reasoning may be employed, depending on the existing state of theory and knowledge."[46]

These are crucially important considerations. Tilly is absolutely correct to insist on the centrality of mechanisms; Goldstone is absolutely correct to insist on the centrality of process-tracing to identify and verify those mechanisms, and to anchor our understanding of mechanisms in causal theory. Mill's methods are silent on all of these topics and therefore we should reject them as self-contained methods. The problem is, however, that identifying and verifying causal mechanisms is not sufficient to validate explanations: every effort must be made to eliminate alternative accounts. These eliminative efforts will embody the spirit, if not the form, of Mill's methods.

One way to imagine the importance of eliminating rival accounts is to listen to Mill as he explains why his methods are inappropriate for the analysis of politics and history. In politics and history, he avers, the

> Plurality of Causes exists in almost boundless excess, and effects are, for the most part, inextricably interwoven with one another. To add to the embarrassment, most of the inquiries in political science relate to the production of effects of a most comprehensive description, such as public wealth, public security, public morality, and the like: results likely to be affected either directly or indirectly

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[45] Charles Tilly, "Means and Ends of Comparison in Macrosociology," *Comparative Social Research* 16 (1997): 43-53.
[46] Jack A. Goldstone, "Methodological Issues in Comparative Macrosociology," *Comparative Social Research* 16 (1997), 112.

> either in *plus* or in *minus* by nearly every fact which exists, or event which occurs, in human society.[47]

Without some means for eliminating at least some hypotheses, we will be unable to distance ourselves from the conclusion that virtually everything matters. Identifying and verifying process-based causal mechanisms is a necessary but not sufficient condition for explanatory adequacy: needed is the systematic refutation of rival arguments.[48] The philosopher Richard Miller, who advocates precisely the mechanism-based explanations that Tilly and Goldstone want to see more of, argues that we can speak of a body of data as confirming an explanatory proposition "just in case its approximate truth, and the basic falsehood of its rivals, is entailed by the best causal account of the history of data-gathering and theorizing out of which the data arose."[49] Data underdetermine their interpretation, and so confirmation is not a "lonely encounter of hypothesis with evidence" but rather the result of maximum efforts to demonstrate, subject to later revision in the face of new data or new comparisons, that the facts are best explained by favored causal hypotheses relative to rival hypotheses.[50] Identifying and verifying causal

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[47] As cited in Tilly, "Means and Ends of Comparison," 44.

[48] If I read him correctly, Tilly downplays the role of eliminating rival arguments because doing so requires "moving far onto the opponents' preferred epistemological, ontological, and methodological terrain." Moreover, he continues, opponents have recourse to abundant theoretical resources and thus can quickly reformulate their arguments. See his "Mechanisms in Political Processes," *Annual Review of Political Science* 4 (2001), 21. The former point is exactly correct, but need not be a counsel of despair: for a detailed account, see Richard Miller, *Fact and Method: Explanation, Confirmation, and Reality in the Natural and the Social Sciences* (Princeton: Princeton University Press, 1987), esp. 200-23. The latter point sounds like it could be a recipe for stagnation, but we could also view it as a recipe for sustained debate and collaborative progress. For this more optimistic view, see Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970), where he writes, (179), "Criticism of a programme is a long and often frustrating process and one must treat budding programmes leniently."

[49] Miller, *Fact and Method*, 156.

[50] Miller's favorite example is an experiment conducted in an integrated elementary school in which children were asked to choose between a small piece of candy now or accepting the promise of receiving a large piece of candy the following week. While a large proportion of non-black children chose to wait for the larger piece of candy, black children asked for the smaller candy. The study's authors concluded that black children chose immediate satisfaction because of their "animal-like impulses." But as Miller points

mechanisms through process-tracing will play a crucial role in this procedure, but putting fairness into fair, causal comparison demands attention to rival hypotheses—to be specific, rival causal mechanisms and processes—as well.

James Mahoney has thus provided two crucial services in his recent writings on comparative methodology.[51] First, he nimbly climbs the ladder of abstraction to subsume Mill's methods under the category "nominal comparison," a location they share with related approaches all of which are designed to fulfill the mandate fair causal comparison by eliminating rival explanatory factors. Valid critiques of Mill's methods do not necessarily imply the absence of any means for rejecting rival theories. Second, he demonstrates not only that process-tracing (narrative appraisal) is based on a logic of causal appraisal distinct from that of nominal comparison, but also that narrative appraisal is usefully employed in conjunction with nominal comparison.

I would add two points to this fine discussion. First, the combination of nominal and narrative appraisal techniques is valuable not only because, as Mahoney concludes, it combines explanatory parsimony with attention to historical detail, a conclusion that seems to value nominal techniques for their aesthetic characteristics; this combination is indispensable because it combines verification with falsification and thus embodies the imperatives of fair, causal comparison. Second, I would argue that nominal comparison must be employed not only at the beginning of the causal analysis, but also at every stage of the causal chain producing the process being traced. That is to say, analysts cannot

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out, a perfectly reasonable alternative hypothesis is that "the typical experiences of black children gave them a greater rational basis for the expectation that promises made by outsiders with official standing will be broken." (175). For a vivid account of the problem of underdetermination, see William McKinley Runyan, "Why Did Van Gogh Cut Off His Ear? The Problem of Alternative Explanations in Psychobiography," *Journal of Personality and Social Psychology* 40 (1981): 1070-1077.

[51] James Mahoney, "Strategies of Causal Inference in Small-N Analysis," *Sociological Methods & Research* 28 (May 2000): 387-424; and "Nominal, Ordinal, and Narrative Appraisal in Macrocausal Analysis," *American Journal of Sociology* 4 (January 1999): 1154-96.

simply eliminate rival variables and then trace the process emanating from their favored variable, as is typically the case. Every stage of the causal process must be tested to eliminate rival causal mechanisms. Process-tracing is a three-dimensional, not a two-dimensional, technique.

Unfortunately, bringing nominal comparisons back in brings us back to our starting point: because nominal comparisons eliminate rival hypotheses that are based on necessary and sufficient conditions, "narratives are structured around the nominal argument and thus tend to be read deterministically."[52] Using this powerful combination of techniques thus demands that we defend a conception of deterministic causality.[53]

4B. Ontological Adequacy: Structuralism versus Unmoored Mechanisms

A substantial body of work contends that an emphasis on causal mechanism is incompatible with a macroanalytic emphasis on structural causes. Structures underdetermine mechanisms; since causal mechanisms are indispensable to explanations, structuralism should be sacrificed. An emerging synthesis thus explains outcomes not via a combination of antecedent conditions and mechanisms, but via unmoored mechanisms. Defending structuralism thus requires defending determinism.

The case against structuralism is a strong one. Most comprehensive is the position defended by Jon Elster. Elster argues that there are two types of causal

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[52] Mahoney, "Nominal, Ordinal and Narrative Appraisal," 1189.
[53] In his recent book *Fuzzy-Set Social Science* (Chicago: University of Chicago Press, 2000), chapter 4, Charles Ragin suggests a way to avoid these problems by modifying our claims of necessity and sufficiency, such as "almost always" or "more often than not" sufficient. I do not consider linguistic qualifiers to be a suitable substitute for difficult but unavoidable epistemological and ontological issues. Indeed, by hewing to a regularity approach to causality and diluting standards of necessity and sufficiency, Ragin's recommendation represents a substantial loss of information.

mechanisms, each of which implies some degree of indeterminism.[54] On the one hand, there are mutually exclusive mechanisms—for example, people can be rebellious or conformist, but not both. For any given situation, we cannot know which of the two mechanisms will be triggered.[55] Structures (more broadly, antecedent conditions) thus underdetermine causal mechanisms and do not explain. Elster calls these Type A mechanisms. Type B mechanisms, on the other hand, are paired mechanisms that are triggered simultaneously but with opposite effects on the outcome. The net effect of mechanisms on outcomes cannot be *ex ante* predicted. Elster thus makes us painfully aware of two distinct forms of indeterminacy: either we cannot know which of two mechanisms will operate (although we might know the effect of either mechanism) or we can know which two mechanisms will operate but we cannot know what they will cause.

Rational-choice theorists have specialized in exploring the consequences of variants of Type A mechanisms in which structures underdetermine mechanisms.[56] Writing of the causes of the end of communism in eastern Europe, Adam Przeworski predicts future futility, writing

> I know that hundreds of macrohistorical comparative sociologists will write thousands of books and articles correlating background conditions with outcomes in each country, but I think they will be wasting their time, for the entire event was a single snowball…As developments took place in one country, people

[54] Elster presents this particular formulation in "A Plea for Mechanisms," in Peter Hedström and Richard Swedberg, eds., *Social Mechanisms: An Analytical Approach to Social Theory* (Cambridge: Cambridge University Press, 1998), 45-73.

[55] In an early version of this argument, Elster argued that it would be plain foolish to argue that intra-capitalist competition forces capitalists to exploit workers, for "nobody is forced to be a capitalist: there is always the option of becoming a worker." Structures, in other words, underdetermine behavior. See his *Nuts and Bolts for the Social Sciences*, 14.

[56] Type B mechanisms have received far less attention. One strand of research considers strategic games in which there are multiple equilibria: in this case, rational choice theory suffers sharply attenuated explanatory power, a situation briefly explored by in the collectively written conclusion to Robert H. Bates, et. al., *Analytic Narratives* (Princeton: Princeton University Press, 1998), 233-34.

> elsewhere were updating their probabilities of success, and as the next country went over the brink, the calculation was becoming increasingly reassuring.[57]

Przeworski writes of n-person assurance games: we cannot predict what triggers their operation, but we can know the outcomes they produce. Similarly, Russell Hardin exerts considerable energy developing a rational choice mechanism for ethnic identification and even ethnic violence. Yet he makes almost no effort to explain when these mechanisms will be triggered. Although he refers to the "grim economics" of Africa, exacerbated in Rwanda and Burundi by land shortages, he explains the origins of violent ethnic conflict there by a seemingly contingent cause, attributing the violence to the actions of Rwandan President Habyarimana who "evidently thought he needed the distractions of racism to maintain his personal control."[58] In a final example of underdetermined mechanisms, Douglass North and Barry Weingast make a strong case that the key to English economic development was the institutionalization of a credible commitment on the part of the Crown to respect private property. The authors, however, make little effort to explaining how and why this mechanism was established in 17th century England, making only passing reference to the absence of a standing army in England and ignoring the considerable efforts of macrostructural scholars to explain the same outcome.[59] I believe that most macroanalytic scholars would find these explanations inadequate; the only response is to defend some version of determinism.

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[57] *Democracy and the Market: Political and Economic Reforms in Eastern Europe and Latin America* (Cambridge: Cambridge University Press, 1991), 3-4.
[58] *One for All: the Logic of Group Conflict* (Princeton: Princeton University Press, 1995), 169.
[59] Douglass C. North and Barry R. Weingast, "Constitutions and Commitment: The Evolution of Institutions Governing Public Choice in Seventeenth-Century England," *The Journal of Economic History* 44 (December 1989): 803-832.

In perhaps the most vivid testimony to the power of arguments contesting the adequacy of structural arguments to explain outcomes in the face of considerable evidence that structures underdetermine mechanisms, scholars who are on the whole sympathetic to macrostructuralism have begun to evince considerable ambivalence about its viability. Charles Tilly heartily endorses a mechanism-based approach to explanation. Mechanism-based explanations, as he characterizes them, shift emphasis away from large-scale social structures; and the effects of mechanisms are highly diverse because they are mediated by varying initial conditions and because they combine heterogeneously with other mechanisms to produce outcomes. Consequently, Tilly advances an agenda that is skeptical of the utility of deep history and is willing to consider bounded streams of social life, or episodes, "snipped…from their historical and social contexts."[60]

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[60] Tilly, "Mechanisms in Political Processes," 36. Elsewhere in this essay (28-31), Tilly evinces his growing dissatisfaction with studies in the tradition of Barrington Moore that explain democratization by way of durable features of polities. He is thus critical of arguments such as the recent work of Jeffery Paige, *Coffee and Power: Revolution and the Rise of Democracy in Central America* (Cambridge: Harvard University Press, 1997) which attributes Central American regime formation to "durable features of class structure" and "structural differences established decades earlier." He endorses in place of this structural determinism Deborah Yashar, *Demanding Democracy: Reform and Reaction in Costa Rica and Guatemala, 1870s to 1950s* (Stanford: Stanford University Press, 1997), which claims that Guatemala and Costa Rica evinced considerably similar political economies and regimes into the 1940s. The differences in their post-war political regime in turn followed from the starkly differing effects of mechanisms common to both countries (the construction of a counter-elite reform coalition). James Mahoney, *The Legacies of Liberalism: Path Dependence and Political Regimes in Central America* (Baltimore: The Johns Hopkins University Press, 2001), 25, convincingly argues that by omitting El Salvador, Nicaragua, and Honduras, Yashar has overestimated the degree of similarity between Costa Rica and Guatemala in the pre-reform period and under-estimated the importance of 19th-century antecedent conditions for 20th-century regime formation. He admits that his account of the political history of all five Central American countries, however, shares much with the social structural approach of Robert Williams, *States and Social Evolution: Coffee and the Rise of National Governments in Central America* (Chapel Hill: University of North Carolina Press, 1994). Mahoney notes, after all, that there is a strong correlation between elite choice of commercialization policies (his independent variable) and pre-reform levels of commercial modernization; his assertion that state-building and commercialization strategies were autonomous of these antecedent structures is not well-supported. Tilly's efforts to defend his non-structural mechanism-based approach from class-analytic, social structural approaches are, in this instance, not very successful.

Both James Mahoney and Paul Pierson exhibit similar skepticism about the role of antecedent structural causes in their important statements about path dependency.[61] Both authors are willing to consider (relatively) deterministic arguments for institutional persistence once institutional patterns have been established. But in considering the origins of institutions, both authors part company with an earlier tradition of "critical junctures" analysis that anchored institutional trajectories in antecedent structural conditions.[62] While paying homage to their social structural predecessors, both authors claim that path dependency begins with unpredictable, partially random, often "small" events that have large consequences. Both authors use the model of a polya urn, in which random events early in a sequence exhibit powerful constraints on all subsequent events. While Pierson evinces more ambivalence, shying away from claiming that institutional origins are completely contingent and averring that analysts often succeed at explaining why one path was chosen over its alternatives, Mahoney states quite clearly that "in a path-dependent sequence, early historical events are contingent occurrences that cannot be explained on the basis of prior events or "initial conditions."[63] In emphasizing not just sensitivity on initial conditions (a feature we will return to below in discussing the butterfly effect), but sensitivity on contingent initial conditions, Mahoney and Pierson converge on Elster's Type A mechanisms: the onset of a sequence is unpredictable. They

[61] James Mahoney, "Path Dependence in Historical Sociology," *Theory and Society* 29 (2000): 507-48; Paul Pierson, "Not Just What, but *When*: Timing and Sequence in Political Processes," *Studies in American Political Development* 14 (Spring 2000): 72-92; and Pierson, "Increasing Returns, Path Dependence, and the Study of Politics," *American Political Science Review* 94 (June 2000): 251-67.
[62] For a review of the tradition of critical junctures arguments that allocated a large explanatory role to antecedent structural conditions, see Ruth Berins Collier and David Collier, *Shaping the Political Arena: Critical Junctures, the Labor Movement, and Regime Dynamics in Latin America* (Princeton: Princeton University Press, 1991), 28-39.
[63] Pierson, "Not Just What," 75; Mahoney, "Path Dependence," 511.

do not, however, incorporate Type B mechanisms, so that once the process is underway, its subsequent stages are (relatively) deterministic.

There is thus a clear relationship between rejecting determinism and rejecting the structural element of macroanalytic scholarship. It may indeed be the case that structuralism is indefensible and dispensable and that it is appropriately supplanted by the new vision of explanation via unmoored mechanisms; as I will argue below, however, that conclusion may be entirely premature.

4C. Explanatory Adequacy: Dueling Criteria

Norms of explanatory adequacy prevalent in macrostructural work join with methodological considerations and ontological preferences to implicate macrostructural scholarship in deterministic understandings of causality. I understand an explanation to be adequate when it adduces one or more conditions that, under the circumstances, are sufficient to produce the outcome. This criterion comports well with extant discussions. In her *States and Social Revolutions*, Theda Skocpol defended her focus on a small number of cases by declaring her intention to provide "valid, complete explanations of revolutions." While Skocpol presented theoretical reasons to reject alternative accounts, she stressed that while the factors of those accounts could certainly be applied to cases of revolution, such an exercise would not constitute an explanation but rather would provide "little more than pointers toward various factors that case analysts might want to take into account, with no valid way to favor certain explanations over others."[64] "Complete" explanations, for Skocpol, meant identifying the total package of relevant necessary and sufficient conditions that produce *particular instantiations* of the complex object of

[64] Skocpol, *States and Social Revolutions*, 5 and 34.

explanation, social revolutions. Because such work is not interested in changes in properties distributed across populations, but rather in particular outcomes, "the explanations which result from applications of the comparative method are not conceived in probabilistic terms…[65]

I am not aware of other macrostructural scholars making explicit parallel claims, but I believe it fair to consider complete explanations to be a shared explanatory goal as long as we understand that the explanandum is not the complete history of a case in all of its infinitesimal detail, but rather a conceptually mediated object of explanation such as social revolutions which are "rapid, basic transformations of a society's state and class structure…"[66] Alan Garfinkel's discussion of "equivalence classes" is relevant here. An equivalence class "consists of the set of "inessentially different" objects, collapsed into one" for the purpose of constituting objects of explanation.[67] We give complete explanations, composed of necessary and sufficient conditions, of the members of an equivalence class. Thus we find Tilly characterizing his account of European state formation as "pursuing a history that oscillates between the somewhat particular and the extremely general," by presenting "enough concrete historical evidence to make the principles comprehensible and credible, but not so much as to bury them in detail."[68]

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[65] Charles Ragin, *The Comparative Method: Moving Beyond Qualitative and Quantitative Strategies* (Berkeley: University of California Press, 1987), 15. To be sure, our understanding of what constitutes a complete explanation must be tempered by pragmatic considerations, for, as John Mackie has pointed out, a full accounting of the etiology of an event includes some factors we willingly call causes and some factors that are necessary conditions but not causal: it was the carelessly tossed cigarette and not the properly stored inflammable material that caused the fire. A full list of these background conditions would have to include all relevant standing conditions such as oxygenation, not to mention countervailing causes that would have blocked the effect had they been present. Complete explanations, in this technical sense, are elusive, and only pragmatic considerations govern the partition of causes and conditions. See his *The Cement of the Universe: A Study of Causation* (Oxford: Clarendon Press, 1974), 34-35.

