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# International Relations Theory and the End of the Cold War

John Lewis Gaddis

Princes have always sought out soothsayers of one kind or another for the purpose of learning what the future holds. These hired visionaries have found portents in the configurations of stars, the entrails of animals, and most indicators in between. The results, on the whole, have been disappointing. Surprise remains one of the few things one can count on, and very few princes have succeeded in avoiding it, however assiduous the efforts of their respective wizards, medicine men, counselors, advisers, and think tank consultants to ward it off.

Surprise is still very much with us. The abrupt end of the Cold War, an unanticipated hot war in the Persian Gulf, and the sudden disintegration of the Soviet Union astonished almost everyone, whether in government, the academy, the media, or the think tanks. Although there was nothing inherently implausible about these events—the Cold War did have to end sometime, war had always been a possibility in the Middle East, and communism's failures had been obvious for years—the fact that they arose so unexpectedly suggests that deficiencies persist in the means by which contemporary princes and the soothsayers they employ seek to discern the future course of world affairs.

No modern soothsayer, of course, would aspire to total clairvoyance. We have no equivalent of Isaac Asimov's famous character, the mathematician Hari Seldon, whose predictive powers were so great that he was able to leave precise holographic instructions for his followers, to be consulted at successive intervals decades after his death.<sup>1</sup> But historians, political scientists, economists, psychologists, and even mathemati-

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1. Isaac Asimov, *Foundation* (New York: Ballantine Books, 1951).

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cians have claimed the power to detect patterns in the behavior of nations and the individuals who lead them; an awareness of these, they have assured us, will better equip statesmen—and states—to deal with the uncertainties that lie ahead.

The end of the Cold War presents an unusual opportunity to test these claims. That event was of such importance that no approach to the study of international relations claiming both foresight and competence should have failed to see it coming. None actually did so, though, and that fact ought to raise questions about the methods we have developed for trying to understand world politics. The following essay suggests some reasons for this failure of modern-day soothsaying; it will also advance a few ideas on how the accuracy of that enterprise might henceforth be improved.

### *Theory, Forecasting, and the Possibility of Prediction<sup>2</sup>*

The claims that those who study world politics have made regarding their ability to forecast the future grow, for the most part, out of efforts to construct theories of international relations. There is a very simple reason for this: visions of any future have to proceed from the awareness of some kind of past; otherwise there can be no conceptual frame of reference—more than that, there can be no language—with which to express them.<sup>3</sup> Theories provide a way of packaging patterns from the past in such a way as to make them usable in the present as guides to the future.<sup>4</sup> Without them, all attempts at forecasting and prediction would be reduced to random guessing.

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2. My own use of these terms—which is not the usage of everyone cited in this paper—follows the distinctions of John R. Freeman and Brian L. Job: “a *forecast* is a statement about unknown phenomena based upon known or accepted generalizations and uncertain conditions (‘partial unknowns’), whereas a *prediction* involves the linkage of known or accepted generalizations with certain conditions (knowns) to yield a statement about unknown phenomena.” Freeman and Job, “Scientific Forecasts in International Relations: Problems of Definition and Epistemology,” *International Studies Quarterly*, Vol. 23, No. 1 (March 1979), pp. 117–118. It follows from this that forecasts can be neither deterministic—“if A, then (inevitably) B”—nor conditional—“if A, then (under specified conditions) B.” They are instead probabilistic statements: “if A, then (probably) B.” I owe this distinction between deterministic, conditional, and probabilistic statements to a suggestion from Alexander George, although I have fit it within my own differentiation between prediction and forecasting. See also, on these problems of definition, Nazli Choucri, “Key Issues in International Relations Forecasting,” in Nazli Choucri and Thomas W. Robinson, eds., *Forecasting in International Relations: Theory, Methods, Problems, Prospects* (San Francisco: W.H. Freeman, 1978), p. 4; and Richard A. Skinner, “Introduction: Research in the Predictive Mode,” in Charles W. Kegley, Jr., Gregory A. Raymond, Robert M. Rood, and Richard A. Skinner, eds., *International Events and the Comparative Analysis of Foreign Policy* (Columbia: University of South Carolina Press, 1975), p. 211.

3. For the centrality to forecasting of theory based on past experience, see Choucri, “Key Issues in International Relations Forecasting,” pp. 5–7.

4. Charles A. McClelland, *Theory and the International System* (New York: Macmillan, 1966), pp. 15–16, provides a succinct but comprehensive summary of the uses of theory in the study of international relations. See also J. David Singer, “The Level-of-Analysis Problem in International Relations,” *World Politics*, Vol. 14, No. 1 (October 1961), pp. 78–80; and Patrick M. Morgan, *Theories and Approaches to International Politics: What Are We to Think?* 4th ed. (New Brunswick, N.J.: Transaction Books, 1987), pp. 18–19.

Hans J. Morgenthau put forward the first comprehensive modern theory of international relations—and the one from which most subsequent theories in that field have evolved—in his 1948 book, *Politics among Nations: The Struggle for Power and Peace*; his approach came to be known, in a somewhat self-congratulatory way, as “realism.”<sup>5</sup> Earlier studies by diplomatic historians, international lawyers, and well-meaning reformers, Morgenthau complained, had failed to identify the “fundamental principles” of world politics, “which are revealed only by the correlation of recent events with the more distant past.” Even the most idiosyncratic event—and it is important to stress that Morgenthau never disregarded the importance of such events—reflected “social forces” which were, in turn, “the product of human nature in action. Therefore, under similar conditions, they will manifest themselves in a similar manner.” Knowledge of these patterns would allow one to “understand international politics, grasp the meaning of contemporary events, and foresee and influence the future.”<sup>6</sup>

What was new in all of this was not Morgenthau’s insistence that the identification and careful examination of past patterns could improve the quality of future statecraft: historians had been saying that all along. Morgenthau’s innovation, rather, was the claim to have developed, as he himself put it, “the science of international politics.”<sup>7</sup> The principal characteristics of this science were its reductionism—the argument that a drive for power inextricably rooted in human nature animated all politics—and its tough-mindedness—the assertion that a focus on power would free the study of international relations from the sentimentality, legalism, and irrelevant empiricism with which it had been afflicted.<sup>8</sup>

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5. Hans J. Morgenthau, *Politics Among Nations: The Struggle for Power and Peace* (New York: Knopf, 1948). There have been six editions of Morgenthau’s classic, the most recent published in 1985. (My references are to the 1948 edition.) Morgenthau was, of course, hardly the first “realist.” The tradition goes back at least as far as Machiavelli, and if one can accept E.H. Carr’s succinct definition of “realism”—“the impact of thinking upon wishing”—then the idea is as old as its opposite, which is utopian idealism. Edward Hallett Carr, *The Twenty Years’ Crisis, 1919–1939: An Introduction to the Study of International Relations* (New York: St. Martin’s Press, 1939), p. 10. For Morgenthau’s primacy in the field of post-1945 international relations, see Martin Hollis and Steve Smith, *Explaining and Understanding International Relations* (Oxford: Oxford University Press, 1990), pp. 22–28; and Robert O. Keohane, “Realism, Neorealism and the Study of World Politics,” in Robert O. Keohane, ed., *Neorealism and Its Critics* (New York: Columbia University Press, 1986), p. 10.

6. Morgenthau, *Politics Among Nations*, pp. 4–5 (emphases added).

7. Morgenthau used this as the title of Chapter Two of *Politics among Nations* beginning with its second edition, published in 1954.

8. Although some of Morgenthau’s writings appear to be scathing attacks on attempts to construct a “science” of politics—see his *Scientific Man vs. Power Politics* (Chicago: University of Chicago Press, 1945); also “Reflections on the State of Political Science,” *Review of Politics*, Vol. 27, No. 3 (October 1955), pp. 431–460; and “Common Sense and Theories of International Relations,” *Journal of International Affairs*, Vol. 21 (1967), pp. 207–214—a careful reading suggests that what Morgenthau really objected to in idealist, behavioral, and quantitative approaches was their unwillingness to place “power” in the central position to which he had assigned it. Morgenthau’s ambivalence about “scientific” approaches is discussed in Hollis and Smith, *Explaining and Understanding International Relations*, pp. 23, 27.

Morgenthau was always careful not to promise too much. "Trustworthy prophecies" in international politics would never be possible, he argued as early as 1948, because "contradictory tendencies" would always be present in every political situation: which of them would prevail was "anybody's guess." What theory could do, though, was to allow scholars to "trace the different tendencies which, as potentialities, are inherent in [the] situation[.]. . . . point out the different conditions which make it more likely for one tendency to prevail than for another, and, finally, assess the probabilities for the different conditions and tendencies to prevail in actuality."<sup>9</sup> It could, therefore, "confront what governments do, and what governments and peoples think, about international relations with independent prudential judgment and with the truth, however dimly perceived and tenuously approximated."<sup>10</sup>

Subsequent theorists of international relations—whether or not they agreed with Morgenthau's insistence on the centrality of power—have nonetheless embraced his assumption that a "scientific" approach enhances the possibility of forecasting. Morton A. Kaplan acknowledged in 1957 that although theory would be of little use in anticipating the specific actions of individuals and nations, it could "predict characteristic or modal behavior within a particular kind of international system" as well as "the conditions under which the characteristic behavior of the international system will remain stable, the conditions under which it will be transformed, and the kind of transformation that will take place."<sup>11</sup> J. David Singer argued several years later that any analytical model should "offer the promise of reliable prediction"—indeed Singer maintained that this task would be less difficult to accomplish than the other two requirements of such models, which were description and explanation.<sup>12</sup> "As our knowledge base expands and is increasingly integrated in the theoretical sense," he added in 1969, "the better our predictions will be, and therefore, the fewer policy disagreements we will have."<sup>13</sup>

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9. Morgenthau, *Politics among Nations*, pp. 6–7. Raymond Aron had an even more modest conception of the role of theory in statecraft: theory, he wrote, would be scientifically valid "to the extent that it does not provide the equivalent of what noble-hearted people and lightweight minds expect, that is, a simple ideology guaranteeing morality or efficiency." Aron, "What Is a Theory of International Relations?" *Journal of International Affairs*, Vol. 21 (1967), p. 204.

10. Morgenthau, "Common Sense and Theories of International Relations," p. 212.

11. Morton A. Kaplan, *System and Process in International Politics* (New York: John Wiley, 1957), pp. xvii–xviii. See also Kaplan, "Systems Theory and Political Science," *Social Research*, Vol. 35, No. 1 (Spring 1968), especially pp. 33 and 45–46; and Kaplan, "The New Great Debate: Traditionalism vs. Science in International Relations," in Klaus Knorr and James N. Rosenau, ed., *Contending Approaches to International Politics* (Princeton: Princeton University Press, 1969), especially pp. 46–51, 56–57.

12. Singer, "The Level-of-Analysis Problem in International Relations," pp. 79–80. See also J. David Singer, *Models, Methods, and Progress in World Politics: A Peace Research Odyssey* (Boulder, Colo.: Westview Press, 1990), p. 249.

13. J. David Singer, "The Incomplete Theorist: Insight Without Evidence," in Knorr and Rosenau, eds., *Contending Approaches to International Politics*, pp. 66–67. See also Singer, *Models, Methods, and Progress in World Politics*, pp. 62, 261, 269; and Melvin Small and J. David Singer, "Conflict in the International System, 1816–1977: Historical Trends and Policy Futures," in J. David Singer and associates, *Explaining War: Selected Papers from the Correlates of War Project* (Beverly Hills, Calif.: SAGE, 1979), p. 76.

Kenneth N. Waltz, whose approach to theory differed sharply from those of Kaplan and Singer, nonetheless shared with them the goal of using theory to forecast the future. Waltz's triple "images" of international relations, set out in *Man, the State, and War* in 1959, had explicitly prescriptive (and thereby implicitly predictive) purposes: "to explain how peace can be more readily achieved requires an understanding of the causes of war."<sup>14</sup> And in his even more influential *Theory of International Politics*, published in 1979, Waltz clarified his claims regarding prediction, in terms that did not differ greatly from those of Morgenthau or Kaplan: "Theory explains regularities of behavior and leads one to expect that the outcomes produced by interacting units will fall within specified ranges. The behavior of states and of statesmen, however, is indeterminate."<sup>15</sup>

The quest for forecasting and prediction has by no means operated exclusively at the level of international systems. The "operational code" technique for studying political leadership evolved from efforts made, during the early Cold War, to forecast the intentions and actions of top Soviet officials.<sup>16</sup> Decision-making theorists set out to produce general propositions—which could be taken as forecasts—regarding behavior of leaders in crises.<sup>17</sup> Deterrence theorists made specific predictions during the 1950s about how the two nuclear superpowers would behave; these in turn directly influenced decisions on the procurement, deployment, and planned use of nuclear weapons in both Washington and Moscow.<sup>18</sup> Political development theory sought to identify patterns in the modernization process that would allow not only an anticipation of events in the Third World, but the formulation of policies aimed at shaping them.<sup>19</sup> Studies of perception and misperception in international relations employed

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14. Kenneth N. Waltz, *Man, the State, and War: A Theoretical Analysis* (New York: Columbia University Press, 1959), p. 2.

15. Kenneth N. Waltz, *Theory of International Politics* (New York: Random House, 1979), p. 68. A theory of international politics, Waltz noted, was not the same thing as a theory of foreign policy; from the standpoint of the former, it was only "to the extent that dynamics of a system limit the freedom of its units [that] their behavior and the outcomes of their behavior become predictable." *Ibid.*, p. 72.

16. Nathan Leites, *The Operational Code of the Politburo* (New York: McGraw-Hill, 1951). See also Alexander L. George, "The 'Operational Code': A Neglected Approach to the Study of Political Leaders and Decision-making," *International Studies Quarterly*, Vol. 13, No. 2 (June 1969), pp. 190–222.

17. Richard C. Snyder, H.W. Bruck, and Burton Sapin, eds., *Foreign Policy Decision-Making* (New York: Free Press of Glencoe, 1954). See also Glenn D. Paige, *The Korean Decision: June 24–30, 1950* (New York: Free Press, 1968); and Glenn H. Snyder and Paul Diesing, *Conflict among Nations: Bargaining, Decision Making, and System Structure in International Crises* (Princeton: Princeton University Press, 1977), especially pp. 470–530.

18. See, in particular, Thomas C. Schelling, *The Strategy of Conflict* (Cambridge: Harvard University Press, 1960); and Schelling, *Arms and Influence* (New Haven: Yale University Press, 1966). Lawrence Freedman, *The Evolution of Nuclear Strategy* (New York: St. Martin's, 1981), provides the best overview of the influence deterrence theory had on policy. Non-nuclear deterrence theory also had predictive aspirations. See Alexander L. George and Richard Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York: Columbia University Press, 1974), especially p. 512.

19. D. Michael Shafer, *Deadly Paradigms: The Failure of U.S. Counterinsurgency Policy* (Princeton: Princeton University Press, 1988), especially pp. 48–78.

psychological literature to identify recurring patterns in the behavior of individuals, a knowledge of which might improve the conduct of statecraft.<sup>20</sup> And the use of game theory to model international rivalries had clear implications for attempts to anticipate the future of Soviet-American relations.<sup>21</sup>

My point, then, is that the major theoretical approaches that have shaped the discipline of international relations since Morgenthau have all had in common, as *one* of their principal objectives, the anticipation of the future. Whether in science or politics, whether by the tough standards of prediction or the more relaxed ones of forecasting, the role of theory has always been not just to account for the past or to explain the present but to provide at least a preview of what is to come. It follows, therefore, that one way to confirm the validity of theories is to see how successfully they perform *each* of the tasks expected of them.<sup>22</sup> The failure to accomplish a particular task would not necessarily invalidate an entire theory, but it should raise questions in our minds. It would be a warning signal, suggesting the need to rethink underlying assumptions. That is the kind of test this essay seeks to apply: how well did international relations theory carry out one of the important tasks it set for itself, which was forecasting the future of the Cold War?<sup>23</sup>

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20. Robert Jervis, *Perception and Misperception in International Politics* (Princeton: Princeton University Press, 1976), especially pp. 6, 409.

21. Anatol Rapoport, *Fights, Games, and Debates* (Ann Arbor: University of Michigan, 1960); Robert Axelrod, *The Evolution of Cooperation* (New York: Basic Books, 1984).

22. "A theory is a good theory if it satisfies two requirements: It must accurately describe a large class of observations on the basis of a model that contains only a few arbitrary elements, and it must make definite predictions about the results of future observations." Stephen Hawking, *A Brief History of Time: From the Big Bang to Black Holes* (New York: Bantam, 1988), p. 9. See also, on this point, Heinz R. Pagels, *The Dreams of Reason: The Computer and the Rise of the Sciences of Complexity* (New York: Simon and Schuster, 1988), p. 204; Skinner, "Introduction: Research in the Predictive Mode," pp. 208–209; Charles F. Hermann, Warren R. Phillips, and Stuart J. Thorson, "Validating International Relations Forecasts to Develop Theory," in Choucri and Robinson, eds., *Forecasting in International Relations*, pp. 69–78; M.R. Leavitt, "Computer Simulation in International Relations Forecasting," in *ibid.*, p. 240; Milton Friedman, "The Methodology of Positive Economics," in May Brodbeck, ed., *Readings in the Philosophy of the Social Sciences* (New York: Macmillan, 1968), pp. 508–528; and Waltz, *Theory of International Politics*, p. 69. Singer, conversely, argues that "despite the folklore to the contrary, *prediction* is neither the major purpose nor the acid test of a theory; the goal of all basic scientific research is *explanation*." But he then goes on to make the point that "a strong explanatory theory will—because it is better able to account for and explain the effects of changing conditions—provide a more solid base for predicting than one that rests on observed covariations and postdictions alone." Singer, *Models, Methods, and Progress in World Politics*, p. 74; see also p. 249.

23. For reasons of space, I have limited this analysis to theoretical approaches that attempted, in one way or another, to forecast the workings of the international system as a whole. There are other ways in which one could use the end of the Cold War to test theory: one could, for example, consider the extent to which sub-systemic level theories relating to deterrence, bargaining, alliances, crisis-management, and collective and individual decision-making provided a basis for anticipating what happened; one could also apply the same scrutiny to the specific field of Soviet studies and the theoretical insights that arose from within it. Some preliminary efforts along these lines include Daniel Deudney and G. John Ikenberry, "The International Sources of Soviet Change," *International Security*, Vol. 16, No. 3 (Winter 1991/92), pp. 74–118; Richard K. Herrmann, "Soviet Behavior in Regional Conflicts: Old Questions, New Strategies,

### Approaches to the Future

Before we can apply that test, though, there is an organizational problem to be got out of the way. It has to do with the fact that although international relations theorists have agreed on the importance of prediction and forecasting, they have by no means agreed on how to construct the theories that must be in place prior to performing these tasks.<sup>24</sup> Differences over theory have long impeded efforts to build a "science" of international relations; they have also affected the assumptions behind, the procedures employed in, and the accuracy of the attempts theorists have made to look ahead.

Morgenthau's "realism" provided little practical guidance on how to use theory to foresee the future. It was true enough that statesmen define their interests in terms of power, Stanley Hoffmann pointed out, "but only at a level of generality that is fatuous":<sup>25</sup> after all, if everyone seeks power because they are human, then the value of a forecast stating that humans will seek power is somewhat limited. Other critics noted that Morgenthau had attempted to derive universally-valid propositions about human behavior from a particular set of human characteristics: there was no explanation of why the craving for power should necessarily take precedence over other human desires, or determine all human actions, or remain immutable for all time to come.<sup>26</sup> Still others accused Morgenthau of failing to specify whether power was an end in itself or a means to an end; if it was both, then what he had achieved was not a theory but a tautology.<sup>27</sup> Finally, Morgenthau's recommendations for policy-makers boiled down to the exercise of prudence and restraint, qualities that seemed at odds with the unchanging characteristics of human nature he had earlier claimed to have identified.<sup>28</sup> Morgenthau's "realism" was a starting point, but clearly much

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and Important Lessons," *World Politics*, Vol. 44, No. 3 (April 1992), pp. 432–465; and John W.R. Lepingwell, "Soviet Civil-Military Relations and the August Coup," *ibid.*, pp. 539–572; as well as papers prepared by Herrmann and Thomas Risse-Kappen for the October 1991 Cornell University conference on "The Transformation of the International System and International Relations Theory" organized by Richard Ned Lebow and Thomas Risse-Kappen.

