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A Normative Turn in Political Science?*

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Traditionally, the scientific study of politics has been associated with a value-neutral approach to the subject. One seeks to uncover what is, not what ought to be, in the political realm. This is what distinguishes a "positive" science from opinionizing, social engineering, or political philosophy. In recent decades, one detects a growing uneasiness with the venerable fact/value dichotomy, at least as it was traditionally understood. It is not clear, however, where this leaves us. (Is the fact/value dichotomy dead?) Against this backdrop, we present the following argument. If political science is to matter to policymakers or citizens, as most political scientists believe it should, authors must be clear about how their subject ties into some broader telos that others might share. Thus, one might fruitfully distinguish three sorts of issues. First, how does a particular subject of political science affect the broader public? (What is its relevance?) Second, how can one demonstrate this relevance empirically? And finally, how might other ways of viewing this issue change the way the "goodness" of the subject is perceived? The first issue is simply a matter of clarification, the second a matter of demonstration, and the third a matter normally reserved for political philosophy. All are necessary components of a relevant and useful political science discipline.
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> Surely, in a world which stands upon the threshold of the chemistry of the atom, which is only beginning to fathom the mystery of interstellar space, in this poor world of ours which, however justifiably proud of its science, has created so little happiness for itself, the tedious minutiae of historical erudition, easily capable of consuming a whole lifetime, would deserve condemnation as an absurd waste of energy, bordering on the criminal, were they to end merely by coating one of our diversions with a thin veneer of truth. Either all minds capable of better employment must be dissuaded from the practice of history, or history must prove its legitimacy as a form of knowledge. But here a new question arises. What is it, exactly, that constitutes the legitimacy of an intellectual endeavor?
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> —Marc Bloch[1]

> An attitude of moral indifference has no connection with scientific "objectivity."
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> —Max Weber[2]

Traditionally, the scientific study of politics has been associated with a value-neutral approach to politics. One seeks to uncover what is, not what ought to be, in the political realm. This is what distinguishes a "positive" science from opinionizing, social engineering, or for that matter from political philosophy. While Plato and Aristotle sought to identify the characteristics of a good polity, most modern political scientists seek to identify the characteristics of polities, their causes and effects, leaving aside moral judgments about their goodness or badness. "A science cannot be a science," writes Levy-Bruhl, "in so far as it is

1. Marc Bloch, *The Historian's Craft* (New York: Vintage Books, [1941] 1953), 9.
2. Max Weber, *The Methodology of the Social Sciences* (New York: Free Press, [1905] 1949), 60.

normative."[3] In the other corner are "normative" theorists, those engaged in a study of the good—without explicit or sustained attention to empirical realities. Thus is the fact/value dichotomy reflected in the disciplinary subdivisions of political science. Empirical research is about facts, while normative theorizing ("political theory") is about values.

The positivistic view of political science seems an apt description of the enterprise at least since the advent of the behavioralist movement in the 1950s. Robert Dahl, in an iconic study of the movement, writes:

> The empirical political scientist is concerned with what *is*. . .not with what *ought* to be. He finds it difficult and uncongenial to assume the historic burden of the political philosopher who attempted to determine, prescribe, elaborate, and employ ethical standards—values, to use the fashionable term—in appraising political acts and political systems. The behaviorally minded student of politics is prepared to *describe* values as empirical data; but, qua "scientist" he seeks to avoid prescription or inquiry into the grounds on which judgments of value can properly be made.[4]

One either studies "democracy" or empirical instances of democracy, but not both. Theorists do not undertake rigorous, methodologically informed empirical study; empiricists do not undertake rigorous, philosophically informed normative study. Not surprisingly, there has been relatively little cross-fertilization between political philosophy and political research. Each cultivates its own garden. Thus, we have, in the words of Harold Lasswell and Abraham Kaplan, "two distinct components in political theory—the empirical propositions of political science and the value judgments of political doctrine."[5]

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3. Quoted in W. Y. Elliott, "The Possibility of a Science of Politics," in *Methods in Social Science*, ed. Stuart A. Rice (Chicago: University of Chicago Press, 1931), 70.

4. Robert A. Dahl, "The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest," *American Political Science Review* 55 (December 1961): 770–71. This perspective is replicated in one of the most widely used methods textbooks in political science and sociology: Earl Babbie, *The Practice of Social Research* (New York: Wadsworth, 1983), 17. See also Austin Ranney, "A Study of Policy Content: A Framework for Choice," in *Political Science and Public Policy,* ed. Ranney (Chicago: Markham Publishers, 1968), 21–22; Albert Somit and Joseph Tanenhaus, *The Development of Political Science: From Burgess to Behavioralism* (Boston: Allyn and Bacon, 1967), 178; David Truman, "The Implications of Political Behavior Research," Social Science Research Council, *Items* (December 1951); Vernon van Dyke, *Political Science* (Stanford: Stanford University Press, 1960), 192.

5. Harold Lasswell and Abraham Kaplan, *Power and Society: A Framework for Political Inquiry* (New Haven: Yale University Press, 1950), xiii. Quoted in Charles Taylor, "Neutrality in Political Science," in *Readings in the Philosophy of Social Science*, ed. Michael Martin and Lee C. McIntyre (Cambridge: MIT Press, 1967), 561. On the attenuation of normative ideals in political science work over the latter part of the 20th century see David Ricci, *The Tragedy of Political Science: Politics, Scholarship and Democracy* (New Haven: Yale University Press, 1984).

In recent decades, one detects a growing uneasiness among social scientists with the fact/value dichotomy, at least as it was traditionally (and rather baldly) understood. Even self-identified positivists have difficulty swallowing the dictums of yesteryear, as quoted above. Interpretivists have always had qualms.[6] It is not clear, however, where this leaves us. Is the fact/value dichotomy dead? Last rites have been administered on numerous occasions. Yet, social scientists continue to insist upon a venerable distinction between arguments that are descriptive and evaluative. If the dichotomy is dead, why do we find this distinction so important? And why does the call for a "positive" social science continue to exert such broad appeal?

This is the subject that the paper attempts to disentangle. As a prologue to this discussion we want to elucidate why the prevailing alienation of "values" and "empirics" might matter for the discipline of political science.[7] The first problem is that of *irrelevance*. A considerable quantity of contemporary work in political science simply does not seem to matter very much. These studies might be methodologically sophisticated, their claims might be true (at standard probability levels), and they may have meaning within the rubric of some theoretical framework or some established empirical question in the discipline. But they do not have any obvious bearing on public policy or on the sorts of things that ordinary citizens might care about. Consider the opening sentence in a recent article by Gary Cox and Octavio Neto: "A classic question in political science concerns what determines the number of parties that compete in a given polity."[8] While this is indeed a classic question, harking back to Duverger, it is not clear that it is a question that policymakers or citizens should be concerned about. Conceivably, the number of parties has an important effect on the quality

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6. For example, Derek Bok, *Beyond the Ivory Tower: Social Responsibilities of the Modern University* (Cambridge: Harvard University Press, 1982); Gary King, Robert O. Keohane, and Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton: Princeton University Press, 1994), 9; Charles E. Lindblom and David K. Cohen, *Usable Knowledge: Social Science and Social Problem Solving* (New Haven: Yale University Press, 1979); George J. McCall and George H. Weber, eds., *Social Science and Public Policy: The Roles of Academic Disciplines in Policy Analysis* (New York: Associated Faculty Press, 1984); Gerardo Munck and Richard Snyder, *Passion, Craft, and Method in Comparative Politics* (Baltimore: Johns Hopkins University Press, 2006); James B. Rule, *Theory and Progress in Social Science* (Cambridge: Cambridge University Press, 1997); Ian Shapiro, Rogers M. Smith, and Tarek E. Masoud, eds., *Problems and Methods in the Study of Politics* (Cambridge: Cambridge University Press, 2004); Rogers M. Smith, "Reconnecting Political Theory to Empirical Inquiry, or, A Return to the Cave?" in *The Evolution of Political Knowledge: Theory and Inquiry in American Politics*, ed. Edward D. Mansfield and Richard Sisson (Columbus: Ohio State University Press, 2003), 60–88; Harold L. Wilensky, "Social Science and the Public Agenda: Reflections of Knowledge to Policy in the United States and Abroad," *Journal of Health Politics, Policy and Law* 22 (October 1997): 1241–265; Mayer Zald, "Sociology as a Discipline: Quasi-Science and Quasi-Humanities," *The American Sociologist* 22 (1990): 3–4, 165–87.

7. The term invokes the title, and arguments, of John G. Gunnell, *Between Philosophy and Politics: The Alienation of Political Theory* (Amherst: University of Massachusetts Press, 1986).

8. Gary W. Cox and Octavio Amorim Neto, "Electoral Institutions, Cleavage Structures and the Number of Parties," *American Journal of Political Science* 41 (1997): 149.

of public policy, political participation, or on other matters that ordinary citizens ought to be concerned about. But this is not a connection made by Cox and Neto (or by other writers who have addressed this question in recent years). Their reference is only to science. Indeed, it is striking that amidst the extraordinary amount of work devoted to the question of *How many parties?* there is a dearth of studies addressing why the size and shape of a party system might matter. (Surely, the number of parties in a party system is not *intrinsically* important.)[9]

At the same time, the ongoing divorce of normative theory and empirical analysis leaves a good deal of work in the Theory camp isolated from things that might matter, here and now. What is the relevance of the "classic questions" of political theory?[10] These may be categorized as follows. (1) What did *A* (Plato, Aristotle, Machiavelli, *et al.*) say about subject *B*? (2) What is the history of concept *B*? (3) What do we mean, or ought we to mean, by concept *B*? This genre of textual exegesis and/or intellectual history has uncertain bearing on what we might think about a subject at the present time, unless of course some new angle or argument is brought to bear that does have relevance for the present time—at which point the work ceases to be intellectual history and commences to be something else (e.g., political philosophy). The latter, in my opinion, is of tremendous importance, but is rarely produced by theorists housed in political science departments. Instead, the self-appointed job of the theorist seems to be to keep track of what the political philosophers have said, or are saying. Explicit normative theorizing about what it would be good to do is relegated to the margins.