[66] Skocpol, *States and Social Revolutions*, 4.

[67] Alan Garfinkel, *Forms of Explanation: Rethinking the Questions in Social Theory* (New Haven: Yale University Press, 1981), 31.

[68] *Coercion, Capital, and European States, AD 990-1990* (Cambridge: Basil Blackwell, 1990), 16.

Despite using the language of "partial explanations," Ruth Berins Collier and David Collier have, I believe, the same explanatory aims when they explain the parallel experiences of paired countries in Latin America. By denying that each member of each pair will be exactly identical, they disclaim any intention to explain all of these contrasting details; rather, their object of explanation is those features of each country that are "more similar than one might otherwise expect" given their "enormous differences [that] might lead one to predict sharply contrasting trajectories of change…"[69] Given that object of explanation, the Colliers also provide a complete explanation.

Contrast this shared understanding of explanatory adequacy to discussions of the relationship between causality and explanatory adequacy in quantitative work, where a probabilistic understanding of causality prevails in alliance with an understanding of explanations as partial, not complete, and an understanding of objects of explanation as properties distributed across populations, not particular instantiations of the dependent variable. The contrast of criteria of explanatory goodness between qualitative and quantitative analysis suggests that completeness and determinism might in fact be inseparable accomplices.

Note first how qualitative and quantitative analyses specify distinct objects of explanation.[70] Qualitative work explains particular instantiations of general phenomena. Quantitative work, on the other hand, makes descriptive and causal inferences about

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[69] Collier and Collier, *Shaping the Political Arena*, 20.

[70] There is no law enjoining one object of explanation with its underlying model of causality and prohibiting the other object of object of explanation with its underlying model of causality. In defending the logic of statistical research from Stanley Lieberson's (*Making it Count*, 88-107) critique that it typically leaves unaccounted basic causes that have constant effects and thus produce no variation across units, King, Keohane, and Verba can legitimately claim that "we must be careful to ensure that we are really interested in understanding [variation in] our dependent variable, rather than background factors that our model holds constant." (*Designing Social Inquiry*, 109). The problem is that King, Keohane, and Verba assume that all research is research into objects of explanation as they conceive them. Their pluralistic embrace of methods thus ultimately runs aground on their metatheoretical dogmatism.

samples, populations, or both. Just as an actuarial table can predict with great accuracy how many members of a given age cohort will die within a specific time frame without identifying the fate of any single member of that group, statistical inferences deal with trends and averages, often quite accurately, without providing specific causal information about individual members of samples or populations. Thus, the regression coefficients yielded by statistical work "summarize information about correlations that exist between measurable properties variably distributed *across some population*."[71]

Given this significant difference in the understanding of objects of explanation, what precisely do major expositions of the logic of statistical explanation say about explanatory adequacy? King, Keohane, and Verba's *Designing Social Inquiry* is remarkably silent about explanatory adequacy. They seem to be distinguishing explanation from causal inferences when they write that "real explanation is always *based on* causal inferences."[72] But the remainder of the chapter discusses how to make causal inferences without discussing when causal inferences constitute adequate explanations. Other advocates of quantitative analysis have, fortunately, gestured more firmly in the direction of explanatory adequacy. In his classic *Data Analysis for Politics and Policy*, Edward Tufte wrote "The causes, explanations, or predictors of the response variable are the describing variables or independent variables. Usually more than one describing variable *will help explain* the response variable."[73] Tufte's brief references to causality and explanation are echoed in more recent discussions. Christopher Achen argues that "social scientists neither have nor want correct, stable functional forms for their explanations…Social theories rarely say more than that, ceteris paribus, certain

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[71] Vaughn McKim, "Introduction," to *Causality in Crisis*, 7.
[72] *Designing Social Inquiry*, 75 at note 1.
[73] (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1974), 2, emphasis added.

variables are related…A great many other factors may also be influential; they are outside the theory and must be controlled somehow."[74] Warren Miller's defense of quantitative methods from the charge of causal inadequacy adds explicit rejection of determinism. Acknowledging the tight link between causation and explanation, so that "without a plausible explanation, we may discard a statistical link as mere coincidence," Miller dismisses the claim that "causal reasoning is based on a deterministic view of the world…Causal models in political studies are not about determinism but about influences, usually only modest influences, and often rather weak influences."[75]

Let's consider an alternative way of capturing differences in norms of explanatory adequacy. Qualitative work, I argued, values explanations in which independent variables, singly or in combination, constitute sufficient causes of particular instantiations of members of equivalence classes. Contrast this to Christopher Achen's discussion of the three ways to think about the significance of an independent variable in statistical analysis. First and most important is what he calls theoretical importance, measured by unstandardized regression coefficients which tell us how, on average and with uncertainty expressed by the standard error, change in an independent variable affects change in the dependent variable. Second is what he calls level importance, which conveys information about the average net effect of a given variable in a particular time and place by multiplying unstandardized regression coefficients by the mean of the independent variable. "This measure," he observes, "has the attractive property that when all these contributions are added, including that of the intercept, the result is precisely the *mean* of

74 Christopher H. Achen, *Interpreting and Using Regression* (Newbury Park, CA: 1982), 16.
75 Warren Miller, 1995, 168.

the dependent variable."[76] Theoretical importance tells us about the potential affect of an independent variable, relative to others in the model and independent of particular circumstances; level importance makes no such general claim, as it is dependent on the mean of a given sample. Finally, there is dispersion importance, which uses standardized betas to explain the variance within a given sample. Because of multicollinearity, summing standardized betas does not explain the total variance of a sample. But if all other variables are held constant, each standardized beta represents "the impact of a variable on the spread of the dependent variable in a given sample."[77]

Because of the difference in objects of explanation, these criteria are largely alien to qualitative researchers. Qualitative researchers are not immune to caring about the general effects of independent variables across time and space (or to theoretical hubris, I suppose), as numerous debates urging the incorporation of this or that variable attest. When comparativists say that "states matter" or "institutions matter," they are making ontological statements about what categories of causes should be included in explanatory ventures; they are not making theoretical claims about the specific causal effects of these variables in specific contexts. These debates are thus preliminary to research design and explanation. The resulting explanations are then linked to particular circumstances,

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[76] Achen, *Interpreting and Using Regression*, 72, emphasis added.

[77] Achen, *Interpreting and Using Regression*, 68-75, quoted material from 75. Note, finally, that a fourth possible measure, the coefficient of determination, provides a measure of how well a model accounts for the spread of points around a regression line for a given sample. But this measure is sensitive to the number of variables included in the model, the degree of similarity between the independent and dependent variables, and the degree of variance of the independent variables. The coefficient of determination is thus a "poor measure" of the spread of points around a regression line, one whose best use is to compare a given model to either a rival model to assess model specification or to the null model with no explanatory variables. Gary King , "How Not to Lie with Statistics: Avoiding Common Mistakes in Quantitative Political Science," *American Journal of Political Science* 30 (August 1986), 675-77. See also Christopher H. Achen, "Measuring Representation: Perils of the Correlation Coefficient," *American Journal of Political Science* 21 (November 1977), 805-815.

widely or narrowly construed, in the form of boundary conditions.[78] Level importance speaks to concerns about historical specificity but does so only by relating the *mean* of the independent variable to the *mean* of the dependent variable. Level importance, like theoretical importance, thus violates the most basic, metatheoretical goal of comparative, qualitative research. As Ragin succinctly put it, qualitative-oriented comparativists "are interested in the cases themselves, their different historical experiences in particular, not simply in relations between variables characterizing broad categories of classes."[79]

There is some overlap between Achen's dispersion importance and the goals of qualitative research. The latter does not typically conceptualize explanatory outcomes in terms of the spread of the dependent variable. But even a study that explicitly looks at only a single instance of revolution or democratization always implies a variant outcome, the counterfactual instance of non-revolution or non-democratization. But qualitative research conceptualizes and thus explains that spread very differently than does quantitative research. Whereas explanations valued by macrostructural scholarship conceives of causality in conjunctural terms (identifying the conditions that collaborate, in the form of necessary and sufficient conditions, to produce outcomes *in toto*), causality in statistical work typically takes the form of a linear additive model where the effect of each variable is estimated *net the effects of other variables.*[80] Additive models can hold background factors constant and look only for factors affecting variations in the

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[78] On boundary conditions, see David Collier and James Mahoney, "Insights and Pitfalls: Selection Bias in Qualitative Research," *World Politics* 49 (October 1996), esp. 68-69.

[79] Charles Ragin, *The Comparative Method: Moving Beyond Qualitative and Quantitative Strategies* (Berkeley: University of California Press, 1987), 6.

[80] Aage B. Sørensen, "Theoretical Mechanisms and the Empirical Study of Social Processes," in *Social Mechanisms,* 248-49, refers to additive models as the "gas station" model, where a model, say, of income portrays a person pulling up at various pumps to get discrete portions of her earnings. On conjunctural causality, see Margaret Mooney Marini and Burton Singer, "Causality in the Social Sciences," *Sociological Method* 18 (1988), 347-409, which is based in large part on Mackie, *Cement of the Universe.* Additive models can of course include interactive terms; but these are still terms whose net effect is to be estimated.

dependent variable; conjunctural models employed on behalf of complete explanations cannot.

In short, part of the debate between advocates of statistical work and advocates of qualitative work stems from profoundly contrasting views about the criteria of what constitutes an adequate explanation. These views are in turn closely—perhaps even necessarily—associated with contending positions on the probabilistic or deterministic nature of causality.

In principle, Carl Hempel's explanatory schema can be used to bridge the gap between explaining features of populations and providing singular explanations. Hempel's deductive-nomological model, and its lesser-known cousin, the inductive-statistical model, explains singular events by subsuming them under more general categories of events whose properties are established by general laws. An explanation of the deductive-nomological model consists of three elements: one or more general laws in the form of universal affirmatives (all p are q: e.g. all gases expand when heated under constant pressure); one or more statements of initial conditions in the form of existential statements (r is p: e.g. neon is a gas); and the event to be explained, the explanandum, which is deduced from the first two elements (therefore, r is q: e.g. the neon in this container expanded because it was heated under constant pressure). We explain the outcome by showing that this particular instance is a deductive implication of a nomological law, the instantiation of a law under appropriate antecedent conditions. A typical example might explain why a particular country C is democratic by claiming that "All countries with a per capita gross national income above $3000 are democracies; country C has a per capital gross national income over $3000; therefore, country C is a democracy. The inductive-statistical model replaces the general law with a statistical law stating that probability that p are q. To explain why a patient recovered from a streptococcus infection, we could observe that the patient was treated with penicillin and then invoke the law that penicillin cures streptococcus infections in a high percentage of cases and so with a high statistical probability. In the case of inductive-statistical explanations, Hempel emphasized that the probabilistic statements had to have a probability of close to 1; and that the inference to the explanandum was not a deductive implication but rather a statement with inductive support.[81]

The problem with using Hempel's model to resolve the conflict between two metatheoretical orientations to explanation is that Hempel's model simply does not explain. The logical relations between the elements of the model, rather, tell us what to expect. Imagine your reaction to the proverbial student who, when pressed for an explanation for a late paper, replies that he always turns in his papers late. This core problem stems directly from Hempel's reliance on a Humean regularity theory of causation; his model of explanation does not explicitly require causal mechanisms. Consider an explanation of why the earth travels in an elliptical orbit. It would be a perfectly acceptable explanation, by Hempel's account, to say the following: All celestial bodies travel in an elliptical orbit (general law); the earth is a celestial body (statement of initial condition); therefore, the earth travels in an elliptical orbit (logical deduction of the explanandum). Explanation, in this example is to attribute a property to an entity by subsuming that entity into a general category all of whose members possess that property.

Now consider an alternative explanation that has the same exact logical form: An

[81] See his "Aspects of Scientific Explanation," in his *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science* (New York: The Free Press, 1965), 381-83.

object on which no net force acts continues in a state of motion at a constant speed in a straight line (law of inertia); centripetal force exerted towards the center of a circle keeps a body moving at a constant speed with uniform circular motion; the combination of the law of inertia and centripetal force creates elliptical orbits (law of gravitation); the earth is a body on which centripetal force is exerted by the sun (initial condition); therefore, the earth moves in an elliptical orbit. Both explanations meet the criteria of the deductive-nomological model; but intuitively we all recognize that the latter explanation "explains" far more and far differently than the former.

Indeed, there is a veritable cottage industry of examples illustrating the non-explanatory nature of Hempel's logical form of explanations. The first set of classic counterexamples consists of arguments that clearly count as deductive-nomological explanations but with equal clarity fail to explain: these examples show that the deductive-nomological model is not sufficient for an explanation. Thus, despite a general law linking sharp drops in the readings of a functioning barometer to the onset of storms, the reading on the barometer does not explain the storm. Two effects of a common cause—a sharp drop in atmospheric pressure—do not explain one another and so the deductive-nomological model does not explain. The second set of classic counterexamples consists of readily acceptable arguments that do explain but do not fulfill the requirements of the deductive-nomological model, which therefore is not necessary for an explanation. I explain the coffee stain on my pants by explaining that in my zeal to finish a paper for the APSA conference, I accidentally jostled a cup of coffee carelessly placed on the edge of my desk, which fell in my lap. This perfectly adequate explanation neither incorporates any laws nor requires the explicit statement of a tacitly assumed law.[82] Far more can be said against the deductive-nomological model (and special defects apply to the inductive-statistical model); but when we see that the model is neither necessary nor sufficient for an explanation, we can quickly agree with Paul Roth when he writes that "The deductive-nomological model, not unlike the verifiability criterion of meaning, remains an interesting and important intellectual artifact, one from which much can be learned by studying why it is mistaken. But its appeal now should be only to the unwary."[83]

In summary, then, norms of explanatory adequacy in the qualitative research tradition favored by macrostructural scholars differ starkly from those norms animating quantitative research. There is no easy way to reconcile these two deeply divergent ways of appraising explanatory goodness. These criteria of explanatory goodness are in turn, I have argued, intimately tied to notions of deterministic or probabilistic causality. Methodological, ontological, and epistemological considerations all point in the same direction: macrostructural analysis is tightly bound to deterministic conceptions of causality. To reject determinism is to reject macrostructural analysis. At this point, then, we can turn our attention (finally) to the key question of this paper: must we reject deterministic conceptions of causality? The first half of the answer to this question pertains to chaos theory, to which we now turn.

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[82] A nice compendium of these classic counterexamples can be found in any of Wesley Salmon's surveys of scientific explanations, especially his "Four Decades of Scientific Explanation," in *Scientific Explanation: Minnesota Studies in the Philosophy of Science*, *volume XIII*, ed. Philip Kitcher and Wesley Salmon. The position that laws are not necessary for explanations is associated with Michael Scriven. See especially his "Truisms as the Grounds for Historical Explanations," which is included in an excellent collection, Patrick Gardiner, ed., *Theories of History*, 443-75.

[83] Paul Roth, "The Full Hempel," *History & Theory* 38 (1999), 254.

5. Chaos, Contingency, and Determinism

The core principles of chaos theory were originally discovered by the mathematician Henri Poincaré in the late 19th century and subsequently ignored for more than half a century. Fittingly, these ideas were reinvented by accident in the early 1960s by Edward Lorenz, who, in the early 1970s, coined the phrase "the butterfly effect," according to which the flapping of a butterfly's wings in Brazil could cause a tornado in Texas.[84] By the late 1980s, chaos was the topic of a best-selling book and academics in the humanities and social sciences were beginning to consider its dramatic implications for their disciplines.[85] Explicit discussion of chaos theory among political scientists is relatively rare, but its effects are strongly felt nonetheless; Paul Pierson's discussion of path dependency, for example, includes features like unpredictability and nonergodicity

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[84] Lorenz's paper on the butterfly effect was delivered in 1972 to the American Academy for the Advancement of Science, and is reprinted in his *The Essence of Chaos* (Seattle: University of Washington Press, 1993). The butterfly effect is frequently understood as the flapping of a butterfly's wings in China, not Brazil: the difference is surprisingly important. As Lorenz speculates, small errors might cause big effects *within* temperate latitudes, but they would not do so moving across the equator from a tropical to a temperate latitude, because these would constitute different fluid systems with different and non-transferable dynamical instabilities.

[85] James Gleick, *Chaos: Making a New Science* (New York: Penguin Books, 1987). For applications to social sciences and the humanities, see Robert W. Batterman, "Defining Chaos," *Philosophy of Science* 60 (1993): 43-66; Alan D. Beyerchen, "Nonlinear Science and the Unfolding of a New Intellectual Vision," *Papers in Comparative Studies* 6 (1989): 25-49; C. Dyke, "Strange Attraction, Curious Liaison: Clio Meets Chaos," *Philosophical Forum* 21 (1990): 369-92; Donald N. McCloskey, "History, Differential Equations, and the Problem of Narration," *History and Theory* 30 (February 1991): 21-36; George A. Reisch, "Chaos, History, and Narrative," *History and Theory* 30 (February 1991): 1-20; idem., "Scientism Without Tears: A Reply to Roth and Ryckman," *History and Theory* 34 (February 1995): 45-58; Paul A. Roth and Thomas A. Ryckman, "Chaos, Clio, and Scientistic Illusions of Understandings," *History and Theory* 34 (Februuary 1995): 30-44; Randolph Roth, "Is History a Process? Nonlinearity, Revitalization Theory, and the Central Metaphor of Social Science History," *Social Science History* 16 (Summer 1992): 197-243; Michael Shermer, "The Chaos of History: On a Chaotic Model that Represents the Role of Contingency and Necessity in Historical Sequences," *Nonlinear Science Today* 2 (1993): 1-13; idem., "Exorcising Laplace's Demon: Chaos and Antichaos, History and Metahistory," *History and Theory* 34 (February 1995): 59-83; Mark A. Stone, "Chaos, Prediction, and Laplacean Determinism," *American Philosophical Quarterly* 26 (April 1989): 123-131; and Immanuel Wallerstein, "The Challenge of Maturity: Whither Social Science," *Review* 15 (Winter 1992): 1-7.

which are hallmarks of chaos theory.[86] Phrases like "sensitive dependence on initial conditions" are now familiar in political science.