24. One recent review of the field has characterized these contending schools of thought as "partisan bands" who seize academic departments, entice graduate students into their camps, and carry on permanent feuds with one another. Yale H. Ferguson and Richard W. Mansbach, *The Elusive Quest: Theory and International Politics* (Columbia: University of South Carolina Press, 1988), p. 18.

25. Stanley Hoffmann, "Notes on the Limits of 'Realism'," *Social Research*, Vol. 48, No. 1 (Winter 1981), p. 655.

26. Waltz, *Man, the State, and War*, pp. 26–41. See also Richard N. Rosecrance, *Action and Reaction in World Politics: International Systems in Perspective* (Boston: Little, Brown, 1963), p. 268; and K.J. Holsti, "Retreat from Utopia: International Relations Theory, 1945–70," *Canadian Journal of Political Science*, Vol. 4, No. 2 (June 1971), p. 169.

27. Kaplan, *System and Process in International Politics*, p. 12. See also Keohane, "Neorealism and World Politics," p. 11.

28. Stanley Hoffmann, "International Relations: The Long Road to Theory," in James N. Rosenau, ed., *International Relations and Foreign Policy: A Reader in Research and Theory* (New York: Free Press, 1961), pp. 423–424.

more would be required if international relations theory was to lead to the detection of laws, and hence to any possibility of forecasting and prediction.

Dissatisfaction with Morgenthau's attempt to build a comprehensive theory of international relations has led to a bewildering array of efforts, over the past several decades, to construct viable alternatives. None of these has come close to commanding universal assent, nor is there even any generally accepted way of categorizing them. For the purposes of this essay—but with the caution that these are oversimplifications—I identify them in terms of three distinctive approaches to theory: the “behavioral,” the “structural,” and the “evolutionary.”<sup>29</sup> I then assess what the major practitioners of each of these approaches either said or implied about the end of the Cold War.

#### THE BEHAVIORAL APPROACH

The behavioral approach bases itself upon a key assumption of classical empiricism: that we can only know what we can directly observe and measure. “History, experience, introspection, common sense, and logic do not in themselves generate *evidence*,” one of the leading behavioralists, J. David Singer, wrote in 1969; they are, rather, “ideas which must then be examined in the light of evidence,” and that procedure can only take place on the basis of observations that are “systematic, explicit, visible, and replicable by other researchers.”<sup>30</sup> A true science of politics would not simply call itself “scientific,” as Morgenthau had described his theory; rather, it would apply methods of the physical and natural sciences, to the maximum extent possible, in analyzing human and state behavior.<sup>31</sup> Without the rigor such methods provide, behavioralists insist,<sup>32</sup> the study of international relations will always be subject to the very utopianism, emotionalism, bias, confusion, and contradiction from which Morgenthau's “realism” had sought to liberate it.

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29. For somewhat similar distinctions, see Johan Galtung, “The Social Sciences: An Essay on Polarization and Integration,” in Knorr and Rosenau, eds., *Contending Approaches to International Politics*, pp. 243–285; also Hayward R. Alker, Jr., and Thomas J. Biersteker, “The Dialectics of World Order: Notes for a Future Archeologist of International Savoir Faire,” *International Studies Quarterly*, Vol. 38, No. 2 (June 1984), pp. 121–142; Daniel Druckman and P. Terrence Hopmann, “Behavioral Aspects of Negotiations on Mutual Security,” in Philip E. Tetlock, et al., eds., *Behavior, Society, and Nuclear War: Volume One* (New York: Oxford University Press, 1989), pp. 152–155; and Yosef Lapid, “The Third Debate: On the Prospects of International Theory in a Post-Positivist Era,” *International Studies Quarterly*, Vol. 33, No. 3 (September 1989), pp. 239–240.

30. Singer, “The Incomplete Theorist,” p. 68.

31. James N. Rosenau, “Moral Fervor, Systematic Analysis, and Scientific Consciousness in Foreign Policy Research,” in Austin Ranney, ed., *Political Science and Public Policy* (Chicago: Markham, 1968), pp. 197–236; J. David Singer, “The Behavioral Approach to International Relations: Payoff and Prospects,” in James N. Rosenau, ed., *International Politics and Foreign Policy: A Reader in Research and Theory*, rev. ed. (New York: Free Press, 1969), pp. 65–69. Singer has always been careful to acknowledge the impossibility of a *pure* science of politics; the behavioralist approach was to be an improvement on, but not a rejection of, more traditional methods like those of Morgenthau and other “realists.”

32. For a recent argument to this effect, see Paul Huth and Bruce Russett, “Testing Deterrence Theory: Rigor Makes a Difference,” *World Politics*, Vol. 42, No. 4 (July 1990), pp. 466–501.

Behavioralists concentrate, for this reason, upon the careful characterization, and where possible quantification, of observable phenomena: examples have included battlefield casualties, voting returns, trade statistics, newspaper stories, and even patterns of communication.<sup>33</sup> Where direct measurement is not possible, they seek to generate measurable data either through the creation of rules for coding the activities of states, organizations, and individuals,<sup>34</sup> or through the simulation of situations in such a way as to yield calculable “inputs” and “outputs.”<sup>35</sup> Considerable emphasis is placed on the use of rigorous mathematical techniques in analyzing information produced by these methods, both as a safeguard against bias and as a means of ensuring comparability.<sup>36</sup> The behavioralists proceed from a determinedly inductive, “bottom-up” approach, deferring the construction of theory until they have collected, measured, and compared as much observable evidence as possible, and after that cumulated, replicated, and thus verified the resulting findings.<sup>37</sup> Only then, presumably, can forecasting on any “scientific” basis take place.

#### THE STRUCTURAL APPROACH

The structural approach differs from the behavioral in that it focuses upon unobservable and hence unmeasurable structures that nonetheless shape international relations

33. Richard A. Brody, “The Study of International Politics qua Science: The Emphasis on Methods and Techniques,” in Knorr and Rosenau, eds., *Contending Approaches to International Politics*, pp. 110–128, provides a good introduction to this kind of analysis. For some representative applications, see Singer and associates, *Explaining War*; Karl Deutsch and others, *Political Community in the North Atlantic Area: International Organization in the Light of Historical Experience* (Princeton: Princeton University Press, 1957); J. David Singer, ed., *Quantitative International Politics: Insights and Evidence* (New York: Free Press, 1968); Bruce M. Russett, ed., *Peace, War, and Numbers* (Beverly Hills, Calif.: SAGE, 1972); Jonathan Wilkenfeld, Gerald W. Hoppole, Paul J. Rossa, and Stephen J. Andriole, *Foreign Policy Behavior: The Interstate Behavior Analysis Model* (Beverly Hills, Calif.: SAGE, 1980); and Paul Huth, *Extended Deterrence and the Prevention of War* (New Haven: Yale University Press, 1988).

34. Singer, *Models, Methods, and Progress in World Politics*, pp. 113, 119–120. See also J. David Singer, “The ‘Correlates of War’ Project: Interim Report and Rationale,” *World Politics*, Vol. 14, No. 2 (January 1972), pp. 243–270.

35. Examples include Thomas H. Naylor, et al., *Computer Simulation Techniques* (New York: John Wiley, 1966); Charles F. Hermann, *Crises in Foreign Policy: A Simulation Analysis* (Indianapolis: Bobbs-Merrill, 1969); and Harold Guetzkow, et al., *Simulation in Social and Administrative Science: Overviews and Case-Examples* (Englewood Cliffs, N.J.: Prentice-Hall, 1972).

36. See, on the use of mathematics, Michael Nicholson, *Formal Theories in International Relations* (New York: Cambridge University Press, 1989), pp. 10–13, 18–21. Nicholson is careful to distinguish between what he calls the “mathematical theory,” “the mathematical model,” and the “mathematical picture.” Behavioralists, following a principally inductive approach, for the most part use the last of these.

37. Singer, “The ‘Correlates of War’ Project,” pp. 249–251. See also Singer and Small, “Conflict in the International System,” p. 76; J. David Singer, “The Peace Researcher and Foreign Policy Prediction,” *Peace Society (International) Papers*, Vol. 21 (1973), pp. 5–8; and John A. Vasquez, “The Steps to War: Toward a Scientific Explanation of Correlates of War Findings,” *World Politics*, Vol. 40, No. 1 (October 1987), pp. 111–114.

in observable and measurable ways.<sup>38</sup> Behavioralists have never denied the existence or the importance of such structures;<sup>39</sup> they maintain only that science lacks the means to deal with them. But structuralists point out that some of the most striking accomplishments of twentieth-century science have arisen from the assumption that unobservable structures produce observable effects: theories about the invisible structures of atoms, after all, brought about the all too visible incineration of Hiroshima and Nagasaki.<sup>40</sup> The only truly inductive method, structuralists insist, is that of the blank mind; one has to assume *a priori* and unobservable structures because without them theories themselves could not exist, reality would be uncharacterizable, and certainly forecasting would be impossible.<sup>41</sup> "Collecting facts is not enough," Stanley Hoffmann has commented; "it is not helpful to gather answers when no questions have been asked first."<sup>42</sup>

International systems are one such structure: no one has ever seen, measured, or even described an international system with any precision; but few would deny that groups of nations in world politics do have characteristics that add up to more than the sum of their parts.<sup>43</sup> Multipolarity and bipolarity are real conditions in international affairs, despite the fact that no state's policies deliberately create them;<sup>44</sup> it makes a difference which of these conditions prevails at any given time.<sup>45</sup> Forms of

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38. My definition of structures here follows Alexander Wendt's discussion of "scientific realism" in "The Agent-Structure Problem in International Relations Theory," *International Organization*, Vol. 41, No. 3 (Summer 1987), especially pp. 351–352. See also Waltz, *Theory of International Politics*, pp. 79–101; and John Gerard Ruggie, "Continuity and Transformation in the World Polity: Toward a Neorealist Synthesis," in Keohane, ed., *Neorealism and Its Critics*, pp. 131–136.

39. See, for example, Singer, "The Incomplete Theorist," pp. 65–68; also Vasquez, "The Steps to War," pp. 113–114.

40. Wendt, "The Agent-Structure Problem in International Relations Theory," p. 352.

41. See, for example, Waltz, *Theory of International Politics*, pp. 4–5. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2d ed., enl. (Chicago: University of Chicago Press, 1970), makes the most influential argument regarding the impossibility of a pure empiricism.

42. Hoffmann, "International Relations: The Long Road to Theory," p. 422. See also, for related arguments, David Hackett Fischer, *Historians' Fallacies: Toward a Logic of Historical Thought* (New York: Harper and Row, 1970), pp. 65–68; and Stephen Jay Gould, *Time's Arrow, Time's Cycle: Myth and Metaphor in the Discovery of Geological Time* (Cambridge, Mass.: Harvard University Press, pp. 48–49, 196).

43. See Robert Jervis, "Systems Theories and Diplomatic History," in Paul Gordon Lauren, ed., *Diplomacy: New Approaches in History, Theory, and Policy* (New York: Free Press, 1979), pp. 212–213; also Waltz, *Man, the State, and War*, pp. 159–223; Waltz, *Theory of International Politics*, p. 40.

44. This is not to say that statesmen have not *tried*, at one time or another, to create these structures: witness the efforts of Castlereagh and Metternich at the Congress of Vienna to build a multipolar system in post-Napoleonic Europe, or the attempts of their principal historical chronicler, Henry Kissinger, to follow their example during the Nixon administration. My point is that such efforts cannot work unless the systemic conditions that favor them are already present.

45. I refer here to the long debate over whether bipolarity or multipolarity is the more stable configuration for international systems. The debate can be conveniently sampled in Waltz, "International Structure, National Force, and the Balance of World Power"; and Karl W. Deutsch and J. David Singer, "Multipolar Power Systems and International Stability," both in Rosenau, *International Politics and Foreign Policy* (1969 ed.), pp. 314–324.

government provide another example: no absolute standard allows one to distinguish democracies from dictatorships in the way that one can specify the differences between apples and oranges; and yet everyone knows that these two forms of government are not the same, and that the effects they produce—whether in terms of free elections, functioning economies, or respect for human rights—are indeed measurable. Unobservable structures can exist within governments, where they take the form of bureaucratic, organizational, and psychological constraints that do not always reflect what might be apparent on the “observable” surface.<sup>46</sup> And it is very likely that such structures also exist in our minds, producing observable effects in the way in which we perceive reality, respond to it, and even, by means of language, characterize it.<sup>47</sup>

Structuralists proceed, then, from a primarily deductive, “top-down” approach that assumes the existence of unobservable phenomena in international relations, uses the collection of empirical evidence—by no means excluding quantitative and simulative techniques—to refine and verify generalizations about them, and then produces forecasts by projecting the resulting patterns into the future.

#### THE EVOLUTIONARY APPROACH

The evolutionary approach combines elements of the structural and the behavioral approaches, but extends them along a third axis, which is that of time. Both structuralists and behavioralists tend toward a static perspective; they pay relatively little attention to the possibility that structures and behaviors in international relations might evolve.<sup>48</sup> But the geological and biological sciences have preoccupied themselves with evolutionary processes—inanimate and animate—for almost two centuries now; historians, of course, have a much longer tradition of temporal analysis. It should have come as no surprise, therefore, that an evolutionary approach to international relations theory would sooner or later make its appearance.<sup>49</sup> Its adherents

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46. See Graham T. Allison, *Essence of Decision: Explaining the Cuban Missile Crisis* (Boston: Little, Brown, 1971); Irving L. Janis, *Victims of Groupthink: A Psychological Study of Foreign-Policy Decisions and Fiascoes* (Boston: Houghton Mifflin, 1972); Richard K. Betts, *Soldiers, Statesmen, and Cold War Crises* (Cambridge, Mass.: Harvard University Press, 1977).

47. See Jervis, *Perception and Misperception in International Politics*, passim; Deborah Welch Larson, *Origins of Containment: A Psychological Explanation* (Princeton: Princeton University Press, 1985), especially pp. 24–65; Robert Jervis, Richard Ned Lebow, and Janice Gross Stein, *Psychology and Deterrence* (Baltimore: Johns Hopkins University Press, 1985); also for a particular manifestation of this phenomenon, Richard K. Ashley and R.B.J. Walker, eds., “Speaking the Language of Exile: Dissidence in International Studies,” *International Studies Quarterly*, Vol. 34, No. 3 (September 1990).

48. Which is not to imply that they ignore this possibility altogether. See, for example, Waltz, *Theory of International Politics*, pp. 199–204; also J. David Singer, “The Global System and its Sub-Systems,” in James N. Rosenau, ed., *Linkage Politics: Essays on the Convergence of National and International Systems* (New York: Free Press, 1969), pp. 21–43.

49. Stanley Hoffmann explicitly encouraged such an approach in 1959; see “International Relations: The Long Road to Theory,” p. 425. See also George Modelski, “Is World Politics Evolutionary Learning?” *International Organization*, Vol. 44, No. 1 (Winter 1990), pp. 1–24; and discussions of the “rediscovery” of history in Joseph S. Nye, Jr., and Sean M. Lynn-Jones, “International Security Studies: A Report of a Conference on the State of the Field,” *International*

have come to see that the passage of time can not only influence both behavior and structure in world politics; it can also obscure the distinction between them.<sup>50</sup>

Theorists have become increasingly interested, for example, in the possibility that periods of war, peace, and political-economic hegemony recur in cyclical patterns extending over several hundred years.<sup>51</sup> This interest, in turn, has spawned a lively concern—extending well beyond the academic community—with the conditions that lead to the rise and decline of great powers.<sup>52</sup> But theorists have also begun to turn their attention to the possibility that irreversible shifts in individual and state behavior can occur on a worldwide scale, and that these can over time modify systemic structures.<sup>53</sup> The assumption here is that human beings and the states they create not only accumulate experience but also learn from it; and that such learning can bring about new ways of doing things, whether at the level of the international system as a whole, aggregations of states within that system, individual states themselves, or groups and individuals within the state.<sup>54</sup> The passage of time itself appears

*Security*, Vol. 12, No. 4 (Spring 1988), pp. 18–19; and Stephen M. Walt, “The Renaissance of Security Studies,” *International Studies Quarterly*, Vol. 35, No. 2 (June 1991), p. 217.

50. See Wendt’s summary of “structuration theory” in “The Agent-Structure Problem in International Relations Theory,” pp. 355–361.

51. See, for example, Robert Gilpin, *War and Change in World Politics* (New York: Cambridge University Press, 1981); George Modelski, ed., *Exploring Long Cycles* (Boulder, Colo.: Lynne Rienner, 1987); Joshua A. Goldstein, *Long Cycles: Prosperity and War in the Modern Age* (New Haven: Yale University Press, 1988).

52. Here the work of a historian, Paul Kennedy, has had the greatest influence through the national debate generated by the appearance of his book, *The Rise and Fall of the Great Powers: Economic Change and Military Conflict from 1500 to 2000* (New York: Random House, 1987). But see also Mancur Olson, *The Rise and Decline of Nations: Economic Growth, Stagflation, and Social Rigidities* (New Haven: Yale University Press, 1982); Aaron L. Friedberg, *The Weary Titan: Great Britain and the Experience of Relative Decline, 1895–1905* (Princeton: Princeton University Press, 1988); Henry R. Nau, *The Myth of America’s Decline: Leading the World Economy into the 1990s* (New York: Oxford University Press, 1990); and Joseph S. Nye, Jr., *Bound to Lead: The Changing Nature of American Power* (New York: Basic Books, 1990).

53. See, for example, Robert O. Keohane and Joseph S. Nye, Jr., *Power and Interdependence: World Politics in Transition* (Boston: Little, Brown, 1977); Richard W. Mansbach and John A. Vasquez, *In Search of Theory: A New Paradigm for Global Politics* (New York: Columbia University Press, 1981); Michael Doyle, “Kant, Liberal Legacies, and Foreign Affairs,” *Philosophy and Public Affairs*, Vol. 12, No. 3 and No. 4 (Summer and Fall 1983), pp. 205–235, 323–353; Richard Rosecrance, *The Rise of the Trading State: Commerce and Conquest in the Modern World* (New York: Basic Books, 1986); John Mueller, *Retreat from Doomsday: The Obsolescence of Major War* (New York: Basic Books, 1989); James Lee Ray, “The Abolition of Slavery and the End of International War,” *International Organization*, Vol. 43, No. 3 (Summer 1989), pp. 405–439; Francis Fukuyama, “The End of History?” *The National Interest*, No. 16 (Summer 1989), pp. 3–18. Waltz in his recent writings has begun to acknowledge this possibility. See “Reflections on *Theory of International Politics: A Response to My Critics*,” in Keohane, ed., *Neorealism and Its Critics*, pp. 327–328, 343.

54. See Stephen D. Krasner, ed., *International Regimes* (Ithaca: Cornell University Press, 1983); Joseph S. Nye, Jr., “Nuclear Learning and U.S.-Soviet Security Regimes,” *International Organization*, Vol. 41, No. 3 (Summer 1987), pp. 371–402; Alexander L. George, “Incentives for U.S.-Soviet Security Cooperation and Mutual Adjustment,” in Alexander L. George, Philip J. Farley, and Alexander Dallin, eds., *U.S.-Soviet Security Cooperation: Achievements, Failures, Lessons* (New York: Oxford University Press, 1988), pp. 641–654.

to be the critical requirement in order for this process to take place: if states are to transcend their own natures and evolve techniques of cooperation, then they must have the opportunity to learn from experience, together with the confidence that existing conditions will continue at least into the near future.<sup>55</sup>

Behavioral, structural, and evolutionary approaches to the construction of international relations theory each have their weaknesses. The behaviorists tend to focus only on observable, measurable phenomena, thereby excluding from their vision those aspects of international relations that do not fall into that category. The structuralists, by taking the opposite approach, produce impressionistic judgments and unverifiable conclusions. And both behaviorists and structuralists neglect the role of time in world politics, a subject the evolutionists focus on, but only at the expense of blurring the distinction between behavior and structure in the first place. It is hardly surprising, therefore, that no grand theory of international relations has arisen to replace Morgenthau;<sup>56</sup> the absence of such a theory, in turn, greatly complicates efforts to forecast world politics. Still the distinction between behavioral, structural, and evolutionary approaches should serve as an adequate framework within which to evaluate such efforts as have been made, and from which to make suggestions about possible improvements.