A second sort of problem concerns work whose relevance is unclear, either by virtue of being untheorized or uninvestigated. (Arguably, both examples cited above are better classified as examples of ambiguity rather than inconsequence. We leave this to the reader's judgment; it matters little for my argument.) Political-economic work on the question of "veto points" seems to imply that multiple veto points are conducive to good governance.[11] Authors usually focus on the

9. Since we have also contributed to the party-system size literature—without any attention to larger purposes and meanings—we are also implicated in this paragraph's charges. See John Gerring, "Minor Parties in Plurality Electoral Systems," *Party Politics* 11 (2005): 79–107.

10. This issue is nicely addressed by Smith, "Reconnecting Political Theory to Empirical Inquiry," 60–88.

11. There are some exceptions, for example, George Tsebelis, *Veto Players: How Political Institutions Work* (Princeton: Princeton University Press, 2002), 19, but this is the general trend. See Witold J. Henisz, "The Institutional Environment for Economic Growth," *Economics and Politics* 12 (2000): 1–32; Witold J. Henisz, "The Institutional Environment for Infrastructure Investment," (Working paper: The Wharton School, 2000); David A. Lake and Matthew A. Baum, "The Invisible Hand of Democracy: Political Control and the Provision of Public Services," *Comparative Political Studies* 34 (August 2001): 587–621; Douglass C. North and Barry R. Weingast, "Constitutions and Commitment: The Evolution of Institutions Governing Public Choice in Seventeenth-Century England," *Journal of Economic History* 49 (1989): 803–32; Torsten Persson, Gerard Roland, and Guido Tabellini, "Separation of Powers and Political Accountability," *Quarterly Journal of Economics* 112 (1997): 1163–202; Charles M. Tiebout, "A Pure Theory of Local Government Expenditure," *Journal of Political Economy* 64 (1956): 416–24; Barry Weingast, "A Rational

association between constitutionally fragmented institutions and institutional and policy outcomes presumed to be good for the community such as independent central banks or economic growth. A variety of reasons, often supplemented by a mathematical model, is hypothesized to account for this fact. Yet, the broader contours of the argument are dim. If veto points enhance the independence of central banks, and independent central banks are effective in curbing inflation and ensuring long-term growth, do veto points have similarly benign effects on other institutions and other policy outputs—and if so, which ones? These matters remain murky. As such, the normative significance of the implied general theory cannot be tested. We are not told whether veto points should be cultivated, how many, what sort, or under what circumstances veto points might contribute to the greater good. The veto points research agenda draws on normative considerations (that institutions affect public policies and that these public policies have important repercussions) and makes implicit normative conclusions, but neither is made explicit. We are left to wonder. And such wonderment beclouds the field of inquiry.

Many other examples of this sort might be cited. Work on federalism often presumes that federalism is good for democracy and for the quality of governance more generally.[12] However, this assumption is rarely formulated in an explicit and falsifiable form; even more rarely is it subjected to any sort of empirical test. Work on social capital, civil society, voluntary associations, participatory democracy, and other related subjects generally assumes that these phenomena are commendable. However, this assumption is rarely tested. Work on political parties generally assumes that strong parties are good for governance;[13] but again the literature is remarkably quiet when it comes to providing empirical proof for this assumption.

Choice Perspective on the Role of Ideas: Shared Belief Systems and State Sovereignty in International Cooperation," *Politics & Society* 23 (December 1995): 449–64.

12. Samuel H. Beer, *To Make a Nation: The Rediscovery of American Federalism* (Cambridge: Harvard University Press, 1993); Martha Derthick, ed., *Dilemmas of Scale in America's Federal Democracy* (Cambridge: Cambridge University Press, 1998); Daniel J. Elazar, *American Federalism: A View from the States*, 2nd ed. (New York: Crowell, 1972); Daniel J. Elazar, "From Statism to Federalism: A Paradigm Shift," *Publius: The Journal of Federalism* 25 (1995): 5–18; Daniel J. Elazar, "Contrasting Unitary and Federal Systems," *International Political Science Review* 18 (1997): 237–51. See also various issues of *Publius: The Journal of Federalism*. This assumption, it should be noted, is much less marked when the subject is federalism in venues other than the U.S.

13. Francis G. Castles, Franz Lehner, and Manfred G. Schmidt, eds., *The Future of Party Government. Volume III: Managing Mixed Economies* (Berlin: Walter de Gruyter, 1988); Francis G. Castles and Rudolf Wildenmann, eds., *The Future of Party Government. Volume 1: Visions and Realities of Party Government* (Berlin: Walter de Gruyter, 1986); Gary W. Cox and Mathew D. McCubbins, *Legislative Leviathan: Party Government in the House* (Berkeley: University of California Press, 1993); Morris Fiorina, "The Decline of Collective Responsibility in American Politics," *Daedalus* 109 (Summer 1980): 25–45; Richard S. Katz, ed., *The Future of Party Government. Party Governments: European and American Experiences* (Berlin: Walter de Gruyter, 1987); Austin Ranney, *The Doctrine of Responsible Party Government: Its Origins and Present State* (Urbana: University of Illinois, 1962).

As a general rule, we imagine that authors believe that their particular object of study has a broader significance. Veto points matter, federalism matters, social capital matters, parties matter, legislatures matter, and communism mattered. Behind the most abstract or rigorously empirical study can be glimpsed a tinge of social importance. Implicitly, the author's work on this subject will help us to lead happier, freer, more just, or more democratic lives. At the same time, these normative components of work in political science are often difficult to grasp. They are treated fleetingly, or not at all. There is an implicit suggestion that the author's subject affects society, but no explicit statement about how, or to what extent. The reader is left to intuit. This lends the practice of normative theorizing a subterranean cast. It is present but unaccounted for. Robert Dahl remarked recently that "many political scientists today. . .feel uncomfortable linking normative political theory with empirically-grounded social science." This, in his view, is "to the detriment of both sides."[14]

A third sort of problem concerns the sort of normative conclusions that an author brings to bear on a subject. Evidently, the goodness of a policy or institution can be differently conceived. An institution or policy might be justified by a consideration of preferences (expressed opinions), efficiency, public goods, rent-seeking, pareto-optimality, utilitarianism, the difference principle, or a deliberative consideration of the public interest—to name just a few options. One's choice among these moral-philosophical options is highly consequential. An institution might be justified from one perspective, but not another.

For example, it is sometimes assumed that good policies are equivalent to pareto-optimal outcomes, outcomes that improve the situation of some actor or actors without depriving any actor; they are good for someone and bad for no one. Once the principle is stated forthrightly it is apparent that this philosophical position privileges some policies over others. Police enforcement presumably benefits virtually everyone except criminals, while redistributive policies have clear winners and losers. Thus, to assume a Paretian moral perspective is a highly consequential choice, and should at the very least be made explicit.[15]

A second example is the topic of "equality," which can be applied to a wide range of subjects—local, national, and international. It is often assumed among social scientists that inequalities are unjust, an assumption that provides the normative foundation for analyses focusing on the causes of inequality, for

14. Interview with Richard Snyder (Munck and Snyder, *Passion, Craft, and Method in Comparative Politics*).

15. For further discussion of these perspectives in a particular policy context see John Gerring and Strom Thacker, *Good Government: A Centripetal Theory of Democratic Governance* (forthcoming) Appendix A.

example, among different races, sexes, social classes, countries, and so forth. This is a common idiom in economics, political science, and sociology. However, the injustice of inequality is a questionable point of departure. Indeed, few moral philosophers take the position that the unequal possession of some good is intrinsically unjust.[16] Inequalities may be considered unjust if they are shown to be to the detriment of the least advantaged (Rawls's difference principle), to the detriment of society at-large (utilitarianism), or an infraction upon some basic principle of human rights or human needs (according to various rights-based perspectives). But this is quite a different claim. From a difference-principle perspective, for example, an egalitarian policy that leaves the poor (the least advantaged) worse off than they would be under a less egalitarian policy is, ceteris paribus, unjust.

Just as the field of econometrics has been accused of resting upon wobbly stilts (where do those standard errors come from?),[17] so might the broader field of social science be accused of resting upon wobbly normative stilts. What do those "ideals" that we speak so confidently about—for example, democracy, equality, non-discrimination, efficiency—mean? And how important are they (relative to each other and to other normative goals)?

The problem with the fact/value dichotomy and its disciplinary extension (empirics/normative theory) is quite apparent once the subject has been properly broached. Normative theorizing must deal in facts just as empirical work must deal in values; they do not inhabit different worlds. The disciplinary segregation of empirical and normative theory is crippling as well as dishonest since both ventures rest on an implicit understanding of the other's territory. The selection of a topic in empirical political science *presumes* a judgment of moral importance. Insofar as this is true, the author had best make these connections explicit. There is nothing to be gained, and potentially a great deal to be lost, by smuggling in normative assumptions through the back door. Inexplicit normative theorizing is apt to be slipshod and escapes counter-argument, for it rests beneath the surface. Indeed, authors may not be fully aware of their own assumptions.[18]

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16. Philosophers sometimes regard equality as a ceteris paribus good, but this is a much weaker claim than seems to be implied by most social scientists who investigate the topic. For a sampling of views see Matthew Clayton and Andrew Williams, eds., *The Ideal of Equality* (Basingstoke, U.K.: Palgrave, 2000); Meghnad Desai, ed., *On Equality: A Centenary Anthology* (New Brunswick, NJ: Transaction, 1995); Neil Devins and Davison M. Douglas, eds., *Redefining Equality* (Oxford: Oxford University Press, 1997); William Letwin, ed., *Against Equality* (London: Macmillan, 1983); J. Roland Pennock and John W. Chapman, eds., *Nomos IX: Equality* (New York: Atherton, 1967); Louis P. Pojman and Robert Westmoreland, eds., *Equality: Selected Readings* (New York: Oxford University Press, 1997).

17. David A. Freedman, "Statistical Models and Shoe Leather," *Sociological Methodology* 21 (1991): 291–313; Bernhard Kittel, "A Crazy Methodology?: On the Limits of Macroquantitative Social Science Research," (Unpublished manuscript, University of Amsterdam, 2005).