I argue below that these vigorous endorsements of the application of chaos theory to social settings is based on some deeply questionable assumptions: chaos theory is about the mathematical analysis of very specific types of physical systems and so its value to the social sciences may be no more than metaphorical and heuristic. Chaos theory may sensitize us to look for how large effects may follow from small causes; but it cannot tell us that such effects indeed exist. In fact, I would argue that the enthusiasm for chaos theory is based on a logical fallacy, that of affirming the consequent. The central implication of chaos theory is sensitive dependence on initial conditions: if a system is chaotic, then (for specific values of its parameters), small causes have large effects. Scholars then claim that history or some other favored discipline exhibits sensitive dependence on initial conditions; history must be, the argument goes, a chaotic system. Such arguments make no effort that I can see to establish that their system is indeed chaotic: as we shall see, such efforts are guaranteed to be futile. Instead, from the claim "if p then q," they reason, "q, therefore p. The references to chaos theory then take the place of a determinate research design: if the system is chaotic, then it is patently obvious that small causes must have huge effects. A rhetorical devise takes the place of disciplined research and causal argument. As I argue in section 6, however, establishing a role for small causes is in fact fraught with difficulty.

The irony is that even if we licensed the application of chaos theory to the social sciences, the implications would simply not be as profound as is frequently imagined. This is because chaotic systems are deterministic systems: chaos theory does not

[86] Pierson, "Increasing Returns," 253.

unambiguously demonstrate that small random *events* have large and totally unpredictable consequences. What chaos theory says is that when the parameters of certain non-linear dynamical systems take on certain values, then arbitrarily and unavoidably small errors in the *measurement* of the initial conditions will, over time, produce large effects on subsequent phase states of the system. Chaos theory, in other words, considers the disproportionate effects of measurement error on our ability to predict subsequent states of a system that are in fact fully determined by the initial conditions.

I address two issues in what follows. First, I discuss chaotic systems as unstable, aperiodic, deterministic, non-linear systems: all of the effects of chaos theory follow from these features. There is almost no reason to believe that social systems exhibit these features. One possible exception to this claim is n-person assurance games, which I discuss below. The second issue I discuss in this section is the meaning of the butterfly effect. In contrast to the popular understanding that this effect means that random and unpredictable events can have monumental consequences, utterly shaping social processes and the course of history, I follow chaos theory more closely in arguing that butterfly effects—to the extent that they exist, which is not yet absolutely established—do not have such fundamental consequences: rather, they change the relative frequency and timing of subsequent events *that would inevitably have occurred without the butterfly effect*.

What is chaos theory? Even the name is in dispute. Most books written by physical scientists refer not to chaos theory but rather to chaos. In doing so, they all hasten to add that their usage of the word chaos differs from ordinary usage. Far from referring to the absence of order, chaos refers to highly organized systems that *appear* to be random.[87] Chaos theory, in turn, is the "qualitative study of unstable aperiodic behavior in deterministic nonlinear systems."[88] This definition is sufficiently precise that it raises profound questions about what does, and does not, qualify as a chaotic system.

A system is, quite simply, an artificial boundary framing a set of objects or processes that scientists study. This relatively simple definition could be applied to history: one sees regular references to phenomena such as the rise and fall of civilizations as a system. Chaotic systems, however, have additional qualifications: they are at least partially mathematized and they are dynamical: the system's features can be quantified and basic rules (difference equations for discontinuous changes, differential equations for continuous functions) can be used to transform the system over time.[89] These features allow us to conceptualize and even visualize systems as the trajectories of phase spaces. A phase space is simply a hypothetical space with as many dimensions as variables constituting the system. A Cartesian grid is thus a non-temporal, two-dimensional phase space. A single point represents a complete description the state of the system at any time; and over time, the movement of the point as it follows the changes of each variable of the system sketch out a trajectory. Not all dynamical systems, however, are chaotic: chaotic systems are non-linear, which means that the functions of the system's variables involve algebraic or more complicated terms. Two implications follow from non-

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[87] All physical systems contain some randomness: the movements of a pendulum, which is the paradigm of a non-chaotic system, can be perturbed by small currents of air produced by people walking by outside. Chaotic systems are those which would appear to be random even if true randomness could be eliminated.
[88] Stephen H. Kellert, *In the Wake of Chaos: Unpredictable Order in Dynamical Systems* (University of Chicago Press, 1993), 2. Kellert's book is the best discussion of the philosophical issues stemming from chaos theory. The most concise and accessible introduction to chaos theory is an online course from the University of Texas, available at http://order.ph.utexas.edu/chaos/.
[89] A very accessible depiction of dynamical systems and their mathematical treatment is provided in David Z. Albert, *Time and Chance* (Cambridge: Harvard University Press, 2000), chapter 1.

linearity. First, current values of the dependent variable have feedback effects, influencing the subsequent values of independent variables: non-linear dynamical systems are reflexive.[90] These non-linear terms represent factors such as friction, limits to population growth, or the rate at which infectious diseases spread. Whereas linear equations of population growth or the spread of disease would depict steady, unending increases in the population; the more realistic, non-linear function includes a term that, for example, reduces the rate of growth when the population level is high. Second, the inclusion of non-linearity makes it impossible to find a closed-form solution to these equations: these models do not supply an exact prediction of future states from a present one, but only a qualitative idea about long-term behavior.[91]

These non-linear dynamical systems have a few other defining features: they are aperiodic, unstable, and deterministic. Aperiodic systems are those in which there is no regular repetition of values: phase space trajectories do not intersect themselves and so the history of the system does not repeat itself. Instability, on the other hand, means that small disturbances to the system persist. A marble resting at the bottom of a bowl that is jarred will return to rest; unstable systems, on the other hand, evince behavior that does not resist small disturbances. As a result of these two properties, chaotic systems exhibit highly complex behavior: values never repeat themselves and they manifest the effects of small disturbances indefinitely. The complex behavior that results *appears* random. Randomness is only an appearance, however, for the final defining feature of chaotic systems is that they are deterministic: their behavior, no matter how complex, can be

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[90] For an excellent illustration, see Randolph Roth, "Is History a Process? Nonlinearity, Revitalization Theory, and the Central Metaphor of Social Science History," *Social Science History* 16 (Summer 1992), 204-208.
[91] Kellert, *In the Wake of Chaos*, 3.

modeled by typically only a few equations, and these equations contain no chance mechanisms. Later states thus evolve from earlier ones according to a fixed law. As Kellert summarizes,

> We are accustomed to thinking that behavior that shows no repeating patterns and responds abruptly to even small disturbances must result from the competing and reinforcing influences of countless subsystems. But chaos theory explores very simple, rigorously defined mathematical models that nonetheless display behavior so complex as to merit description as random.[92]

The most interesting implication of chaos theory is the so-called butterfly effect: sensitive dependence on initial conditions. The butterfly effect has two components: ineradicable uncertainty and disproportionate effects. On the one hand, it is impossible, both in practice and in principle, to avoid arbitrarily small uncertainty in measurements of initial conditions. On the other hand, in stark contrast to the common belief that large effects must have had large causes while small effects can be safely ignored, a form of cause-effect thinking with its roots in Newtonian deterministic mechanics, the non-linearity of chaos theory means that even vanishingly small differences in initial conditions produce dramatically large differences in outcomes. Chaotic systems, in other words, are "error amplifying."[93] To see how errors get amplified, try balancing a pencil on its point. You cannot do it because even smallest error will cause the pencil to fall in the direction of its displacement from the central point. The pencil will then fall faster and faster because its velocity is a function of the square of time elapsed. The initial error—the initial displacement of the pencil from its central equilibrium point—amplifies over time.

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[92] Kellert, *In the Wake of Chaos*, 5.
[93] Mark A. Stone, "Chaos, Predictions, and Determinism," 127.

Is this conception of time-dependent evolution with its dramatic disjuncture between the scale of cause and effect relevant to comparative politics? I have argued that considering this question is critical to the macrostructural project. To repeat, that project is based on causal intuitions that large outcomes stem from large structural effects. And yet important essays have used chaos theory to argue that this entire enterprise is essentially obsolete. Alan Zuckerman argues, for example, that there are "obvious parallels between political phenomena and the world described by these new fields of study [chaos and complexity theory]."[94] He cites approving David Ruelle's speculations about the implications of chaos theory:

> Historical determinism must thus be corrected (at least) by the remark that some historically unpredictable events have important long-term consequences. I think that more in fact can be said. I think that *history systematically generates unpredictable events with important long-term consequences.*[95]

While Zuckerman ends the quote here, it is worth considering how Ruelle finishes his thought, however.

> Remember indeed that momentous decisions are often taken by individual political leaders. In many cases these political figures act quire predictably under the pressures of the moment. But if they are intelligent and act rationally, the theory of games will often force them to put a random element in their decisions. The decisions that shape history, when they are taken rationally, involve therefore a random, unpredictable event. This is not to say that the president of the United States could explain to Congress that he made an important decision by flipping a coin. Maybe that is just what he did, and maybe that was the rational thing to do, but he will have to find something else to say, explaining somehow that there was no reasonable alternative to his decision.[96]

I think it highly unlikely that Ruelle has accurately depicted a major component of decision-making. Contemporary political science has placed most of its bets on the

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[94] Zuckerman, "Reformulating Explanatory Standards," 285.
[95] Ruelle, *Chance and Chaos*, 90, emphasis in original, cited in Zuckerman, "Reformulating Explanatory Standards," 289.
[96] Ruelle, *Chance and Chaos*, 90.

idea that political leaders are rational and that their rationality leads them to act predictably. But even if we grant Ruelle's point about the behavioral implications of rationality, there is still a huge difference between deriving sensitive dependence on initial conditions from the properties of aperiodic, unstable, deterministic non-linear dynamical systems and deriving randomness from rationally motivated erratic behavior. Ruelle is simply not describing the consequences of a chaotic system and Zuckerman is wrong to invoke his speculations to demonstrate the utility of chaos theory to comparative politics.[97] To the contrary, Ruelle is highly skeptical about applying the term "system" in its mathematical sense to the social sciences. He notes that in some domains, such as solar astronomy, fluid dynamics, and atmospheric sciences, we can speak of systems because we know the equations governing the time-dependent evolution of the states of the system. In the social sciences, we neither know these equations nor do we know if they exist; we do know that if such equations exist, they are not invariant, but rather they themselves change over time as the system "learns." From this distinction Ruelle concludes that in the soft sciences, "the impact of chaos remains for the time being at the level of scientific philosophy rather than quantitative science."[98]

Let's add a further complication: not all dynamical systems exhibit sensitive dependence on *all* initial condition. Some systems have no such dependence; others have such dependence for all initial conditions. But still other dynamical systems have sensitive dependence for some initial conditions but long-term predictability for others.

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[97] Indeed, at other points of the book, Ruelle demonstrates acute perception of this difference. Commenting on the work of Brian Arthur, whose work on technological innovation demonstrates that accidental choices can have essentially irreversible long-term effects, Ruelle denies that this conclusion about historical processes follows from chaos theory. Arthur's work "sounds like sensitive dependence on initial condition, although mathematically it is something different." *Chance and Chaos*, 90.

[98] *Chance and Chaos*, 79. Ruelle concludes this observation on the cautiously optimistic claim that "progress is possible: remember that Poincaré's considerations on predictability in meteorology were just scientific philosophy, and this domain is now quantitative science."

The degree of sensitivity is captured by a measure called the Lyapunov exponent; for each system, there is a Lyapunov exponent corresponding to each dimension of the system that measures the propensity for displacement from the system's center along that axis. For any direction away from the central point, if the Lyapunov exponent is negative, displacement from the center will shrink exponentially; if the exponent is zero, the displacement will retain its magnitude. Only if the Lyapunov exponent is positive will small displacements grow exponentially over time. Thus, someone who wishes to argue that a hypothetical system is dynamical must also be prepared to argue that it will display chaotic behavior for the relevant initial conditions.

A simple metaphor might illustrate this how sensitivity to initial conditions can itself vary. Baseball, it is often said, is a game of inches. At Shea Stadium, the deepest part of the ballpark is 410 feet from home plate. For a ball hit approximately 410 feet, a few inches here or there can be the difference between a home run and a long out and thus can be the difference between victory and defeat. For this initial condition, small differences matter. But imagine a ball hit approximately 410 feet close to the left-field foul pole, which is 338 feet from home plate. Small differences in our measurement of that ball are inconsequential: it is an absolutely mammoth home run.

Note: section on n-person assurance games and Lyapunov exponents to be added here (or as an appendix).

Finally, let's allow that political phenomena are properly modeled as dynamical systems, and that sensitivity on initial conditions obtains for the relevant conditions.

What precisely does it mean to invoke sensitive dependence on initial conditions? What are butterfly effects? Edward Lorenz, the MIT meteorologist, was rerunning the second half of a convection current simulation by using the values obtained at the halfway point of the first simulation as the initial conditions of the second simulation. The first half of the second simulation should have exactly replicated the second half of the first simulation; but after a coffee break, he discovered that the new simulation was radically different from its predecessor. Why? Because in entering the numbers for the second simulation, he lopped off the last three digits of a six-digit number: .506127 became .506. In essence, introducing the flap of a butterfly's wings is equivalent to losing some of the digits, to measurement error. The flapping of a butterfly's wings is the causal analogue of measurement error: if the latter amplifies exponentially, the former should have the same effect.

The implications of this equivalence between measurement error and the flapping of a butterfly's wings, however, are not as dramatic as is commonly depicted. One of the most common tropes employed in discussion of the butterfly effect is that of Cleopatra's nose: had her nose been shorter and thus not charmed Marc Antony, the entire history of the Western world would have been different.[99] Small causes not only have large effects; they produce outcomes that *would not have otherwise occurred.*[100]

[99] The original version is from Pascal. More recently, see G. Lively, "Cleopatra's Nose, Naso and the Science of Chaos," *Greece and Rome* 49 (April 2002), pp. 27-43. Lively considers Pascal to have anticipated chaos theory with his comment that had Cleopatra's nose been shorter, the entire history of the western world would have been different.

[100] James Fearon does not deny the existence of butterfly effects; he denies, rather, that they form the basis for meaningful counterfactuals. Had Cleopatra's nose been different, it is not the case that World War I would not have occurred in a world that otherwise closely resembled that of 1914; rather, all aspects of the 1914 world would have been tremendously different, so that it would make no sense to ask whether World War I would have occurred in that alternative world. See his "Counterfactuals in Social Science," 57.

Whatever else we think of Pascal's claim, chaos theory simply does not support this interpretation. Chaotic systems do not allow the wildly implausible to happen; chaos is only apparent, a complex representation of order. To see why, we have to understand the role of attractors and see how they influence the outcomes of sensitive dependence on initial conditions. As a basic rule for the application of chaos theory, we cannot speak of sensitive dependence on initial conditions without also speaking about attractors.

Recall that a phase space is a graphical representation of the time-dependent evolution of an n-dimensional system. The key point is that a trajectory does not visit every possible location in the volume composed by these dimensions. This is because of attractors. Certain conceivable states of the world do not occur; other states of the system occur repeatedly. Still other states of the world are repeatedly approximated, closely and more closely. These last states of the system belong to the set of attractors. "The essence of an attractor is that it is some portion of the phase space such that any point which starts nearby gets closer and closer to it."[101] Trajectories in chaotic systems converge on the set of attractors without ever visiting the *exact* position twice (aperiodicity) and without resisting even minor perturbations (instability).

To illustrate this point, consider an attractor that is quasi-periodic and stable. Consider jogging in a two-dimensional grid representing all open spaces. We could quite literally jog along all contiguous points, but most of us do not visit every conceivable point. In Charlottesville, I avoid busy commercial strips and fraternity row, preferring a path along tree-lined streets with mountain views down to and along the Rivanna River.

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[101] Ian Stewart, *Does God Play Dice? The New Mathematics of Chaos* 2nd ed (Malden, Massachusetts: Blackwell Publishing, 2002), 99.

The absence of heavy traffic, the shade, and the views attract me to a relatively well-defined trajectory through the phase space. Unlike an aperiodic chaotic system, in which the trajectory never passes through the same point twice, this route is quasi-periodic: I follow it with some regularity, and I stay pretty close to the same spot along the route. Unlike an unstable chaotic system in which small perturbations have lasting effects on the trajectory, this system is fairly stable: if I swerve to avoid an obstacle, I return fairly quickly to the original path. Knowing where I am on my route at any point in time would permit an observer to make a fairly good prediction of where I will be in the future. Not so for the trajectories of chaotic systems. Minor perturbations create growing divergences in trajectories; no point is visited twice; chaotic attractors are complex and strange attractors.[102] But like the attractor of my jogging route, attractors impose order; new trajectories still converge on the set of attractors, and so chaos is only apparent, and *some* predictability is possible.

Understanding attractors along with sensitive dependence allows us to see what the butterfly effect really means. Chaos theory is all about the strange combination of unpredictability and stability. Given the emphasis placed on instability in defining chaotic systems, it seems odd to now emphasize stability. Attractors resolve this tension: if the system is displaced from its attractor, it rapidly returns to it, but not to the same point on the attractor. Because, as we have seen, small differences enlarge rapidly, the shift to a slightly different point on the attractor means that you cannot predict subsequent states even though they are completely determined by the system's basic equations. Because the antecedent condition cannot be fully measured, even knowing the equations

[102] The most common attractors are the two-lobed, kidney shaped figures first discovered by Lorenz.

does not allow prediction of precisely where on the attractor the point trajectory will go next.

Other features of chaotic systems, however, can be predicted: it all depends on what you want to predict. A point in the phase space representing the earth's weather can be perturbed off of an attractor briefly, but it rapidly returns to the same attractor. This means that measurements of that system constitute a time-series with a shared "texture." The pre-butterfly trajectory and the post-butterfly trajectory diverge rapidly, but as they are on the same attractor, they have the same texture: the two trajectories produce the same weather motif. This means, Ian Stewart explains, that

> a hurricane…cannot occur in the perturbed time-series unless it was (eventually) going to occur in the original one. So what the butterfly does is to alter the timing of a hurricane that—in a sense—was going to happen anyway. Don't take that too literally: the butterfly may trigger the conditions needed to make a hurricane form, or prevent one that would have formed. But most of the time it will just have a minor effect on where and when a hurricane that has been building for global reasons—the right kind of warm, humid air in the right place—will occur. Hurricanes are a recurrent and characteristic feature of the time-series that we call 'normal global weather-patterns', and as such they are evidence that this time-series lives on a single attractor. The butterfly does not flip the weather to a new attractor; it just displaces it a bit on the same attractor.[103]

It is worth giving the final word on this matter to Edward Lorenz, who first coined the phrase "the butterfly effect." Concerned that he might appear frivolous in even suggesting the question, Lorenz noted that there was no reason to privilege the flap of any one butterfly's wings over those of millions of other butterflies, not to mention the actions of other more powerful species. He also hastened to recognize that the flap of a single butterfly's wings could just as easily prevent a tornado as to generate one. What then is the possible effect of the butterfly? Lorenz proposed that

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[103] Stewart, *Does God Play Dice?*, 132.