### *Theory, Theorists, and the End of the Cold War*

Establishing criteria for success, in forecasting, is no easy thing to do.<sup>57</sup> How much weight should one give, for example, to a vision of the future that turns out to be right, but for the wrong reasons? What if the reasons are right but the timing is wrong? How much precision should one demand, and how much detail can one expect? To what extent should one reward lucky guesses? How does one take into account the possibility that forecasts might make themselves inaccurate by encouraging action to alter existing trends? The complexities are such that one is tempted to fall back on Justice Potter Stewart's famous rule for recognizing pornography: "I know it when I see it."<sup>58</sup>

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55. These points are made on the basis of both theoretical and historical evidence in Axelrod, *The Evolution of Cooperation*; and Kenneth A. Oye, ed., *Cooperation Under Anarchy* (Princeton: Princeton University Press, 1986).

56. For the decline of grand theory, see K.J. Holsti, "Retreat from Utopia," pp. 165–177.

57. Some efforts to define criteria for success in forecasting appear in William Ascher, *Forecasting: An Appraisal for Policy-Makers and Planners* (Baltimore: Johns Hopkins University Press, 1978), pp. 2–13; Singer, "The Peace Researcher and Foreign Policy Prediction," p. 5; Choucri, "Key Issues in International Relations Forecasting," p. 4; and Edward E. Azar (drawing on earlier work by Davis Bobrow), "Behavioral Forecasts and Policymaking: An Events Data Approach," in Kegley, et al., eds., *International Events and the Comparative Analysis of Foreign Policy*, pp. 216–217. I have also benefited from an informal seminar on this problem organized by Kennette Benedict and Philip E. Tetlock at the John D. and Catherine T. MacArthur Foundation in October 1990.

58. James B. Simpson, compiler, *Simpson's Contemporary Quotations* (Boston: Houghton Mifflin, 1988), p. 79.

For the purposes of this essay, though, let us establish a relatively easy test. Let us say that a successful anticipation of the Cold War's end need not have been a deterministic or a conditional prediction, but only a probabilistic forecast.<sup>59</sup> Let us absolve it of any obligation to have foreseen all or even most of the circumstances that brought about that event. Let us require of it only the specification in advance of *at least one* of the following as *likely*: (1) the asymmetrical outcome—that is, the fact that only one of the two Cold War superpowers, not both, lost that status; (2) the manner in which this happened—that is, an abrupt but peaceful collapse of Moscow's authority both within and beyond the borders of the former Soviet Union; (3) the trends that caused this loss of authority to occur—that is, the increasing unworkability of command economies, together with the infeasibility of using authoritarian means to rescue them; (4) the approximate timing of these developments—that is, the last half of the 1980s and the early 1990s; or (5) the rough outlines of a world without the Cold War—especially one in which German reunification has taken place, NATO has survived despite the Warsaw Pact's demise, and democratization has revived ancient ethnic, linguistic, and religious rivalries in territories that once lay within, or adjacent to, the Soviet sphere of influence.

What is immediately obvious, on reviewing this list, is that very few of our theoretical approaches to the study of international relations came anywhere close to forecasting *any* of these developments. One might as well have relied upon stargazers, readers of entrails, and other "pre-scientific" methods for all the good our "scientific" methods did;<sup>60</sup> clearly our theories were not up to the task of anticipating the most significant event in world politics since the end of World War II.<sup>61</sup> The following discussion of international relations theorists and the end of the Cold War must necessarily concentrate, therefore, on what did not happen rather than on what did. Still, as Sherlock Holmes noted long ago, dogs that do *not* bark in the night have their own important messages to convey.

#### BEHAVIORAL APPROACHES

The behavioralist research agenda, J. David Singer argued in 1972, was to move from the collection of data through the construction of theory to the generation of forecasts: "The number one task for peace research always turns out to be that of prediction," which, in turn, was "the ability to forecast, with increasing reliability, the outcomes

59. For definitions of these terms, see note 2.

60. For evidence that the Reagan administration did at times employ such methods, see Donald T. Regan, *For the Record: From Wall Street to Washington* (New York: St. Martin's Press, 1988), pp. 78–83, 409–412; and Lou Cannon, *President Reagan: The Role of a Lifetime* (New York: Simon and Schuster, 1991), pp. 583–586.

61. They have not been particularly successful in anticipating other major events either. See, on this point, David Easton, "The New Revolution in Political Science," *American Political Science Review*, Vol. 63, No. 4 (December 1969), pp. 1053, 1057; also James N. Rosenau, "Assessment in International Studies: Ego Trip or Feedback," *International Studies Quarterly*, Vol. 43, No. 3 (September 1974), p. 344; and Michael D. Wallace, "Early Warning Indicators from the Correlates of War Project," in J. David Singer and Michael D. Wallace, eds., *To Augur Well: Early Warning Indicators in World Politics* (Beverly Hills, Calif.: SAGE, 1979), p. 17.

which are most likely to emerge out of a given set of background conditions and behavioral events." Existing methods of forecasting were inadequate: "if we peace researchers are to nudge human history onto a slightly different course—and we can strive for nothing less—we must radically revise the style and method of social forecasting as we know it today."<sup>62</sup>

Nine years earlier, Singer and his colleagues had founded the Correlates of War Project at the University of Michigan, a careful effort to catalog the causes and nature of modern wars that must now be the most frequently-cited of all data-collection enterprises in the field of international relations. The research program he laid out in 1972 is of special interest, therefore, not simply for its clarity but also for the central position its author has occupied in the study of war and peace over the past three decades. It provides a standard against which to measure what the behavioralist approach has achieved.

Dismissing such familiar—or fashionable—forecasting techniques as simple extrapolation, "seat of the pants" guessing, Delphi methods,<sup>63</sup> and simulations, Singer called first for work to identify relationships between variables that were likely to influence conditions of war or peace. These correlations could be tested against the historical record; if they held up—and if one could reasonably assume their continuation into the future—then they might provide a means of forecasting what was to come. What would be needed if one were to accomplish this, though, Singer insisted, was "theory-based prediction."<sup>64</sup>

Singer defined theory as "a reproducible and compelling explanation of a given class of events." Theories were superior to correlations because they could not only identify patterns, but also explain why, when, and under what circumstances patterns occurred. They could describe the dynamics of systems as well as their static characteristics. "In sum," Singer concluded, "correlational knowledge can carry us part way, but until we have built and empirically tested a theory which offers a compelling explanation of the changing as well as the constant associations in the past, we make predictions of less than desirable solidity."<sup>65</sup>

The construction of such a theory would require, first, "a reasonably extensive and accurate data base." Computer modeling could then allow the manipulation of variables at different magnitudes, after which the results could be checked—and refined—by reference back to historical experience. This technique would allow one, in effect, to "reproduce" diplomatic history, while at the same time projecting it into various futures as specified by the researcher. Out of these efforts would come, first, "the most feasible way of ascertaining when, and in what fashion, the theoretical dynamics of the international system change"; second, "the factors that most strongly account

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62. Singer, "The Peace Researcher and Foreign Policy Prediction" pp. 1–2.

63. Popularized by the RAND Corporation in the 1950s, the Delphi method involved eliciting successively more specific predictions on a particular problem from a group of experts who were in touch with one another only to the extent that they reviewed each other's predictions.

64. Singer, "The Peace Researcher and Foreign Policy Prediction," pp. 2–5.

65. *Ibid.*, pp. 5–6.

for those changing relationships"; and third, the ultimate identification of "the mechanisms which account for and produce such systemic dynamics."<sup>66</sup>

In the years that have followed the appearance of Singer's article, behavioralists have indeed made heroic efforts to collect and analyze data, not just on the causes and nature of war but on the workings of the international system generally.<sup>67</sup> They have done this in a manner consistent with their determination to build theory only on a base of observable and measurable phenomena. They have identified key variables from this body of data; they have established correlations among them using an ingenious array of statistical and computer techniques; they have checked and rechecked these findings against the historical record; they have communicated the results honestly and openly; and they have trained an entire generation of students to carry on this research agenda into the future.

Unfortunately, though, the behavioralist approach has produced neither theory, nor forecasts, nor usable policy recommendations.<sup>68</sup> At the time the Cold War ended it was still gathering and correlating data, a process from which few firm conclusions of any kind have emerged. The behavioralists themselves have often commented on this phenomenon: "Regardless of the theoretical interpretation, the empirical investigations led once more to inconsistent results."<sup>69</sup> "Although the goal of a social scientific perspective on negotiations is cumulation, the development of the literature in this field to date suggests that the results fall well short of this goal."<sup>70</sup> "Unquestionably, one of the greatest disappointments experienced by early [comparative foreign policy] proponents has been their perceived failure to generate intellectual products even roughly commensurate with early expectations."<sup>71</sup>

There are, to be sure, good reasons why the behavioral study of world politics has yielded such inconclusive results. For one thing, fewer directly measurable entities exist at the level of international relations than at other levels of human activity. It is

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66. *Ibid.*, pp. 7–8.

67. I refer here, not just to the Correlates of War Project, but also to such other data collection enterprises as the Dimensionality of Nations Project, the World Event Interaction Survey, the Conflict and Peace Data Bank, the Cooperative Research on the Events of Nations Project, and the Foreign Relations Indicator Project, all of which have operated according to different rules and for different purposes. I have chosen to focus this analysis on the Correlates of War Project because I believe its influence on the field of international relations has been, and remains, greater than that of the other data collection efforts; it is also the case that its principal founder, Singer, has made the most explicit claims regarding the utility of such data collection efforts in forecasting.

68. See, on this point, Ferguson and Mansbach, *The Elusive Quest*, pp. 29–30; John A. Vasquez, "Statistical Findings in International Politics: A Data-Based Assessment," *International Studies Quarterly*, Vol. 20, No. 2 (June 1976), pp. 171–218; and Charles F. Hermann and Gregory Peacock, "The Evolution and Future of Theoretical Research in the Comparative Study of Foreign Policy," in Charles F. Hermann, Charles W. Kegley, Jr., and James N. Rosenau, eds., *New Directions in the Study of Foreign Policy* (Boston: Allen and Unwin, 1987), pp. 13–32.

69. J. David Singer, "Accounting for International War: The State of the Discipline," *Journal of Peace Research*, Vol. 28 (1981), p. 9.

70. Druckman and Hopmann, "Behavioral Aspects of Negotiations on Mutual Security," p. 96.

71. Hermann and Peacock, "The Evolution and Future of Theoretical Research in the Comparative Study of Foreign Policy," p. 16.

not all that difficult to accumulate data from questionnaires documenting the habits and preferences of individuals, or from votes cast in national elections, or from statistics on national economic performance, but how does one translate concepts like “polarity,” or “hostility,” or “deterrence”—fundamental as they are to an understanding of global systems—into calculable expressions?<sup>72</sup> It is also worth remembering that behavioralism began as a young science,<sup>73</sup> and that the behavioralists never claimed the ability to forecast anything with authority until theories derived from scientifically valid evidence were solidly in place, however long this took. From these perspectives, then, it is unfair to criticize behavioralist scholarship for not having anticipated the end of the Cold War.

Still, the protracted delay in producing what was promised cannot help but create doubts as to the ultimate viability of the behavioralist enterprise. It makes one wonder whether the approach may not be stuck in a permanent condition of adolescence. Certainly it raises question as to whether theory has not become, for the behavioralists, something like what the classless society once was for Marxist-Leninists: a goal to which one pays deference and toward which one works, but without ever getting there. The behavioral approach to international relations theory remains just that—an approach: it has never gotten beyond the generation of correlational knowledge that Singer specified as the first step toward theory construction. The absence of theory is a major reason, therefore, as well as an excuse, for why the behavioralist literature has given so little attention to forecasting, and hence to the end of the Cold War.

What insights, though, has behavioralism produced? Behavioralists have put forward *some* generalizations relating to present conditions and future prospects in world politics, but these tend to be highly tentative, imperfectly integrated, and drawn almost entirely from statistically demonstrated correlations.<sup>74</sup> I have tried to summarize the most important of these below, with a view to determining what forecasting utility, if any, they might have had:

WARS ARE BECOMING LESS FREQUENT, BUT MORE DANGEROUS. The Correlates of War Project has shown that the frequency of both international and civil wars has been declining—when measured against the increasing number of states in the international system—and that it has done so dramatically since 1945.<sup>75</sup> But behavioralists have not concluded from this, as have other scholars,<sup>76</sup> that great power war is

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72. David Dessler, “Beyond Correlations: Toward a Causal Theory of War,” *International Studies Quarterly*, Vol. 35, No. 3 (September 1991), pp. 340–342, discusses the problems heterogeneous variables have posed for the Correlates of War Project.

73. See, on this point, Bruce M. Russett, “The Young Science of International Politics,” *World Politics*, Vol. 12, No. 1 (October 1969), pp. 87–94.

74. Dessler, “Beyond Correlations,” *passim*.

75. Small and Singer, “Conflict in the International System,” pp. 61–74. See also Jack S. Levy, *War in the Modern Great Power System, 1495–1975* (Lexington: University Press of Kentucky, 1983), pp. 144–149, which argues that wars among great powers have been declining in frequency over the past five hundred years, but that they have been increasing in their severity.

76. For example, Mueller, *Retreat from Doomsday*; Ray, “The Abolition of Slavery and the End of International War.”

becoming obsolete; on the contrary, they have stressed the increasing severity of the wars that do occur, together with the persistence of arms races, the dangers of nuclear and conventional weapons proliferation, and the absence of safeguards that would keep wars from breaking out. They have tended to conclude, as Singer and Melvin Small did in 1979, that the international system remains "fundamentally as war-prone as it has been since the Congress of Vienna."<sup>77</sup>

**ALLIANCES RARELY BRING SECURITY.** One generally forms alliances for the purpose of making one's nation more secure, but the behavioralist literature suggests strongly that one ought not to count on this. Past efforts to bolster security by aligning against potential adversaries have more often than not set off arms races, thereby diminishing security in the long run.<sup>78</sup> Except during the nineteenth century, most participants in alliances over the past five centuries—and all great power participants—have found themselves at war within five years after the alliance was formed.<sup>79</sup> One cannot rely on alliance partners to meet their obligations if war breaks out; alliances also tend to expand wars once they have begun.<sup>80</sup> "In sum," Michael D. Wallace concluded in 1979, "most of the evidence seems to be against those who see military alliances as necessary to peace, and on the side of those who see them as a danger."<sup>81</sup>

**PREPARATION FOR WAR RARELY ENSURES PEACE.** The dictum "Let him who desires peace prepare for war" has long been used to justify the existence and expansion of large military establishments. But Correlates of War Project statistics suggest that preparation for war has most often caused arms races, with all their attendant risks, rather than the peace this ancient maxim promises. The "far safer course of action," Wallace noted in 1981, "is to maintain unilateral restraint in acquiring new weapons systems while seeking every opportunity to negotiate bilateral and multilateral agreements to limit development and deployment."<sup>82</sup> Subsequent research in this area has shown a close correspondence between increases in military spending and involvement in military conflict: excuses tend to be found to use the weapons one develops.<sup>83</sup> Meanwhile, quantitative studies of deterrence successes and failures have revealed little correlation between military superiority, on the one hand, and success in deterring adversaries, on the other.<sup>84</sup>

**POWER DISPARITIES PROMOTE PEACE.** Behavioralists have also argued, though, that in the twentieth century at least, a well-defined international system—that is, one in

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77. Small and Singer, "Conflict in the International System," p. 80.

78. Singer, *Models, Methods, and Progress in World Politics*, pp. 194–195.

79. Jack Levy, "Alliance Formation and War Behavior: An Analysis of the Great Powers, 1495–1975," *Journal of Conflict Resolution*, Vol. 25, No. 4 (December 1981), pp. 581–613.

80. Vasquez, "The Steps to War," pp. 120–123.

81. Michael D. Wallace, "Early Warning Indicators from the Correlates of War Project," in Singer and Wallace, *To Augur Well*, p. 97.

82. Michael D. Wallace, "Old Nails in New Coffins: The Para Bellum Hypothesis Revisited," *Journal of Peace Research*, Vol. 28 (1981), pp. 94–95.

83. Singer, "Research, Policy, and the Correlates of War," pp. 54–55.

84. Jack S. Levy, "Quantitative Studies of Deterrence Success and Failure," in Paul Stern, Robert Axelrod, Robert Jervis, and Roy Radner, eds., *Perspectives on Deterrence* (New York: Oxford University Press, 1989), pp. 117–118, 120.

which each state clearly understands the intentions and capabilities of the others—makes the events that take place within that system more predictable; war, which tends to arise from the inability of statesmen to foresee consequences, therefore becomes less likely.<sup>85</sup> It would appear to follow from this that a hierarchical system of international relations—a situation in which a few great powers dominate a much larger number of weaker states—encourages stability. Great powers are more likely than smaller ones to be cautious in their dealings with one another, while smaller powers, whether cautious or not, lack the means and the inclination to challenge larger ones.<sup>86</sup>

BIPOLARITY MAY, OR MAY NOT, PROMOTE PEACE. Behavioralists have long sought to settle, by scientific means, the important question of whether bipolar or multipolar systems are more stable. But as one recent review of this literature has noted, the findings have been “exceedingly complicated and sometimes inconsistent.”<sup>87</sup> Some evidence suggests that war is more likely under conditions of extreme bipolarity and extreme multipolarity, but less likely if the situation falls between these extremes.<sup>88</sup> Other research concludes—even less helpfully—that the increasing “tightness” of bipolar alignments tends to lead to war, but that an expansion of “poles” within the international system is likely to have the same result: shifts toward either bipolarity or multipolarity, it appears, are dangerous.<sup>89</sup> One can only conclude from all of this, as Singer himself does, that “depending on the variables used, the ways in which they were measured, the spatial-temporal domain covered, and the statistical models that were applied to the data, we obtain appreciably different results.”<sup>90</sup>

This brief summary of behavioralist findings on the problem of war and peace fails to do justice to the complexities and nuances of the research that produced them. But the above propositions will serve, I think, as a fair approximation of what a policy-maker interested in applying behavioral research to world politics would have drawn from this body of work. They also suggest the difficulties such a policy-maker would have in seeking to base any reasonably coherent course of action on them.

What is one to make, for example, of the observation that wars are becoming less frequent but more dangerous? Is this insight likely to have escaped the attention of policy-makers unfamiliar with behavioralist research? How is one to reconcile the

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85. J. David Singer, “Research, Policy, and the Correlates of War,” in Øvind Østerud, ed., *Studies of War and Peace* (Oslo: Norwegian University Press, 1986), pp. 51–52; Singer, *Models, Methods, and Progress in World Politics*, pp. 242–244. It is important to note, however, that Singer’s evidence suggests the opposite to have been the case during the nineteenth century. For his non-quantitative speculation as to the reasons for this, see *ibid.*, pp. 252–255.

86. See Manus I. Midlarsky, “A Hierarchical Equilibrium Theory of Systemic War,” *International Studies Quarterly*, Vol. 30 (1986), especially pp. 85–87; also Manus I. Midlarsky, *The Onset of World War* (Boston: Unwin Hyman, 1988).

87. Vasquez, “The Steps to War,” p. 123.

88. Michael D. Wallace, “Alliance Polarization, Cross-Cutting, and International War, 1815–1964: A Measurement Procedure and Some Preliminary Evidence,” in Singer and associates, *Explaining War*, p. 105.

89. Bruce Bueno de Mesquita, “Systemic Polarization and the Occurrence and Duration of War,” in Singer and associates, *Explaining War*, pp. 129–32.