18. For further arguments about the importance of bringing theory and empirics back together again see Richard Ned Lebow, *The Tragic Vision of Politics: Ethics, Interests, and Orders* (Cambridge: Cambridge

But it is not enough simply to clarify. There must also be some logic to the normative assumptions that undergird an empirical investigation. If these assumptions are faulty, or questionable, then so is the study—regardless of how sound its empirics might be.

Against this backdrop, we wish to present the following argument. If political science is to matter to policymakers or citizens, as most political scientists believe it should, authors must be clear about how their subject ties into some broader telos that others might share. In some cases, where an outcome that is generally valued is the immediate topic of concern—for example, economic growth, poverty, peace, genocide—this connection may be obvious. But where it is *not* obvious it should be clarified, empirically and philosophically. Thus, one might fruitfully distinguish three sorts of issues. First, how does a particular subject of political science affect the broader public? (What is its relevance?) Second, how can one demonstrate this relevance empirically? And finally, how might other ways of viewing this issue change the way the "goodness" of this subject is perceived? How well grounded are the philosophical assumptions of a study? The first issue is simply a matter of clarification, the second a matter of demonstration, and the third a matter normally reserved for political philosophy. All are necessary components of a relevant and useful political science discipline.

We do not mean to suggest that these are easy maxims to follow, or that they are beyond challenge. Thus, the rest of this essay will be devoted to clarifying why one might wish to strive for a political science that is normatively oriented, empirical, and philosophically engaged, and what this entails.[19] It is my contention that we cannot conceptualize the scholarly significance of a theoretical framework or a particular empirical puzzle without also contemplating its relevance to society, its normative importance. This underlying feature of social science provides the missing organizing element, without which the activity of social science is, quite literally, meaningless.

University Press, 2003); Bo Rothstein, *Just Institutions Matter: The Moral and Political Logic of the Universal Welfare State* (Cambridge: Cambridge University Press, 1998); Ricci, *The Tragedy of Political Science*; Rule, *Theory and Progress in Social Science*; Smith, "Reconnecting Political Theory to Empirical Inquiry."

19. In this essay, we employ the words *normative*, *value-oriented*, and *evaluative* more or less interchangeably. Each refers to broad social norms—that which is thought to be good or bad by large sections of a community. This should not be confused with disciplinary or methodological norms—Larry Laudan, *Progress and its Problems: Toward a Theory of Scientific Growth* (Berkeley: University of California Press, 1977)—or the force of norms in structuring political life—Martha Finnemore and Kathryn Sikkink, "International Norm Dynamics and Political Change," *International Organization* 52 (Autumn 1998): 887–917; and Jack Snyder, "'Is' and 'Ought': Evaluating Empirical Aspects of Normative Research," in *Progress in International Relations Theory: Appraising the Field*, ed. Colin Elman and Mirium Fendius Elman (Cambridge: MIT Press, 2003): 349–80. We mean, rather, the normative importance of the topic under research. Does X matter to society, or should it matter? The concept of *relevance* (or *utility*) refers to that which might be relevant to the basic norms and values of a community. In most contexts, making a normative argument also implies a *prescriptive* argument. To label something as good or bad usually (but not always) brings in its train a policy or action proposal.

The Relevance of "Relevance"

Stipulatively, the purpose of social science is to help citizens and policymakers better understand the world, with an eye to changing that world. Social science ought to provide useful answers to useful questions. Robert Lynd has made this argument at greater length, and with greater acumen, than anyone else. Social science, he writes, "is not a scholarly arcanum, but an organized part of the culture which exists to help man in continually understanding and rebuilding his culture. And it is the precise character of a culture and the problems it presents as an instrument for furthering men's purposes that should determine the problems and, to some extent, the balance of methods of social science research."[20]

This is not a new idea, of course. Lynd made this argument in 1939, and many others have echoed the sentiment.[21] Indeed, the presumed connection between social science and social progress has been present from the very beginning of the disciplines we now label social science. The Statistical Society of London, one of the first organized attempts to develop the method and employment of statistics, proposed in 1835 to direct their attention to the following question: "What has been the effect of the extension of education on the habits of the People? Have they become more orderly, abstemious, contented, or the reverse?"[22] Whatever one might think about the perspectives embedded in this research question, it is clear that early statisticians were interested in *society*, as well as technique. They were eager subscribers to Karl Marx's famous thesis that

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20. Robert Staughton Lynd, *Knowledge For What?: The Place of Social Science in American Culture* (New York: Grove Press, [1939] 1964), ix.

21. For example, Christian Bay, "Politics and Pseudopolitics: A Critical Evaluation of Some Behavioral Literature," *American Political Science Review* 59 (1965): 39–51; Bloch, *The Historian's Craft*; Bok, *Beyond the Ivory Tower*; David Easton, "The Current Meaning of Behavioralism," in *Approaches to the Study of Politics*, ed. Bernard Susser (New York: Macmillan, [1967] 1992), 47–48; David Easton, "Tenets of Post-Behavioralism" *American Political Science Review* 63 (1969): 1052; Norma Haan, Robert Bellah, Paul Rabinow, and William M. Sullivan, eds., *Social Science as Moral Inquiry* (New York: Columbia University Press, 1983); King, Keohane, and Verba, *Designing Social Inquiry*, 9; Daniel Lerner and Harold D. Lasswell, eds., *The Policy Sciences* (Stanford: Stanford University Press, 1951); Lindblom and Cohen, *Usable Knowledge*; McCall and Weber, *Social Science and Public Policy*; C. Wright Mills, *The Sociological Imagination* (New York: Oxford University Press, 1959); Gunnar Myrdal, *The Challenge of World Poverty: A World Anti-Poverty Program in Outline* (New York: Pantheon, 1970), 258; Rule, *Theory and Progress in Social Science*; Herbert Alexander Simon, "Are Social Problems Problems that Social Science Can Solve?" in *The Social Sciences: Their Nature and Uses*, ed. W. H. Kruskal (Chicago: University of Chicago Press, 1982); Wilensky, "Social Science and the Public Agenda"; Zald, "Sociology as a Discipline." For a different twist on "relevance" see Leo Strauss, *The Rebirth of Classical Political Rationalism: An Introduction to the Thought of Leo Strauss* (Chicago: University of Chicago Press, 1989), ch. 1.

22. Quoted in Stephen P. Turner, '"Net Effects': A Short History," in *Causality in Crisis?: Statistical Methods and the Search for Causal Knowledge in the Social Science*, ed. Vaughn R. McKim and Stephen P. Turner (Notre Dame, IN: Notre Dame Press, 1997): 25–26. Originally quoted in T. M. Porter, *The Rise of Statistical Thinking 1820–1900* (Princeton: Princeton University Press, 1986), 33. See also Randall Collins, *Three Sociological Traditions* (New York: Oxford University Press, 1985), 19.

the point of scholarly reflection is not merely to interpret the world, but also to change it. Patrick Edward Dove, in one of the first textbooks on political science (published in 1850), writes that "*Mere* numbers, however accurately collected, do not form *political* statistics. It is necessary that those numbers should involve *social benefit* or *social detriment*, and that a conclusion relating to such benefit or detriment may be drawn."[23]

Social science, in all its guises, is *practical* knowledge. "Any problem of scientific inquiry that does not grow out of actual (or "practical") social conditions is factitious," John Dewey writes. "All the techniques of observation employed in the advanced sciences may be conformed to, including the use of the best statistical methods to calculate probable errors, etc., and yet the material ascertained be scientifically "dead," that is, irrelevant to a genuine issue, so that concern with it is hardly more than a form of intellectual busy work."[24] If social scientists cannot tell us something useful about the world, then they (we) are serving very little purpose at all.

These statements might also be applied to the work of the natural sciences, whose social utility is readily apparent. Most of the great technological advances of the twentieth century (satellites, fertilizers, computers, pharmaceuticals) have built upon the discoveries of academics working in the sciences. (This was, of course, less true in previous eras.) Yet, the greatest advances of science often do not have immediate practical repercussions for humankind. Were we better off after Einstein's discovery of the theory of relativity, or Watson and Crick's discovery of the double helix structure of DNA? The point here is that natural science sometimes does its best work when *not* focused directly on the interests and needs of lay citizens. Scientists engaged in basic research generally do not ask citizens and policymakers for guidance on what sorts of questions they should be working on. And rightly so. Whatever it is that guides natural scientists, these rules and rewards are sometimes quite removed from everyday concerns and everyday ways of thinking. Laudan writes,

> Much theoretical activity in the sciences, and *most of the best of it*, is not directed at the solution of practical or socially redeeming problems. Even in those cases where deep-level theorizing has eventually had practical spin-off, this has been largely accidental; such fortuitous applications have been neither the motivation for the research nor the general rule. Were we to take seriously the utilitarian approach to science, then a vast re-ordering of

23. Patrick Edward Dove, *The Elements of Political Science. Book II: On Doctrine* (Edinburgh: Johnstone and Hunten, 1850), 220–21.

24. John Dewey, *Logic: The Theory of Inquiry* (New York: Henry Holt, 1938), 499. See also Dewey, *The Public and its Problems* (Athens, GA: Swallow Press, 1954), ch. 5.

> priorities would have to follow, since the present allocation of talent and resources within science manifestly does not reflect likely practical priorities.[25]

Paradoxically, one could also make the same argument about the humanities. It is not clear what guides, or should guide, academics exploring the regions of art, literature, language, music, religion, and philosophy.[26] But it is probably not—or at least only partially—social utility. This is not to say that the humanities are irrelevant. It is to say that they are probably not doing their best work when in hot pursuit of relevance. Indeed, it may be that they are of greater relevance when pursuing truth, beauty, and knowledge in a less interested fashion.

What we are suggesting is that although all segments of the academy can lay claim to social utility, only social science must pursue that goal in a fairly direct and unmediated fashion. *Art for art's sake* has some plausibility, and *science for science's sake* might also be argued in a serious vein. But no serious person would adopt as her thesis *social science for social science's sake*.[27] Social science is science for *society's* sake. These disciplines look to provide answers to questions of pressing concern, or questions that we think should be of pressing concern, to the general public. We look to pursue issues that bear upon our obligations as citizens in a community—issues related, perhaps, to democracy, equality, justice, life-satisfaction, peace, prosperity, violence, or virtue, but in any case, issues that call forth a sense of duty, responsibility, and action.