> over the years miniscule disturbances neither increase nor decrease the frequency of occurrences of various weather events such as tornados; the most they do is modify the sequence in which these events occur. The question that interests us is whether they can do even this—whether, for example, two particular weather situations differing by as little as the immediate influence of a single butterfly will generally after sufficient time evolve into two situations differing by as much as the presence of a tornado.[104]

Surprising, given all the fanfare that this paper has provoked, in response to this, more modest, question, Lorenz concludes, “We must therefore leave our original question unanswered for a few more years, even while affirming our faith in the instability of the atmosphere.”[105]

Certainly, differences in the timing of hurricanes or changes in the frequency with which they occur can matter for all sorts of reasons. But it is a difference within the same kind of weather system, and it is factors like warm, humid air masses that shape that weather, not butterflies. In the terms of Adam Garfinkel, the two runs of the weather system belong to the same equivalence class: they are not fundamentally different from one another and we can safely ignore the causes of minor variations. Put different, weather can be variable but climate is stable. The real changes we should worry about—the ones that genuinely produce something new--are not the timing of hurricanes or the addition or subtraction of one of them from a given, fully predictable, hurricane season. The really important issue is whether the weather is going to jump to a new attractor. How, most centrally, does the rise in the level of carbon dioxide affect the average summer temperature or the average number of winter snowstorms? This is a question about climate, and the butterfly is completely irrelevant to it. Thinking in even longer terms, the entire climate of the earth is one huge attractor, but there is a second

[104] Lorenz, *Essence of Chaos*, 181-82.
[105] Lorenz, *Essence of Chaos*, 184.

attractor—ice ages. Over time, the rising level of greenhouse gases produced by industrialized and industrializing economies could flip us to that second attractor. Again, the butterfly is irrelevant to this story.[106]

Indeed, there is something highly artificial about the entire butterfly story, based as it is on the counterfactual of holding everything constant while allowing a butterfly to flap its wings. The problem with this is that the butterfly does not cause hurricanes; the butterfly in conjunction with everything else causes hurricanes. And among that group of causes, it is warm, humid air masses that most proximately cause hurricanes. As for the proverbial butterfly, whose flapping wings combine with the flapping of billions of other butterflies to create just a tiny portion of the perturbations of the atmosphere: the average butterfly "is just as likely to cancel out a hurricane as to create one—and it might just raise the average temperature of India by a hundredth of a degree, or generate a small grey cloud over Basingstoke."[107]

One nagging question remains, even after minimizing the consequences of the butterfly effect: is chaos theory a genuine *causal* theory? Is the butterfly a genuine analogue to measurement error? Stephen Kellert makes a strong case that the answer to both of these questions is no.[108] First, the object of explanation of chaos theory is not facts, events, or phenomena; rather, chaos theory studies mathematical models of physical systems, models that produce patterns and bifurcations represented by curious graphics on computer screens. Chaos theory is not, then, a causal theory, but rather a theory that helps us to understand how unpredictable behavior appears in simple systems,

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[106] This discussion of weather versus climate is based on Stewart, *Does God Play Dice?*, 133, and Jack Cohen and Ian Stewart, *The Collapse of Chaos: Discovering Simplicity in a Complex World* (New York: Penguin Books, 1994), 212.
[107] Stewart, *Does God Play Dice*, 132.
[108] Kellert, *In the Wake of Chaos*, chapter 4.

both in the sense of how that behavior comes to be and also in the sense of what that behavior looks like. Moreover, chaos theory accomplishes this task by building mathematical models that contain geometric mechanisms, not causal mechanisms. Chaos theory does shed light on any causal processes at work; it is not even designed to do so.[109] Chaos theory, rather, supplies "the geometry of behavior," and these geometrical features tell us about the qualitative features of the physical systems that are being modeled. The butterfly effect, however, is a *causal* effect: the butterfly is said to cause tornados. This raises the rather sticky question of whether a causal effect can be derived from the "theoretical hypotheses of chaos theory [which] assert *relations of topological similarity, not congruence of physical causes, between its exemplary models and actual systems.*"[110] The significance of this divergence between a mathematical model with geometric mechanisms and a physical system with causal mechanisms is that "mathematical ideas of chaos give an idea of a range of possible behaviors, but do not tell you what will really happen."[111]

6. Small Cause and Large Effect in the Absence of Chaos

But so what? Even if we eliminate chaos theory as the source of our thinking about sensitive dependence on initial conditions, why can't we still make arguments that small causes have large effects? Writing of the Cuban missile crisis, James Fearon doubts that the color of Khrushchev's shirt had causal relevance, but is willing to consider the weather or passing conversations as causes that "might conceivably have

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[109] These geometric mechanisms by which geometric shapes are folded and twisted are the source of strange attractors; they are the source of error amplification. See Gleick, *Chaos*, 81-117.
[110] Kellert, *In the Wake of Chaos*, 107.
[111] David Ruelle, personal communication, May 7, 2000.

mattered."[112] There are absolutely no a priori grounds for prohibiting arguments connecting small causes to large effects. Let's imagine three types of transformative processes: those that amplify initial divergences, those that reduce initial divergences, and those that retain initial divergences. Amplifying processes transform small differences in initial conditions into large outcomes. Paul Pierson argues, for example, that when increasing returns processes operate, "Small events are remembered": over time, accidental events occurring early in a sequence become "locked in" as vicious or virtuous cycles operate to build seemingly insuperable advantages.[113] In contrast, reductive processes take even large differences in initial conditions and transform them into relatively equivalent outcomes. This seems to be what many dependency and world-systems theorists have in mind when arguing that incorporation into the periphery of the capitalist world system erases often large initial differences and creates a relatively homogeneous form of dependent capitalism; it is also what early modernization theorists envisioned (and some contemporary globalization theorists envision) when predicting future convergence on a single model of modernity. Finally, there are magnitude-retaining transformative processes. In Robert Putnam's account of institutional behavior in Italy over an eight hundred year period, differences in the propensity for collective action produced different political institutions in 13th-century Italy and in 20th century Italy as well.[114]

I see no a priori reason to privilege one of these types of processes. But while I cannot rule out butterfly-*type* effects—small and unpredictable causes with large

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[112] Fearon, "Counterfactuals in Social Science," 46.
[113] Pierson, "Increasing Returns," 253.
[114] Robert Putnam, *Making Democracy Work: Civic Traditions in Modern Italy* (Princeton: Princeton University Press, 1993).

effects—neither can devotees of this type of analysis, whether explicitly relying on chaos theory or not, simply assert both that butterfly effects exists and that they have identified unproblematically the proper butterfly. In fact there are four problems of causal attribution that any such explanation will have to overcome: causal transitivity, causal confirmation, causal depth, and causal substitution.

The first hurdle to be overcome is that of causal transitivity. If A causes B; and B causes C; when are we correct to say that A causes C? Consider the well-known nursery rhyme that connects the want of a nail to the loss of a kingdom by way of four intermediary steps: the loss of a shoe, of a horse, of a rider, and of a battle. It seems as if the different parts of the causal chain all have a common explanation: the loss of the nail. But this is not correct for two reasons. There is no mechanism directly linking missing nails to lost kingdoms. Each subsequent step in the causal chain, moreover, is explained by its antecedent only in conjunction with a great deal of causally relevant background conditions specific to that stage in the argument.[115] Losing single battles causes the loss of a kingdom only under conditions unspecified by the "explanation" in its current form; lacking exactly one rider causes the loss of a battle only under other unspecified conditions; and so on. The explanation we seek must specify those causally relevant conditions, and when it does so, we will see that in fact a large outcome was caused not by a single small cause but by the conjunction of variously sized causes.

The problem of causal transitivity assumes that we all agree that the causal chain begins with the loss of a nail. The second hurdle to be overcome is that of causal confirmation. How do we know that it was the loss of the nail that started the whole

[115] David Owens, *Causes and Coincidences* (Cambridge: Cambridge University Press, 1992), 15-19.

tragic process? Paul Roth and Thomas Ryckman are not being flippant when they write that

> If a kingdom is lost for want of a nail, then an account of the blacksmith's inattention—the bad meal that upset his stomach, his two-year old's dirty nappy, the distraction outside his shop at a crucial moment—all become causally relevant to the (not too distant) future state of the system. Every conceivable detail or variation affecting same, in turn, potentially acquires causal significance regarding defeat in battle and so any explanation of why an empire was lost.[116]

Every conceivable detail: this point is worth stressing. Not only is every conceivable detail affecting the blacksmith of potential causal significance, let us continue, but so also are all of the conceivable details affecting other direct or ancillary participants in the war. To stick with the nursery rhyme for a moment, what of the other riders, the colleagues of the lost rider? Why are their actions not considered? Or what of the officers? In his accounts of Civil War battles, James McPherson regularly stresses how key battles were won or lost due to the decisiveness or indecision of individual generals. Union Colonel Chamberlain, for example, led his under-armed and dispirited men on a desperate bayonet charge against massing Confederate troops; this audacious maneuver shocked the Alabamians into surrender and turned the tide at the battle of Gettysburg. If the absence of ammunition did not spell Union defeat at Gettysburg, why should we believe that it was indeed the loss of a single horseman rather than a poor decision by a commander that spelled defeat for our mythical, nail-challenged kingdom? In his account of the rise of the Republican Party in the 1850s, Robert Fogel emphasizes that there was no inevitability to the victory of the anti-slavery coalition. Fogel does not list a single small cause, but rather four: the split in the Know-Nothing movement; the tactical brilliance of Free Soil leaders; favorable economic developments in the North that reduced intra-labor

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[116] Roth and Ryckman, "Chaos, Clio and Scientific Illusions of Understanding," 37.

conflicts between nativists and immigrants; and acts of southern violence against Northerners. Why four factors and not some other number? Why these four factors? And what of the factors that did not occur but that, had they occurred, would have blocked any of the above putative causes from occurring? Donald McCloskey correctly observes that "*any* of an unbounded set of little people and little events could be brought into the story."[117] McCloskey considers this "flock of butterflies" effect to be the inevitable implication of non-linear history. But like so many others, McCloskey assumes non-linearity from the assumed presence of butterfly effects, and invokes sensitive dependence on initial conditions *without invoking attractors*. We could, I suppose for the sake of argument, claim that there is one attractor for Republican or Union victory and one attractor for Democratic or Confederate victory; but as we have seen, butterflies do not select from among these attractors.

The third issue to consider in our evaluation of "small-large" causality is causal depth. A cause can have a genuine effect but still not count as an adequate explanation if it lacks sufficient causal depth. Are there, in other words, relatively large causes of the small causes invoked by butterfly arguments? And are these less proximate causes the better explanation? Consider Cleopatra's nose and the fate of the western world. The queen's nose was itself caused in some way by her genetic heritage. Similarly, we might speculate about the origins of Marc Antony's fixation with oversized nasal protuberances as the primary mark of beauty. But these speculations are worthless. No single cause produced both the nose and its effect; their coming together is pure coincidence, and

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[117] McCloskey, "History, Differential Equations, and the Problem of Narration," 33. McPherson's argument about contingent causes in civil war battles, and Fogel's argument about contingent causes and the origins of the civil war, are both drawn from this article.

therefore the causal chain that interests us (but that is ultimately rendered irrelevant for reasons argued above) begins with their meeting.[118]

In other instances, however, there are good reasons to push further back in the causal chain. Consider the outcome of the Civil War. It has long been argued by military historians that Northern victory was inevitable due to its vastly overwhelming superiority of men and material. The brilliance of southern generals only delayed an outcome that was so inevitable that it would have occurred even if Sherman and Grant had not eventually redressed the earlier imbalance of leadership. In a recent review essay, James McPherson, who we saw above stressing the contingent outcomes of individual battles in explaining northern victory, skillfully explodes the myth of northern invincibility, arguing forcefully that "Numbers and resources do not prevail in war without the will and skill to use them."[119] We might say that McPherson distinguishes between macrolevel factors such as material resources; mesolevel factors such as will; and microlevel factors such as strategic and tactical skill. He places causal emphasis on the meso- and the microlevel and in doing so rejects the macrolevel with its large/large causality.

The problem with this account is that there may be intimate connections between the levels. Rather than beginning the causal chain with Lee's strategic genius and tactical brilliance, we need to endogenize these factors and explore their relationship to the broader structure of northern superiority. According to Gary Gallagher's recent account, Lee recognized clearly that the preponderance of material power guaranteed northern

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[118] This distinction between causes and coincidences was first noted by J.B. Bury in his 1916 essay, "Cleopatra's Nose," in Harold Temperley, ed., *Selected Essays of J.B. Bury* (Cambridge: Cambridge University Press, 1930), 60-69. That same distinction is explored to great effect by Owens, *Causes and Coincidences*.

[119] James M. McPherson, "Could the South Have Won," *The New York Review of Books* (June 13, 2002), http://www.nybooks.com/articles/15481.

victory if the population of the north remained resolute. The key to victory, he believed, was to convince the North that the costs of not granting independence to the South would be higher than expected given the North's superior resources. While many historians have criticized Lee for pursuing an offensive strategy, Gallagher argues that Lee felt forced to pursue "battlefield victories as a means to undermine northern national morale in a conflict that pitted the Confederacy against a foe with huge advantages of manpower and material resources."[120] Lee was equally concerned with the effects of battlefield victories on southern morale. If McPherson is right to stress the importance of will, he is wrong to consider will as a factor independent of northern advantages in men and material.

And what of skill? Northern military predominance did not uniquely determine any broad strategy; many southern generals chose a defensive strategy. The problem is that this strategy almost always ended in southern defeats that in turn deflated southern morale. Recognizing the link between overall resources and strategy, in part as a result of his experience during the war with Mexico, Lee became convinced "that a predominately defensive posture would allow the enemy to muster and apply his strength at leisure." He turned instead to the strategic offensive "to keep the Federals off balance, find opportunities to bring them to battle under favorable circumstances, and inflict defeats that would bolster morale in the South and weaken it in the North."[121] Finally, northern material superiority shaped Lee's decisions about battlefield tactics as well. Given northern battlefield advantages in engineering, artillery, and naval power, the only battlefield tactic that has any chance of success was "to employ the strategic turning

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[120] Gary W. Gallagher, *Lee & His Army in Confederate History* (Chapel Hill: University of North Carolina Press, 2001), 163.
[121] Gallagher, *Lee & His Army*, 177-78.

movement and open-field maneuvering by infantry and cavalry in order to gain an advantage, attack the enemy, and inflict heavy losses."[122] Both will and skill, in other words, were endogenous effects of northern advantages.

Southern generals could select a defensive strategy, but when they did, northern superiority produced victories. Northern superiority could, alternatively as in the case of Lee, induce an offensive strategy with well-designed tactical innovations. This option produced brilliant victories that deeply shook northern morale, and so it seems that northern victory was not inevitable. Why then did the south lose? McPherson tells us that "Union leaders and armies were learning the skills needed to win, and each time the Confederacy seemed on the edge of triumph, Northern victories blunted the Southern momentum…these contingent turning points…eventually made it possible for superior numbers and resources to prevail."[123] But these northern battlefield victories were not fully contingent at all: they were made possible by superior numbers and resources, so that southern battlefield victories did not cause the north to grant independence and union generals like Grant and Sherman thus had the time to learn the skills needed to win. The only way that McPherson can argue for the ultimate contingency of northern victory is if he is willing to argue that *no* northern general could learn how to use his superior resources more effectively given ample battlefield experience: either northern officers would have had to have been incapable of learning, or northern political would have had to have been unrelentingly unwilling to give command to more effective generals. These assumptions seem quite far-fetched.

[122] Mackubin Thomas Owens, "Review of Gary W. Gallagher, *Lee & His Army in Confederate History*,"*Civil War Book Review* (Spring 2002), http://www.nybooks.com/articles/15481.
[123] McPherson, "Could the South Have Won?"

This extended illustration of how to endogenize seemingly contingent events is an example of what Miller calls "depth as necessity." X may in fact be a cause of Y, but it is a shallow cause, one that lacks causal depth, and hence a poor explanation, because "If X had not occurred, Y would have happened anyway; Z would have produced some causal substitute for X, bringing Y about in some other way."[124] Putting some sociological flesh on this philosophical skeleton, Stanley Lieberson suggests that we distinguish between basic and superficial causes and thus to distinguish between

> the consequence of a given causal principle (or hypothesis or theory) and the manner in which that consequence unfolds in a given context. In most cases, a given end product is generated by probably one significant basic causal principle; the form through which that basic cause is channeled may well be highly variable and affected by other causal principles that operate, as well as modified by the idiosyncratic history of that setting. Focusing on the channeling process will lead us to think that we are finding many causes, but these are what I have called the superficial causes…Chasing after the unique ramifications of that principle is harmless enough, just as long as we do not think that the superficial causal patterns are a substitute for a causal explanation in the basic sense used earlier.[125]

Reaching a determination about causal depth will not be easy. We will make no progress if our research designs do not explicitly address this issue. All too often, proponents of macrostructural arguments have often not explored in detail the determining effect of structures on microlevel action. But equally often, advocates of contingent explanation write as if evoking leadership, skill, will, and other individual-level properties exhausts explanatory possibilities. Neither alternative is satisfactory.

Finally, a fourth impediment to small/large explanations follows directly from the problem of causal depth: the problem of counterfactuals. The butterfly effect, as we have seen, involves a counterfactual scenario: run the weather a second time with the addition

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[124] Miller, *Fact and Method*, 98.
[125] Stanley Lieberson, *Making it Count: The Improvement of Social Research and Theory* (Berkeley: University of California Press, 1985), 189-90.

or subtraction of a butterfly's wings and observe the differences. Run history again with or without the contingent action of a particular individual and observe the differences. Counterfactual analysis has received a great deal of attention in recent methodological debates.[126] Although excellent, those discussions have not considered some restrictive conditions on the use of counterfactuals to establish causality. We can argue that "X caused Y just in case Y would not have occurred, under the actual circumstances, unless X had occurred," if and only if we make the following auxiliary claims.[127] The first condition is that we have properly distinguished between causes and symptoms. The light of distant stars evinces a red shift in their spectra. If there were no red shift, there would be no expansion. But the red shift is not the cause of the expansion; it is a symptom of an expansion caused by the Big Bang origins of the universe. Examples of this sort can be readily multiplied.