90. Singer, *Models, Methods, and Progress in World Politics*, p. 255.

arguments (a) that alliances rarely achieve security and (b) that preparation for war provides no protection against it, with the assertion (c) that disparities in power correlate with peace? How have power differentials developed in the past, after all, if not largely through the accumulation of the military strength that alliances and armaments provide? And what policy implications would behavioralist findings on the respective merits of bipolarity and multipolarity suggest, given the uncertainty of the behavioralists themselves as to what those findings are? In short, the major “policy-relevant” conclusions behavioralist research has produced are either self-evident, self-contradictory, or self-confusing.

Nor, if the fading of Soviet-American competition can serve as a test, do these findings provide any very good basis for forecasting. The declining incidence of war would have been a good indicator of what was to happen, but behavioralists chose not to make the forecast one might have expected from such data; instead they concluded that the danger of a great power conflict would be at least as high during the 1980s as it had been in the past. The Western military buildup during the early part of that decade—together with the strengthening of the NATO alliance that accompanied it—does not appear to have delayed the end of the Cold War; on the contrary, these initiatives may have hastened it.<sup>91</sup> The behavioralists’ point about power disparities does help to explain the relative stability of the Cold War international system, but it would have provided no warning of that system’s impending collapse. Nor—given their uncertainty on the effects of bipolarity and multipolarity—are the behavioralists able to make any coherent forecasts of what might replace it.

It is not my purpose here to question the competency of those scholars who have embraced behavioralism in the study of international relations. They have often been their own toughest critics; few of the criticisms I have made above have not also been made by behavioralists themselves in assessing their own work or that of their

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91. This point is a controversial one, and the evidence necessary to confirm it is not yet available. It is clear that the initial response by the Brezhnev-Andropov leadership was a dangerous war scare, best discussed in Christopher Andrew and Oleg Gordievsky, *KGB: The Inside Story of its Foreign Operations from Lenin to Gorbachev* (New York: HarperCollins, 1990), pp. 581–605. But it also seems clear that President Reagan’s announcement of the Strategic Defense Initiative in March 1983, together with the Soviet government’s failure to prevent the NATO intermediate-range nuclear forces deployment later that year, did set off a fundamental reassessment of foreign and military policy inside the Kremlin. See Jerry Hough, *Russia and the West: Gorbachev and the Politics of Reform*, 2d ed. (New York: Simon and Schuster, 1990), pp. 118–121; also Michael McCgwire, *Perestroika and Soviet National Security Policy* (Washington, D.C.: Brookings, 1991), pp. 115–173. The key question is what influence this reassessment had on Mikhail Gorbachev when he came to power in 1985: did the Soviet Union’s failures in these areas push him into *perestroika*, or would he have gone in that direction in any event? Preliminary attempts to answer this question include Deudney and Ikenberry, “The International Sources of Soviet Change,” *passim*; Thomas Risse-Kappen, “Did ‘Peace Through Strength’ End the Cold War? Lessons from INF,” *International Security*, Vol. 16, No. 1 (Summer 1991), pp. 162–188; Daniel Deudney and G. John Ikenberry, “Who Won the Cold War?” *Foreign Policy*, No. 87 (Summer 1992), pp. 123–138; and the account of a well-informed journalist, Don Oberdorfer, *The Turn: From the Cold War to a New Era: The United States and the Soviet Union, 1983–1990* (New York: Poseidon, 1991).

colleagues.<sup>92</sup> But if we are to determine why behavioralism's performance with respect to forecasting has fallen so far short of what was promised, we must consider certain difficulties that have affected the approach as a whole:

THE TYRANNY OF METHOD OVER SUBJECT. Despite the self-chosen association of many behavioralists with the field of "peace studies," the data bases they have assembled have concerned themselves, to a striking degree, with the subject of war. Their focus has been on conflict, escalation, deterrence, crisis management, and crisis decision-making. They have shown more interest in the circumstances that cause peace to break down than with those that cause it to break out. This has happened, I think, for two reasons.

First, these projects all began at the height of the Cold War, when the prospects of a hot war were uppermost in people's minds. Given the massive character of these projects, and especially their emphasis on reproducible cumulative research, it has not been easy to redirect priorities as circumstances have changed. There is a considerable irony in the fact that it proved easier to modify the official policies of the United States and the Soviet Union toward one another than to shift the focus of the major behavioralist data-collection projects that were supposed to provide insights into how those governments' policies might be modified.

Second, the behavioralists' concentration on the causes and manifestations of conflict reflect what the historical logician David Hackett Fischer has called the "quantitative fallacy": this is the assumption that "facts are important in proportion to their susceptibility to quantification."<sup>93</sup> It was simply easier to count events related to war than to peace. War is an exceptional event in international relations; despite the frequency with which it has occurred, it is always a departure from the normal state of affairs, and departures from the norm are always less difficult to measure than the norm itself.

There was nothing inherent in behavioralism that required its practitioners to proceed in this manner. One of the few even partially successful anticipations of the Cold War's end came from a pioneer in the behavioral approach to international relations, Karl Deutsch, in a 1966 article entitled "The Future of World Politics." In it, Deutsch focused not so much on the causes of conflict or the nature of the global system as on its impending transformation, a process he saw taking place because of the growth of literacy and urbanization, diminishing income inequalities, and the increasing involvement of the masses in politics. From these trends, he forecast that autocracies would find it more and more difficult to govern, that the costs of intervention in foreign countries would mount, that threats would carry less and less credibility, that nationalism would erode ideological blocs, that economic influence

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92. This is especially true of Singer, whose recent collection of essays, *Models, Methods, and Progress in World Politics*, repeatedly reflects his willingness to submit behavioralist research to critical scrutiny. See also, in particular, Hermann and Peacock, "The Evolution and Future of Theoretical Research in the Comparative Study of Foreign Policy," *passim*.

93. Fischer, *Historians' Fallacies*, p. 90. For related criticisms, see Waltz, *Theory of International Politics*, p. 64; and Hoffmann, "The Long Road to Theory," pp. 427–429.

would become more important than military force and that, in the end, a more mature condition of international society would develop than the one that had existed throughout most of the twentieth century.<sup>94</sup>

If a pioneer in the behavioralist movement could come up with these prescient observations a quarter of a century prior to the end of the Cold War, one has to wonder why the field as a whole was unable to accomplish anything like this. The answer would appear to be that Deutsch was prepared to depart from quantitative analysis when that technique was inappropriate: for him, subject determined method, rather than the other way around. Too many other behavioralists let method determine subject,<sup>95</sup> with the consequences one might expect in any situation in which means are allowed to overshadow avowed ends.

PROBLEMS OF DATA COLLECTION AND ANALYSIS. Any behavioralist would acknowledge, in principle, that one can never be totally atheoretical, otherwise one could never define research priorities. But Singer has made a point, in the Correlates of War Project, of deferring a commitment to any single theory until as much evidence as possible has been gathered. This procedure allows the testing of theories without preconceptions; presumably it would also make the endorsement of a particular theory, were that to occur, more convincing than it otherwise would have been.<sup>96</sup> The difficulty here is that such a deferral also vastly increases the task of data collection, because one thereby loses a major function that theories serve, which is to provide a basis for distinguishing between significant and trivial information. The amount of potentially useful but still unassimilated evidence does not noticeably diminish, in an atheoretical research enterprise, with the passage of time.<sup>97</sup> Or, to paraphrase a famous law of administrative science, data expands to fill the vacuum left by the absence of theory, and one never gets past the first step in one's research agenda.

A related difficulty has to do with the kind of data one collects. Behavioralists limit themselves to measuring directly observable phenomena. They by no means ignore unobservable influences, but they count them only when they manifest themselves in some quantifiable form. That approach may work in fields like politics, economics, or academia, where a single universally accepted unit of measure—votes, for example, or money, or ponderous publications—exists. But there is no such unit in the field of international relations;<sup>98</sup> one must choose, instead, between two alternatives, neither of them completely satisfactory: One can confine one's analysis to that limited sphere of world politics in which quantifiable entities do exist—war casualties, arms races, trade statistics, population movements, and the like—but this can lead to the quan-

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94. Karl W. Deutsch, "The Future of World Politics," *Political Quarterly*, Vol. 37, No. 1 (January–March 1966), pp. 9–32.

95. Waltz, *Theory of International Politics*, p. 13; Ferguson and Mansbach, *The Elusive Quest*, pp. 28–30.

96. See, on these points, Singer, "The 'Correlates of War' Project," pp. 248, 251.

97. Waltz, *Theory of International Politics*, p. 5. The behavioralists have fallen victim, I believe, to the fallacy of "holism." For more on this, see Fischer, *Historians' Fallacies*, pp. 65–68.

98. See Hoffmann, "The Long Road to Theory," p. 428.

titative fallacy, and hence to the danger of missing the kind of non-quantifiable information Deutsch focused on in his 1966 article. Or one can artificially create quantifiable entities, a procedure that has been the basis for the “events data” movement, an important part of the behavioral approach to theory construction over the past several decades.

But this latter course raises other difficulties, which have to do with the way in which one interprets the data one collects. Because information gathered has to be coded if it is to be quantified—and because coding is inescapably dependent on the subjective perceptions of those doing the coding—“events data” tends to fall short of scientific standards for objectivity and reproducibility.<sup>99</sup> Comparisons of data bases covering the same subject have shown an unsettling lack of correspondence, as have efforts to replicate coding procedures.<sup>100</sup> One is left with the suspicion that our supposedly objective data-collection efforts may not be much freer from impressionistic and arbitrary judgments than are the old-fashioned historical narratives they sought to replace.

MODELING REALITY. Even if methods were subordinated to subjects and problems of data collection and analysis had been solved, the behavioralist approach to theory and forecasting would confront a remaining difficulty: it has to do with relating the generalizations that emerge from correlations to the real world. When Singer composed his 1972 research agenda, he envisaged constructing “the most plausible and parsimonious model” one could devise that would explain the recurrence of war and peace; one would then “examine how closely that model fits the historical patterns which have been observed and recorded earlier.” Adjustments in the model would bring it progressively closer to historical experience; computer simulation would then allow movement “from runs of the past to runs of the future,” without committing “the sin of mechanical extrapolation from past into future.”<sup>101</sup>

There have always been doubts about the possibility of accomplishing this kind of thing. Gabriel Almond and Stephen Genco summarized them well in 1977 when, borrowing from Karl Popper, they pointed out that reality comprises a range of phenomena extending from the determinate to the indeterminate—from predictable clocks to unpredictable clouds, to use Popper’s metaphor—and that “models, pro-

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99. “No matter how detailed and thorough an historical inquiry may be, it certainly cannot leave us with a unique correlation between the various empirical variables which will force all observers to make identical inferences and conclusions.” Olav Njølstad, “Learning from History? Case Studies and the Limits to Theory-Building,” in Nils Petter Gleditsch and Olav Njølstad, eds., *Arms Races: Political and Technological Dynamics* (London: SAGE, 1990), p. 223.

100. See Llewellyn D. Howell, “A Comparative Study of the WEIS and COPDAB Data Sets,” *International Studies Quarterly*, Vol. 27 (1983), pp. 149–159; Jack E. Vincent, “WEIS vs. COPDAB: Correspondence Problems,” *ibid.*, pp. 161–168; Charles A. McClelland, “Let the User Beware,” *ibid.*, pp. 169–177; Charles H. Anderton, “Arms Race Modeling: Problems and Prospects,” *Journal of Conflict Resolution*, Vol. 33, No. 3 (June 1989), pp. 350–353; Gary King, “Event Count Models for International Relations: Generalizations and Applications,” *International Studies Quarterly*, Vol. 33, No. 2 (June 1989), pp. 125–128; and Richard Ned Lebow and Janice Gross Stein, “Deterrence: The Elusive Dependent Variable,” *World Politics*, Vol. 42, No. 3 (April 1990), pp. 336–369.

101. Singer, “The Peace Researcher and Foreign Policy Prediction,” pp. 7–8.

cedures, and methodologies created to explore a world in which clocklike and cloud-like characteristics predominate will capture only a part of the much richer world of social and political interaction." In their determination to be "scientific," social scientists had "overlooked the fact that much of social and political change has to be explained . . . by accidental conjunctions—by events that had a low probability of occurring." The implication of all of this, Almond and Genco insisted, was that "the explanatory strategy of the hard sciences has only a limited application to the social sciences."<sup>102</sup>

This argument made little impact at the time, given the conviction of behavioralists that a scientific approach, even to the study of apparently unpredictable phenomena, was indeed possible: all that was necessary was to get the proper "fit" between models and reality. But even as the social scientists were insisting on the need to apply "hard" scientific techniques to their field if it was ever to succeed at forecasting, the "hard" scientists themselves were backing away from the view that all phenomena could be modeled and their future behavior therefore predicted. What is even more ironic, in the light of Almond and Genco's critique, is that this shift away from scientific certainty began, quite literally, with the study of clouds.

Why, meteorologists had long asked, could one not build a computer simulation of the atmosphere that would allow reliable long-range weather forecasting? In what has now become a famous experiment, Edward Lorenz, a mathematically-inclined meteorologist, sought to construct such a model on a primitive computer at the Massachusetts Institute of Technology in 1961. Lorenz had his computer calculate certain meteorological correlations, based on known variables and a single starting point. But he quickly found that tiny variations in his parameters—the rounding of a number from six decimal points to three, for example—produced startling effects on his computer screen: patterns that should have corresponded in fact diverged dramatically, and did so on the basis of statistical variations so minute that no real-world measuring device could possibly compensate for them. What this suggested, Lorenz noted in another cloud-related metaphor, was that something as unpredictable as the fluttering of a butterfly's wings over Beijing could produce a hurricane over New York. Thus was born the principle of "sensitive dependence on initial conditions," the "butterfly effect," that makes long-term weather forecasting—the transformation of clouds into clocks, if you will—impossible.<sup>103</sup>

Not all phenomena, to be sure, are subject to the butterfly effect. The motions of planets, and of spacecraft traveling between them, do proceed like clockwork, and the familiar principles of Newtonian physics provide an entirely adequate method of forecasting their behavior for, if necessary, centuries to come. But these are systems in which only a few critical and easily calculable variables are at work. Equally reliable weather forecasting over a time scale extending only into next week would require

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102. Gabriel A. Almond and Stephen J. Genco, "Clouds, Clocks, and the Study of Politics," *World Politics*, Vol. 20, No. 4 (July 1977), pp. 493, 496–497.

103. The story is well told in James Gleick, *Chaos: Making a New Science* (New York: Viking, 1987), pp. 11–21. Dessler, "Beyond Correlations," pp. 342–344, employs a similar meteorological metaphor to critique Correlates of War Project methodology.

calculating an infinite number of variables with infinite precision, a task well beyond the abilities of even the most sophisticated computer today, or in the foreseeable future. As a consequence, scientists have had to learn to live with the fact that some phenomena can be predicted with great accuracy, but that other phenomena can never be. Regularity and apparent randomness co-exist quite easily in a real world, which does not require their measurement, if not always in our minds, which do.<sup>104</sup>

Surely human affairs, and the history they produce, come closer to falling into the unpredictable rather than the predictable category: not only are the potentially relevant variables virtually infinite, but there is the added complication—not found in either clouds or clocks—of self-awareness, which means that the “variables” themselves can often foresee the consequences of contemplated actions, and reconsider them accordingly. The behavioralist enterprise of attempting to theorize about, and then to forecast, the actions of individuals, societies, nations, and groups of nations on the basis only of observable, calculable evidence and without taking into account the critical variable of self-awareness is, ultimately, an attempt to transform clouds into clocks. It is an incomplete, misleading, and washed-out representation of reality; no wonder, therefore, that it was so unsuccessful in forecasting the end of the Cold War.

#### STRUCTURAL APPROACHES

What about the structuralists? Did an approach to theory construction that incorporated the role of unobservable—and unquantifiable—phenomena in world politics do any better than the behaviorists in anticipating recent developments?

Morton A. Kaplan’s 1957 book, *System and Process in International Politics*, was the first major attempt at a structural analysis of world politics. In it, Kaplan identified six distinctive international systems, only two of which had actually existed in modern history.<sup>105</sup> He described the characteristics of these systems in considerable detail, as well as the processes operating within them that would contribute to systemic perpetuation or disintegration. Kaplan claimed no ability to forecast what particular states within any of these systems might do in specific situations. He pointed out, though, that although physical scientists had no means of mapping in advance the paths of individual gas molecules, they could reliably predict how an aggregation of such molecules would behave under known pressures and temperatures. Theories of international politics, Kaplan thought, ought to work in much the same way: they should allow one to detect patterns of behavior within international systems, and they should be capable of specifying the conditions under which those systems would remain stable, or be transformed into something else.<sup>106</sup>

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104. See also, on the limited possibilities of prediction in the physical sciences, Kaplan, *System and Process in International Politics*, p. xvii.

105. *Ibid.*, pp. 21–53. For an even more elaborate typology, based on actual historical experience, see Rosecrance, *Action and Reaction in World Politics*, pp. 219–275. As Waltz has pointed out, however, Rosecrance’s typology is not a structural theory. Waltz, *Theory of International Politics*, pp. 41–43.

106. Kaplan, *System and Process in International Politics*, pp. xvii–xviii. It is worth noting that

The two historical systems Kaplan identified were the familiar “balance of power” system, which had lasted throughout the eighteenth, nineteenth, and early twentieth centuries, and the “loose bipolar” system, which had been functioning since 1945. As its name implied, the balance of power system had operated without a dominant power or combination of powers; instead each major state sought to counter bids for dominance on the part of other states. The loose bipolar system, in contrast, evolved from the fact that two predominant states had emerged from World War II capable of incorporating less powerful states into coalitions they controlled; the configuration was “loose” because some other states remained apart from these alignments, and because a few significant actors within the system—the United Nations, in particular—were not states.<sup>107</sup> Kaplan was able to draw upon historical evidence in describing these two systems, and in explaining how the first of them had evolved into the second. No historical evidence was available in 1957, though, to illustrate the breakup of a loose bipolar system, or to answer the question of what might replace it. Kaplan’s theoretical description of this process is of interest, therefore, as an early attempt to forecast, solely by deductive means, how the end of the Cold War might come about.

Total war in a loose bipolar system, Kaplan anticipated, would bring about a unipolar international system if one side won, or chaos if both sides were exhausted. A stalemate, he thought, would produce a “tight bipolar” system, in which both antagonistic coalitions would become hierarchical. But what if these blocs should begin to disintegrate without war taking place? Here Kaplan argued that the greater the amount of hierarchy within a bloc, the more resistant to fragmentation it would be: coalitions that had come together freely would tend to fly apart more easily than those that had been forged, and sustained, by tight central control. Instability within voluntary coalitions would probably push the system as a whole toward unipolarity; in the unlikely event that involuntary coalitions should break up, the international system would revert to a balance of power configuration, or toward some form of international organization. A simultaneous weakening of both coalitions would also probably revive the balance of power system or encourage movement toward some form of central world government.

Loose bipolarity, Kaplan noted, contained “a considerable degree of inherent instability,” because so much depended upon the kind of relationship that existed between the two dominant states. There would be, on the one hand, a strong temptation for each of them to seek to eliminate its rival, if for no other reason than to avoid the danger of being eliminated itself. But, on the other hand, if “the destructive power of weapons increases to an inordinate degree, this fact may raise the costs of military action so greatly that the blocs arrive at some form of accommodation.”

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Kaplan’s claims regarding the structuralist approach to forecasting did not differ significantly from those Singer had made from a behavioralist perspective. See especially Singer, “The Peace Researcher and Foreign Policy Prediction,” p. 8.

107. Kaplan, *System and Process in International Politics*, pp. 22–25, 36–38. Loose bipolarity did not require equivalent behavior by each pole: Kaplan made a point of stressing that the coalitions within such a system could be organized either hierarchically or non-hierarchically, so that integration within them could come about by coercion or by choice.