Among the social sciences, the argument for relevance is particularly strong for political science. To understand this point it is necessary to consider the definition of its primary subject matter, *politics*. Formerly, it was common to equate politics with government or the state. Nowadays, this notion is anachronistic. A narrow conception of politics limited to some "official'" realm seems unduly restrictive. Indeed, popular usage has moved to the other extreme,

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25. Laudan, *Progress and its Problems*, 224–25 (emphasis in original). See also Zald, "Sociology as a Discipline," 175–76. For an extended discussion see Loren R. Graham, *Between Science and Values* (New York: Columbia University Press, 1981).

26. For thoughts on this question, see Andrew Delbanco, "The Decline and Fall of Literature," *New York Review of Books* 46 (November 4, 1999): 32–38; John Ellis, *Literature Lost: Social Agendas and the Corruption of the Humanities* (New Haven: Yale University Press, 1999); Alvin Kernan, ed., *What's Happened to the Humanities?* (Princeton: Princeton University Press, 1997); John Seery, *America Goes to College: Political Theory for the Liberal Arts* (Albany: State University of New York Press, 2002).

27. At a dinner for scientists in Cambridge, someone is supposed to have made the following toast: "To pure mathematics, and may it never be of any use to anybody!" Quoted in Robert K. Merton, *Social Theory and Social Structure* (New York: Free Press, [1949] 1968), 597. It is difficult to imagine the same toast being given at a dinner of *social* scientists. Similarly, Laudan (*Progress and its Problems*, 32) remarks that "certain problems [in the natural sciences] assume a high importance because the National Science Foundation will pay scientists to work on them or, as in the case of cancer research, because there are moral, social, and financial pressures which can "promote" such problems to a higher place than they perhaps cognitively deserve." Again, one cannot imagine somebody arguing against funding a social science problem because, although socially and politically important, it was not "cognitively" so.

equating politics with power, and understanding the latter in all realms of human life, including private and public spheres. Yet, this also seems problematic. Instances of power—define it how you will—are not always instances of politics. The power exercised by a parent over her child is not political (except in certain circumstances). The power exercised by an employer over an employee is not political (except, again, in certain circumstances). So "power" seems too capacious a term to capture what we mean by politics. A third option, building on colloquial usage, defines the parameter of politics through subsidiary notions such as self-interest and duplicity. "Politics" is said to occur when narrowly defined interests govern behavior in a sphere rightly governed by other principles (e.g., merit, virtue). This makes sense of utterances such as "office politics" or "It's all politics." Here, the implicit inference is that rightful duties are being neglected, or merely given lip-service. To be sure, this is not usually what is meant by politics in the context of political science. Indeed, this definition seems to *exclude* many instances that we normally label politics—for example, a candidate who means what she says (rather than something else), a government program that helps people in need (rather than politicians and bureaucrats). We suggest that we regard this colloquial definition as a cynical commentary on politics rather than an alternative definition. (It recalls the exchange between Socrates and Thrasymachus on the meaning of justice, where Thrasymachus charges that justice is nothing but the rule of the strongest and Socrates defends the more traditional notion of justice as giving to each his due.)[28]

Instead, we would argue that the term politics pertains minimally to any human action that is decisional (a choice rather than an inevitability) and that affects a whole community or a substantial portion of that community. Maximally (as an ideal-type), the use of the term politics also implies that such an action is oriented towards, or judged according to, some normative ideal pertaining to the entire community. This is politics. Let us see how it might apply to various realms of human activity.

A public law evidently satisfies all of the foregoing criteria. It is a choice (not an inevitability), it is binding on an entire community, and is justified by some normative principle or principles pertaining to the entire community.[29] Some hypothetical actions are harder to classify. Consider the case of a factory closing. Normally, such an action would not be considered political since it does not affect everyone in a community and may be prompted by economic necessity (e.g., the factory is losing money). However, special circumstances may augur otherwise. If the factory is large relative to the community in question, the effect

28. Hanna Fenichel Pitkin, *Wittgenstein and Justice* (Berkeley: University of California Press, 1972), ch. 8.

29. This is true, we imagine, even in an authoritarian context, where it is no longer sufficient to justify a law by reference to the sovereign's will.

of this action on the community becomes more significant. By virtue of this fact, the hypothetical plant closing is more "political." Much depends, of course, on the way in which community is defined. That which is political in one context may not be political in another. Similarly, if the closing was not inevitable—if the plant owner's decision was *not* governed by economic laws beyond her control—it is also rendered more political, for it could be otherwise. If, finally, the closing of the plant is judged according to some ideal pertaining to the larger community—for example, it is looked upon as an instance of class injustice—it is also, by virtue of this fact, rendered more political. To assert that an otherwise nonpolitical act has importance for a larger community, and to assert that it could be otherwise, is to *politicize* that act.

The point of this definitional exegesis is to show that it is impossible to think about politics without also considering the impact of a subject—whether it be a single action or a patterned set of actions (an institution)—on the commonweal. And it is difficult and perhaps impossible to do this without some attention to normative considerations, that is, what would be good or bad for the community in question. Politics and normative inquiry are intimately conjoined.

This is not to say that political science should be preoccupied only with *policy* relevance. One would be hard-pressed, for example, to uncover a single policy prescription in work by David Brion Davis, Edmund Morgan, and Orlando Patterson on slavery.[30] Yet, arguably, no one ignorant of these writers' work can fully comprehend contemporary social policy debates in the United States. Similarly, while it would be difficult to derive discrete policy implications from work on the American Revolution, the Constitution, the Civil War, and various other historical topics, each of these seems important for understanding where we are today. The point, then, is not that every study should have a policy "lesson," but that every study should reflect upon something that citizens and policymakers care about, or might care about.

Alternatives

We shall presume to have convinced the reader (if she is not already convinced) that political science should be relevant to practical, public concerns—to matters that vitally affect the commonweal. The question then becomes how this goal of relevance should be pursued. We have argued provisionally that the best way to do this is not to abandon science but rather to tether science to normative concerns. This argument may be contrasted with four

30. See David Brion Davis, *The Problem of Slavery in Western Culture* (Oxford: Oxford University Press, 1988); Edmund S. Morgan, *American Slavery/American Freedom: The Ordeal of Colonial Virginia* (New York: W. W. Norton, 1975); Orlando Patterson, *Slavery and Social Death: A Comparative Study* (Cambridge: Harvard University Press, 1982).

alternatives: (1) the abandonment of science in favor of partisan politics, (2) the abandonment of a "positivist" science in favor of an interpretivist approach to the study of politics, (3) the abandonment of the fact/value dichotomy, and hence the disappearance of the problem of value-neutrality, and (4) the abandonment of politics, which might pave the way for a more value-neutral approach to political science. We shall argue that none of these alternative routes is promising.

Alternative #1: Abandon Science for Politics

Arguably, the best way for social science to be relevant to the broader society in which we are situated is to abandon the dictates of "science"—which are, it is usually averred, simply ideological traps—for a more open embrace of partisan politics. If something is right we should fight for it; if something is wrong we should fight against it, with all the weapons we have at our disposal. Justice, not science, should determine our civic and cosmopolitan obligations.[31] Arguably, insofar as social scientists wish to change the world they ought to forsake their dismal sciences so as to engage more directly with the world's problems—for example, organizing protests, endorsing candidates, running for office, writing checks to worthy causes, working in public administration, issuing jeremiads on the state of the world, and so forth.

We must remind ourselves that, however mightily social science strives for social utility, solving problems like racism, poverty, and the spread of AIDS requires much more than good social science. It requires, among other things, good polemic and compelling dramatizations. The cause of civil rights, for example, was advanced more by visual images—of peaceful protesters being sprayed with water cannons and beaten over the head by policemen—than by social science. The sermons of Martin Luther King resounded with greater force than the lengthy and detailed analysis of Gunnar Myrdal's *An American Dilemma*. Beyond rhetoric, social change requires *power*, as the role of social movements, and national political elites during the 1950s and 1960s attests. We do not wish to argue, therefore, for the primacy of social science.

Yet, we must also consider the implied counterfactual. Would the world be a better place if social science were to cease, or if an openly partisan/polemical form of social science were to replace the positivistic orientation that currently presides? It is perhaps obvious from the way that we have posed this question that we have doubts about whether the answer to this question would be affirmative. Granted, some social scientists are good polemicists, and the talents of these individuals are perhaps better employed in more active roles. But for the rest of

31. Howard Zinn, *You Can't Be Neutral on a Moving Train: A Personal History of Our Times* (Boston: Beacon Press, 2002).

us—many of whom must struggle to keep our students awake—the greater good is probably not served by forsaking the academy for the campaign stump, the barricades, or the editorial page.

Whatever its relative impact on policy, politics, and public opinion (a matter that is difficult to determine), the work of social science is probably best carried forth by remaining true to standards proper to its domain.[32] It will not aid citizens and policymakers to have a field of political science indistinguishable from party ideology. If Christopher Jencks, a noted social policy expert, approached problems in the same manner as Edward Kennedy—or Ronald Reagan, for that matter—then we would have no need whatsoever to consult the views of Professor Jencks. What academics like Jencks have to *add* to the political debate is premised on their expertise. And what are the grounds for expertise, if not the practice of good social science? One may disagree about the norms that constitute good social science—none of the criteria advanced in textbooks on methodology are sacrosanct—but it is difficult to deny the necessity of norms. Colloquially stated, there is some utility to good social science, and none at all to bad social science.