The second condition for counterfactual analysis is that we have properly ruled out the possibility of causal substitution. To say that X is the cause of Y because in the absence of X, Y would not have occurred is to ignore the difference between basic and superficial causes and thus to have denied the possibility of multiple superficial causes. If basic causes can operate through multiple channels, then no single channel can be understood, in counterfactual terms, as the cause of the outcome, for in its absence, the basic cause would work through an alternate channel. When financial markets crash, somebody made the first panicked move to liquidate her position. Someone *had* to be first, but that move is not a unique cause. Basic causes induce multiple triggers, but these triggers fail the counterfactual test because there are substitutes for them. When an

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[126] Tetlock and Belkin, *Counterfactual Thought Experiments in World Politics*.

[127] Miller, *Fact and Method*, 64. The following discussion follows Miller's treatment on 64-73, omitting the discussion of determinism.

explanation fails for lack of causal depth, it will also fail the counterfactual notion of causality. When causal substitutes exist, we are in a position exactly analogous to the butterfly effect; any individual cause might influence the idiosyncratic features of its operation, but it produces only effects that would have occurred in its absence.

The problems of causal transitivity, confirmation, depth, and substitution are neither unique to explanations linking small causes to large effects, nor are they irremediable. Addressing these issues will only make these explanations more convincing. My bet, however, is that in systematically addressing these challenges, scholars will find that lurking behind micro-causal explanations of macrolevel outcomes are other macrolevel conditions. To make that case, we turn now to the issue of rendering compatible microlevel indeterminism with macrolevel determinism: we turn, in other words, to the issues raised by quantum mechanics.

7. The Fate of Determinism in a World of Quantum Mechanics

Chaos theory poses an epistemic challenge to determinism. The equations modeling dynamical systems are fully deterministic, but the impossibility of sufficiently precise measurement imposes inherent limits to predictability. Chaos theory tells us that dynamical, non-linear systems are aperiodic and unstable: unlike classical determinism, in which state A is always followed by state B, in chaotic systems state A *never* recurs and we can *never* precisely measure the difference between A and A'. Moreover, because errors multiply, A' will not be followed by B', where the difference between A and A' is proportional to the difference between B and B'; A', rather, will be followed by C. If we grant that contingent events are analogous to measurement error (and we have seen reasons to not grant this assumption), then we can see how contingent events can have disproportionately large consequences. Chaos theory thus drives a wedge between determinism and prediction; in that sense, and only in that sense, does it defeat Laplace.

Quantum mechanics, in contrast, poses an ontological challenge to determinism: it says that if we observe A followed by B, and A recurs, then B will follow with probability p. And that is all that we can say, for there is no reason to believe that there are hidden variables determining whether A will be followed by B or not-B. The problem is thus not one of measurement: the probability functions that lie at the heart of quantum mechanics provide overwhelming reasons to believe that at the subatomic level, reality is indeterminate. To give just one striking example, an atom of uranium^{235} is an unstable element that undergoes radioactive decay by emitting an alpha particle from its nucleus. Alpha particles are in constant motion inside the nucleus, bouncing against its walls about 10^{21} times per second. When the particle breaks through the wall of the nucleus and escapes, we say that it has "tunneled out" of its nucleus. On average, an alpha particle tunnels out in 4.46 billion years: it has, in other words, about a one in 10^{38} chance of escaping every time it assaults the walls of its cage. What is the explanation for this? Wesley Salmon explains

> When we ask why a particular uranium atom decayed in this manner at this particular time, the answer is that an alpha-particle "tunneled out" of its nucleus. When we ask why the alpha-particle escaped on that particular trial, having failed on countless other occasions, the answer is simply that there is a probability of 10^{-38} of such as outcome on any particular bombardment of the wall. That is all there is to it. Perhaps you want to say that there must be some reason for the success on this trial and the failures on the others, but we do not yet know what it is. According to the most common current interpretation of quantum mechanics, however, that is not the case. We are, according to that view, dealing with an irremediably indeterministic process.[128]

[128] Wesley C. Salmon, *Causality and Explanation* (Oxford University Press, 1998), 41.

To put this same point in the language of observation, "There is no detectable difference between a uranium atom that is about to decay and one that's still far from splitting up."[129] The only explanation of the instant that a uranium atom decays is chance.

Consider, then, two ways to formulate a principle of universal determinism. The first is the principle of sufficient reason—an effect cannot occur with a cause producing it." The second is the principle of universal causation—everything that happens follows lawfully from its cause." Quantum mechanics forces us to drop both principles. As we shall see, there are effects which have no genuine causes: they just happen.

I argue in this section that quantum mechanics has no significant implications for the study of macrostructural phenomena. If it is true that at the subatomic level effects have no causes, then we have to reject both the principle of sufficient reason and the principle of universal causation. We do not, however, have to accept a new principle of universal acausality. We should accept instead a principle of segmented determinism: some effects have deterministic causes.[130] While quantum mechanics itself has no significant implications for macrostructural phenomena, it does force us to rethink the relationship of microlevel action and macrolevel phenomena in order to reconcile microlevel probability with macrolevel determinism. Subsequent sections will seek insights for thinking about this relationship in statistical mechanics and equilibrium statistical mechanics, not in quantum mechanics.

I want to spend a few pages reviewing briefly the history of quantum mechanics. I believe that this material is necessary to understanding why quantum mechanics offers a

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[129] Ivars Peterson, *The Jungles of Randomness: A Mathematical Safari* (New York: John Wiley & Sons, Inc., 1998), 181.
[130] Tim McKeown urges scholars "to extract the clocklike aspects from a social situation…" in his "Case Studies and the Statistical Worldview," *International Organization* 53 (Winter 1999), 179.

revolutionary understanding of reality at the subatomic level but has few or no consequences for our understanding of the macrolevel. Just as consumers of chaos theory extracted the idea of butterfly effects without understanding how they fit into the broader theoretical framework, so have consumers of quantum mechanics generalized ideas such as the uncertainty principle without regard to their context which sets the boundaries of their applicability.

The story of quantum mechanics can, for our purposes, be told in three stages: in the first stage, physicists grappled with some pressing problems that classical physics could not explain, resulting in a new view of atomic processes that were discontinuous or quantized; in the second stage, physicists developed a fundamental theory to explain the ad hoc quantization of stage one; and in the third stage, physicists formulated an interpretation, the Copenhagen Interpretation, of the fundamental equations of quantum theory. The most important components of the Copenhagen Interpretation are Heisenberg's uncertainty principle and Bohr's principle of complementarity. (A fourth stage, the development of quantum electrodynamics to explain the interaction of light and matter, has not been invoked as relevant to the social sciences and so will not be discussed here).

One of the great successes of 19th-century physics was Maxwell's theory of electromagnetic waves, which established that light was just the visible spectrum of electromagnetic radiation. That theory bequeathed several puzzling features, as it yielded incorrect predictions for blackbody radiation, the photoelectric effect, and the structure of the atom and its attendant stability. The solution to each of these three problems was to "quantize" radiation, light, and the structure of the atom. Max Planck began the revolution by proposing a formula for electromagnetic radiation, E = nhv. Energy is a function of the frequency of the radiation, multiplied by a constant, h, which is in turn multiplied by n, a positive integer.[131] Because n cannot have a value between 0 and 1, energy (E) cannot have a value between vh and 0. If quantities like $1/2vh$ and $1/4vh$ are excluded, then energy can exist only in discrete packets, not in continuous amounts. Planck's formula is thus "a measure of the amount of discreteness in atomic processes."[132] Similarly, overturning two centuries of theory and evidence that seemingly established that light is a wave, Einstein proved in his study of the photoelectric effect that light is emitted in packets of energy called photons. The quantization of light was even more revolutionary, however, because now light had to be understood as having a dual nature, acting alternatively as waves or particles. Einstein forced physicists to explain the wave-particle duality for light and reconcile these apparently contradictory properties of point particles with no spatial extension versus waves diffusing over large spatial regions.[133]

The last puzzle left over from Maxwell's theory was the stability of the atom, whose existence had finally become accepted over the objections of positivists such as Ernst Mach following Einstein's 1905 paper on Brownian motion. The model of the atom most of us learned in high school resembles a solar system, with negatively charged electrons orbiting the nucleus. Although this model, proposed by Ernest Rutherford, was consistent with experiments that examined how alpha particles scatter when they

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[131] Most of the books I consulted present Planck's formula without the positive integer n. For the inclusion of n and its importance for understanding the discreteness of energy, see Robert P. Crease and Charles C. Mann, *The Second Creation: Makers of the Revolution in Twentieth-Century Physics* (Rutgers University Press), 24.

[132] Heinz R. Pagels, *The Cosmic Code: Quantum Physics as the Language of Nature* (London: Penguin Books, 1982), 60.

[133] For details, see Ralph Baierlein, *Newton to Einstein: The Trail of Light* (Cambridge: Cambridge University Press, 1992).

bombard metal foil, it is inconsistent with classical physics. The problem was that as electrons orbit the nucleus, they are constantly accelerating and hence, according to Maxwell's theory, constantly radiating electromagnetic energy. As they lose energy, however, electrons should collapse into the nucleus; according to classical physics, atoms were highly unstable, a prediction quite obviously at odds with all observations. Moreover, electrons should radiate light in an unbroken spectrum corresponding to the full and continuous range of frequencies; observations of the spectra of atomic light, however, showed it to be discontinuous. The atom itself had to be quantized. Applying quantum theory to the structure of the atom, Niels Bohr deduced that electrons are allowed to inhabit only specific orbits, the lowest of which kept the electron from falling into the nucleus. Because only certain orbits are allowed, electrons can absorb and emit light—they can "jump" to higher orbits or fall to lower ones—only in discrete quantities. Since the energy emitted as light is quantized, and the energy of light is related to its color, Bohr's model explained why the spectrum of emitted light is not only discontinuous but displays colors unique and distinct to different atoms. The radical implication, however, was that electrons were not tiny planets orbiting the solar nucleus. Electrons do not physically jump between higher and lower orbits: they make a non-spatial transition. Bohr thus insisted that we not visualize the atom: we can have accurate quantitative predictions only at the expense of visualization. Mathematical representations were slowly supplanting visual representations.

These three accomplishments completely overturned the classical conception of energy while blurring the distinction between waves and particles. Whereas classical physics had treated energy levels as fully continuous, quantum mechanics discovered that energy levels are in fact discontinuous: energy exists in discrete amounts, as Planck's formula depicts. If classical physics understood reality like a guitar, where bending the strings created all of the frequencies or pitches between the twelve tones of western musical scales, quantum mechanics depicted reality as a piano which could play the note B (494 oscillations per second) or B-flat (466 oscillations per second) but nothing in between.

If the first stage of the development of quantum theory replaced a discontinuous for a continuous notion of reality, the second stage substituted a statistical for a deterministic understanding of that reality. Quantum theory provides only statistical information about subatomic particles in the form of probability distributions. Building on Einstein's argument that light possessed a wave-particle duality, Louis de Broglie reasoned by analogy that if a wave could sometimes act like a particle—Einstein's photon—then an electron, which was clearly a particle, could sometimes act like a wave. The behavior of waves is in turn captured by a mathematical description called a wave function. Erwin Schrödinger's equation provided a wave function for the standing wave of an electron in an atom that is not emitting or absorbing energy—not jumping between allowable orbits. This wave function raised the question: if one could count electrons as individual particles using a Geiger counter or cloud chamber, what were the waves? Electron wave functions are purely mathematical abstractions which do not refer to the physical existence of the atom: electron wave functions are not material objects. Rather, they are waves of probability that act as a mathematical atlas, a source of information about the electron. "By manipulating the wave function mathematically, for example, you can obtain what's called an electron's *probability distribution*, which is a kind of tabulation of how the probability for locating the electron varies or is distributed throughout the space."[134] More specifically, the square of the amplitude (height) of a wave at any point in space gave the probability of finding an electron there. This meant that quantum theory could not provide exact locations for position of a particle, but could only give the shape of the particle's wave function and hence the probability that a quantum particle would have certain properties. Just as knowledge of basic probability theory can give us the probabilities of any given hand of poker being dealt from a deck, wave functions specify only the probability that particles will have certain properties. The wave function, moreover, tells us that probabilities can propagate through space and thus change from point to point. Thus, only probabilities are causally determined, not individual events.[135]

In the third stage of the development of quantum theory physicists formulated interpretations of the basic equations of quantum theory. The basic interpretation that resulted was split into a physical and a mathematical interpretation. Both interpretations imposed radical limits on what could be known about the physical world. In the mathematical version, the uncertainty principle, Werner Heisenberg demonstrated that if p and q represent the momentum and position of a particle, and p and q are matrices, then

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[134] Roger S. Jones, *Physics for the Rest of Us* (Chicago: Contemporary Books, 1992), 158.

[135] Werner Heisenberg reached the same conclusion by attempting to avoid a physical interpretation and giving only a mathematical interpretation. In classical physics, physical variables describing particles are simple numbers obeying the laws of commutation. If the position of a particle (q) is 5 units from a fixed point, and the momentum (mass times velocity, or p) of that particle is 3, then we can multiply them as 5 x 3 or 3 x 5 and obtain 15. In Heisenberg's matrix mechanics, variables like position and momentum are not simple numbers, but matrices. Matrices do not have to obey the commutative law of multiplication: if p and q are matrices, not simple numbers, then p x q does not necessarily equal q x p. In fact, the difference between p x q and q x p is proportional to Planck's constant, which measures the degree of discreteness of atomic processes. This means that properties like the position and the momentum of a particle do not have definite values, just as they do not have definite values but only probabilities in Schrödinger's wave function. Indeed, Paul Dirac later showed that matrix and wave mechanics are completely equivalent.

no observer can simultaneously measure both properties with arbitrarily high precision. Repeated measurements of both properties will yield a range of numbers for both. Let ?p represent the spread of p around an average value; and let ?q represent the spread of q around an average value. Heisenberg's uncertainty principle then states that

(?p) x (?q) = h, where h = Planck's constant[136]

A number representing our uncertainty about the position of a particle, multiplied by a number representing our uncertainty about the momentum of that particle, must be a positive number. This means that if we had absolutely certain knowledge about the position of the particle, ?q would be equal to 0. The price of our certain knowledge about the location of the particle, however, would be that ?p would have to be infinite.[137]

The more typical interpretation of the Heisenberg uncertainty principle was actually advanced by Niels Bohr. Bohr's basic idea was that we cannot talk about nature without talking about the experimental apparatus which gives us the information we use in answering questions about nature. For quantum objects like electrons, unlike the objects studied by classical physics, experiments alter the state of the object: observations change what is being observed. Thus, to determine the position of an electron, we can shine on it a light of short wavelength; but the photons making up light of short wavelength are of high momentum, and so the momentum of the electron so observed is

[136] If your printer, like mine, won't print these symbols, read the equation as "the average variance in measures of p times the average variance in measures of q is greater than or equal to h.
[137] This interpretation of Heisenberg's uncertainy principle is drawn from Pagels, *The Cosmic Code*, who stresses that the principle applies to repeated measurements, about statistical averages over lots of measurements; it does not apply to single measurements of a single particle. Thus, it is a mistake to assume that the uncertainty principle implies that quantum objects are "fuzzy." Most other authors, however, refer to particles quite literally as "smudges."

altered by the photon's momentum. "Thus the more accurately we try to measure the position of an electron, the less we know after the measurement about the electron's momentum."[138] Bohr thus introduced the idea of "observer-created reality, which is the

> fact that the unavoidable interaction between the objects and the measuring instruments sets an absolute limit to the possibility of a behavior of atomic objects which is independent of the means of observation…As soon as we are dealing…with phenomena like individual atomic processes which, due to their very nature, are essentially determined by the interaction between the objects in question and the measuring instruments necessary for the definition of the experimental arrangements, we are, therefore, forced to examine more closely the question of what kind of knowledge can be obtained concerning the objects…[N]o result of an experiment concerning a phenomenon which, in principle, lies outside the range of classical physics can be interpreted as giving information about independent properties of the objects, but is inherently connected with a definite situation in the description of which the measuring instruments interacting with the objects also enter essentially.[139]

Bohr ideas culminated in the principle of complementarity. The particle interpretation and the wave interpretation, for example, are complementary interpretations: they are two different representations of the same thing, inconsistent with one another and yet complementary. The reason for the uncertainty principle is that complementarity implies simultaneous exclusion: knowledge of one property excludes knowledge of the other property. There is no experimental setting that will yield simultaneous knowledge of both properties. The two pieces of knowledge, each of which can be individually obtained, cannot be combined into a single picture. When we try to create that single picture, we achieve only contradiction: light becomes both a wave and a particle, which is seemingly impossible. Bohr's point is that there was no conflict within

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[138] Steven Weinberg, *Dreams of a Final Theory: The Scientists' Search for the Ultimate Laws of Nature* (New York: Vintage, 1992), 73.
[139] In his *Atomic Physics and Human Knowledge* (New York: John Wiley and Sons, Inc., 1958), 25-26.

any single experiment; the conflict arises only when we try to create a unified picture. Since no experiment shows light to be both a wave and a particle, no experiment evinces the contradiction. And to the objection that light must have a single nature independent of our experiments, Bohr nudged his interlocutors back to the starting point: “It is wrong,” he remarked, “to think that the task of physics is to find out how Nature *is*. Physics concerns what we can say about Nature.”[140] What we can say about nature is what we have learned from our experimental apparatus. All we can say, therefore, is that sometimes light is a wave, and sometimes light is a particle.

It is very tempting to jump immediately into applying the Copenhagen interpretation to human affairs: Bohr himself led that charge, arguing that the moral dilemmas comprising the tragedy of Sophocle’s *Antigone* represented the how the principle of complementarity operated in human affairs.[141] I think this intuition should be firmly resisted. As even the massively truncated summary of the history of quantum mechanics presented above demonstrates, two central planks of quantum mechanics are the idea of discontinuity, represented by Planck’s formula, which is absolutely essential to the Heisenberg uncertainty principle, and wave-particle duality. Neither of these key elements have direct analogues in human affairs. As in chaos theory, quantum mechanics applies to a very particular domain: the world of atomic and subatomic particles and processes. The next section considers the specific reasons why the lessons of this domain do not apply outside of it.

[140] Pagels, *Cosmic Code*, 84.
[141] The massive literature on the social construction of reality continues this inquiry into observer-created reality. See, for a fine summary, Ian Hacking, *The Social Construction of What?* (Cambridge: Harvard University Press, 1999).