And if such weapons should become relatively cheap, yet another kind of international system might evolve: a “unit veto” arrangement in which “each actor responded to the negative golden rule of natural law by not doing to others what he would not have them do to him.” In short, all of the above options were possible: “Depending upon conditions, the loose bipolar system can be transformed . . . into a tight bipolar system, into a hierarchical international system, a universal international system, a ‘balance of power’ system, or a unit veto system.”<sup>108</sup>

Kaplan’s book was a remarkable feat of theoretical imagination, but that was also its problem. Critics found his discussion of four systems that had never existed to be puzzling: how could one know, Hedley Bull asked, that these were the *only* four possible systems and that, even if they were, all of the relevant variables that might shape their character had been included?<sup>109</sup> Kaplan’s terminology was confusing—he used the term “subsystem dominant,” for example, to suggest that the international system dominated units within it, and he applied the adjective “hierarchical” both to systems and to blocs within systems.<sup>110</sup> His structuralist approach did generate forecasts, but these tended to be so abstract and indecisive—so inclined toward “all of the above” conclusions—that they were of little greater use to policy-makers than those of the behavioralists would be. And in those few instances in which Kaplan did make specific predictions—for example, his assertion that tightly controlled coalitions would be more durable than those that functioned by mutual consent—they have not held up particularly well.

The principal criticism of Kaplan’s method, though, was that he had failed to distinguish the structure of his respective international systems from the behavior of states within them. In an effort to explain how systems can become unstable and evolve into something else, he fell into the argument that *processes within states* could shape systemic structures. The point would have been unexceptionable had Kaplan not also insisted that *international systems* determine the behavior of states. But since Kaplan had made that assertion, the logic of his analysis, and hence its capacity for forecasting, was questionable.<sup>111</sup>

That, at least, that was the argument that Kenneth N. Waltz, Kaplan’s chief critic and the most influential “structuralist” in contemporary international relations theory, made in 1979. In his book, *Theory of International Politics*, Waltz sought to rescue the structuralist approach by making a sharp distinction between what he called “systems level” and “unit level” phenomena. Any theory that sought to account for or to anticipate the workings of an international system, he insisted, had to concern itself only with the characteristics of that system; it could not confuse the issue by introducing the behavior of individual states within it. The reason for this was that international systems imposed their own limits upon state action. Even the most revolutionary state would not revolutionize world politics if systemic influences re-

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108. *Ibid.*, pp. 40–43, 50.

109. Hedley Bull, “International Theory: The Case for a Classical Approach,” in Knorr and Rosenau, eds., *Contending Approaches to International Politics*, pp. 32–33.

110. See Kaplan, *System and Process in International Politics*, pp. 17, 40–41, 48–50.

111. Waltz, *Theory of International Politics*, pp. 54–59.

sisted that objective; even the most conservative nation would fail to stabilize an international order if the systemic prerequisites for stability were not present. The internal character of states—whether democratic or autocratic, capitalist or communist, peace-loving or aggressive—made no difference; what defined an international system was the anarchic environment in which it operated, together with the distribution of capabilities across the states that existed within it. Changes in this distribution produced shifts in systemic structure.<sup>112</sup>

Waltz agreed with Kaplan that there had been only two international systemic structures in modern history: the multipolar system that had characterized interstate relations from approximately the time of the Treaty of Westphalia through the end of World War II, and the bipolar system that had replaced it. But Waltz went well beyond Kaplan in insisting, on both theoretical and historical grounds, that bipolar systems were inherently more “stable” than their multipolar counterparts. From a theoretical perspective, the existence of only two major adversaries minimized the possibilities of misperception, confusion, and unpredictable interaction: as any physicist could explain, two-body problems are far easier to solve than those involving three or more.<sup>113</sup> From a historical perspective, Waltz could point to the success of the United States and the Soviet Union in managing crises and maintaining alliances without resort to war over three and a half decades, a record that compared favorably with what the pre-1945 great powers had accomplished in a multipolar international environment.<sup>114</sup>

What did all of this imply about the future of the Cold War? A superficial reading of Waltz would suggest that, because he described bipolarity as more stable than multipolarity and because he defined “stability” as simply the capacity of the system to endure,<sup>115</sup> he had been quite wrong in 1979: multipolarity, after all, lasted for three hundred years; bipolarity would survive only for another ten. But Waltz had been careful to point out that the principal actors in the pre-World War II multipolar system had changed frequently: of some seven great powers in 1700, only France and Great Britain continued to enjoy that status in 1939. Turkey, Sweden, Spain, Austria, and the Netherlands had all lost their preeminence by the time World War II broke out; Germany, Italy, Japan, the Soviet Union, and the United States had arisen to take their place. Soviet-American bipolarity seemed robust forty years later because no third power had developed capabilities comparable to those commanded by Moscow and Washington, but the system was “unlikely to last as long as its predecessor.”<sup>116</sup>

Common threats, Waltz believed, could transform enemies into allies. The emergence of the Soviet Union and the United States as adversaries after World War II

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112. *Ibid.*, pp. 97–98.

113. *Ibid.*, p. 192. See also Waltz, “The Stability of a Bipolar World,” *Daedalus*, Vol. 93, No. 3 (Summer 1964), pp. 881–909. A succinct explanation of why three-body problems defy solution appears in Peter Coveney and Roger Highfield, *The Arrow of Time: A Voyage Through Science to Solve Time’s Greatest Mystery* (New York: Fawcett Columbine, 1990), p. 267.

114. Waltz, *Theory of International Politics*, pp. 163–176.

115. *Ibid.*, pp. 132n, 161–162.

116. *Ibid.*, p. 162.

had had the paradoxical effect of reconciling once antagonistic states in Western Europe. Conditions of insecurity that had caused Europeans to distrust one another for so long disappeared in the face of greater danger; long-term cooperation became possible, even as the Cold War itself intensified.<sup>117</sup> Even if no third state seemed likely to threaten Russians and Americans in a way that might cause them to settle their differences, Waltz came to see that a common technological threat might have the same result. Waltz had minimized the effects of nuclear weapons in *Theory of International Politics*—"in shaping the behavior of nations, the perennial forces of politics are more important than the new military technology"<sup>118</sup>—but he soon reconsidered this position, so much so, indeed, that by 1981 he was advocating the proliferation of nuclear capabilities to smaller powers as a sure way to guarantee peace among them. The possibility of an all-out nuclear war might well serve as the functional equivalent of a third party threat in driving the United States and the Soviet Union toward the discovery of common ground; certainly concentration on the destructive capabilities of nuclear weapons "has obscured the important benefits they promise to states trying to coexist in a self-help world."<sup>119</sup>

It was also the case that the very character of bipolar confrontation carried within it the causes of its own eventual demise. Citing conclusions drawn from the study of oligopolistic competition among corporations, Waltz pointed out that the passage of time makes it easier for rivals to cooperate: "The increasing similarity of competitors' attitudes, as well as their experience with one another, eases the adjustment of their relations." Bipolar situations, in particular, encouraged this process: "Tension in the system is high because each can do so much for and to the other. But because no appeal can be made to third parties, pressure to moderate behavior is heavy. . . . The simplicity of relations in a bipolar world and the strong pressures that are generated make the two great powers conservative."<sup>120</sup> By this logic, the bipolar structure of the post-1945 international system suggested an eventual moderation of Soviet-American hostility, if not an end to it altogether.

Waltz was not at all certain, therefore, that the Cold War would continue: indeed *Theory of International Politics* holds up rather well in its anticipation of several influences that would bring that conflict to an end. He did maintain, however, that bipolarity would survive: "The barriers to entering the superpower club have never been higher and more numerous. The club will long remain the world's most exclusive one."<sup>121</sup> The maturation of a bipolar relationship did not necessarily mean its passing: "American and Russian behavior has changed somewhat over time, but it has changed

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117. *Ibid.*, pp. 70–71.

118. *Ibid.*, p. 173.

119. Kenneth N. Waltz, "Toward Nuclear Peace," in Robert J. Art and Kenneth N. Waltz, eds., *The Use of Force: Military Power and International Politics*, 3d ed. (Lanham, Md.: University Press of America, 1988), p. 689. This essay originally appeared as *The Spread of Nuclear Weapons: More May Be Better*, Adelphi Paper No. 171 (London: International Institute of Strategic Studies, 1981). See also Kenneth N. Waltz, "Nuclear Myths and Political Realities," *American Political Science Review*, Vol. 84, No. 3 (September 1990), pp. 733–734.

120. Waltz, *Theory of International Politics*, pp. 173–174.

121. *Ibid.*, p. 183.

in the direction one may expect it to take so long as the world remains bipolar."<sup>122</sup> This is where Waltz went wrong: he allowed for the possibility that Soviet and American behavior within a bipolar structure might evolve from confrontation to cooperation; but he made no allowance for the possibility that the structure itself might shift, or that changes in the policies of nations within it might contribute to that process.

The failure to account for structural change has always been the weakest point in Waltz's theory.<sup>123</sup> For if systemic structures do in fact reflect the distribution of capabilities across units, and if shifts in this distribution can in fact alter such structures, then it is difficult to see where those shifts might come from except from changes in the capabilities of states within the system. Those changes may arise, in turn, from a decision to make peace with rivals, or from a recognition that one can no longer keep up with rivals, or from both—the two perspectives are not incompatible, as Soviet policy after 1985 showed. But in either case they result from actions taken *within units*, and yet because they shape capabilities they also *affect structures*. Waltz himself acknowledged that structure does not account for everything that happens in world politics: "To explain outcomes one must look at the capabilities, the actions, and the interactions of states, as well as at the structure of their systems. . . . Causes at both the national and the international level make the world more or less peaceful and stable."<sup>124</sup>

If unit-level behavior as well as system-level constraints can cause cooperation to evolve, though, it is difficult to see what is gained by insisting that students of world politics emphasize the latter at the expense of the former. Waltz had concentrated on the systemic level, he explained near the end of *Theory of International Politics*, "because the effects of structure are usually overlooked or misunderstood and because I am writing a theory of international politics, not foreign policy."<sup>125</sup> But this was only to make the same error the behavioralists had made, which was to let the method of one's inquiry shape its conclusions. It was also to imply that the behavior of states as well as systems is critical to an understanding of international relations—precisely what Waltz had criticized Kaplan for having asserted. In the end, then, the rigid separation of systems from units provided no firmer basis for theory—or for forecasting—than had an approach that had taken both of them into account.

Apart from these early and highly theoretical efforts by Kaplan and Waltz, structuralism produced few significant insights as to how the Cold War might end until the publication of Stephen Rock's *Why Peace Breaks Out* in 1989, literally on the eve

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122. *Ibid.*, p. 204.

123. See Ruggie, "Continuity and Transformation in the World Polity," pp. 142–152; Robert O. Keohane, "Theory of World Politics: Structural Realism and Beyond," pp. 169–173; Robert W. Cox, "Social Forces, States and World Orders: Beyond International Relations Theory," p. 243; and Richard K. Ashley, "The Poverty of Neorealism," p. 288, all in Keohane, ed., *Neorealism and Its Critics*.

124. Waltz, *Theory of International Politics*, pp. 174–175. See also Waltz, "Reflections on *Theory of International Politics*: A Response to My Critics," pp. 327–329, 343.

125. *Ibid.*, p. 175.

of that event.<sup>126</sup> This book deserves special emphasis, not just because it is the only explicitly structural analysis we have of the circumstances that have caused cold wars in the past to disappear, but also because Rock sought to use the resulting hypotheses to specify what it would take would bring our own Cold War to a peaceful conclusion. Rarely does history provide so rapid an opportunity to test theory against experience.

Rock began by arguing that traditional balance of power theory fails to account for several important historical instances of great power rapprochement. Why, for example, did the unification of Germany in 1871, the most significant challenge to European equilibrium since the Napoleonic wars, produce more than four decades of European peace? Why did Great Britain in the 1890s suddenly stop trying to counter the rising power of an old antagonist whose rapidly increasing capabilities posed the greatest of all potential threats to British global hegemony—the United States? In neither of these cases, Rock maintains, did the emergence of any common threat force former adversaries to cooperate,<sup>127</sup> considerations other than those of pure power were obviously involved. Complementary economies and ideologies muffled geopolitical conflicts: there were times when appeasing an ascending rival was likely to be less costly and more beneficial, from the standpoint of national priorities, than the path of resistance that strict adherence to balance of power principles would require. The causes of peace, therefore, lay not just in the configurations of international systems, but also in the internal structures of the states that make them up.<sup>128</sup>

A careful analysis of these causes, Rock argued, would not reveal in advance exactly when two previously antagonistic states might reconcile their differences, but it would nonetheless have “predictive value.” It could, in particular, “provide us with clues as to whether or not a [Soviet-American] reconciliation is probable, and to the kinds of developments that would make one more (or less) likely in the future.”<sup>129</sup> Rock extracted, from his study of great power rapprochements in the past, four specific hypotheses that could be used to evaluate the prospects for an end to the Cold War:

1. “A state of peace is most likely to emerge among states that are heterogenous in the exercise of national power.” By this somewhat murky formulation, Rock meant simply that states whose geopolitical interests do not clash tend not to clash militarily.

2. “A state of peace is most likely to emerge among states that are heterogenous in their economic activities.” Here Rock was making the useful point that the complementarity of economies, not the volume of transactions between them, encourages peace: states whose economies are not directly competitive with one another—whose exports do not displace the other’s domestic producers—will maintain more friendly

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126. Stephen R. Rock, *Why Peace Breaks Out: Great Power Rapprochement in Historical Perspective* (Chapel Hill: University of North Carolina Press, 1989). As Rock notes (pp. 3–4), the only previous modern effort to build a theoretical explanation of how peace evolves was Deutsch, *Political Community in the North Atlantic Area*, the first stage of a research design that was never completed.

127. Not all diplomatic historians would agree with Rock’s assertion that growing concern about Germany did little to influence Britain’s determination to improve relations with the United States.

128. Rock, *Why Peace Breaks Out*, pp. 8–12.

129. *Ibid.*, p. 149.

relations than states in which similar commodities are produced and competitive efforts to market them therefore ensue.

3. "A state of peace is most likely to emerge among states that are homogenous in their societal attributes." This proposition was straightforward enough: states that resemble one another tend not to fight.

4. "Even if the exercise of power, economic activities, and societal attributes favor pacific relations, some catalytic event may be required to set the process of reconciliation in motion. The most probable candidate for this role is an acute crisis between the two states." Or, the imminence of military conflict may force greater attention to economic and ideological complementarities.<sup>130</sup>

The Soviet-American relationship, Rock noted, had never come close to meeting these standards. It was—and had been since 1945—one involving "intense geopolitical competition, a keen sense of ideological estrangement and mistrust, [and] potentially strong but actually weak economic connections." Not even a crisis like the one over Cuba in 1962 had been sufficient to overcome these unpromising conditions and produce anything approaching a lasting reconciliation between Moscow and Washington. It was hardly surprising, therefore, that the Cold War had gone on for so long; "nor does any fundamental improvement in Soviet-American relations seem likely without some alteration in these conditions." In the light of these structural impediments, it would be "naive," Rock argued, to claim "that changes conducive to a far-reaching transformation of the Soviet-American relationship are probable, or that a rapprochement could be easily effected."<sup>131</sup>

The situation was not, however, entirely hopeless. The rise of a third power like China could cause Washington and Moscow to develop common geopolitical interests. A "long-term decline in Soviet and/or American military capabilities could force a strategic retrenchment on the part of one or both superpowers, reducing the extent to which their interests overlap"; the experiences of Vietnam and Afghanistan had already shown that the superpowers' capacity for intervention in the Third World was not what it once had been. Both the Soviet Union and the United States faced potentially serious internal economic difficulties, and "although the correlation between a nation's economic strength and its military capabilities is not perfect, there is clearly a relationship between the two factors." There was no immediate prospect of ideological reconciliation: the United States was not about to relinquish its democratic principles; "nor can one expect the Soviet Union to renounce socialism, particularly since Marxist-Leninist doctrine serves to legitimize the existing Soviet regime." Historical, linguistic, and cultural traditions were vastly different, "and will surely remain so." But the economies of the two countries were potentially compatible, and there were some indications that Mikhail Gorbachev was seeking to jettison ideological orthodoxies, although by no means to the same extent that the Chinese government was doing.<sup>132</sup>

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130. *Ibid.*, pp. 12–18.

131. *Ibid.*, p. 151.

132. *Ibid.*, pp. 151–154.

For the immediate future, Rock concluded, “perhaps the best for which we can hope is an end to . . . shrill ideological rhetoric.” More fundamental changes might be possible over the long run as domestic politics in the United States swung back toward liberalism and as Gorbachev’s reforms took hold.<sup>133</sup> The existence of nuclear weapons had had “a considerable and even profound impact on the relations between states,” even if they had not removed “the need to analyze and to understand other, more fundamental, sources of states’ attitudes and behaviors toward one another.” Effective statesmanship could certainly make a difference at the margins once the structural prerequisites for a reconciliation were in place.<sup>134</sup> But nothing in Rock’s book would have led a reader to expect the Cold War to end clearly and decisively within months of its publication. Nor did *Why Peace Breaks Out* come anywhere close to explaining how—or why—that event took place.

What actually happened, after all, was the abrupt and asymmetrical collapse of one superpower, not the gradual and symmetrical decline of both. The government of the Soviet Union did give up Marxism-Leninism, despite the fact that its own authority derived from that ideology. The Cold War ended without any significant increase in Soviet or Eastern European economic contacts with the West; indeed one could argue that it was precisely the absence of such contacts that hastened the Cold War’s demise. No obvious third party threat existed: far from forcing cooperation between Moscow and Washington to counter the growing influence of China in the world, Beijing’s aging gerontocracy aborted a once-thriving reform movement and turned that nation inward upon itself. Nor was any catalytic crisis required to shock Soviet and American leaderships into recognizing their mutual dependence upon one another; instead the crises that developed in Eastern Europe in the fall of 1989 shocked Marxist governments throughout that region by demonstrating that they could no longer depend upon Moscow to prop them up. In short, the most serious structuralist effort to forecast the end of the Cold War failed, and failed thoroughly.

I say this not to demean Stephen Rock’s attempt. After all, he alone among structuralists (and behavioralists as well) had the courage to venture clear theory-based forecasts of how the Cold War might end.<sup>135</sup> It is a daunting thing to freeze one’s vision of the future in the highly-visible and unforgiving medium of cold type; perhaps that is why so many theorists—however confident they may be about the validity of their theories—avoid that exercise altogether. It is also the case that failed forecasts can provide insights into the causes of failure: in that sense, they can be just as valuable as forecasts that succeed. Rock’s inability to foresee what turned out to be a very near future reflects, not so much his own shortcomings as an analyst of international politics, but rather a more general weakness in the structuralist approach to theory as a whole, and thus to whatever forecasts might be based upon it.

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133. *Ibid.*, p. 153.

134. *Ibid.*, pp. 155–159.

135. Sean M. Lynn-Jones did collaborate with Rock on an essay, “From Confrontation to Cooperation: Transforming the U.S.-Soviet Relationship,” in Joseph S. Nye, Jr., Graham T. Allison, and Albert Carnesale, eds., *Fateful Visions: Avoiding Nuclear Catastrophe* (Cambridge, Mass.: Ballinger, 1988), pp. 111–131, the conclusions of which roughly parallel those of Rock’s book.

This weakness is the tendency to treat time as a dimension—like length, width, and depth—but not as a process. Structuralists see time as a scale against which to measure events, but they pay little attention to the fact that the passage of time, in and of itself, also shapes events. In this respect, they resemble those pre-Darwinian paleontologists who believed in the immutability of species: despite being surrounded by evidence showing that animals, plants, and even land forms had evolved over time, these scientists simply assumed the absence or the unimportance of evolution and therefore lacked the means to understand, account for, and anticipate structural change.<sup>136</sup> Like pre-Darwinian paleontologists, structural theorists of international relations have produced firm and at times startling conclusions; these go well beyond the range of behavioralist analysis, which normally extends from the cautious confirmation of the obvious to the inability to confirm anything at all. But the static character of the structuralists' conclusions—the failure to account for change—left that approach little better equipped than behavioralism to forecast the quite dramatic changes that brought about the Cold War's end.