Again, we must avoid misleading analogies to natural science, where "expertise" and "science" are easier to define and more generally respected. There will always be contention on these matters within the social sciences, and rightly so. Yet, there remain important differences between social science and party politics, and social science and journalism, which justify the use of the loaded term "science" in the context of serious academic work in anthropology, economics, history, psychology, sociology, and—our present topic—political science. A lecture, Weber points out, "should be different from a 'speech.'"[33] He also says that

> To take a practical political stand is one thing, and to analyze political structures and party positions is another. When speaking in a political meeting about democracy, one does not hide one's personal standpoint; indeed, to come out clearly and take a stand is one's damned duty. The words one uses in such a meeting are not means of scientific analysis but means of canvassing votes and winning over others. They are not plowshares to loosen the soil of contemplative thought; they are swords against the enemies; such words are weapons. It would be an outrage, however, to use words in this fashion in a lecture or in the lecture-room.[34]

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32. See Harry Eckstein, *Regarding Politics: Essays on Political Theory, Stability, and Change* (Berkeley: University of California Press, 1992), chapter 2, who invokes Weber in support of this limited and differentiated role for social science (*vis-a-vis* the public sphere).

33. Weber, *The Methodology of the Social Sciences*, 4.

34. Max Weber, "Science as a Vocation," in *From Max Weber: Essays in Sociology*, ed. Hans Gerth and C. Wright Mills (New York: Oxford University Press, [1918] 1958), 145.

It should also be pointed out that the willful avoidance of "science" has doleful long-term consequences for social science, and for those who would like social science to play a role in the transformation of society. To the extent that social scientists forego systematic analysis in favor of polemic, they compromise the legitimacy of the enterprise of which they are a part and from which they gain their prominence and public attention. As judges walk a fine line between their assigned constitutional roles and their desire to affect public policy, so must social scientists walk a line between methodological rigor and socio-political importance. The day when this line disappears is the day when social science no longer has a calling.

Nor is it clear that an openly polemical prose is always the best way to undertake social advocacy. Albert Hirschman observes of Karl Marx that the apostle of socialism remained firmly in the positivist tradition established by Machiavelli, Montesquieu, and Adam Smith,

> In [his] attempt to interpret and, above all, to change the prevailing social and political order, he consistently refused to appeal to moral argument. He scoffed at the "Utopian socialists" precisely for doing so in their critique of capitalist society and for resorting to moral exhortation in putting forth their proposed remedies. In spite of the ever-present moralistic undertone of his work, Marx's proudest claim was to be the father of "*scientific* socialism." To be truly scientific, he obviously felt that he had to shun moral argument. True science does not preach; it proves and predicts. So he proves the existence of exploitation through the labor theory of value and predicts the eventual demise of capitalism through the law of the falling rate of profit. In effect, Marx mixed uncannily these "cold" scientific propositions with "hot" moral outrage; and it was perhaps this odd amalgam, with all of its inner tensions unresolved, that was (and is) responsible for the extraordinary appeal of his work in an age both addicted to science and starved of moral values.[35]

Whether Marx's avoidance of overt moral commentary in his academic work (e.g., *Das Kapital*) is an appropriate model for us to follow might be doubted. Certainly, it left many questions unexamined and perforce unresolved. But there can be little doubt about the impact of Marx's self-consciously scientific approach. Evidently, a scientific approach does not preclude a work from attaining a broad public—even world-historical—significance.

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35. Albert O. Hirschman, "Morality and the Social Sciences: A Durable Tension," in *Social Science as Moral Inquiry*, ed. Norma Haan, Robert Bellah, Paul Rabinow, and William M. Sullivan (New York: Columbia University Press, 1983), 23.

Alternative #2: Abandon Positivist Science for Interpretation

A second argument is similar to the first in that it envisions an abandonment, or at least the circumscribing, of positivist science. However, rather than (or in addition to) an overtly political guise, this opposition takes the form of proposing a different form of science, one based on interpretation rather than prediction, nomothetic laws, and experimental control. Of concern to us here are not the various methodological arguments for and against interpretivism but rather the ability, or inability, of mainstream positivist science to integrate moral deliberation into political analysis. Richard Rorty finds that the social sciences have "coarsened the moral fiber of our rulers."

> Something happens to politicians who are exposed to endless tabulations of income levels, rates of recidivism, cost-effectiveness of artillery fire, and the like—something like what happens to concentration camp guards. The guards in the Gulag see that nothing matters to the prisoners save bread and pain, so they cease to think of the prisoners in the terms they use for their fellow guards. The rules of the liberal democracies come to think that nothing matters but what shows up in the expert's predictions. They cease to think of their fellow citizens as fellow citizens.[36]

Accordingly, suggests Rorty, we may have to accept the existence of two vocabularies—one for social science and one for moral deliberation. This is consonant with the view of many interpretivists, who are often suspicious of social-science's long-term impact on society.

Yet, Rorty also points out that the use of interpretivist (or any other) language is no assurance that one will think and act in a moral fashion. "The friends of value freedom are doubtless right when they say that any departure from liberal *Zweckrationalität*, any attempt to avoid hard problems of social engineering as raising income levels and lowering recidivism by thinking in "hermeneutical," "textualist" terms, is a temptation to avoid the responsibility to take hold of social forces and use them to alleviate human suffering. They are also right when they say that Nazi and Communist rhetoric depends heavily on replacing liberal jargon with Hegelian Jargon."[37]

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36. Richard Rorty, "Method and Morality," in *Social Science as Moral Inquiry*, ed. Norma Haan, Robert Bellah, Paul Rabinow, and William M. Sullivan (New York: Columbia University Press, 1983), 164. See also Strauss, *The Rebirth of Classical Political Rationalism*; Sheldon Wolin, *The Presence of the Past: Essays on the State and the Constitution* (Baltimore: Johns Hopkins University Press, 1989); essays by Carol Gilligan, Paul Rabinow, Michelle Rosaldo, Reiner Schurmann, William Sullivan, and Robert Bellah in *Social Science as Moral Inquiry*. For a discussion of the various meanings of interpretivism see John Gerring, "Interpretations of Interpretivism," *Qualitative Methods: Newsletter of the American Political Science Association Organized Section on Qualitative Methods* 1 (Fall 2003): 2–6.

37. Rorty, "Method and Morality," 164.

Rorty's point is well taken. In order to integrate moral deliberation with positivist logic we may need to broaden the latter's vocabulary. Political science, in my understanding, is the application of reason to politics—not the application of physics to politics.[38] Political science is a branch of the social sciences, not of the natural sciences. Within this context, we need not—and ought not—recoil from the science moniker, which allows plenty of scope for normative inquiry. We do not think that it is necessary to accept the existence of two vocabularies—or three, or four—as Rorty somewhat pessimistically suggests. To do so would be equivalent to saying that moral and empirical inquiry are incompatible, or even incommensurable. (One can either talk about *what is*, or *what should be*, but never both.) This is of course the crude positivist position, which we have argued against. The difference is that here it is argued from the interpretivist (antipositivist) premise that science is faulty.

Our position, following Weber, is that these two modes of analysis should be distinguished from one another but are also necessarily implicated in one another.[39] To suggest otherwise, to suggest an independence of vocabularies, is to impugn the purpose of moral inquiry, which is (we take it) to enhance our ability to act morally in the world. Acting morally, we assume, presupposes that we understand that world and also, we should add, presupposes some investigation of causal and predictive relationships (in a "positivist" manner). Otherwise, how shall we achieve our moral ends?

Alternative #3: Abandon the Fact/Value Dichotomy

A third argument, closely associated with the second, suggests that the Humean fact/value distinction, upon which the positive/normative distinction is based, is itself illegitimate.[40] There are no empirical facts without normative

38. Gerring, *Social Science Methodology*, chapter 1; Donald McCloskey, *If You're So Smart* (Chicago: University of Chicago Press, 1990), 7.

39. There are many ways to read Weber, and it is not my primary purpose to suggest a reading of his oeuvre. For a more in-depth analysis see Richard W. Miller, "Reason and Commitment in the Social Sciences," *Philosophy and Public Affairs* 8 (Spring 1979): 241–66.

40. G. E. M. Anscombe, "Brute Facts," in *Ethics, Religion and Politics: Philosophical Papers*, *Volume III* (Oxford: Blackwell, 1981); Philippa Foot, "Moral Beliefs." *Proceedings of the Aristotelian Society* 59 (1958–59); Peter Herbst, "The Nature of Facts," in *Essays in Conceptual Analysis*, ed. Antony Flew (London: Macmillan, 1956); Martin Hollis and Steven Lukes, eds., *Rationality and Relativism* (Oxford: Basil Blackwell, 1982); Alasdair MacIntyre, "Is a Science of Comparative Politics Possible?" in *Against the Self-Images of the Age: Essays on Ideology and Philosophy* (London: Duckworth, 1971), 260–79; Alasdair MacIntyre, "Hume on 'Is' and 'Ought,'" in *Against the Self-Images of the Age* (Notre Dame, IN: University of Notre Dame Press, 1978), 109–24; Alasdair MacIntyre, *After Virtue: A Study in Moral Theory* (Notre Dame: University of Notre Dame Press, 1984); Leo Strauss, "Natural Right and the Distinction Between Facts and Values," in *Philosophy of the Social Sciences: A Reader*, ed. Maurice Natanson (New York: Random House, [1953] 1963); Taylor, "Neutrality in Political Science"; Bernard Williams, *Morality: An Introduction to Ethics* (New York: Harper & Row, 1972).

judgments; facts and values are inextricably intertwined. After all, science itself is a value and is regulated by norms. Facts, however dry, gain meaning from a social context and that context is inevitably value-laden. And language, with which we express the facticity of reality, is not morally neutral. Social science is compelled to accept an affectively charged vocabulary as a condition of doing business. Indeed, not everything can or should be stripped of evaluative sentiment. Imagine writing about the Holocaust or slavery in a wholly dispassionate manner. (What would an even-handed treatment of these subjects look like?) Leaving aside such extreme examples, it is difficult to conceive of important work focused on social behavior that does not carry normative freight.