8. Schrödinger's Cat: What Does Microlevel Indeterminism Mean at the Macrolevel?

Quantum mechanics has unambiguously destroyed the deterministic worldview derived from classical physics. Quantum reality is irreducibly indeterministic; and progress in fields such as chemistry and molecular biology has demonstrated that processes above the atomic level can ultimately be reduced to quantum physics and thus share this indeterminism.[142] This pervasive indeterminism has consequences for the form of explanations as well. As Wesley Salmon and others have argued, many of our explanations will be probabilistic, not deterministic, and the probabilities used in them to explain will likely be far lower than the near unity demanded by Hempel in his inductive-statistical model. To cite Salmon's most famous example, persons with untreated syphilis that passes through the primary, secondary, and latent stages may contract paresis, a form of tertiary syphilis. Untreated latent syphilis is absolutely a necessary condition for contracting paresis, but it is by no means a sufficient condition, for far less than one-half of the people with untreated latent syphilis contract it. At the moment, we have no understanding of what further characteristics might determine who does and who does not contract paresis: we cannot say, in other words, whether this is an example of epistemic or ontological indeterminism. Whether or not future research resolves this issue, for now we accept as a valid explanation the fact of untreated latent syphilis, even though the probability of paresis developing is considerably less than one half. Probabilistic statements, even those of low probabilities, can support valid explanations.[143]

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[142] On reductionism, see Steven Weinberg, *Facing Up: Science and Its Cultural Adversaries* (Cambridge: Harvard University Press, 2001).
[143] Salmon, *Causality and Explanation*, 38-42.

I have argued, however, that rejecting the doctrine of universal determinism does not necessitate rejecting the doctrine of segmented determinism. The implications of microlevel indeterminism for the macrolevel are not as unambiguous as they might seem. There are some reasons to believe that microlevel indeterminism can be sequestered at the microlevel so that our explanations can attribute causal effects to macrolevel deterministic structures.

Consider the paradox of Schrödinger's cat. It is said that when Schrödinger heard that Max Born had interpreted his wave mechanics equation as governing waves of probability, not material waves, he remarked that had he known of this implication, he would never have written his paper. Schrödinger's simple thought experiment is this: a cat is placed in a sealed box along with a detector of radioactive particles, a radioactive source, a glass bottle containing arsenic, and a mechanism that will break the bottle and release the arsenic if decay of the radioactive material is detected. The entire devise is calibrated so that there is precisely a fifty-fifty chance that the radioactive decay will be detected, killing the cat. Central to the Copenhagen interpretation is the idea of superposition: both states—radioactive decay causing feline death and no radioactive decay—are real *until the box is opened and the results of the experiment are observed*. Only with the act of observation does the wave function collapse into one of the two states. Until we look in the box, in other words, "there is a radioactive sample that has both decayed and not decayed, a glass vessel of poison that is neither broken nor unbroken, and a cat that is both dead and alive, neither alive nor dead."[144] Thus, at hard as it is to imagine an electron in a state of superposition, simultaneously occupying two

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[144] John Gribbin, *In Search of Schrödinger's Cat: Quantum Physics and Reality* (Toronto: Bantam Books, 1984), 205.

contradictory states, it is several orders of magnitude harder to imagine a cat suspended between life and death.[145] And the effect can get even weirder. For the scientists who first open the box to make the observation, the cat has a definite state, but until they communicate that observations to others, the entire experimental apparatus, including the scientists, are now in a state of superposition with no definite state, on and on in an infinite regress. Reality can be determinate for some people but simultaneously indeterminate for others. In the final act of weirdness, if the price of accepting the Copenhagen interpretation is that we accept the reality of the dead/live cat, inflation sets in and we might have to accept that the entire universe depends for its "realness" on its being observed. Or, alternatively, in the many worlds interpretation of the paradox of superposition, alternative realities do not disappear when their wave functions collapse upon observation: instead, observation cuts the ties between alternative realities so that Schrödinger's cat is alive and dead but in two different worlds.[146]

What implications do these rather curious paradoxes and thought experiments have for observers of the macroworld of social phenomena? In some domains, those implications are quite tangible. Microscopic switches in high-speed computers are so small that they can be directly affected by quantum effects in the microworld. In these soft errors, the radioactive material from which microchips are made randomly releases high-energy quantum particles that cause malfunctions. Closer to home, quantum

[145] The weirdness of superposition, as well as an accessible discussion of the technical experiments demonstrating it, is well conveyed in David Z. Albert, *Quantum Mechanics and Experience* (Harvard University Press, 1992), chapter 1. The technical demands of this book increase after chapter 1, but if you read to the end you'll obtain a rudimentary understanding of Bohm's theory which combines the mathematical formalism of quantum mechanics with the metaphysics of classical mechanics: in this theory, every particle has a determinate position which evolves according to completely deterministic motions. But if we are ignorant of that exact state, then the world will appear to evolve probabilistically. Wave functions, in other words, are real but do not collapse.

[146] These two alternative interpretations of the implications of the Copenhagen interpretation were put forth by John Wheeler and Hugh Everett, respectively.

features of chemical bonds affect the random combining of DNA molecules at the moment of conception.[147] More generally, however, there are good reasons to argue that the Copenhagen interpretation does not apply to the world of ordinary objects and experience; just as incorrect lessons were drawn from the butterfly effect, "Those who do apply [the Copenhagen interpretation] to the macroworld do so gratuitously."[148] There are two general considerations, a quantitative one related to size and a qualitative one related to time.

Size matters. Both de Broglie's original hypothesis about the wave nature of particles and the uncertainty principle relate quantum effects to mass. "The larger and more massive a body is, the less pronounced is its wave nature."[149] Indeed, as de Broglie wrote in 1939, the size of the theoretical indeterminism in any bodies larger than molecules is far smaller than the limits of experimental accuracy; therefore, the effects of theoretical indeterminism of quantum effects will be swamped by and hence masked by the perfectly acceptable level of errors introduced in the experiment. "In practice, as also in experiment, everything happens as though…there were a strict Determinism."[150]

More specifically, multiply the size of an object times its typical momentum, and compare this product to Planck's constant (6.62 x 10^{-27}), which sets the limits of certainty in measuring both position and momentum in Heisenberg's uncertainty principle. Because Planck's constant is so small, in order to undergo a substantial quantum effect, an object must be smaller than even a bacterium. For a tennis ball, the uncertainties due to quantum effects are about 10^{-34}; even for the bacterium, the uncertainties due to

[147] These examples are from Pagels, *Cosmic Code*, chapter 10, from which much of this section is drawn.
[148] Pagels, *Cosmic Code*, 140.
[149] Jones, *Physics for the Rest of Us*, 167.
[150] Louis De Broglie, *Matter and Light* (New York, 1939), 230, cited in Nagel, *Structure of Science*, 316.

quantum effects are only 10^{-9}, so quantum effects are negligible here as well.[151] Only the wave function of electrons is large enough to be diffracted by objects of comparable size in the microworld, like the atoms of a crystal. This means that macroscopic objects have the dual nature of microworld objects, but their wave functions are so small that no physical object can diffract them and thus we cannot detect their wave nature. With such negligible quantum effects, we can treat these objects like the particles of classical physics obeying classical deterministic laws.

Size matters even within the atomic level.[152] As atoms combine to form molecules and then crystals, the quantum effect diminishes and the new collectivity exhibits properties not present in the individual units. Two atoms that bond by sharing electrons (covalent bonding which can be understood only through quantum theory, as opposed to ionic bonding between positively and negative charged ions which was well understood prior to quantum theory) have a lower energy system that is thus a more stable form.[153] Now see what happens when molecules combine into crystals. Some of the electrons are shared by the entire crystal, producing “collective” quantum states and imparting them with unusual properties. The energy values of these collective states are, like blackbodies, photons, and individual atoms, still quantized into discrete levels, but the gaps between levels are much smaller than in individual states confined to microscopic regions. Indeed, in some cases these energy levels are so closely spaced that they are nearly continuous. This spacing in turn determines the level of conductivity of the crystals. Thus, in contrast to the fully quantized state of an electron, as we move to

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[151] Pagels, *Cosmic Code*, 82-3.
[152] This paragraph is based on Jones, *Physics for the Rest of Us*, 223-32.
[153] Electrons shared by two atoms have more room to move around. As the electron’s wave function spreads out over this greater space, it has a larger wavelength which means a reciprocally smaller frequency and thus lower energy. Lower energy systems are always more stable.

large-scale crystals the quantum effect is sharply reduced and the collectivity exhibits new properties.

Thus, as we increase size from the microworld to the macroworld, quantum effects become negligible and new properties such as conductivity emerge. This disjuncture between the microworld and the macroworld has many important implications, for it means that there are *two* causally relevant levels of descriptions. Consider, in the example favored by John Searle, hammers and nails whose usefulness to us is based on their solidity. That solidity is a real causal property of the macrosystem—the entire hammer and the entire nail. But that same solidity "is caused by the behaviour of particles at the microlevel and it is realised in the system which consists of micro-elements." Searle then reasons to an analogous relationship between mind and brain. Consciousness is a real property of the brain. At the lower level of description, we have the firing of neurons resulting in events like the contraction of muscles. At the higher level of description, we have intentions—states of consciousness—causing the movement of the arm. Both levels of description "are causally real, and the higher level causal features are both caused by and realised in the structure of the lower level elements."[154]

Consider next the qualitative differences between the micro- and macroworlds. The qualitative disjuncture between the two levels pertains to time: at the microlevel, time is reversible; at the macrolevel, time is irreversible: it has an arrow. Our experience is therefore not reducible to the world of atomic events. Heinz Pagels asks us to imagine a camera with a remarkable zoom lens that switches easily from the microworld to the macroworld. Using that camera, we film Niels Bohr smoking his pipe. Zooming into the microworld of particles, we see individual particles moving through the air and bouncing

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[154] John Searle, *Minds, Brains, and Science* (Cambridge: Harvard University Press, 1984), 26.

into one another, all fully obeying Newton's laws of motion. Now imagine the projectionist secretly running the film backward: nothing would change, we would still see the same particles bouncing around obeying the same laws of motion. At the microlevel, time has no direction because Newton's laws do not distinguish the past from the future. Indeed, this time neutrality is what allowed Laplace's demon to know both the past and the future.

Now we zoom to an intermediate level where the particles coalesce into wisps of smoke. We have lost information: instead of seeing individual particles, we now see their average distribution over time. When the projectionist runs this clip backwards, again without notifying us first, we might notice wisps of smoke condensing rather than expanding; we would quickly suspect that the film was running backwards because the condensation of smoke violates the second law of thermodynamics: over time, entropy or disorder is supposed to increase, not decrease. At the level of average effects, time begins to matter. As the camera pulls back to take in Bohr and his pipe, time is quite apparent. We would be quite shocked if we watched Bohr sucking in lungfuls of smoke as the level of tobacco in the pipe grew. We would know, without a doubt, that this film was running backwards; at the macrolevel, time's arrow constitutes our experiences.

The laws governing motion in the microworld are time invariant; laws governing the macroworld are not. The second law of thermodynamics cannot, therefore, be deduced from Newton's laws of motion. When we move from the micro-world to the macro-world, two things happen: we lose precise information, moving from detailed knowledge of particles to knowledge of their average distribution; and we increase entropy.

Considering the quantitative and the qualitative differences between the microworld and the macroworld demonstrates a clear disjuncture between the two domains, with quantum effects and the Copenhagen interpretation limited to the former. Three critical implications follow from this disjuncture between the micro- and macroworlds. First, descriptions of the macrolevel are not reducible to descriptions at the microlevel. This is because new properties emerge at the macrolevel. With reference to gases, for example, temperature and pressure are macroscopic variables. Their source is at the microlevel, but they describe the bulk properties of a gas. Second, because these new properties that emerge at the macrolevel frequently have causal significance—the solidity of hammers, the intentionality of minds, the pressure and temperature of gases--descriptions at this level may play absolutely essential roles in our explanations. Macrosystems have causal properties that are not reducible to the microlevel. Third, microlevel probability is perfectly consistent with macrolevel, non-statistical propositions, such as the claim of Boyle's gas law that the product of the pressure times the volume of a gas is proportional to its temperature. Boyle's law was discovered and considered to be an absolute law well before Boltzmann and Gibbs gave it a statistical foundation at the microlevel.

Why should this be so? Why should we reject the claim that every conclusion drawn from a statistical theory must also be statistical?[155] The answer to these questions comes from the field of statistical mechanics, to which we now turn.

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[155]This discussion is from Nagel, *Structure of Science*, 313-15.

9. Rendering Microlevel Indeterminism Compatible with Macrolevel Determinism

Imagine a set of microlevel objects which can interact with one another in various ways, producing configurations featuring time-dependent evolution. Each possible configuration of these micro-objects is a microstate, m_i to m_j. These microstates are in turn responsible for various macrolevel properties that are rooted in but not reducible to the microlevel configuration: temperature, solidity, or intentionality, as in the examples cited above. Our explanation of the various macrostates thus depends on the correspondence between macro- and microstates. One simple way to think about that correspondence is this: How many microstates correspond to a single macrostate?

The total number of microstates m_i to m_j is a function of the total number of microlevel objects raised to the power of the number of states each can inhabit. For example, if there are four units, each of which can occupy 1 of 10 states, the total number of microstates is 4^{10} or 1,048,576. Because the total amount of variation that is possible at the microlevel is so vast, some of which cannot be detected and most of which is too fine-grained to catch our attention, in almost all instances, the number of macrostates will be less than the number of microstates. Let m_k refer to a subset of the total number of microstates m_i to m_j that corresponds to a given macrostate, M_1. That is to say, there may be a large number of possible configurations of the micro-objects that, for all intents and purposes, all correspond to the same macrostate. This is, of course, the subject of combinations in probability theory, which asks how many different ways a set of *r* objects can be chosen from *n* distinct objects, e.g., how many committees of six can be formed from the entire U.S. Senate. Each particular combination of six senators is a microstate corresponding to the same macrostate M_1: they thus all belong to m_k, which is a subset of all of the possible ways of configuring 100 objects.

If m_k is a large proportion of m_i to m_j, then the transition between microstates within m_i to m_j may be fully statistical but because virtually all of the possible microstates will still be within m_k, then M_1 will overwhelmingly be the outcome. For all intents and purposes, M_1 is the inevitable outcome of a probabilistic process. The statistical nature of transitions at the microlevel does not affect the stability of the macrostate.[156] This is the case, for example, in the kinetic theory of gases developed by Boltzmann, Maxwell, and Gibbs in the latter half of the 19th century; in their statistical interpretation of the second law of thermodynamics (entropy always increases), an overwhelmingly large number of microstates corresponds to a single macrostate, the mean kinetic energy of the gas. As each gas molecule changes its position, its momentum, and thus its arrangement with all other molecules—as the molecules cycle through all of the possible microstates, each of which has the same (low) probability of occurring as every other one—the macrostate "average kinetic energy" stays the same. This is why, when we release a volume of chilled air into a heated room, the cooler air does not remain together in one corner of the room; rather, the average temperature of the room decreases as the chilled air becomes randomly distributed throughout the room. The reason for this is purely statistical: of all the possible arrangements of the trillions and trillions of particles, only a very few of them group all of the chilled air together. The vast majority of arrangements, all of which are equally possible, ensures that the original configuration of a solid block of cold air will recur only once in billions and billions of lifetimes. More precisely, the initial condition of uniform blocks of hot and cold air occupies a very small portion of the

[156] Nagel, *Structure of Science*, 314, is the source of this description of statistical mechanics.

volume of phase space; it is a very improbable arrangement. Time evolution then leads to a new region of phase space with a very large volume; this is a very probable state of the system. The laws of physics are not irreversible; what we see instead are the highly probable happening and the highly improbable not recurring. A return to the original condition is possible and so after a long period of time the system will return to it. How long? When physicists create idealizations of these situations, they allow the number of particles to tend to infinity, and so the time of the infamous eternal return also tends to infinity.[157] Thus, while it is possible for a beaker of water at room temperature to spontaneously freeze, those of us who like ice in our cocktails should probably not unplug the freezer.

The implication of this analysis is this: we cannot make a priori judgments about determinism versus probability; rather we have to make estimates of the ratio of macro-inducing microstates to the total number of microstates and enter into arguments about correspondence rules. Statistical mechanics is the limiting case, where virtually all of the microstates correspond to a single macrostate. This limiting case is not an analogy or a model of social life: it does establish that, in principle, microlevel indeterminism can be reconciled with macrolevel determinism. Making that principle relevant to social analysis requires adding several additional elements to the discussion.

Note first that the correspondence between microstates and any given macrostate depends on the level of abstraction with which we define the specified macrostate. Consider the proverbial ton of bricks. We bring the bricks up in a helicopter to a height of one thousand feet and drop them on an empty field. Will the bricks form an exact replica of Thomas Jefferson's rotunda which graces the lawn of the University of

[157] Ruelle, *Chance and Chaos*, 111-13.

Virginia? While statistically possible, the answer is no, because only one of the astonishingly huge number of microstates comprisable from a ton of bricks exactly corresponds to Mr. Jefferson's rotunda. In fact, we can say with virtually complete certainty that the bricks will form nothing but a pile, because the class of microstates m_k corresponding to macrostate M_1 "pile of bricks" is an overwhelmingly huge proportion of the total number of microstates m_i to m_j. We would not be able to predict which microstate would result, but we could predict quite easily which macrostate would result: that described as a random pile of bricks. For all intents and purposes, the random pile of bricks is the predetermined outcome of a probabilistic process.

But suppose further that for some reason (I can think of none, I'll admit), we suddenly begin to care about piles of bricks, making finer and finer distinctions based upon the spatial dimensions of the pile, the number of bricks that lay atop one another, the number of bricks perpendicular to one another, etc. As we make finer and finer distinctions, the number of microstates would not change, but the number of corresponding macrostates would increase. As the number of macrostates increases, the number of sets of microstates corresponding to each macrostate increases as well, and, since the total number of microstates remains the same, the ratio of each set of microstates m_k decreases as a proportion of the total number of microstates m_i to m_j. As that proportion decreases, the probabilistic nature of the microstates is transferred to the macrolevel. We could no longer say that a "random pile of bricks" would result: rather, we would have to give equal odds to different types of piles. Thus, the relationship of microlevel probability to macrolevel determinism is in part a function of the level of abstraction at which we define our macrostate.

Let us here recall Alan Garfinkel's idea of an equivalence class, which "consists of the set of 'inessentially different' objects collapsed into one for this purpose."[158] If we are to argue that every member of a subset m_k of all microstates m_i to m_j corresponds to macrostate M_1, then we have to have a clear sense of what counts as M_1. M_1 refers to a single equivalence class. Thus, to consider using the example of the rotunda, piles of bricks that basically replicated the rotunda but differed from it, and from each other, due to relatively minor differences in the dimensions or in spatial orientation would all count as members of the same macrostate.