#### EVOLUTIONARY APPROACHES

“Evolutionists” assume the interaction of behavior and structure in world politics, and incorporate observable and unobservable phenomena into their explanations of how that happens. But their chief distinguishing characteristic—the one that differentiates evolutionists most sharply from other theorists—is the attention they give to changes in behaviors and structures over time. Evolutionists see time itself as influencing what happens, even as it provides the chronological framework we use to make sense of what has happened. A static representation of behavior and structure may work reasonably well when the objects being described are inanimate, or when the organisms being cataloged are incapable of learning from experience.<sup>137</sup> But human beings do learn from the past: history allows for the inheritance of acquired characteristics, even if biology does not.<sup>138</sup> For this reason, the passage of time, which is the process through which experience accumulates, affects behavior and structure in observable and unobservable ways: it constitutes a third axis along which the search for a theory of international relations must proceed.

Evolutionists disagree, though, on how time produces its effects. Linear evolutionists tend to see historical processes as irreversible: like time itself, history flows in one direction only; a return to prior conditions is as improbable as it would be for an arrow to reverse its course in mid-flight. The future, from this perspective, will not resemble the past; one can nonetheless foresee certain aspects of it by calculating the trajectories of historical trends—or arrows—that seem likely to continue. Cyclical

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136. See Gould, *Time's Arrow, Time's Cycle*, pp. 146–149.

137. Although not perfectly well: as Darwin pointed out long ago, natural selection provides a way for even the most primitive organism to “learn” at least indirectly from experiences of the past.

138. See E.H. Carr, *What Is History?* (New York: Vintage Books, 1961), pp. 150–151; also Ernest Gellner, *Plough, Sword and Book: The Structure of Human History* (Chicago: University of Chicago Press, 1988), p. 14.

evolutionists believe that although time does indeed move forward and not backward, historical processes may do both: they can reverse themselves even as time proceeds; cycles rather than arrows provide the appropriate metaphor. From this angle of vision, the future will at times resemble, even if it does not precisely replicate, the past; one can foresee certain aspects of it by understanding the frequency, amplitude, and implications of historical cycles.

To be sure, distinctions between linear and cyclical views of history are rarely this sharply drawn in practice. For if the future were completely unlike the past, then we would have no categories with which to characterize it: each morning—indeed each moment—would be totally novel,<sup>139</sup> and forecasting of any kind would be impossible. Conversely, if the future always resembled the past, nothing would be unexpected and there would be no need for forecasting in the first place.<sup>140</sup> Still, evolutionists do tend to work within linear or cyclical frames of reference when they generalize about the past; the choice they make, in turn, affects how they see the future.

LINEAR EVOLUTION. The conviction that historical processes operate in a linear manner goes back as far as the ancient Hebrews;<sup>141</sup> but it was Karl Marx who created the most influential theory of irreversible historical change by inverting the Hegelian dialectic to insist that deeply-rooted economic forces—shifts in the “means of production,” to use Marx’s term—determine the structure of societies and the behavior of states, driving them forward in time in ways that are inexorable and, therefore, largely predictable.<sup>142</sup> The progression from feudalism through capitalism to socialism and ultimately communism was as certain as was the Darwinian process of natural selection, Marx’s collaborator Friedrich Engels insisted; individuals could harness these forces only by aligning their own objectives with them.<sup>143</sup> Subsequent history—not least the end of the Cold War—has shown Marx and Engels to have been wrong about the *direction* in which history was moving: it certainly did not bring about the death of capitalism, the triumph of communism, and the consequent disappearance of the state. Marxism’s botched forecasts have by no means disproven the Marxist assumption that underlying historical processes do exist, though, and that they function rather in the way we now know tectonic forces move continents around on the surface of the earth. These processes may operate very slowly, with no visible

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139. For a poignant clinical analogy, see Oliver Sacks, *The Man Who Mistook His Wife for a Hat, and Other Clinical Tales* (New York: Harper and Row, 1987), pp. 23–42.

140. The above discussion of cycles and arrows has been very much influenced by Gould, *Time’s Arrow, Time’s Cycle*, a book that shows brilliantly how insights drawn from geology, biology, and paleontology can sharpen one’s understanding of history.

141. See Herbert Butterfield, *The Origins of History* (New York: Basic Books, 1981), pp. 80–117.

142. Ernst Breisach, *Historiography: Ancient, Medieval, and Modern* (Chicago, Ill.: University of Chicago Press), pp. 293–297. Marx did not, however, completely deny the possibility of individual autonomy in history: “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.” Karl Marx, “The Eighteenth Brumaire of Louis Bonaparte” (1852), in Robert C. Tucker, ed., *The Marx-Engels Reader*, 2d ed. (New York: Norton, 1978), p. 595.

143. Friedrich Engels, “Socialism: Utopian and Scientific” (1892), in Tucker, *The Marx-Engels Reader*, pp. 696–697, 712.

consequences for long periods of time. But when their effects do appear, they can be as dramatic as the earthquakes that result from the buildup of strains along geologic fault lines. And once such upheavals happen, they cannot be undone.<sup>144</sup>

Evolutionists have devised no general theory of linear change in world politics, but they have advanced specific theories based on the workings of irreversible processes in several areas that relate to the problem of how the Cold War would end:

*Development.* The dismantling of European colonialism in Africa, the Middle East, and Southeast Asia during the 1950s and 1960s created a strong demand for explanations that would not only account for what was happening within a bewildering array of newly-independent states, but also provide a basis for future policies toward them. There emerged, in response, a theory of “development” that purported to show how economic and social evolution shape politics. Based on the assumption that stages of modernization exist, much like Marx’s stages of production, this literature sought to identify corresponding political structures; it even attempted to forecast points at which “developing” countries would be most vulnerable to communism’s seductive claim that it could accelerate what Walt Rostow liked to call the “takeoff” to mass production and consumption. It was no accident—as the Marxists themselves would have said—that Rostow chose for his influential 1960 book, *The Stages of Economic Growth*, the sub-title *A Non-Communist Manifesto*.<sup>145</sup>

Development theory proved to be of little use in anticipating events in the Third World: it overestimated the appeal of communism and underrated that of nationalism; it failed to foresee the durability of markets as against command economies; and it encouraged a hyperactive interventionism on the part of the United States, which found it necessary to try to “guide” new nations along the path to social stability and geopolitical reliability, at times with disastrous results.<sup>146</sup> Particularities of events taking place on several different continents and within dozens of different cultures overwhelmed the capacity of theorists to advance valid generalizations about them. But attempts to link stages of economic growth with political evolution have worked much better when applied to the narrower task of evaluating what was happening within the Soviet Union, Eastern Europe, and China.

As early as 1960 Rostow and other development theorists were predicting that economic modernization without political democratization—the path Marxist-Leninist

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144. For more on this “tectonic” metaphor, see John Lewis Gaddis, *The United States and the End of the Cold War: Implications, Reconsiderations, Provocations* (New York: Oxford University Press, 1992), pp. 155–167.

145. W.W. Rostow, *The Stages of Economic Growth: A Non-Communist Manifesto* (Cambridge: Cambridge University Press, 1960), especially pp. 1–3, 145–167. For other examples of applied modernization and development theory, see Max Franklin Millikan and Donald L.M. Blackmer, eds., *The Emerging Nations: Their Growth and United States Policy* (Boston: Little, Brown, 1961); Edward Shils, *Political Development in the New States* (The Hague: Mouton, 1962); Cyril E. Black, *The Dynamics of Modernization: A Study in Comparative History* (New York: Harper and Row, 1966); Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968); and Edward L. Morse, *Modernization and the Transformation of International Relations* (New York: Free Press, 1976).

146. See Shafer, *Deadly Paradigms*, especially pp. 276–290.

states were following at the time—was certain to fail. The reason had to do with what Lenin had added to Marx: a rigidly centralized political structure superimposed upon what was supposed to have been a largely spontaneous process of economic development. Authoritarian government might indeed set industrialization in motion, as the Soviet Union's experience under Stalin had shown. But the effective management of an industrial economy would require mass education; peasants do not automatically become technocrats. Education, though, would raise political consciousness, thereby creating the risk that a politically aware population would not indefinitely accept political repression. The choice Marxism-Leninism would eventually face, then, would be a bleak one: either dismantle authoritarianism in order to save the economy, or ruin the economy in order to save authoritarianism. Marx had the process right but the outcome wrong: it turned out to be communism, not capitalism, that carried within it the seeds of its own destruction.<sup>147</sup>

*Interdependence.* Modern industrial economies make their requirements felt within capitalist societies as well; by the mid-1970s these had elicited, in the rise of "interdependence" studies, a second linear evolutionist approach to international relations theory. The emergence of this field reflected widespread dissatisfaction with the "realist" tendency to reduce all of world politics to a simple struggle for power. Such reductionism, critics argued, overlooked the post-World War II expansion in commerce and communications that had already altered the nature of traditional geopolitical competition. No single nation, or group of nations, had set this trend in motion; instead it was the product of something Marx himself might have recognized—a fundamental shift in the means of production with both structural and behavioral consequences. Relationships based on integration and cooperation were becoming at least as important as those conducted according to old-fashioned balance of power rules; collaborative international "regimes" were emerging in certain areas, even as competitive international rivalries continued in others.<sup>148</sup>

Perhaps because the end of the Cold War seemed so distant during the late 1970s and early 1980s, regime theorists were slow to apply their findings to the study of

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147. Rostow, *The Stages of Economic Growth*, pp. 159–162. See also Seymour Martin Lipset, "Some Social Requisites of Democracy: Economic Development and Political Legitimacy," *American Political Science Review*, Vol. 53, No. 1 (March 1959), especially pp. 75–85; Zbigniew Brzezinski, *Between Two Ages: America's Role in the Technetronic Era* (New York: Viking Press, 1970), pp. 154–176; and Morse, *Modernization and the Transformation of International Relations*, pp. 191–192. For excellent retrospective descriptions of this process, see Theodore S. Hamerow, *From the Finland Station: The Graying of Revolution in the Twentieth Century* (New York: Basic Books, 1990), pp. 210–225, 302–309; also William H. McNeill, "Winds of Change," in Nicholas X. Rizopoulos, ed., *Sea-Changes: American Foreign Policy in a World Transformed* (New York: Council on Foreign Relations Press, 1990), pp. 168–171. Rostow applied his own analysis to the future of Soviet-American relations in "On Ending the Cold War," *Foreign Affairs*, Vol. 65, No. 3 (Spring 1987), pp. 831–851.

148. Keohane and Nye, *Power and Interdependence*, especially pp. 3–22, provides the best introduction to this line of argument. See also Krasner, *International Regimes*; and, for a longer historical perspective, Rosecrance, *The Rise of the Trading State*, passim. Waltz anticipated many of the basic elements of regime theory in *Theory of International Politics*, especially pp. 173–174.

that conflict.<sup>149</sup> But the fact that several previously-established patterns of Soviet-American cooperation survived the “era of stagnation” in Moscow and the first Reagan administration in the United States, together with the rapid decline in Cold War tensions that followed the advent of Mikhail Gorbachev in 1985, left little doubt that mutual suspicion was no longer the only force driving the superpower relationship.<sup>150</sup> Meanwhile, new developments in game theory and in the study of corporate behavior were revealing that competitors might well reciprocate cooperative initiatives if they had reason to believe that their competition would continue.<sup>151</sup> Historical and theoretical developments converged, therefore, to show how the passage of time could make possible the evolution of cooperation—which is to say, the emergence of regimes—even under conditions of anarchy. For the first time there appeared to be both a practical and a conceptual way out of the “prisoner’s dilemma” that had for so long confounded those seeking a model for how the Cold War might end.<sup>152</sup>

There was one difficulty, though, in extending interdependence from economics into geopolitics. If in fact the requirements of modern industrial economies linked nations more closely than ever before, then the likelihood of war among them should have diminished: classical liberalism had long argued that nations who traded with one another would have few incentives to fight one another.<sup>153</sup> But security regimes, if understood in the context of the Cold War, grew out of a security dilemma:<sup>154</sup> it was the fear of war, not the desire for profit, that induced cooperation, and if war was improbable, then it was not clear why the Soviet Union and the United States should cooperate, given the infrequency of economic contacts between them. The improvement in Soviet-American relations that was so obviously taking place in the 1980s seemed to require more than the purely economic explanation that development and interdependence theories had provided.

*The obsolescence of war.* If large-scale and long-term processes were so important in the economic realm, some linear evolutionists wondered, why should comparable mechanisms not also shape social institutions and the actions that take place within

149. See Robert Jervis, “Security Regimes,” in Krasner, ed., *International Regimes*, pp. 173–194.

150. See John Lewis Gaddis, “The Long Peace: Elements of Stability in the Postwar International System,” *International Security*, Vol. 10, No. 4 (Spring 1986), pp. 99–142; also the case studies in Dallin, George, and Farley, *U.S.-Soviet Security Cooperation*.

151. These developments are discussed in Axelrod, *The Evolution of Cooperation*; and in Oye, *Cooperation under Anarchy*.

152. “Prisoner’s dilemma” games have figured so prominently in the theoretical literature on international relations over the past three decades that it hardly seems necessary to describe them here. Those in need of elucidation may find it in Axelrod, *The Evolution of Cooperation*, pp. 7–12. I cannot refrain, however, from calling attention to Robert Jervis’s observation that: “It is not a good sign [when] prisoners confronted by a District Attorney do not behave as the [prisoners’ dilemma] model would lead us to expect.” Jervis, “Realism, Game Theory, and Cooperation,” *World Politics*, Vol. 40, No. 3 (April 1988), p. 319. Jervis is citing here the work of Brian Forst and Judith Lucianovic, “The Prisoner’s Dilemma: Theory and Reality,” *Journal of Criminal Justice*, Vol. 5 (Spring 1977), pp. 55–64.

153. The argument is clearly made in Rosecrance, *The Rise of the Trading State*.

154. Alexander L. George, “Factors Influencing Security Cooperation,” in George, Farley, and Dallin, *U.S.-Soviet Security Cooperation*, pp. 655–678.

them? Historians had long understood that societies can change over time: despite the fact that the keeping of slaves, the denial of education to women, and even the abandonment of children were all ancient traditions that had been socially acceptable for centuries, they were so no longer. Once attitudes shifted against each one of these practices, its future differed quite dramatically from its past.<sup>155</sup> Three different linear evolutionist arguments arose, during the 1980s, suggesting that something like this might be happening to the institution of war itself.

The first of these had to do with the nuclear revolution. The quantum jump in destructive capabilities that had suddenly become available in 1945, many experts argued, revolutionized statecraft as well as warfare by virtually ruling out the use of military force in relations between great powers. Technological innovation had produced a geopolitical shock of the most fundamental proportions, and as a result “nuclear learning” had taken place, so that the world’s most powerful nations were far less inclined than ever before to risk war with one another.<sup>156</sup> Evolution had worked, in this instance, not through the slow accumulation of desirable adaptations, but rather through an abrupt “punctuation” that instantly and irrevocably altered the international environment and the requirements for survival within it.<sup>157</sup>

A second evolutionist argument came to the same conclusion by a different route. War had been well on the way to becoming obsolete before nuclear weapons had been invented, John Mueller insisted in his 1989 book *Retreat from Doomsday*; even without the bomb, the escalating costs of military operations—because of the increasing lethality of weapons and vulnerability of targets—would have made a war among great powers no more likely in the last half of the twentieth century than it would have been for statesmen from those countries to try to settle their differences through the nineteenth-century expedient of fighting a duel.<sup>158</sup> By this logic, industrialization and modernization, even as they produced instruments of war, became forces for peace.

A third argument for the evolving obsolescence of war stressed the influence of democratization. Building on a suggestion made by Immanuel Kant in 1795, Michael Doyle argued that liberal democracies have strong ideological and psychological inhibitions about fighting one another, quite apart from the quantity and character of the arms they possess. Through careful historical research, Doyle documented an

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155. See, on these points, David Brion Davis, *The Problem of Slavery in Western Culture* (Ithaca: Cornell University Press, 1966); Lawrence Stone, *The Family, Sex and Marriage in England, 1500–1800* (New York: Harper and Row, 1979); and John Boswell, *The Kindness of Strangers: The Abandonment of Children in Western Europe from Late Antiquity to the Renaissance* (New York: Pantheon, 1989).

156. Nye, “Nuclear Learning and U.S.-Soviet Security Regimes,” *passim*. See also McGeorge Bundy, *Danger and Survival: Choices About the Bomb in the First Fifty Years* (New York: Random House, 1988), pp. 463–516; and Michael Mandelbaum, *The Nuclear Revolution: International Politics Before and After Hiroshima* (Cambridge: Cambridge University Press, 1981).

157. My analogy here is to the concept of “punctuated equilibria” in evolution. See Niles Eldredge, *Time Frames: The Evolution of Punctuated Equilibria* (Princeton: Princeton University Press, 1985).

158. Mueller, *Retreat from Doomsday*, pp. 3–13.

accelerating trend toward democratic forms of government over the past two centuries; he also pointed out that there has never been a war between two liberal democracies.<sup>159</sup> Wars between democracies and non-democratic states were still possible, Doyle acknowledged, but as the former became more numerous and the latter less—a proportion significantly shifted in democracy's favor by the end of the Cold War—his findings appeared to reinforce what Mueller and the “nuclear learning” theorists have suggested about the diminishing likelihood of great power war.

From these linear evolutionist perspectives, then, a Soviet-American reconciliation should have been an entirely predictable development. The end of the Cold War was “over-determined,” in that several separate historical processes—the invention of nuclear weapons, the steadily-mounting costs of conventional war, and progress toward democratization, as well as the development dilemma of Marxism-Leninism, the trend toward interdependence, and the emergence of regimes—all pointed toward the same outcome. These processes became apparent only along a temporal axis of analysis: the passage of time was required for their effects to appear, but once they did they were as irreversible as time itself. History was like Humpty-Dumpty: old ways of doing things, once broken up, could never be put back together again.

But linear evolutionists in fact came no closer than behavioralists or structuralists to forecasting the actual circumstances that brought the Cold War to an end. One reason, I suspect, is that long-term historical processes are indeed, as Marx suggested, subterranean phenomena. One discovers their existence from the consequences they produce; it often requires an earthquake to determine where a fault line really is.<sup>160</sup> A second explanation has to do with the familiar problem of compartmentalization: theorists may well identify a particular process and even forecast its consequences, but few if any theories are built on the *convergence* or *intersection* of complementary processes or, for that matter, on the potential fratricide of contradictory ones.<sup>161</sup> Finally, linear evolutionists tend to commit what I would call the “Fukuyama fallacy,” named after the political scientist Francis Fukuyama, who chose the summer of 1989 to publish an article arguing that because Western liberal democracy had triumphed over Marxism-Leninism, Hegel's old vision of an end to history had finally come to

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159. Michael Doyle, “Liberalism and World Politics,” *American Political Science Review*, Vol. 80, No. 4 (December 1986), pp. 1151–1163. See also Doyle, “Kant, Liberal Legacies, and Foreign Affairs,” *passim*; also Melvin Small and J. David Singer, “The War-Proneness of Democratic Regimes, 1816–1965,” *Jerusalem Journal of International Relations*, Vol. 1, No. 3 (Summer 1976), pp. 50–68, a Correlates of War Project study which anticipated Doyle's findings, but expressed pessimism about “the continuing democratization of the world” (p. 68).

160. Or, as Alexander L. George has put it: “I believe that theory does better in explaining what has happened than in predicting it.” George, “The Transition in U.S.-Soviet Relations, 1985–1990: An Interpretation from the Perspective of International Relations Theory and Political Psychology,” *Political Psychology*, Vol. 12, No. 3 (September 1991), p. 469.

161. I have tried to elaborate on this problem in *The United States and the End of the Cold War*, pp. 168–192. The only forecast I have seen that argued explicitly for the possibility of a *near-term* end to the Cold War is Eric A. Nordlinger, “Prospects and Policies for Soviet-American Reconciliation,” *Political Science Quarterly*, Vol. 103, No. 3 (Summer 1988), pp. 197–222. But, interestingly, this forecast was a projection of several converging historical trends. In this sense, it resembles the 1966 Karl Deutsch article cited in note 94.

pass. “Centuries of boredom” affording minimal opportunities for “daring, courage, imagination, and idealism” lay ahead, Fukuyama lamented,<sup>162</sup> six months before the Berlin Wall came down, a year before an unprecedented international effort to liberate Kuwait began, and two years before Boris Yeltsin and a few of his supporters, through the sheer force of their moral and political authority, so thoroughly humiliated the KGB, the Soviet government, and the Communist Party of the Soviet Union as to call into question the very survival of those institutions.