While all of this is true enough, and familiar enough (at least to those who follow current trends in the philosophy of science), it does not follow from this deconstruction of dichotomies that we should forswear the fact/value distinction or the value-neutral goal in science. Language is built upon multiple distinctions, and this particular distinction is no more fluid than most. If we were to decide to jettison all loose distinctions we should quickly find ourselves unable to say anything at all. For all distinctions are, in some sense, matters of degree. Skepticism towards a supposedly hard-and-fast, bright-yellow-line dichotomy is useful as a rejoinder to those who take these things too seriously (the hard-core positivists, if there be any left). But it does not offer a very persuasive argument for dropping the distinction, once we have acknowledged its ambiguities.[41] Indeed, the philosophical debate stemming from John Searle's influential article, upon which the skeptical attitude toward value-neutral social science is (to some extent) based, addresses quite a different question: namely, whether it is *possible* to derive an "is" from an "ought."[42] Our question is somewhat different: namely, whether it is desirable to maintain some sort of distinction between "is" statements and "ought" statements within the language region of social science. We are arguing that it is both possible and desirable to do so.

Note that while the choice of subject matter is almost inevitably value-laden, the *conduct* of research may be undertaken in a value-neutral ("positivist") manner. We do not want to condone biased work, where only those facts pleasing to a theory are discussed, where coding categories pre-judge the result, or where contending theories and points of view are mis-represented. These are high crimes, not misdemeanors, in social science, for they impugn the very idea of

41. Ernest Nagel, "The Value-Oriented Bias of Social Inquiry," in *Readings in the Philosophy of Social Science*, ed. Michael Martin (Cambridge: MIT Press, [1961] 1994); Felix E. Oppenheim, "'Facts' and 'Values' in Politics: Are They Separable?" *Political Theory* 1 (February 1973): 54–68.

42. Searle's article is reprinted in A. G. N. Hudson, ed., *The Is/Ought Question* (London: Macmillan, 1969), which also features a series of articles on the is/ought question.

science (to adopt Peter Winch's stinging rebuke).[43] Weber speaks strongly and passionately to this point, enjoining the social scientist to set

> as his unconditional duty, in every single case, even to the point where it involves the danger of making his lecture less lively or attractive, to make relentlessly clear to this audience, and especially to himself, which of his statements are statements of logically deduced or empirically observed facts and which are statements of practical evaluations. Once one has acknowledged the logical disjunction between the two spheres, it seems to me that the assumption of this attitude is an imperative requirement of intellectual honesty; in this case it is the absolutely minimal requirement.[44]

Nothing could be more damaging to the discipline than an inability, or unwillingness, to distinguish between normative and empirical arguments. My argument is for greater clarity in both, not for doing away with the distinction between the two. Indeed, clarity depends upon maintaining this distinction. Integrating facts and values in the same work does not entail their conflation.

Alternative #4: Abandon Politics

A fourth counter-argument takes quite a different tack. Plausibly, good social science is impugned by a desire to be relevant. Earnestness and commitment may lead the researcher away from the path of truth and towards something else to which he/she retains an *a priori* commitment. Indeed, social science inquiry undertaken in a polemical mode is of little utility, and may cause harm. From this perspective, social scientists should care only for science, and not a whit for social relevance. This is the value-neutral approach often linked to Comte (perhaps unfairly, in light of the latter's diverse, explicit, and highly prescriptive work), and his new term "positivism."

The positivist model is usually associated with natural science, where theoretical and empirical questions are—at least by some accounts—the primary driver of research and relevance a hoped-for by-product. Physics, it is said, does not proceed by contemplating questions of social importance. It proceeds, if it proceeds at all, by contemplating empirical regularities, methodological principles, and theoretical explanations. To be sure, relevance impinges upon scientific research on occasion, particularly in the applied disciplines such as medical research, where the quest for knowledge is often very explicitly a quest

43. Peter Winch, *The Idea of a Social Science, and its Relation to Philosophy* (London: Routledge & Kegan Paul, 1958).

44. Weber, *The Methodology of the Social Sciences*, 2.

to save lives (one thinks, e.g., of medical research on cancer and AIDS). However, natural scientists take most of their cues from the book of nature, not the needs and desires of humankind. Political philosophy is hardly implicated in a study of the cosmos.

Whether or not this is a correct description of the process of natural science might be disputed. But even if it is correct it does not follow that the same model could be applied profitably to the *social* sciences. Decisional behavior, the focus of the social sciences, is quite different from non-decisional behavior. When we study humans as animals we are studying non-decisional behavior, reflexes or otherwise innately programmed behavior. When we study humans as social beings we are reflecting upon human *actions*. This introduces a high level of indeterminacy into our subject matter. Equally important, it raises the problem that studies of human action may affect those actions that we study, thus introducing a circularity to social research that is hardly present in natural science. These considerations suggest that it might not be possible to practice social research by relying solely on the book of nature as our guide. Actions of such an irregular quality are unlikely to cohere in neat and tidy bundles (aka as general theory); moreover, highly abstract theories are unlikely to indicate what the interesting questions are, and therefore where we might profitably focus our research. Arguably, the human sciences are structurally incapable of the sort of paradigmatic unity that characterizes the natural sciences.

This is a classic debate in philosophy of science and we do not pretend to have added anything to the work of philosophers and historians of science on this venerable topic.[45] Moreover, it is a debate that cannot be answered definitively. Conceivably, a unified vision of social science will one day appear, uniting

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45. Among prominent naturalists see Carl G. Hempel, *Aspects of Scientific Explanation: And Other Essays in the Philosophy of Science* (New York: The Free Press, 1965); Harold Kincaid, "Defending Laws in the Social Sciences," in *Readings in the Philosophy of Social Science*, eds. Michael Martin and Lee C. McIntyre (Cambridge: MIT Press, 1994); King, Keohane, and Verba, *Designing Social Inquiry*; Hilary Kornblith, ed., *Naturalizing Epistemology* (Cambridge: MIT Press, 1985); Lee C. McIntyre, *Laws and Explanation in the Social Sciences: Defending a Science of Human Behavior* (Boulder: Westview, 1996); Edward O. Wilson, *Consilience: The Unity of Knowledge* (New York: Alfred A. Knopf, 1998); and earlier work by the so-called "logical positivists"—for example, Alfred Jules Ayer, *Language, Truth and Logic, 2d ed.* (New York: Dover, [1936] 1950); Rudolf Carnap, *The Logical Structure of the World: Pseudoproblems in Philosophy,* trans. Rolf A. George (Berkeley, University of California Press, 1967); Ludwig Wittgenstein, *Tractatus logico-philosophicus*, trans. D. F. Pears and B. F. McGuinness (London: Routledge & Kegan Paul, [1921] 1988).

Critiques of naturalism can be found in William H. Dray, *Laws and Explanations in History* (Oxford: Oxford University Press, 1952); Hans-Georg Gadamer, *Truth and Method*, trans. Garrett Barden and John Cumming (New York: Seabury Press, 1975); David Couzens Hoy, *The Critical Circle: Literature, History, and Philosophical Hermeneutics* (Berkeley: University of California Press, 1982); Maurice Natanson, ed., *Philosophy of the Social Sciences: A Reader* (New York: Random House, 1963); Paul Rabinow and William M. Sullivan, eds., *Interpretive Social Science: A Reader* (Berkeley: University of California Press, 1979); Charles Taylor, *The Explanation of Behavior* (New York: Routledge & Kegan Paul, 1962); Taylor, "Neutrality in Political Science"; Charles Taylor, "The Explanation of Purposive Behavior," in *Explanation*

empirical efforts in each of the social sciences in much the same fashion as the paradigm of natural selection provided the theoretical architecture for the discipline of biology in the nineteenth century. Conceivably, rational choice is such a paradigm. Indeed, the movement from behavioralism to rational choice (and its various incarnations and subfields including public choice, game theory, and political economy) may be understood as a movement toward a more theoretically informed approach to the study of politics. If rational choice is truly unifying, in the sense of offering a single paradigm for the study of politics, then the solution is at hand.

However, even the advocates of this approach rarely make such grand claims.[46] For most practitioners, the point of a formal model is simply to provide the tools (usually mathematical) by which to understand a particular set of empirical relationships. Rational choice models tell us how to analyze something;

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in the Behavioral Sciences, ed. Robert Borger and Frank Cioffi (Cambridge: Cambridge University Press, 1970); Charles Taylor, "Interpretation and the Sciences of Man," in *Philosophy and the Human Sciences: Philosophical Papers*, vol. II (Cambridge: Cambridge University Press, 1985); Winch, *The Idea of a Social Science*.

For arguments pro and con, see William H. Dray, ed., *Philosophical Analysis and History* (New York: Harper & Row, 1966); Michael Martin and Lee C. McIntyre, eds., *Readings in the Philosophy of Social Science* (Cambridge: MIT Press, 1994); Bruce Mazlish, *The Uncertain Sciences* (New Haven: Yale University Press, 1998); Donald Polkinghorne, *Methodology for the Human Sciences: Systems of Inquiry* (Albany: State University of New York Press, 1983); David Thomas, *Naturalism and Social Science: A Post-empiricist Philosophy of Social Science* (Cambridge: Cambridge University Press, 1979). The early history of these debates is covered in Archie Brown, ed., *Political Culture in Communist Studies* (London: Macmillan, 1984); and Rogers M. Smith, *Civic Ideals: Conflicting Visions of Citizenship in U.S. History* (New Haven: Yale University Press, 1997). Sieyes's view, perhaps too simple, is nonetheless pithy: "Nothing can be more correct for the physical scientist than to observe, collect, and examine the relationship between facts. His aim is to understand nature, and since he is not required to assist the plan of the universe with his advice or practical skills (since the physical universe exists and is able to preserve itself independently of his corrective meditation), it is perfectly right for him to limit himself to the examination of facts. Physics can only be the knowledge of *what is*. Art, which is bolder in its horizons, aims to modify and adapt facts to meet the purposes of our needs and enjoyments. It asks *what should be* for the utility of human beings. Art has us as its object. Its speculations, combinations, and operations are for our own use." Emmanuel Joseph Sieyes, *Political Writings* (Indianapolis: Hackett Publishing, 2003), 15.