Increasing the size of the equivalence class decreases the total number of macrostates; decreasing the number of macrostates, as we saw above, increases the size of each subclass m_k as a proportion of the total set of microstates m_i to m_j. Increasing the size of the macrostate by imposing on it a large equivalence class stabilizes our explanations: many perturbations which affect the highly idiosyncratic differences between members of the same equivalence class are filtered out by the large-grid mesh we have used in constructing our equivalence class. As the specific details of the object of our explanation are omitted, the specific features of the explanation itself can also be omitted.

We have, of course, seen this relationship already. In the discussion of the butterfly effect, we noted that the hypothesis of the butterfly effect means that minor butterfly-like perturbations "neither increase nor decrease the frequency of occurrence of

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[158] Alan Garfinkel, *Forms of Explanation: Rethinking the Questions in Social Theory* (New Haven: Yale University Press, 1981), 31, n. 7. Fearon suggests as an example different ways of conceptualizing World War I, asking "does 'World War I' mean (1) a war between the European great powers that begins in 1914; (2) a war between at least four great powers beginning in the period 1890-1920; or something in between?" The first formulation is a narrowly construed equivalence class, while the second is a highly permissive class. See his "Counterfactuals in Social Science," 58.

various weather events like tornados; the most they do is modify the sequence in which these events occur." If we postulate that the butterfly effect causes a specific tornado with a specific force at a specific time in a specific place—if we construe our equivalence class quite narrowly so that includes in essence only one event—then the butterfly effect seems quite powerful. But what Lorenz is suggesting is that we construe the equivalence class quite broadly—every year, a relatively stable number of tornados visit some part of Texas—then the butterfly effect, in effect, does not matter, for the same equivalence class occurs with or without it.

The equivalence class constitutes the object of explanation. Large equivalence classes are more stable than narrow equivalence classes: because they include more events with fewer specific details, they are less affected by microlevel heterogeneity. Because we can, for pragmatic reasons, manipulate the size of equivalence classes, we can also manipulate the significance of microlevel heterogeneity and we can thus manipulate the significance of contingent events and irreducible probabilism. This is the lesson of statistical mechanics.

The lesson of statistical mechanics, however, ends with a whimper, not a bang. *If* each microstate has an equal probability of being realized, then the only way to derive deterministic outcomes is to make m_k essentially equivalent to the entirety of m_i to m_j. If that condition is not met, then M_1 will occur with a frequency proportional to the size of m_k, but other macrostates will repeatedly occur: microlevel probability will become manifest at the macrolevel.

But not all microstates have an equal probability of occurring; this is why have social theories of power, authority, institutional constraints, normative regulation, and culture. These theories all consider how the macrolevel constrains the microlevel. They have frequently done so, however, by illegitimately assuming uniform responses at the local level. That assumption is not necessary, as we see when we turn to equilibrium statistical mechanics.

The basic point about equilibrium statistical mechanics is this: if we impose a global restrictive condition upon a complex system, we can determine its microscopic probabilistic structure; the configurations satisfying the global restrictive condition "usually have a cluster of probabilistic features that uniquely characterizes their configurations."[159] In other words, the global condition usually guarantees that from the total number of microstates m_i to m_j, one of the microstates m_k corresponding to macrostate M_1 will be "selected," without determining which of the microstates within m_k is realized. The global condition usually determines M_1 by determining a microscopic probabilistic structure—the set m_k--corresponding to M_1.

Two examples, courtesy of David Ruelle, illustrate equilibrium statistical mechanics: boiling and freezing water:

> If we take a liter of water and lower the temperature, it is not unreasonable that it should become more and more viscous. We may guess that at low enough temperature it will be so viscous, so still, as to appear quite solid. This guess about the solidification of water is wrong. As we cool water we see that at a certain temperature it changes to ice in a completely abrupt manner. Similarly, if we heat water it will boil at a certain temperature, i.e., it will undergo a discontinuous change from liquid to water vapor. The freezing and boiling of water are familiar examples of *phase transitions*…According to our general philosophy, imposing a global condition (in this case fixing the temperature) has the result that all kinds of things about the system are uniquely specified (*usually*). Given a snapshot of the configuration of atoms in helium at 20°C, you should be able to distinguish it from a snapshot corresponding to another temperature or another substance, in the same way as you distinguish a van Gogh from a Gaugin at a glance. The "cluster of probabilistic features" changes with temperature, and the change is usually gradual…And then the unexpected occurs. At a certain

[159] Ruelle, *Chance and Chaos*, 116-17.

> temperature, instead of gradual change you have a sudden jump—from helium gas to liquid helium, or from water to water vapor or to ice.[160]

The idea that a global condition can determine the probabilistic features that uniquely characterize configurations satisfying that global condition—that we can, under certain circumstances, move from a global condition (temperature) to the microlevel (configurations of water molecules) and back to a macrolevel feature (water molecules crystallized into ice) provides one half of a bridge linking the non-social reality of statistical mechanics to the social reality of politics. The examples tell us to consider the possibility that a global condition restricts microlevel probability, so that not all microstates occur with equal probability.

Consider the challenge to determinism posed by Stanley Lieberson. While expressing a preference for deterministic statements—"They are cleaner, simpler, and more easily disproved than probabilistic ones."—Lieberson gives several reasons for casting our explanations in more modest probabilistic terms. Some of those reasons are technical or pragmatic; deterministic explanations are frequently either infeasible or impractical, or both. But even if these problems could be resolved, Lieberson argues, there remains the role of chance events, or random influences which could not be taken into account even by Laplace's demon.

Lieberson illustrates this claim by looking at the determinants of who wins the Super Bowl. He provides a series of compelling illustrations of how the outcomes of close games could have gone the other way had not "small "random" events seriously impacted the outcome." Lieberson ends the essay with a fine discussion of why random factors play a role not only in sports but also in broad macrosocial events. Again, his

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[160] Ruelle, *Chance and Chaos*, 122-23.

arguments are generally pragmatic, consisting of reasons for adopting the probabilistic perspective even while believing that deterministic processes are actually operating. But Lieberson insists, as well, that "The role of random factor is, of course, relevant to a wide variety of social contexts."[161]

Two features of the discussion above of equilibrium statistical mechanics can be brought to bear on Lieberson's argument. First, in discussing the impracticality if not infeasibility of deterministic explanations of historical events, he defines the outcomes to be explained quite narrowly, essentially eliminating all other potential members of the equivalence class:

> Suppose we take a deterministic view of history such that a given outcome, say a war or the assassination of a prominent political figure, *had* to happen when and how it did. What knowledge would it take to locate and understand every conceivable influence for and against the outcome's occurring in the exact time and place that it did?[162]

Insofar as we define the dependent variable to include the exact time and place of its occurring in the exact manner that it occurred, Lieberson is absolutely correct. As I argued above, as we narrow our equivalence class by making membership conditional on things happening "when and how they did," we allow for more macrostates, thus emphasizing the role of chance. But we are under no compulsion to draw our equivalence classes so narrowly.

Second, Lieberson's example of the Super Bowl is one that manifestly lacks a global condition that restricts variation at the microlevel. Although the two teams competing for the championship may not ever be precisely evenly matched, there is generally a rough balance. If we could calculate all of the possible microstates—the

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[161] Lieberson, "The Big Broad Issues in Society and Social History," quotations from 364, 373.
[162] Lieberson, "Big Broad Issues in Society and Social History," 373.

positions and momentum of all 22 players over approximately 150 plays—and assign each microstate to one of two macrostates (Team A wins and Team B wins), we would find, I think that roughly equal numbers of microstates correspond to each macrostate. A random event that chooses this microstate over that microstate could indeed turn the outcome of the game.

But imagine if we imposed the following global condition: of the two teams playing a basketball game, one team was composed of the starting lineup of the New York Knicks, and the other was composed of the members of the APSA panel on Probability, Contingency, and Determinism: Messrs. Adcock, Ertman, Hall, Lieberson, and Stephens. The huge imbalance of talent, athleticism, and size does not eliminate all of the probabilistic features of the microlevel; at any given point in time, to continue the physical metaphor, the position and momentum of each player relative to the rest would not be determined by the global condition. But the huge imbalance of talent would overwhelmingly select, from among the large number of possible microstates, a subset of microstates, m_k, which corresponds to a single macrostate, M_1, defined as "Knicks win." It is quite difficult to conceive of *any* microstate corresponding to the macrostate "social scientists win."

Neither the argument from equivalence classes nor the argument from equilibrium statistical mechanics address all of the points raised by Lieberson; and neither of them impose upon us a preference for deterministic statements. They create, I hope, some grounds for thinking about the ontological conditions under which deterministic theorizing will be more useful and valid.

Still, even allowing that a global condition might coexist with microlevel probability and restrict the range of microlevel configurations does not create a secure foundation for macrostructural analysis. The example just cited continues to make use of the highly artificial assumption that all of the microstates correspond to a single macrostate. That assumption does not correspond to all social settings, as Lieberson's example of the Super Bowl; and there is no reason to believe that it corresponds to even a small minority of settings. Heterogeneity at the microlevel is greater than the example allows. Microlevel heterogeneity is, in many if not most cases, underdetermined by any macrolevel conditions. I argue, however, that even microlevel heterogeneity that cannot be explained by a global or structural condition does not *necessarily* preclude structuralist arguments.

Consider again Jon Elster's response to the argument that inter-capitalist competition forces capitalists to exploit workers.[163] Even if we accept the existence of the macrostructure, Elster responds, the conclusion does not follow: the behavior of individual capitalists is not determined by the overarching structure but rather contains an inherently probabilistic element. We know this because autonomous agents have choice, and so any capitalist has the option of becoming a worker. Elster makes the fully legitimate argument that the global condition does not fully determine its corresponding microstates; these are influenced both by the global condition and by a set of heterogeneous and idiosyncratic factors that we cannot possibly account for. In a word, the microstates evince irreducibly probabilistic features.

But we might ask what effect this microlevel probability has on the macro-structure. The free will of the compassionate capitalist, which is the source of the

[163] Elster, *Nuts and Bolts*, 14.

microlevel indeterminism, does not affect the macrostate. Why? Because two things happen when the capitalist joins the labor force. First, the labor pool is incrementally enlarged, incrementally driving down wages. Second, the average expression of compassion among capitalists drops as the most compassionate capitalists leave. Any normative aversion to exploitation that might counteract the incentive to exploit is correspondingly reduced. Thus, the global condition—competition-induced need to exploit workers—is reproduced and continues to exert pressure on capitalists, who will continue to exploit workers. In this example, microlevel indeterminism is perfectly compatible with macrolevel determinism up until the point that *all* capitalists have opted to become workers, an outcome I consider to be as likely as the spontaneous freezing of water at room temperature. Put differently, Elster is right to insist on the underdetermination of the microstate by the macrostructure: the microlevel is irreducibly probabilistic. But he errs in not considering whether the underdetermined microstate remains a member of the subset m_k of the total set of microstates m_i to m_j that corresponds to M_1.

There are two important reasons why my account differs from Elster's. First, he takes as his object of explanation the behavior of individual capitalists. This is fully consistent with his long-stated views on methodological individualism. But it completely misses the point about the object of explanation of structuralist arguments, which is not individual-level behavior (the causal mechanism linking macrostates) but rather macrolevel phenomena. Consider Alan Garfinkel's discussion of a closed system of foxes and rabbits, in which the population of foxes is increased so that individual rabbits have no choice but to more frequently pass through fox-inhabited zones. We cannot predict with any certainty which foxes will eat which rabbits; any individual rabbit might turn left rather than right, avoid the lurking fox, and thus survive. At the microlevel, multiple microstates exist that are not predetermined by the global condition, an increase in the fox population. But we do not need every piece of information about the microlevel to know, with a great deal of certainty, that an increase in the level of foxes will reduce the population of rabbits. The object of explanation here is the movement from one macrostate to another, and a relatively deterministic law governing that transition is compatible with microlevel probability.[164] To repeat a point made earlier, new causal properties emerge at the macrolevel, and, depending on our object of explanation, these properties may take precedence in our explanations.

Second, despite his emphasis on mechanisms, Elster's account remains wedded to the regularity interpretation of causality and determinism. Elster has adopted the language of mechanisms but not the ontology of mechanisms. He defines mechanisms as "frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with indeterminate consequences."[165] Key to the definition is that mechanisms are not defined as entities having causal powers that produce outcomes, but rather as causal patterns—as regular conjunctions of events. The suspicion that Elster still adheres to a regularity principle, despite the fact that he explicitly contrasts mechanisms to black-box regularity, is confirmed when he contrasts mechanisms and mechanism-based explanations to lawlike phenomena and covering-law explanations:

> In more abstract terms, a law has the form "If conditions C_1, C_2,…C_n obtain, then always E." A covering law explanation amounts to explaining an instance of E by

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[164] Garfinkel, *Forms of Explanation*, 54-58.
[165] Elster, "A Plea for Mechanisms," 48.

> demonstrating the presence of C_1, C_2,…C_n. At the same abstract level, a statement about mechanisms might be "If C_1, C_2,…C_n obtain, then sometimes E."

Elster's conception of mechanisms, then, overlaps extensively with Carl Hempel's definition of the inductive-statistical method, lacking only a formal requirement for the inclusion of statements of initial conditions in the explanation. As I argued above, Hempel's entire explanatory scheme fails to explain. Mechanism-based explanations are a replacement for the entire Hempelian apparatus in both its deductive-nomological format and in its inductive-statistical format. The problem with Hempel's model, to repeat a point made earlier, is that it "explains" by way of logical deduction from laws, but these laws are neither necessary nor sufficient for explanations. Inserting "mechanisms" as intermediary steps does not solve the problem at all; nor does it help much to drop the condition of "invariant association" in favor of "occasional association."[166] We need to think of mechanisms as causal agents, not as patterns of association. In the next section, I argue that microlevel probability is consistent with macrolevel determinism if and only if the subset of microstates m_k is 1). a necessary but not exclusive effect of a global condition; and 2). a sufficient condition for the macrostate M_1.

10. Making Macrolevel Determinism Safe for Social Science

Deriving lessons from statistical mechanics and equilibrium statistical mechanics might create the impression that determinism, far from being hard to establish, is too

[166] Elster bases his approach on an important argument by Nancy Cartwright that demonstrates why covering laws are not *needed* to explain. He has omitted discussion, however, of why covering laws, whether deterministic or statistical, are not *sufficient* to explain. This is why his version of mechanism-based explanations neglects the ontological dimension of causality and remains instead at the level of regularity and patterns. For a brief introduction to Cartwright's distinction between laws of association and underlying causal structures, see her "The Reality of Causes in a World of Instrumental Laws," in Richard Boyd, et. al., eds., *The Philosophy of Science* (Cambridge: The MIT Press, 1991), 379-86.

easily established. That intuition is correct, for I have built a biased case. In statistical mechanics, the microstates, in effect, do not matter: pour a quart of warm water on top of a quart of cold water and you get two quarts of lukewarm water. Of the trillions of microstates, just about every one gets you to the lukewarm water. Likewise in the case of freezing water, the microstates essentially all correspond to the same macrostate.

In Elster's account, on the other hand, this does not at all seem to be the case. He has stressed irreducible indeterminateness in the conditions triggering mechanisms and in their consequences, so that micro- and macrostates do not correspond so neatly. Elster illustrates his point with the example of children of alcoholics who might become either alcoholics or teetotalers. "Both reactions embody mechanisms: doing what your parents do and doing the opposite of what they do. We cannot tell ahead of time what will become of the child of an alcoholic, but if he or she turns out either a teetotaler or an alcoholic, we may suspect we know why."[167] Elster does not note how in his example both ends of the relationship are indeterminate. Parental behavior underdetermines their children's behavior; we do not know and probably cannot know which conditions trigger which mechanism for which children. But we might add that children can pick one or the other mechanism and still stop short of either full-blown alcoholism or abstention: they might become social drinkers. Given this indeterminacy, all we can do is point to mechanisms, to add some fine-grained detail. Being the child of an alcoholic is not equivalent to placing a layer of cold water on top of a layer of warm water: while the latter always produces lukewarm water, the former does not always produce social drinkers. If it is true that a given set of conditions do not sufficiently trigger mechanisms

[167] Elster, "A Plea for Mechanisms," 45.

and thus underdetermine microstates; and if it is true that microstates underdetermine consequences; then how can we make causal arguments from structures?

Just as the recognition of irreducible indeterminism in quantum mechanics does not force acceptance of acausality in all causal orders, neither do Elster's many ingenious examples of indeterminism in human action force complete rejection of determinism in all causal orders.[168] We can make explicit arguments (and not merely assume) that structural conditions determine outcomes by hypothesizing the existence of a global restrictive condition—a structure, S_1--that is sufficient for the production of a causally privileged subset of microstates: some sets of microstates have more causal capacity than other microstates that result from the same initial conditions. The presence of these privileged microstates in the overall mix of microstates is then subsequently sufficient to produce determinate outcomes. If m_i to m_j is the total set of microstates; if m_{k1} to m_{kn} denotes the set of subclasses of microstates, each of which corresponds to a particular macrostate M_1 to M_n; then m_{k*} refers to the privileged subclass of microstates that is sufficient to produce M_1. If S_1 is sufficient to produce m_{k*}; and if m_{k*} is sufficient to generate M_1; then S_1 is a structural and deterministic cause of M_1, albeit a structural cause that is compatible with heterogeneity at the level of microlevel causal mechanisms.

The basic idea is that *not everyone has to act uniformly for the resulting array of actions to produce an outcome.* If a global condition causes some but not all people to act in a particular way, then we say that the global condition underdetermines the behavior at the microlevel. But if the actions of some of those people inevitably fall within the subclass of privileged microstates m_{k*}, so that their behavior is sufficient to

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168 We might note, however, that while indeterminism in quantum mechanics is ontological, the indeterminism in Elster's examples is epistemological: something other than pure chance presumably determines behavior.

produce an outcome at the level of description set by the analyst, then the global condition causes the outcome: microlevel probability is perfectly consistent with macrolevel determinism.

There are two halves to this claim, corresponding to Elster's two types of indeterminism. On the one hand, we must forge a necessary link between one or more global conditions, S_1, to a subset of microstates, m_{k^*}, even when we know that other subclasses are consistent with the global condition. On the other hand, we have to argue that m_{k^*} is genuinely sufficient for the outcome M_1.