The Fukuyama fallacy is the tendency for those who advance propositions about irreversible forces in history to conclude that history will stop with them. Hegel, for a time, believed that history had ended with Napoleon. Marx committed a similar error when he made the proletarian revolution the final stage in historical development;<sup>163</sup> and so too, although to a less egregious extent, have more recent linear evolutionists. Certain that they have exposed *an* engine that drives history forward, they never seem to ask whether there might be others, or whether the one they have focused on might also operate in reverse. Confident that they have identified *a* direction in which history is proceeding, they rarely tell us how they have determined what the ultimate destination actually is. A flea creeping along the inside of a hula hoop might well see its progress as linear, purposeful, and irreversible: curved surfaces often appear flat to those with limited horizons. Or, as Mark Twain once warned: “The past does not repeat itself, but it rhymes.”<sup>164</sup>

CYCLICAL EVOLUTION. Cyclical evolutionism is a useful corrective to the Fukuyama fallacy. Its antecedents go back at least to the ancient Greeks; certainly they were implicit in Thucydides’s hope that “these words of mine [will be] judged useful by those who want to understand clearly the events which happened in the past and which (human nature being what it is) will, at some time or other and in much the same ways, be repeated in the future.”<sup>165</sup> Modern interest in historical cycles has grown largely out of the field of economics, where recurring patterns exist at several different levels of analysis.<sup>166</sup> Evolutionary theorists of international relations have used cyclical approaches to explain—and make forecasts about—the course of revolutions, alternations between democratic and authoritarian forms of government and, most extensively, the relationship between war, peace, and national power over extended periods of time.<sup>167</sup>

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162. Francis Fukuyama, “The End of History?” *The National Interest*, No. 16 (Summer 1989), p. 18.

163. Breisach, *Historiography*, pp. 231–232, 297.

164. Quoted in David Pratt, “The Functions of Teaching History,” in Stephen Vaughn, ed., *The Vital Past: Writings on the Uses of History* (Athens: University of Georgia Press, 1985), p. 208.

165. Thucydides, *History of the Peloponnesian War*, trans. by Rex Warner (New York: Penguin Books, 1954), p. 48. See also Butterfield, *The Origins of History*, pp. 121–126; and a classic book on this subject, Mircea Eliade, *The Myth of the Eternal Return*, trans. by Willard R. Trask (New York: Pantheon, 1954).

166. See Edward R. Dewey and Edwin F. Dakin, *Cycles: The Science of Prediction* (New York: Henry Holt, 1947); also W.W. Rostow, *The World Economy: History and Prospect* (Austin: University of Texas Press, 1978).

167. Strictly speaking, a cyclical view of history would appear to rule out evolution: if everything

*Revolutions.* One of the most fruitful efforts to employ historical cycles in forecasting has had to do with the phenomenon of revolution, and why it has so rarely produced the results Marx anticipated. Marx himself, along with Lenin, Trotsky, and the other architects of the 1917 upheaval in Russia, recalled very vividly how the French Revolution of 1789 had fallen into an autocratic “Bonapartist” phase; they worried that a similar gap between intentions and consequences might arise as socialism supplanted capitalism.<sup>168</sup> That concern did not prevent the rise of Stalin, but his totalitarian rule did provoke a good deal of thought during the 1930s and 1940s about what causes revolutions to go astray. Trotsky made significant contributions to this analysis prior to his assassination; so too did his biographer, Isaac Deutscher.<sup>169</sup> But it was the historian Crane Brinton who provided the most durable explanation of how revolutions originate, evolve, and eventually degenerate in his 1938 book, *The Anatomy of Revolution*.

Basing his findings on a comparative study of the English civil war and the American, French, and Russian Revolutions, Brinton identified a cycle through which such disruptions tend to proceed: the collapse of the old regime, the euphoria of revolution itself, the failure of moderates to match ideals with accomplishments, the rise of extremists, their use of—but ultimately consumption by—terror, and finally the reassertion of a central authority whose oppressiveness might well exceed anything that existed under the old regime in the first place.<sup>170</sup> Brinton claimed no scientific rigor for this model, and refused to regard it as a basis for theory.<sup>171</sup> But as another historian, Theodore S. Hamerow, showed half a century later, Brinton’s cycles of revolutionary evolution came remarkably close to anticipating what would happen to Marxism-Leninism, not just inside the Soviet Union, but also in Eastern Europe, China, Vietnam, and Cuba during the Cold War: they explain how once vigorous revolutions lose their momentum, ossify, and eventually turn into old regimes themselves, vulnerable to new revolutionary challenges.<sup>172</sup>

Brinton’s work parallels—and in terms of predictive potential holds up considerably better than—Marx’s own use of linear evolutionist analysis to forecast the overthrow of capitalism a century earlier.<sup>173</sup> From Brinton’s vantage point one might well have

repeats, how can anything change? But as the Mark Twain quotation in the text suggests, the argument is not that *everything* repeats but that *some things do*. Once certain processes are set in motion—like revolutions, reforms, wars, and the building of empires—certain patterns tend to recur, even as time (as always) moves on.

168. Richard Pipes, *The Russian Revolution* (New York: Knopf, 1990), pp. 468–469.

169. Leon Trotsky, *Stalin: An Appraisal of the Man and His Influence* (New York: Harper, 1946); Isaac Deutscher, *Stalin: A Political Biography* (New York: Oxford University Press, 1949). George Orwell’s caustic novels, *Animal Farm* (New York: Harcourt, Brace, 1946) and *1984* (New York: Harcourt, Brace, 1949), of course fit into this tradition as well.

170. Crane Brinton, *The Anatomy of Revolution*, rev. ed. (New York: Prentice Hall, 1952), *passim*. Brinton’s inclusion of the American Revolution has always struck critics as having strained his argument, which would have held up just as well without this peculiar case.

171. *Ibid.*, p. 18. However, Brinton did see himself as following “scientific methods” in his study.

172. Hamerow, *From the Finland Station*, *passim*.

173. See also, on Marx’s failures as a forecaster, Theda Skocpol, *States and Social Revolutions: A*

foreseen, in a way that no behavioralist, structuralist, or Marxist perspective would have allowed one to foresee, the otherwise unexpected combination of petrification and fragility that has come to characterize once-revolutionary regimes in our time, and that accounts, to a large degree, for the asymmetrical manner in which the Cold War ended.<sup>174</sup>

*Liberalism and authoritarianism.*<sup>175</sup> One should not assume, though, that democracies are exempt from the kind of cyclical evolution that afflicts revolutionary regimes. Our understanding of how democratic governments rise and decline has not advanced as far as our knowledge of how revolutions evolve; but we have more than enough historical evidence to know that democratization is by no means an irreversible process. Athens lost its democracy and Rome its republic; fascism and communism originated, during and after World War I, in states that had appeared to be well on the way to representative constitutional government. The second half of this century has indeed seen a remarkable expansion of democracy throughout the world; certainly that ideology has proven to be more durable than its Marxist-Leninist alternative. But there is no clear guarantee that this process will continue;<sup>176</sup> hence the importance of attempting to determine whether the present movement toward liberalism really is irreversible, or whether it simply alternates, over long periods of time, with authoritarianism.

Recent work by the sociologist John A. Hall provides a starting point for such an undertaking. Hall accepts the linear evolutionist view that democratization is necessary to sustain economic development; but he does not conclude from this that liberalism is necessarily the wave of the future.<sup>177</sup> For one thing, governments may conclude that the danger of losing their authority exceeds the costs of political repression and of resulting economic regression: presumably this is what happened in China in 1989. More significant in the long run, though, is the possibility that Marx may have been partially right after all: that capitalism and the liberalism it generates do carry within them the seeds of their own periodic decline, if not destruction altogether. Societies do not determine qualities of life solely by calculating economic advantage. Spiritual, psychological, and emotional needs have to be satisfied as well, and liberalism—depending as it does upon rationality—is often ill-equipped for that

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*Comparative Analysis of France, Russia, and China* (Cambridge: Cambridge University Press, 1979), especially pp. 284–293; and Zbigniew Brzezinski, *The Grand Failure: The Birth and Death of Communism in the Twentieth Century* (New York: Scribner's, 1989).

174. "The ideas, the promises of orthodox Marxism as now embodied in Stalin's Russia," Brinton wrote in the revised edition of *The Anatomy of Revolution* in 1952, "may well prove in the next few years almost as embarrassing in Russian internal politics as useful in Russian external politics. The Marxist heaven on earth will do as a mere promise in Indonesia or Iran, for a while, but in Moscow, it has got pretty soon to become in part visible—or the whole doctrine must undergo a still unpredictable transformation" (p. 248.)

175. I am using the term "liberalism" here in its original sense, that is, one that emphasizes the value of individual liberties and seeks to minimize government control.

176. See John J. Mearsheimer, "Back to the Future: Instability in Europe After the Cold War," *International Security*, Vol. 15, No. 1 (Summer 1990), pp. 48–51.

177. John A. Hall, *Powers and Liberties: The Causes and Consequences of the Rise of the West* (Berkeley: University of California Press, 1985), pp. 197–209.

task.<sup>178</sup> Where did fascism and communism come from, after all, if not from the disillusionment their followers felt with late nineteenth-century liberal capitalism?

Threats to contemporary liberalism are already becoming apparent, even as that ideology consolidates its victory in the Cold War. Religious, linguistic, and ethnic tensions have risen dramatically in Central and Eastern Europe and even within the former Soviet Union itself as the heavy hand of Moscow's authority has disappeared: these provide infertile ground for the growth of democratic institutions. The resurgence of religious fundamentalism in the Middle East has already shown that the creation of wealth and indulgence in consumption do not always ensure democratic politics. The lowering of barriers to trade and immigration within the European Community and North America is causing protectionist and restrictionist pressures to build in those parts of the world; improvements in transportation have also facilitated the spread of illicit drugs and AIDS; economic development, we now understand, can bring about ecological dangers not just in the form of pollution but also ozone holes, disappearing rain forests, and rising ocean levels. And in the United States, an uneasy compromise tolerates the existence of an economic and social "underclass," a deteriorating physical and educational infrastructure, corporate greed, ballooning deficits, and vapid politics in return for the short-term gratifications of minimally-intrusive government and low taxes.<sup>179</sup>

From these perspectives, it is not all that difficult to see how late twentieth-century *laissez-faire* liberalism could give rise to a collective alienation comparable to that induced by its late nineteenth-century counterpart. It may be, then, that neither liberalism nor authoritarianism is foreordained, but rather that a dialectical relationship exists between them in which the excesses of one create opportunities for the other. The end of the Cold War could turn out to be the precursor of something worse.

*War and peace.* Theories of cyclical evolution are most fully developed with respect to the issue of war and peace, where there has been a major effort over the past two decades to determine whether conflict in the international system really is like earthquakes along fault lines: a recurring phenomenon brought about by the accumulating pressures of underlying economic, social, and geopolitical forces.<sup>180</sup> Proponents of

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178. John A. Hall, *Liberalism: Politics, Ideology, and the Market* (Chapel Hill: University of North Carolina Press, 1987), especially pp. 71–100. For a critique of "rational actor" models, see Hollis and Smith, *Explaining and Understanding International Relations*, pp. 144–146.

179. For an elaboration of this screeed, see Gaddis, *The United States and the End of the Cold War*, pp. 202–208. Arthur M. Schlesinger, Jr.'s cyclical theory of American politics suggests that a shift toward more active government and a greater concern for public as opposed to private interests is on the way. See Schlesinger, *The Cycles of American History* (Boston: Houghton Mifflin, 1986), pp. 23–48.

180. The best review of this literature is Jack S. Levy, "Long Cycles, Hegemonic Transitions, and the Long Peace," in Charles W. Kegley, Jr., ed., *The Long Postwar Peace: Contending Explanations and Projections* (New York: HarperCollins, 1991), pp. 147–176. But see also George Modelski and Patrick M. Morgan, "Understanding Global War," *Journal of Conflict Resolution*, Vol. 29, No. 3 (September 1985), pp. 391–417; Nathaniel Beck, "The Illusion of Cycles in International Relations," *International Studies Quarterly*, Vol. 25, No. 4 (December 1991), pp. 455–476; and Joshua S. Goldstein, "The Possibility of Cycles in International Relations," *ibid.*, pp. 477–480.

“long cycle” or “power transition” theory accept Marx’s view that uneven rates of economic and technological development cause shifts from one phase in history to another.<sup>181</sup> But where Marx saw these phases as linear progressions in forms of economic organization, scholars in this field have seen them as part of a cyclical process—extending over periods of anywhere from 100 to 150 years—by which rising powers challenge dominant “hegemons” for control of the international system.

Such challenges, according to the theory, produce a “hegemonic” war from which a single superpower emerges: the most recent examples have been Great Britain after the Napoleonic Wars and the United States after World War II. The new hegemon need not have initiated the challenge that overthrew its predecessor; it does not even have to be the strongest nation in the postwar international arena. It is, however, the state best positioned to establish and maintain a worldwide system of international economic and political relationships, and it does this as much by eliciting the cooperation of other nations as by intimidating them. The resulting hegemonic management produces, for a time, a long peace. Eventually, though, the rise of other states that have chosen to follow the hegemon’s example—together with the hegemon’s own exhaustion, bureaucratization, and consequent loss of imagination—creates instabilities that lead to major war and to the emergence of a new hegemon, thereby starting the cycle all over again.<sup>182</sup>

The Cold War, from this angle of vision, was a brief, unsuccessful, and not even particularly interesting challenge by the Soviet Union to the hegemonic position the United States established for itself in world politics after 1945.<sup>183</sup> Predictably, the challenge failed, not just because of the economic and technological backwardness of the USSR, but also because international systemic conditions themselves worked against a successful challenge to the dominant hegemon at such an early point in a historical long cycle. Washington wrote the “rules” for the international “game” that emerged from World War II: it was hardly surprising that the deck was stacked against Moscow.<sup>184</sup>

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181. Gilpin, *War and Change in World Politics*, p. 93; Goldstein, *Long Cycles*, p. 282. See also Kennedy, *The Rise and Fall of the Great Powers*, pp. 436–437.

182. I have attempted to summarize here: Gilpin, *War and Change in World Politics*, pp. 9–15; Goldstein, *Long Cycles*, pp. 15–17; Olson, *The Rise and Decline of Nations*, p. 45; and A.F.K. Organski and Jacek Kugler, *The War Ledger* (Chicago: University of Chicago Press, 1980), pp. 13–63; as well as a paragraph from my own essay, “Great Illusions, the Long Peace, and the Future of the International System,” in Gaddis, *The United States and the End of the Cold War*, p. 187.

183. Witness the extent to which some theorists of hegemonic stability neglect Cold War history altogether. For more on this, see Gaddis, *ibid.*, pp. 175–176.

184. Adherents to the “world system” approach to international affairs would argue that it is not just the United States but capitalism in general that has “stacked the deck” against both the Second and Third Worlds, severely inhibiting progress toward social and economic development in those regions. See, for example, Immanuel Wallerstein, *The Capitalist World-Economy* (New York: Cambridge University Press, 1979); and Wallerstein, *The Politics of the World-Economy: The States, the Movements, and the Civilizations* (New York: Cambridge University Press, 1984); also the discussion in Ole R. Holsti, “Models of International Relations and Foreign Policy,” *Diplomatic History*, Vol. 12, No. 1 (Winter 1989), pp. 27–29. But this approach is a crude form of static structuralism which allows no role whatever for particularities of history, personality, politics,

Long cycle theorists have had less to say about the end of the Cold War than about the possibility that the United States may have already begun the gradual decline in power that eventually afflicts all hegemonies, and that more serious challenges than the one the Soviet Union posed are likely to arise in the twenty-first century. Future historians may find it odd that this concern over “decline” should have intensified during the late 1980s, the very point at which the United States was emerging as the world’s sole surviving superpower. But long cycle theory never saw the Soviet Union as a credible competitor with the United States in anything other than brute military strength, and that capability, the theorists would argue, is of relatively little importance in maintaining hegemonic control. The real threat to American predominance lies, they believe, in what the United States is doing to itself by failing to maintain its competitive edge in the global economy, together with what new competitors like Japan and the European Community are doing to exploit the opportunities thereby handed them.<sup>185</sup>

No cyclical theorist would claim that cycles—long-term, short-term, or in between—repeat themselves precisely. Some linear evolution always takes place: cycles may operate but time pushes them forward as they do, thereby subjecting them to modification by non-cyclical forces.<sup>186</sup> Robert Gilpin has explicitly raised the possibility, for example, that a linear development—the invention of nuclear weapons—may have broken the old cycle of recurring hegemonic wars; Mueller and Doyle have implicitly suggested a similar effect as the result of shifting social consciousness and growing political awareness.<sup>187</sup> But this is what makes forecasting from a cyclical evolutionist perspective difficult. Long cycle theory provides no very good way of determining the extent to which linear progression has modified cyclical patterns, and the longer the cycles are, the harder it is to resolve this question: it is frustrating to lack the means of verifying one’s vision of the future other than by awaiting completion of the next historical cycle. Only history, by this logic, can confirm theory, and that may take a while.

*Decline.* Late in 1987, a surprise best-seller brought the implications of long cycle theory to the attention of a mass audience in a way that the theorists themselves could never have managed; indeed Paul Kennedy’s *The Rise and Fall of the Great Powers* may well have had as great an impact on American society during the final stages of the Cold War as did Edward Gibbon’s *The Decline and Fall of the Roman Empire* at the time of its publication in Great Britain during the War for American Independence.<sup>188</sup>

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or culture. Change can occur, presumably, only when the worldwide Marxist revolution finally comes, which now looks to be a while.

185. The argument is most thoroughly laid out in Robert Gilpin, *The Political Economy of International Relations* (Princeton: Princeton University Press, 1987). But see also David P. Calleo, *Beyond American Hegemony: The Future of the Western Alliance* (New York: Basic Books, 1987); and Kennedy’s *The Rise and Fall of the Great Powers*, discussed below.

186. If the term did not already have specific connotations in deterrence theory, a “spiral” model would be a good way of illustrating how cyclical processes actually work.

187. Gilpin, *War and Change in World Politics*, pp. 213–219. For Mueller and Doyle, see notes 158 and 159, above.

188. See Roy Porter, *Gibbon: Making History* (New York: St. Martin’s Press, 1988), p. 161.

The condition of being a great power is in fact transitory, Kennedy argued; the United States can no more exempt itself from the historical cycle of ascendancy and enfeeblement than any other powerful state has ever been able to do. "It simply has not been given to any one society to remain *permanently* ahead of all the others, because that would imply a freezing of the differentiated pattern of growth rates, technological advance, and military developments which has existed since time immemorial."<sup>189</sup>

The resulting uproar—for it was nothing short of that<sup>190</sup>—over Kennedy's thesis reflected the transition to the post-Cold War world that was already beginning to develop within the American public consciousness. Previous outbreaks of anxiety over national inadequacies, most notably the one that occurred in the wake of the *Sputnik* launch in 1957, had focused on what the United States had to do to keep up with the Russians. But the Soviet Union hardly figured in the "declinism" debate of the late 1980s: Kennedy himself had seen its power as eroding even more rapidly than that of the United States,<sup>191</sup> and most of his readers no doubt worried more about Japan as a potential challenger to the American position in the world than they did about the Soviet Union.

Kennedy did warn, though, along with many others,<sup>192</sup> that the disintegration of Soviet authority would be a dangerous thing. His argument is worth quoting in full, because it reflects views that were almost universally held prior to the revolutionary year 1989:

There is nothing in the character or tradition of the Russian state to suggest that it could ever accept imperial decline gracefully. Indeed, historically, *none* of the over-extended, multinational empires which have been dealt with in this survey—the Ottoman, the Spanish, the Napoleonic, the British—ever retreated to their own ethnic base until they had been defeated in a Great Power war, or (as with Britain after 1945), were so weakened by war that an imperial withdrawal was politically unavoidable. Those who rejoice at the present-day difficulties of the Soviet Union and who look forward to the collapse of that empire might wish to recall that such transformations normally occur at very great cost, and not always in a predictable manner.<sup>193</sup>

Whatever the accuracy of Kennedy's views on American "decline," it is clear now that he was quite wrong—as was almost everyone else—in failing to foresee how suddenly, how thoroughly, and how *peacefully* the Soviet Union would relinquish its position as a superpower, indeed its own existence as a state. History contains no

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189. Kennedy, *The Rise and Fall of the Great Powers*, p. 533.