46. An exception is Gary S. Becker, *The Economic Approach to Human Behavior* (Chicago: University of Chicago Press, 1976). More cautious presentations are found in James S. Coleman, *Foundations of Social Theory* (Cambridge: Harvard University Press, 1990); Jon Elster, ed., *Rational Choice* (New York: New York University Press, 1986); Jon Elster, *Nuts and Bolts for the Social Sciences* (Cambridge: Cambridge University Press, 1989); Dennis C. Mueller, ed., *Perspectives on Public Choice: A Handbook* (Cambridge: Cambridge University Press, 1997); Peter C. Ordeshook, *Game Theory and Political Theory: An Introduction* (Cambridge: Cambridge University Press, 1986). For critical views of rational choice, see Mark Blaug, *Economic Theory in Retrospect* (Cambridge: Cambridge University Press, 1978); Mark Blaug, *The Methodology of Economics, Or How Economists Explain* (Cambridge: Cambridge University Press, 1980); Jeffrey Friedman, ed., *The Rational Choice Controversy: Economic Models of Politics Reconsidered* (New Haven: Yale University Press, 1996); Donald P. Green and Ian Shapiro, *Pathologies of Rational Choice Theory: A Critique of Applications in Political Science* (New Haven: Yale University Press, 1994); Mark Irving Lichbach, *Is Rational Choice Theory All of Social Science?* (Ann Arbor: University of Michigan Press, 2003).

they do not tell us which descriptive, causal, and/or predictive phenomena we should analyze, or why we should be more concerned with one phenomenon than another. Thus rational choice, whatever its merits and demerits, offers no obvious deliverance from what I shall refer to (below) as the empiricist dilemma.

At present, therefore, the sensible conclusion to the naturalist thesis is: *not proven*. And in the absence of such a unifying paradigm the enterprise of social science retains a fundamental openness and contingency. Reality is underdetermined. It is not clear what we should be studying.

The Empiricist Dilemma

The openness of social-scientific endeavor raises a problem that may be summarized as the *empiricist dilemma*. Among the infinite variety of political phenomena available for analysis, which ones should we study? Which descriptive, causal, and predictive inferences merit attention? How, finally, should disparate topics and findings be integrated into a coherent discipline?

Although the empiricist dilemma is by no means a new phenomenon, the affliction seems to have become increasingly acute in recent decades. The contemporary fragmentation of the social sciences offers evidence on this point. While it seems reasonable to view the proliferation of knowledge categories in the natural sciences as a matter of specialization (since this proliferation takes place within a more unified field of inquiry), in the social sciences it is otherwise. Although we are learning more and more, this knowledge-gathering process appears to be about less and less. The increasing sub-division of labor in political science has revealed that beneath the apparent unity of any identifiable subject matter—for example, parties, interest groups, or legislatures—lies an infinite variety of facts and theories. Since these theories are micro-theories (i.e., hypotheses), pertaining to some small subfield or to some small aspect of a larger field, the recourse to theory offers little relief from the inexorable, fragmenting process of theoretically based inquiry. The regress towards finer and finer distinctions is unlimited, and the course of contemporary political science appears to bear this principle out to its logical conclusion.[47]

Consider Anthony Downs's attempted synthesis of the complex subject of *bureaucracy*. In this influential book, Downs enumerates three central hypotheses, 16 laws, 23 propositions concerning the life cycle of bureaus, 16 propositions concerning the internal characteristics common to all bureaus, four limitations

47. Gabriel A. Almond, *A Discipline Divided: Schools and Sects in Political Science* (Newbury Park, CA: Sage, 1990); David Waldner, "Anti Anti-Determinism: Or What Happens When Schrodinger's Cat and Lorenz's Butterfly Meet Laplace's Demon in the Study of Political and Economic Development," Presented to the Annual Meetings of the American Political Science Association, Boston, MA (August–September 2002).

and biases common to all officials, five propositions concerning officials' milieu, motives, and goals, 29 propositions concerning how specific types of officials behave, 18 propositions concerning communications in bureaus, eight propositions concerning the basic nature of control problems in bureaus, nine propositions concerning control processes and devices, four propositions concerning the rigidity cycle, two propositions concerning the basic dynamics of search and change, 11 propositions concerning search problems in bureaus, 20 propositions concerning processes of change, 10 propositions concerning bureau territoriality, 13 propositions concerning goal consensus, recruitment, and indoctrination, seven propositions concerning bureaucratic ideologies, one proposition concerning how ignorance affects the government budgeting process, and three propositions concerning increasing bureaucratization, social efficiency, and individual freedom.[48] Many of these propositions contain subhypotheses, bringing the grand total (including central hypotheses, laws, and propositions) to 349. Evidently, each individual hypothesis might be operationalized in a variety of ways (this is a purely theoretical exercise; Downs does not even pretend to subject his propositions to empirical tests), further complicating the empiricist dilemma. And, if we were to canvas work on bureaucracy appearing in the intervening decades (Downs's book was published in 1967) we would undoubtedly arrive at a much larger set of plausible hypotheses.

Needless to say, the same problem of propositional overload besets all subfields of the discipline, for it is the business of those in the knowledge business to generate new hypotheses! The implicit hope of most practitioners is that they might be contributing to a larger stock of knowledge by pursuing its myriad capillaries. This micro-foundational approach may be productive in the natural sciences. But it has not proven to be similarly productive in the social sciences.

Consider the question from the perspective of the classic debate between *deductive* and *inductive* epistemologies. Deductive approaches purport to be grounded in theory—either broad paradigms such as Marxism, Weberianism, rational choice or subfield-specific theoretical frameworks like realism and idealism (in international relations). Inductive approaches, associated with the behavioral tradition, purport to be grounded in specific research problems as articulated by a field or subfield—for example, Why do states go to war? or Why do states develop social welfare policies? (A third perspective cultivates the middle ground—some combination of deduction and induction—and probably serves as the offhand epistemology of most practitioners.)

Yet, neither the inductive nor the deductive approach (nor, for that matter, the middling approach), is sufficient to explain the organizing principle of social science. Of course, social scientists do employ macro- and micro-theoretical

48. Anthony Downs, *Inside Bureaucracy* (Boston: Little Brown, 1967).

frameworks and these frameworks necessarily influence the way a topic is conceptualized. Equally, the nature of the specific problem that is being investigated also helps to structure the work of social science. However, neither deductive nor inductive epistemologies can fully explain why some theories and some problems are considered *more important* than others.

From the deductivist perspective, it is easy to see that the attractiveness of a theory rests in part on its explanatory potential, as Lakatosians rightly emphasize.[49] However, the "novel facts" that determine the goodness of a theory from a Lakatosian perspective can be of many different sorts. Only some novel facts are truly noteworthy. Others are simply trivial. Insofar as political scientists focus on trivial facts, they lead the discipline down a pleasant (theoretically speaking) garden path to nowhere. Insofar as political scientists focus on intrinsically important facts, they are following a normative telos that is largely unacknowledged.

From the inductivist perspective, we may grant that when considering a single problem—say, the democratic peace—the explanation that is strongest in light of the available evidence will be accorded pride of place, as behavioralists and falsificationists suggest that it should.[50] Even so, not all problems are equally problematic. Some are simply curiosity pieces. Again, we come to the same conclusion. The pursuit of trivial problems is trivial; the pursuit of important problems is important, but unacknowledged by the epistemology adopted by behavioralists and falsificationists (whom we shall treat as part of the same philosophy of science).

Thus, both the deductive and inductive sides of the ongoing epistemological debate in political science have ignored the normative component of social science research. Lakatosians presume that theories can be judged by the scope of their empirical purview (the number of different facts that can be explained by a theory), without reference to the substantive importance of their empirical phenomena. Behavioralists presume that problems generate solutions, without reflecting upon the origins of an empirical problem. What is a problem? What makes a set of facts "problematic," that is, worthy of investigation?

The only way that we can resolve the empiricist dilemma in political science, in my opinion, is by recourse to normative thinking. Amidst the infinite complexity of the empirical world, we can usually distinguish between subjects that matter and those that matter less (or not at all). By focusing resolutely on the former we may recover a greater sense of cohesion in the social sciences. Purpose and unity go together. Weber makes the point nicely. "The problems of

49. Colin Elman and Mirium Fendius Elman, *Progress in International Relations Theory: Appraising the Field* (Cambridge: MIT Press, 2003).

50. Dahl, "The Behavioral Approach in Political Science"; Karl Popper, *The Logic of Scientific Discovery* (New York: Harper & Row, [1934] 1968).

the empirical disciplines are, of course, to be solved 'nonevaluatively'. . .But the problems of the social sciences are selected by the value-relevance of the phenomena treated."[51]

Normative Inquiry in Economics

This argument is brought home by a brief look at political science's principal disciplinary colleague and rival, economics. This field is traditionally regarded as a more "normal" science, one that approaches, even if it does not always achieve, the virtues of natural science. (It is also the origin of that would-be unifying paradigm known as rational choice, which we have already discussed.) Somewhat paradoxically, economists are probably more likely than political scientists to invoke an openly normative perspective on their subject matter. Consider the following rumination offered by Persson and Tabellini in the introduction to their textbook, *Political Economics*:

> The book asks positive questions about policy: we primarily want to know why policy is the way it is, not what it ought to be. Nevertheless, in many instances we contrast the positive predictions against a normative benchmark. Without much discussion, and in the absence of clearly superior alternatives, our normative benchmark throughout the book is a Benthamite utilitarian optimum, namely the policy maximizing the sum of individual utilities. If the analysis has normative implications, these typically concern the design of institutions, such as the structure of budgetary procedures, the degree of central bank independence, or the form of the electoral rule.[52]

What is interesting about this passage is the authors' admission that it is insufficient to take a purely positive approach to political economics. There is simply too much out there, and no Archimedean point by which to orient the data analysis. The formal modeling of political behavior (i.e., after all, a textbook in the application of economic methods to political analysis) does not clarify any particular road to knowledge. Thus, the authors turn sensibly to political philosophy, specifically utilitarianism.

Persson and Tabellini unequivocally unite positive and normative concerns in a way that would be shocking, or at least highly questionable, in the discipline of political science (or for that matter in sociology or anthropology). Economics and political science are quite different in this regard. Normative theory is not an

51. Weber, *The Methodology of the Social Sciences*. Quoted in Martin and McIntyre, *Readings in the Philosophy of Social Science*, 545.