With reference to the first issue, consider the game of chess, a favored metaphor for indeterminism for so many moves are possible that predictability at any point in time is highly unlikely. Yet there are some circumstances that constitute what Daniel Dennett calls a "forced move." As he explains,

> Such a move is not forced by the rules of chess, and certainly not by the laws of physics (you can always kick over the table and run away), but by what Hume might call a "dictate of reason." It is simply dead obvious that there is one and only one solution, as anybody with an ounce of wit can plainly see. Any alternatives are immediately suicidal.[169]

Do such forced moves exist in the social reality studied by political scientists? We cannot establish them as an ontological category, but we can make highly reasonable arguments for their existence in concrete settings. Ertman seems to have something like a forced move in mind when he writes of those groups in early modern European society who control administrative, financial, and military resources needed by the king and who subsequently "seek to negotiate or extract terms of service which will protect and/or extend their privileges, status, and income in the face of the potentially unlimited and

[169] Daniel Dennett, *Darwin's Dangerous Idea: Evolution and the Meanings of Life* (New York: Simon and Schuster, 1995), 128.

arbitrary authority of the patrimonial monarch or prince."[170] We are just not surprised when people seek to protect and extend their privileges; we are surprised when they do not. All I am saying here is that at least some of the people faced with incentives and some capacity to protect and extend their privileges will indeed do so.

Similarly, Rueschemeyer, Stephens, and Stephens surely raise few eyebrows when they argue that "those who have only to gain from democracy will be its most reliable promoters and defenders, those who have the most to lose will resist it and will be most tempted to roll it back when the occasion presents itself."[171] Rueschemeyer, Stephens, and Stephens never argue that these are "objective" interests that unproblematically are recognized by every member of the class. Interests, they insist, are ambiguous because they are socially constructed and because they are mediated by forms of collective action. Consequently, they fully recognize heterogeneity at the microlevel when, for example, they express absolutely no surprise that "the goals of different movements claiming to act on behalf of the same class differ considerably from each other." This heterogeneity stems from a diverse array of "situational determinants" ranging from the "mentalities, outlooks, and ideological inclinations" of the members of a class to the prevailing economic, political, and social situation which influence evaluations of "what makes sense under the circumstances."[172] All of these sources of heterogeneity and historical particularity aside, it still strikes most of us as eminently sensible to depict large landowners, particularly those using labor-repressive mechanisms, as typically anti-democratic. We cannot predict that all large landowners will choose this stance, but we would be quite surprised by the historian who uncovered

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[170] Ertman, *Birth of Leviathan*, 8.
[171] Rueschemeyer, Stephens, and Stephens, *Capitalist Development and Democracy*, 57.
[172] Rueschemeyer, Stephens, and Stephens, *Capitalist Development and Democracy*, 53-7.

evidence of large landowners who uniformly and voluntarily granted unrestricted franchise to landless peasants.

Not all moves are forced moves; perhaps most moves are not forced moves. But if some forced moves exist—and this is ultimately a matter of theory and evidence, not ontology and epistemology—then we have a necessary but not a sufficient condition for macrolevel determinism.

I want to stress that even when talking about forced moves, we have not in any way ruled out a role for essentially random causes. If most but not all people respond in the predicted way to forced moves, then something else is going on that explains the variance. Commenting on Albert Soubol's treatment of the *sans-culottes* of the French Revolution, Richard Cobb writes

> He does name the militants, but he does not give any of them the benefit of a personality. The result is that we can see how they operated, but we gain virtually no impression of what they were like, whether they were sincere or were time-servers, whether they were out for publicity or for the fruits of office, whether they had sound sense or were crackpots. We just have to accept that they were militants and that something, whether ambition or sincerity, distinguished them from the general mass of their neighbours.[173]

Perhaps no more can be said about what distinguishes the militants from their neighbors. Cobb makes a strong case for studying the militants more intensively, but he does not and cannot make the case that he knows what distinguishes militants from their non-militant neighbors, for the evidence of his study is generated by the actions of the militants themselves: he has no grounds on which to compare them to the non-militants. But this does not matter: the point for macrostructural analysis is that nothing more *needs to be said*, if we can establish that the existence of those militants is sufficient for the outcome at hand. Just as, in the example discussed above, we do not need to know which foxes

[173] Richard Cobb, *The French and Their Revolution* (London: John Murray, 1998), 175.

ate which rabbits, so we do not need to know which agents exposed to a global condition did or did not become militants. It is the existence of the militants that is important, not their constituent elements.

This brings us to the second half of the macrostructural argument, to the challenge of Elster's "Type B" causal mechanisms with their attendant indeterministic relationship to outcomes. Even if we grant that macro-condition S_1 inevitably produces a subset of microstates m_{k*}, even as we recognize that other subsets of microstates also follow from S_1, we still have to establish that m_{k*} is itself sufficient for M_1. Our question is, then, is it ever plausible to argue that m_{k*} is sufficient for M_1?

In principle, the answer is absolutely yes: there are fairly easy ways to establish that there are circumstances under which m_{k*} is sufficient for $M_{1.}$ Jack Goldstone provides the most concise reasoning behind this claim.[174] Imagine, he offers, that under condition X, there is an even chance that people will do Y. This statement, of course, exactly mirrors, Elster's regularity-based definition of a mechanism; and like Elster, Goldstone does not inquire into *why* those people do (or do not do) Y. Goldstone's point is one Elster does not consider: if Y means "engage in revolutionary activity," then condition X suffices for literally thousands or even millions of people to engage in revolutionary activity. The behavior Y that is a probabilistic response to X is also a sufficient condition for B: revolution.

We cannot generalize Goldstone's point for two reasons. First, his dependent variable is revolution, not, as in the case of Skocpol, *successful* revolution. Goldstone's logic could be sufficient for his outcome but still not be sufficient for Skocpol's outcome. More generally, whether m_{k*} is sufficient for M_1 is in part a function of how we

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[174] Goldstone, "Methodological Issues in Comparative Macrosociology," 117.

conceptualize our object of explanation. As this act of conceptualization is a pragmatic choice and not a logical operation, there cannot be any algorithmic rules governing it and so we cannot say a priori whether a given object of explanation will have a sufficient cause.

The second barrier to the generalization of Goldstone's powerful logic is that size matters. Because we cannot be certain about how any individual will react, when the relevant population is small, stochastic variation may be relatively large and thus eliminate certain outcomes. When the population affected by the structural condition is large, on the other hand, "it really doesn't matter much what particular individuals do; the standard deviation of the expected group outcome (which is proportional to the inverse of the square root of the number of individuals involved) will be miniscule."[175]

Indeed, these two considerations probably interact: for revolutions, which require that large numbers of people act, the initial pool of potential participants must be quite large; for a coup, on the other hand, a smaller subset of actors is sufficient for the outcome and they can be drawn from a smaller subset of potential participants.

But we must still distinguish between motivating action and motivating action that achieves specified outcomes. In Goldstone's scenario, the motive for action—the forced move—is entirely too proximate to the ultimate dependent variable to constitute a general model for macrostructural sufficiency. A subset of potential participants must have the motivation to act—they must respond to the forced move—and the capacity to achieve outcomes. The literature on social movements and revolutions (to which, needless to say, Goldstone has made multiple important contributions) starts from grievances but then documents the critical role played by organizational resources,

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[175] Goldstone, "Methodological Issues in Comparative Macrosociology," 117.

cultural frames, and repertoires of contention in permitting sustained social action.[176] Any one of these factors may be underdetermined by the macrostructure, driving a wedge between the three links in the deterministic edifice.

Needed, then, are substantive theories of power crafted to analyze concrete historical circumstances. Consider two forms those theories can take: theories of state structure and theories of long-term shifts in the balance of power. Theories of state structure identify causally privileged subsets of microstates as the behavior of actors occupying strategically central roles in the state apparatus. A prominent variant of this form of structural argument looks at pre-modern, mediated states in which central state actors rely on alliances with local elites to rule territory outside of major cities. Those local elites are valuable allies because of their autonomous control over politically-relevant social, cultural, military, and economic resources: those resources constitute a significant portion of what Mann calls the "infrastructural power of the state" and so local elites have tremendous capacity to block central state initiatives.[177] A paradigmatic example is Skocpol's theory of social revolutions. Social revolutions begin with state crises, engendered by the resistance of a landed nobility to state-sponsored modernization reforms that threaten their power and privilege.[178] If and only if that nobility has leverage within the semi-bureaucratic state are they able to block reforms and throw the *ancien regime* into crisis. State-sponsored defensive modernization which threatens local control over resources acts as the "forced move," spurring the nobility (or, more

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[176] A recent synthesis of these themes is Sidney Tarrow, *Power in Movement: Social Movements and Contentious Politics* 2nd ed (Cambridge: Cambridge University Press, 1998).
[177] On infrastructural power and the logic of mediated states, see Michael Mann, *The Sources of Social Power, Volume I: A History of Power From the Beginning to A.D. 1760* (Cambridge: Cambridge Univerity Press, 1986).
[178] Skocpol, *States and Social Revolutions*.

precisely, groups within the nobility) into action: occupying a powerful location within the state apparatus gives them causally privileged status and makes their opposition a sufficient cause of "state crisis" and the onset of social revolutions.

A second way to formulate propositions about causally privileged subsets of microstates is to analyze long-term changes in the balance of power. The paradigmatic example of this form of analysis is Rueschemeyer, Stephens, and Stephens, *Capitalist Development and Democracy*. Although the authors consider the causal significance of state structures, their primary hypothesis details how capitalist development changes the balance of power between social classes. Capitalist development is the principle cause of democracy because it not only enlarges the working class, but it also moves subordinate classes from locations that are inhospitable to organization and collective action to new locations that are conducive to those activities. At the same time, capitalist development weakens landed elites who are the most recalcitrant opponents of democracy. Structural change over time transforms the balance of power and translates the action of workers and their allies, with all of its probabilistic features, into a causally privileged subset of microstates.

Thomas Ertman's *Birth of Leviathan* is a final example of how to argue that a subset of microstates is causally privileged. His account combines state structure with long-term structural change. When state building began prior to 1450, state builders seeking expert personnel recruited from among a very tight labor market, swinging the balance of power sharply in favor of the latter who wasted little time patrimonializing their offices and then using their entrenched position within the state to block subsequent projects at bureaucratic rationalization. When state building began post-1450, on the other hand, the growth of universities enlarged the pool of expert personnel, and so the balance of power swung back in favor of statebuilders, who rebuffed patrimonializing efforts and built rudimentary legal-rational states.

To be sure, each of these accounts is susceptible to challenge on methodological, conceptual, theoretical, and empirical grounds. I have claimed only that their explanations largely fulfill the formal conditions for a valid deterministic argument. The point is that these arguments cannot be easily dismissed on purely ontological grounds. They each identify a structural condition, S_1 that influences configurations of microstates with probabilistic features; they each identify a subset of such microstates, m_k, that is a necessary but not necessarily unique outcome of S_1; and they each give powerful reasons why subset m_k is in fact a causally privileged subset of microstates, m_{k*}, that is a sufficient cause to generate the final macrostate, M_1.

That, in principle, microlevel probability is consistent with macrolevel determinism, is a lesson derived from statistical mechanics. That in principle, a global restrictive condition can influence configurations of microstates without stripping them of their probabilistic features so that these microstates correspond to a given macrostate, is a lesson derived from equilibrium statistical mechanics. I have argued that we can make these lessons relevant to political science by adding the concept of a causally relevant subset of microstates. That subset exists when it is virtually always included in the heterogeneous set of microstates resulting from a structural condition; and that subset is causally privileged when it is a sufficient condition for a macrostate, M_1.

The argument, to summarize, is not that all outcomes are determined by structural conditions; it is not even that some outcomes are determined by structural conditions.

Rather, the argument is that a set of formal conditions exists according to which some outcomes may be determined by structural conditions, even in the presence of tremendous indeterminism at the microlevel. The explanatory accounts by Skocpol, Rueschemeyer, Stephens, and Stephens, and Ertman, are paradigmatic examples of how those conditions can be fulfilled by substantive propositions.

11. Conclusion: Accidents Will Happen

This paper began by reviewing a diverse array of criticisms of the deterministic perspective that subtends macrostructural analysis. I have attempted in this paper to take those criticisms seriously and indeed to use them to build a stronger foundation for macrostructural scholarship. Let me conclude this paper by reconsidering one of the most substantial of those critiques, that of Stanley Lieberson. Lieberson wields the example of an automobile accident to systematically expose to light the many hidden assumptions required to sustain use of Mill's methods. For the purposes of this essay, the most important of those assumptions is that the causal forces at work are deterministic. How reasonable is the assumption of determinism? Lieberson comments

> I have selected the automobile-accident example because it should be patently clear that the special deterministic logic does not operate in that instance. Perhaps one may counter that nations and major institutions are neither persons nor roulette wheels; surely their determination is less haphazard, and therefore deterministic thinking is appropriate for these cases. Hence, one might argue, the points made are true for automobile accidents but not for major social institutions or other macrosocietal phenomena. This sounds plausible, but is it true? It turns out that many deep and profound processes are somewhat haphazard too, not so easily relegated to a simple determinism. Elsewhere, I have cited a wide variety of important phenomena which appear to involve chance processes or processes that are best viewed that way. These include race riots, disease, subatomic physics, molecules of gas, star systems, geology, and biological evolution. One must take a very cautious stance about whether the methods used in these small-N studies are appropriate for institutional and macrosocietal events. At the very

> least, advocates of such studies must learn how to estimate if the probabilistic level is sufficiently high that a quasi-deterministic model will not do too much damage.[179]

Lieberson's challenge to macrostructural determinism is both penetrating and fair-minded. It does not simply assert that determinism is wrong; it carefully disentangles the ontological, epistemological, and methodological dimensions of the issue; and it encourages macrostructural scholars to either adopt a probabilistic perspective or learn how to estimate when a deterministic model is viable. My goal in this paper has been to meet that last challenge. Let me thus conclude this essay by responding directly to Lieberson's points cited above.

First, it is indeed my intuition, as Lieberson anticipates, that deterministic thinking is more appropriate for thinking about nations and institutions than it is for automobile accidents. But it is most definitely *not* my contention that all aspects of nations and institutions can be explained by a deterministic logic. Rather, I have articulated a set of formal requirements that deterministic analyses of macrostructural phenomena must fulfill to self-inoculate against ontological critiques. They would still be vulnerable to methodological, empirical, conceptual, and theoretical critiques.

Second, I have urged caution before drawing lessons for the social sciences from the physical sciences. Lieberson is correct to point to "disease, subatomic physics, molecules of gas, star systems, geology, and biological evolution" as examples of "important phenomena which appear to involve chance processes." The spread of disease can appropriately be modeled as a chaotic phenomenon; but as we have seen, the lessons of chaos theory are limited to very specific types of systems—aperiodic, unstable, deterministic, non-linear dynamical systems. Quantum mechanics, as we have seen, has

[179] "Small N's and Big Conclusions," 117-18.

revolutionary implications for the study of subatomic particles; it quite arguably has no implications for the macroworld. As for molecules of gas, I have argued that statistical mechanics is in fact the basis for thinking about structuralism. But here as well, I have urged extreme caution in treating statistical mechanics as an analogy. The lessons we can derive from it have relevance only when embedded in a broader framework, one I articulated in sections nine and ten.

Third, I have argued that part of the debate about probabilism and determinism is not about ontology at all, but rather about contrasting objects of explanation. Jeffrey McKee begins his study of chance and coincidence in biological evolution with a quick calculation of the odds against his being born. As the youngest of four boys born to a mother who wanted a daughter, there was just one chance in eight that "there should have been any need for my conception."[180] Once conceived, the odds of his being a boy drop to 1 in 16. The odds of his being born get lower and lower if we take matters further back to look at each generation of McKee's ancestors. This is all true; biological evolution is a deeply probabilistic process. And yet our appreciation of McKee's story changes somewhat when we substitute a different object of explanation:

> Some…may look at things from a population perspective: it was *likely* that a boy such as myself would have been born, and *probable* that some families would consist of four unruly boys. True, but I cannot help thinking that if time were rolled back and the sequence started over, with just one tiny difference in the fertilization of any egg that became a member of the McKee family, I would not be here and you would not be reading this book.[181]

It is important to see just how little is at stake here. From the individual perspective, McKee is absolutely right; from the population perspective, it was indeed, as McKee

[180] Jeffrey K. McKee, *The Riddled Chain: Chance, Coincidence, and Chaos in Human Evolution* (New Brunswick: Rutgers University Press, 2000), 3. As the youngest and sole male of four children born to a father who wanted a boy, this calculation resonates especially strongly for me.
[181] McKee, *Riddled Chain*, 3.

agrees, "likely that a boy such as myself would have been born." The key phrase, we should now recognize, is "such as myself." This is a clear statement of what Garfinkel calls an "equivalence class," a class of essentially similar objects. The broader the equivalence class, the more plausible the deterministic perspective becomes. Pragmatically driven concept-formation does all of the work here, not debates about epistemology and ontology.

Fourth, and finally, the framework presented here provides a basis for estimating "if the probabilistic level is sufficiently high that a quasi-deterministic model will not do too much damage." I have argued that the plausibility of a deterministic theory rests on three claims: the identification of a structural condition, S_1 that influences configurations of microstates with probabilistic features; the identification of a subset of such microstates, m_k, that is a necessary but not necessarily unique outcome of S_1; and the identification of reasons why subset m_k is in fact a causally privileged subset of microstates, m_{k*}, that is a sufficient cause to generate the final macrostate, M_1.

We can use this same framework to estimate the *high* level of probabilism influencing automobile accidents. First, we can identify a few structural conditions that influence configurations of microstates: weather, traffic, and the mean level of alcohol consumption by drivers all influence the position and momentum of drivers. Second, we cannot say that there is a subset of such microstates, m_k, that is a necessary but not necessarily unique outcome of S_1. Accidents, quite obviously, are not intended: S_1 cannot induce a "forced move" whereby a particular course of action "makes sense" to lots of people. Third, we could argue that there is a causally privileged subset of microstates, m_{k*}, that is a sufficient cause to generate the final macrostate, M_1. When the position and momentum of an automobile is such that neither braking nor turning can avert a collision, we have a sufficient condition for an accident. But this is a trivial statement, for it tells us nothing of the causes of accidents.

Thus, the framework elaborated here permits us to grant ontological plausibility to particular versions of deterministic theories of social revolutions, democratization, and state formation. It does not permit us to grant ontological plausibility to deterministic theories of automobile accidents. The estimates are not mathematically precise, but they point in the right direction. Many other issues remain to be tackled, such as measurement error, the quality of data, the problem of rejecting rival probabilistic theories, and others. But we have made some progress on the ontological issue, which may be the thorniest of them all.