190. For the debate, see Peter Schmeisser, "Taking Stock: Is America in Decline?" *New York Times Magazine*, April 17, 1988, pp. 24–27, 66–68, 96; Joseph S. Nye, Jr., "Understating U.S. Strength," *Foreign Policy*, No. 72 (Fall 1988), pp. 105–129; Samuel P. Huntington, "The U.S.—Decline or Renewal?" *Foreign Affairs*, Vol. 67, No. 4 (Winter 1988/89), pp. 76–96; and Paul Kennedy, "Can the U.S. Remain Number One?" *New York Review of Books*, March 16, 1989, pp. 36–42.

191. Kennedy, *The Rise and Fall of the Great Powers*, pp. 488–514.

192. Including John Lewis Gaddis, "How the Cold War Might End," *The Atlantic*, Vol. 260 (November 1987), pp. 88–100. My own retrospective critique of this mostly unsuccessful effort at forecasting appears in Gaddis, *The United States and the End of the Cold War*, pp. 132–154.

193. Kennedy, *The Rise and Fall of the Great Powers*, p. 514.

precedent for so striking an example of abrupt but amicable collapse. Either the world has been extraordinarily lucky, or linear evolution has pushed familiar cycles of war, peace, and decline into a new and wholly unfamiliar environment. How does one account for—and how might one have anticipated—this development?

One obvious answer is that we give too little thought to how cyclical and linear patterns interact with one another. Some attention to what development and interdependence theorists were saying about the Soviet economy, combined with what the “nuclear learning” theorists, Mueller, and Doyle were suggesting about shifts in collective social consciousness, might have hinted that old habits of using force to repress change would no longer work. Certainly Kennedy’s materialist analysis of the nature of power did not give sufficient weight to the role immaterial forces have played in this situation: the Cold War ended as much because of what people believed as because of what they possessed. As one of Kennedy’s most thoughtful critics, Joseph S. Nye, Jr., has pointed out, new “soft” forms of power are emerging, especially in the form of culture, education, and mass communications, the nature of which cannot be calculated according to traditional geopolitical equations.<sup>194</sup>

It is also the case that we tend to bias our historical and our theoretical analyses too much toward continuity. Despite our awareness that abrupt change occurs frequently in history and in personal experience, despite our understanding that intellectual breakthroughs more often result from sudden flashes of insight than from the diligent piling up of evidence,<sup>195</sup> we rarely find a way to introduce discontinuities into theory, or to attempt to determine what causes them to happen. This is another area in which social scientists could learn from recent developments in mathematics and the “hard” sciences, where a new understanding of complexity, chaos and catastrophe is providing ways to anticipate otherwise unexpected shifts in what had seemed to be gradual evolutionary processes. The circumstances surrounding the end of the Cold War may not be all that different, at least by way of analogy, from the sudden die-offs that occur among animal species under certain conditions, or the unexpected collapse of bridges due to metal fatigue, or wild fluctuations in markets, or even the transition from regular to turbulent flow that happens every time we turn on a water tap.<sup>196</sup>

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194. See Joseph S. Nye, Jr., “Soft Power,” *Foreign Policy*, No. 80 (Fall 1990), pp. 153–171; also Nye, *Bound to Lead*, passim.

195. Kuhn, *The Structure of Scientific Revolutions*, has become the classic text on this subject.

196. See Gleick, *Chaos*, pp. 59–86, 262–267. In addition to Gleick, helpful guides to these new developments—at least for laypeople—include Pagels, *The Dreams of Reason*; Coveney and Highfield, *The Arrow of Time*; Nicholson, *Formal Theories in International Relations*; and (now somewhat out of date), Alexander Woodcock and Monte Davis, *Catastrophe Theory* (New York: Dutton, 1978). Preliminary efforts to apply these techniques to the study of history and international relations include Alan D. Beyerchen, “Nonlinear Science and the Unfolding of a New Intellectual Vision,” *Papers in Comparative Studies*, Vol. 6 (1988/89), pp. 25–49; Beyerchen, “Clausewitz, Nonlinearity, and the Unpredictability of War,” *International Security*, Vol. 17, No. 3 (Winter 1992/93), pp. 59–90; C. Dyke, “Strange Attraction, Curious Liaison: Clio Meets Chaos,” *The Philosophical Forum*, Vol. 21 (Summer 1990), pp. 369–392; George A. Reisch, “Chaos, History, and Narrative,” *History and Theory*, Vol. 30 (1991), pp. 1–20; Donald N. McCloskey, “History, Differential Equations, and the Problem of Narration,” *ibid.*, pp. 21–36; and, interestingly, James

The geological and biological sciences, from which the idea of evolution arose in the first place, have always allowed for the integration of linear, cyclical, and even catastrophic phenomena: landforms rise up and collapse but never in exactly the same way; ontogeny recapitulates phylogeny but genetically-unique individuals (almost) always result from the process; violent eruptions and abrupt extinctions periodically occur. It is odd that the evolutionary approach to international relations theory—which will have to incorporate both pattern and particularity if it is ever to provide a basis for forecasting—seems to find these kinds of juxtapositions so difficult to manage.

### Conclusion<sup>197</sup>

“If you are a student, switch from political science to history.” Such was the blunt reply of Robert Conquest, the distinguished Anglo-American historian of the Soviet Union, when asked to draw lessons from the abortive coup against Mikhail Gorbachev in August 1991.<sup>198</sup> Conquest is hardly a neutral observer, but he does have a point. The efforts theorists have made to create a “science” of politics that would forecast the future course of world events have produced strikingly unimpressive results: none of the three general approaches to theory that have evolved since 1945 came anywhere close to anticipating how the Cold War would end.

It will not do to claim that forecasting was never an objective of these theories in the first place, because the theorists repeatedly set that task for themselves. Nor was the “case” in question an insignificant one: the end of the Cold War brought about nothing less than the collapse of an international system, something that has happened in modern history only once before—if one accepts structuralism’s emphasis on the shift from multipolarity to bipolarity at the end of World War II.<sup>199</sup> Nor is the test at issue here an unfair one: after all, more than one generation of theorists made the Cold War their central preoccupation. If their forecasts failed so completely to anticipate so large an event as that conflict’s termination, then one has to wonder about the theories upon which they were based. Either those theories were themselves artifacts of the Cold War, in which case they lacked the universal applicability so often claimed for them; or they were universally applicable, in which case they were simply wrong.

This failure of international relations theory arose primarily, I believe, because of a methodological passing of ships in the night. The social sciences, seeking objectivity,

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N. Rosenau, *Turbulence in World Politics: A Theory of Continuity and Change* (Princeton: Princeton University Press, 1990). I have also benefited a great deal from having read several unpublished papers on this subject by Stefan Rossbach which grew out of his work as a Social Science Research Council/MacArthur Foundation Fellow at the Ohio University Contemporary History Institute and the Department of War Studies at King’s College, London.

197. Portions of this section appeared, in a somewhat different form, as a “Point of View” essay in the *The Chronicle of Higher Education*, July 22, 1992, p. A44.

198. Quoted in the *Wall Street Journal*, August 22, 1991, editorial page.

199. One ought not to judge theories by how well they perform with respect to a single case—unless it is a very big one.

legitimacy, and predictability, set out to embrace the traditional methods of the physical and natural sciences. But they did so at a time when physicists, biologists, and mathematicians, concerned about disparities between their theories and the reality they were supposed to characterize, were abandoning old methods in favor of new ones that accommodated indeterminacy, irregularity, and unpredictability—precisely the qualities the social sciences were trying to leave behind. To put it another way, the “soft” sciences became “harder” just as the “hard” sciences were becoming “softer.”

The old Newtonian vision of a totally deterministic science—one that could not only account for but predict all phenomena—had begun to fade as early as the beginning of the twentieth century: “He could not affirm with confidence, even to himself,” a worried Henry Adams wrote of himself at the time, “that his ‘largest synthesis’ would turn out to be chaos, since he would be equally obliged to deny the chaos.”<sup>200</sup> And yet, Einstein’s physics was already making time, like space, a relative concept; another element of certainty dropped away with Heisenberg’s unsettling discovery, in 1927, that the very act of *observing* certain particles altered them, so that the precise measurement of one characteristic obscured others.<sup>201</sup> By the 1960s, it was becoming apparent that this entanglement of observation with reality extended across a very broad spectrum indeed: two whole classes of phenomena existed, one of which lent itself to prediction, and one that did not. Prediction was possible where one or two variables interacted under known or controlled conditions. But if the number of variables increased even slightly, or if the conditions under which they operated changed even a little, all bets were off. One was into Adams’s feared realm of chaos, and although there is much that one can say about the boundaries and behavior of chaotic systems, one cannot predict the specific actions of their specific parts at any specific time.<sup>202</sup>

The classical scientific method had been to generate laws, and hence predictions, from experiments that limited the number of variables involved and that controlled—sometimes quite arbitrarily—the conditions within which they operated. Newton’s laws of motion, for example, assumed perfectly smooth balls rolling down frictionless inclines with no air resistance, a condition never actually encountered in the real world. Generations of students were taught that feathers and stones fall to earth at the same speed, despite the fact that they never really do. Predictability was achieved by removing the object being studied from its origins and its surroundings: one gained a vision of the future by shutting one’s eyes to the past and the present. But the more one observed past and present, the more Heisenberg’s principle came into play, and the less confidence one could have in the forecasts one made.

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200. *The Education of Henry Adams: An Autobiography* (Boston: Houghton Mifflin, 1961 [first published in 1918]), p. 408. Adams followed carefully the pioneering work of the French mathematician Henri Poincaré, whose early demonstration of how complexity persists regardless of scale, Adams wrote, “promised eternal bliss to the mathematician, but turned the historian green with horror” (p. 455). Newtonian determinism is well summarized in Coveney and Highfield, *The Arrow of Time*, pp. 64–69.

201. *Ibid.*, pp. 125–26.

202. Gleick, *Chaos*, pp. 48, 145.

Theorists of international relations are using the methods of classical science when they conduct their investigations exclusively along a behavioral, structural, or—within the evolutionary approach—a linear or cyclical axis of analysis. They are excluding other variables and controlling conditions in order to produce theories from which they can forecast events. They know that if they do not impose such exclusions and controls, complications will quickly overwhelm their calculations, and predictability will suffer. Exercises of this kind can yield useful insights: so too can simple experiments in freshman physics. But generalizations of this kind perform badly when applied to the real world, which functions along behavioral, structural, and evolutionary axes *simultaneously*.<sup>203</sup> The generation of theory—at least in the traditional scientific method—requires departures from reality; if forecasts derived from theory are to succeed, however, they must also account for reality. That is the paradox that theorists of international relations have been struggling, with such lack of success, to resolve. Theorists in the “hard” sciences gave up on resolving it some time ago.

The “predictability paradox” is not the only difficulty that confronts theorists of international relations, though: they face another that no physicist in a laboratory has ever had to worry about. It has to do with the fact, as Stanley Hoffmann once reminded his colleagues, that human beings are not “gases or pistons.”<sup>204</sup> They are conscious entities capable of reacting to, and often modifying, the variables and conditions they encounter. They can at times see the future taking shape; they can devise, within limits, measures to hasten, retard, or even reverse trends. If molecules had minds of their own, chemists would be much less successful in predicting their behavior.<sup>205</sup> It is no wonder that the effort to devise a “molecular” approach to the study of politics did not work out.<sup>206</sup>

The simple persistence of *values* in politics ought to be another clue that one is dealing here with objects more complicated than billiard balls. Not only does this kind of “input” into political behavior resist expression in scientific terms; it also means that the “scientists” themselves—because they are human—can never be totally objective about what they are studying.<sup>207</sup> A biological scientist must view battles between viruses and antigens with strict impartiality; otherwise his or her career will suffer.<sup>208</sup> Political scientists who fail to achieve that standard survive quite comfortably, which suggests that the science they do is of a rather different character.

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203. For more on this point, see Robert Jervis, “The Future of World Politics: Will It Resemble the Past?” *International Security*, Vol. 16, No. 3 (Winter 1991/92), especially pp. 39–46.

204. Hoffmann, “The Long Road to Theory,” p. 429.

205. Recent evidence suggests that inorganic molecules can have a surprising capacity for self-organization under certain conditions. See, on this point, Coveney and Highfield, *The Arrow of Time*, pp. 35–37, 159–168; also Roger Penrose, *The Emperor’s New Mind: Concerning Computers, Minds, and the Laws of Physics* (New York: Penguin Books, 1991).

206. See John Lewis Gaddis, “Expanding the Data Base: Historians, Political Scientists, and the Enrichment of Security Studies,” *International Security*, Vol. 12, No. 1 (Summer 1987), especially pp. 5–8.

207. For a good discussion of this point, Ferguson and Mansbach, *The Elusive Quest*, pp. 32–48; also Njølstad, “Learning from History?” pp. 222–225.

208. This is a point that several prominent biological scientists have recently discovered.

One might—at least as a thought experiment—construct a model capable of simulating human behavior in all of its complexity, but it would have to be of such complexity itself as to render it indistinguishable from the object being modeled.<sup>209</sup> Even then it might not predict behavior: identical twins do not have identical personalities, and there is no reason to expect that clones would either. In practice, therefore, we “model” human actions by falling back upon the only known simulative technique that successfully integrates the general and the specific, the regular and the irregular, the predictable and the unpredictable: we construct narratives.<sup>210</sup> But that is also what novelists and historians do.

We come, therefore, full circle: the “scientific” approach to the study of international relations appears to work no better, in forecasting the future, than do the old-fashioned methods it set out long ago to replace.<sup>211</sup> Novelists and historians make forecasts all the time, but they do so more by analogy than by scientific theory. They assume, in what seems to social scientists a distressingly imprecise way, that if a particular occurrence is “like” something that has already happened, and if the surrounding circumstances are much the same, then the chances are it will produce a similar result. But then again it may not.<sup>212</sup> The track record for this kind of forecasting is, as one might expect, mixed. For all of its insights into the nature of authoritarianism, George Orwell’s 1948 vision of 1984 could hardly have been less accurate; and the historiographical landscape is now littered with the failed predictions of historians, my own included,<sup>213</sup> who thought that the Soviet Union would never peacefully tolerate its own collapse. “Stranger things have happened,” George F. Kennan commented years ago when discussing this possibility, “though not much stranger.”<sup>214</sup>

209. Pagels, *The Dreams of Reason*, p. 229.

210. Reisch, “Chaos, History, and Narrative,” pp. 17–18. A fine commentary on the value of narratives appears in Richard E. Neustadt and Ernest R. May, *Thinking in Time: The Uses of History for Decision-Makers* (New York: Free Press, 1986), pp. 247–70.

211. “We are arguing for a new research agenda that will explore the evolution, overlapping, and interaction of authority patterns and attendant human loyalties from past to present. Such analysis will necessarily identify dominant and competing patterns, as well as continuities and changes over time, and attempt to explain both the reasons for the patterns observed and the important consequences that have flowed from them.” Yale H. Ferguson and Richard W. Mansbach, “Between Celebration and Despair: Constructive Suggestions for Future International Relations Theory,” *International Studies Quarterly*, Vol. 35, No. 4 (December 1991), p. 382. Perhaps a historian might be pardoned for asking: isn’t that what we have been doing all along?

212. Neustadt and May, *Thinking in Time*, provides the best guide to how to do forecasting by analogy responsibly, but also to how easily things can go wrong. See also Ernest R. May, *“Lessons” of the Past: The Use and Misuse of History in American Foreign Policy* (New York: Oxford University Press, 1973); and Yuen Foong Khong, *Analogies at War: Korea, Munich, Dien Bien Phu, and the Vietnam Decisions of 1965* (Princeton: Princeton University Press, 1992), especially pp. 255–257, which is more pessimistic than Neustadt and May about the possibility of training policymakers to use analogies wisely.

213. See note 192; also a particularly short-sighted commentary of mine in Robert K. German, ed., *The Future of U.S.-USSR Relations: Lessons from Forty Years without World War* (Austin, Tex.: Lyndon B. Johnson School of Public Affairs, 1986), pp. 163–166.

214. George F. Kennan, “America and the Russian Future,” *Foreign Affairs*, Vol. 29 (April 1951), p. 368.

But novelists and historians never advertised their forecasting abilities with the frequency and self-confidence once common among political scientists. Their chief concern was rather to make sense out of the past and, if possible, the present; if in the process they shed a little light in the direction of the future, then so much the better. This does sometimes happen: insights derived from careful narration and thoughtful analogy—not from an excessive deference to a now outmoded scientific method—can illuminate even quite distant futures. Brinton's cycles of revolution were one such example; Kennedy's cycles of great power rise and fall may yet turn out to be another. And consider, as a third example, this observation from the historian James Billington, buried on page 594 of his 1966 book, *The Icon and the Axe*:

That the phantasmagoria of Soviet construction seems to us the most real thing about Soviet history may only be a reflection of our own essentially materialist conception of reality. The Russians, on the other hand, have always been a visionary and ideological people, uniquely appreciative of the ironic perspectives on reality offered in such works as . . . Shakespeare's *Tempest*. It may be that only those who have lived through the tempest of Stalinism will be able, like Prospero, to look on it as "the baseless fabric of a vision"; to see in "the cloud-capped towers, the gorgeous palaces, the solemn temples" only an "insubstantial pageant faded," and to find fresh meaning in Prospero's final affirmation that man is, indeed, "such stuff as dreams are made on."<sup>215</sup>

Or, for that matter, an even earlier insight, also drawn from a literary analogy, that occurs in the most basic of all Cold War texts, Kennan's 1947 article on "The Sources of Soviet Conduct":

Observing that human institutions often show the greatest outward brilliance at a moment when inner decay is in reality farthest advanced, [Thomas Mann] compared the Buddenbrook family, in the days of its greatest glamor, to one of those stars whose light shines most brightly on this world when in reality it has long since ceased to exist. And who can say with assurance that the strong light still cast by the Kremlin on the dissatisfied peoples of the western world is not the powerful afterglow of a constellation which is in actuality on the wane?<sup>216</sup>

These observations hardly qualify as forecasts. They were vague, impressionistic, and would certainly have been maddeningly elusive for anyone trying to pin down exactly what they were anticipating or when it would occur. Still, it is not at all clear that Conquest's student would have been any less well off, in seeking to foresee the events of 1989–91, had she or he avoided the reading of theory altogether, and concentrated instead on the admittedly imprecise and necessarily intuitive insights that can be drawn from well-constructed narratives.

My point, though, is not to suggest that we jettison the scientific approach to the study of international relations; only that we bring it up to date by recognizing that

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215. James H. Billington, *The Icon and the Axe: An Interpretive History of Russian Culture* (New York: Knopf, 1966), p. 594.

216. "X" [George F. Kennan], "The Sources of Soviet Conduct," *Foreign Affairs*, Vol. 25, No. 2 (July 1947), p. 580.

good scientists, like good novelists and good historians, make use of *all* the tools at their disposal in trying to anticipate the future. That includes not just theory, observation, and rigorous calculation, but also narrative, analogy, paradox, irony, intuition, imagination, and—not least in importance—style.<sup>217</sup> If today's physical and natural sciences can benefit from, and even enrich themselves by, a recognition of how imperfectly the old scientific method "modeled" the real world, then surely the social sciences can do the same. We may not gain greater clairvoyance as a result. But we will learn more about the limits of our vision, and hence more about ourselves.

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217. It is interesting to note that some of our best literary stylists these days are "hard" scientists: the names of Stephen Jay Gould, Stephen W. Hawking, Lewis Thomas, Philip Morrison, and the late Heinz Pagels come to mind. How many social scientists write as well, or have as many readers?