52. Torsten Persson and Guido Tabellini, *Political Economics: Explaining Economic Policy* (Cambridge: MIT Press, 2000), 4.

impoverished backwater of economics; it is, instead, at the forefront of the discipline. In many ways, it exemplifies the highest—purest, most theoretical—stage of modern economics. This has been true of economics for at least a century. In the opening pages of Pigou's mammoth *Wealth and Welfare* (1912), the writer proclaims that his purpose "is not primarily scientific, if by science we intend the single-eyed search after knowledge for its own sake. It is rather practical and utilitarian, concerned chiefly to lay bare such parts of knowledge as may serve, directly or indirectly, to help forward the betterment of social life."[53] Pigou's classic treatise, *The Economics of Welfare*, which also takes welfare as its telos, purports to provide a foundation for economic inquiry. In this respect, Pigou builds on earlier work by Bentham, Dupuit, Edgeworth, Marshall, Mill, Sidgwick and Walras, and presages later work by Arrow, Baumol, Coase, Debreu, Harrod, Hicks, Hotelling, Kaldor, Musgrave, Robbins and Samuelson. Pigou quotes Macaulay's "Essay on History" with approval (though he finds the exposition a trifle shrill):

> No past event has any intrinsic importance. The knowledge of it is valuable, only as it leads us to form just calculations with regard to the future. A history which does not serve this purpose, though it may be filled with battles, treaties and commotions, is as useless as the series of turnpike tickets collected by Sir Matthew Mite.[54]

Comte, the presumed founder of modern social science, is also enlisted: "It is for the heart to suggest our problems; it is for the intellect to solve them. . .The only position for which the intellect is primarily adapted is to be the servant of the social sympathies."[55] For economists, therefore, it has long been recognized that the purpose of social investigation is to enlist empirical and theoretical effort in the service of valued goals.[56]

Plausibly, the frank recognition of relevance as an orienting goal of economics may have much to do with the actual relevance of this discipline in the policymaking world. Other social sciences including anthropology, history, sociology, and political science have been more reticent. The quotations drawn

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53. A. C. Pigou, *Wealth and Welfare, Volume II of A.C. Pigou: Collected Economic Writings* (London: Macmillan, [1912] 1999), 4.

54. A. C. Pigou, *The Economics of Welfare*, 4th ed. (London: Macmillan, [1920] 1938), 4.

55. Comte as quoted in Pigou, *The Economics of Welfare*, 5.

56. For a review of normative (as well as positive) economics work focused on the question of constitutions, see Stefan Voigt, "Positive Constitutional Economics—A Survey," *Public Choice* 90 (March 1997): 11–53. For a general consideration of this question see Daniel M. Hausman and Michael S. McPherson, *Economic Analysis and Moral Philosophy* (Cambridge: Cambridge University Press, 1996), Appendix. See also J. R. Hicks, "The Foundations of Welfare Economics" *Economic Journal* 49 (1939): 696–712.

from Macaulay and Comte, and even Pigou's more moderate formulation, grate harshly on the ears of contemporary practitioners in these fields. The general sense is that we can't, or should not, make normative judgments about political topics. (We speak, as always, of the mainstream adherents of these disciplines. Obviously, there are exceptions.) But where has this reticence taken us? Among other things, it has surely helped to remove us from the halls of political power.

The Empirical Investigation of the Greater Good

Our argument for a more normatively oriented study of politic does not rest solely on the need to enhance the practical import. We are concerned, equally, with the contribution this reorientation might make to theoretical development, productive empirical inquiry, and disciplinary unity. This is, on the face of things, a difficult argument. Conventionally, one considers the entry of normative concerns to be a confusing element—one leading to endless, and essentially irresolvable, questions about ultimate ends. It would appear that the normative turn I am advocating would lead to an *ideologization* of the field, not to greater theoretical and conceptual unity. If we turn from the descriptive (the way politics works) to the normative (the way politics should work) one might suppose that the veneer of civility between liberals and conservatives would quickly dissipate, to be replaced by hard-edged controversies with no obvious resolution.

This is of course a distinct possibility.[57] But it is not an inevitability. To begin with, many moral-philosophical debates lie primarily at a philosophical level. When it comes to specific policy outcomes such as economic growth, racial equality, or human development, there is often greater agreement. These things are desirable, all other things being equal. And they are *measurable*. A glance at the neighboring discipline of economics suggests that the identification of a set of "dependent variables" that are both measurable and of clear normative import is essential to the progress of a discipline. Traditionally, economics has regarded economic growth as such an outcome. There is no reason, in principle, why political science might not identify an array of outcomes (including but not limited to growth) to be approached as dependent variables for an array of causal questions. With this as our empirical fulcrum we might gain greater perspective on many specific debates about the optimal shape of public policy and political-economic institutions. Do strong parties matter? Does social capital matter? Do presidential or parliamentary regimes lead to better outcomes? These are empirical questions. We do not mean to suggest that they are easy to resolve, but

57. Kevin Smith, "Data Don't Matter? Academic Research and School Choice," *Perspectives on Politics* 3 (June 2005), 285–99.

they are certainly worth pondering.[58] While we are accustomed to testing specific public policies for their social effects, there is no reason in principle why we might not also test basic-level political institutions in a similar manner. This sort of endeavor directly addresses what lay citizens and policymakers wish to know, for it addresses the consequential question: what shall we do? What sort of institutions, and what sort of policies, shall we adopt? Which will advance justice and human welfare, and which will not?

In this fashion, a great many ideologically charged issues might be subjected to empirical test. Insofar as this move is successful, it seems reasonable to anticipate a lessening of ideological conflict within the discipline. Indeed, the viability of a normative turn in political science rests to a large extent on the success of this sort of empirical exercise. To recall, what we are calling for is not a rejection of "positive" science but rather its application to normatively valued topics, the application of science to politics.

Granted, the success of this normative/empirical maneuver in economics owes something to the greater moral-philosophical consensus that characterizes that discipline, most of whose members adopt an implicit utilitarian framework. We should not expect—nor, in my opinion, should we desire—such a high level of consensus on basic normative principles and reasoning in political science. Thus, a normative turn in political science is likely to culminate in a degree of moral-philosophical strife. Matters normally kept in the background would now be in the foreground. This sort of debate, it seems to me, is perfectly healthy. If the social impact of a study's findings hinge on one's basic moral commitments—if outcomes would be judged differently by utilitarians and Rawlsians—then this must be considered as more than philosophical baggage. It ought to be featured prominently in a study's introduction or conclusion. This does not mean that it is incumbent upon every empirical study to wage a moral-philosophic war; obviously, if one has nothing new to say on the time-honored debate between Rawls and Mill, then one had best keep one's commentary to a minimum. The same might be said for any subject. At the very least, however, one ought to clarify the connection between an outcome and some judgment of its normative value. In many circumstances, we can imagine that it would be profitable to include a short explanation of how an outcome might be interpreted from various philosophical standpoints. Disagreement, therefore, is not a compelling argument for leaving moral claims by the wayside. If anything, it militates toward a greater and more public engagement.

An example of a fruitful union between moral philosophy and empirical social science can be found in recent work devoted to questions of international public health. Here, researchers looking for ways to measure health cross-nationally and

58. For discussion of these difficulties, see Gerring and Thacker, *Good Government*, ch. 4.

cross-temporally found themselves bumping up against the question of what health *is*. Evidently, in order to measure something it is first necessary to ascertain how that something, the underlying concept of interest, might be defined. Giovanni Sartori's well-chosen words—"concept formation lies prior to quantification"—come to mind.[59] While this is not always true, it is certainly true with respect to concepts of high abstraction such as those identifying important normative goals. In order to measure the health of human populations one must have a reasonably good idea of how diverse societies currently define health (in their own terms) and how they *might* define health (in a situation of full information and extensive deliberation). This has led to a series of collaborative studies among doctors, economists, public health experts, philosophers, and anthropologists, all attempting to determine the best way to define, and measure, this elusive concept. Representative titles include the following (drawn from a recent compilation): "The Concern for Equity in Health" (Sudhir Anand), "Why Health Equity?" (Amartya Sen), "Health and Inequality, or, Why Justice is Good for Our Health" (Norman Daniels, Bruce Kennedy, Ichiro Kawachi), "Health Equity and Social Justice" (Fabienne Peter), "Personal and Social Responsibility for Health" (Daniel Wikler), "Relational Conceptions of Justice: Responsibilities for Health Outcomes" (Thomas Pogge), "Just Health Care in a Pluri-National Country" (Philippe Van Parijs), "Ethical Issues in the Use of Cost Effectiveness Analysis for the Prioritisation of Health Care Resources" (Dan W. Brock), "Deciding Whom to Help, Health-Adjusted Life Years and Disabilities" (Frances Kamm), "The Value of Living Longer" (John Broome), "Ethics and Experience: An Anthropological Approach to Health Equity" (Arthur Klienman).[60]

There is a widespread assumption, carried over from the normative/positive distinction, that normative concerns are somehow beyond argument, or at any rate beyond any sort of argument that could be reasonably (rationally) settled. The positivist's perspective is that empirical disputes are amenable to resolution because they rest upon evidence. Normative disputes, by contrast, rest on subjective values, and are not really open to rational debate. It all comes down to basic moral commitments, about which there is little to say. Hence the identification of objectivity with empirical study and subjectivity with normative claims.

This is much too facile. Political philosophy has very little to do with personal desires, interests, or emotions—except insofar as these subjective elements are to

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59. Giovanni Sartori, "Concept Misformation in Comparative Politics," *American Political Science Review* 64 (December 1970): 1033–46, at 1038.

60. All of the foregoing are chapters in Sudhir Anand, Fabienne Peter and Amartya Sen, eds., *Public Health, Ethics and Equity* (Oxford: Oxford University Press, 2004). See also Christopher J. L. Murray, Joshua A. Salomon, Colin D. Mathers, and Alan D. Lopez, eds., *Summary Measures of Population Health: Concepts, Ethics, Measurement and Applications* (Geneva: World Health Organization, 2002).

be *isolated* from an investigation of right and wrong. Instead, political philosophy deals traditionally with obligations, obligations that flow from circumstances quite independent of our personal positions. Political philosophy thus strives self-consciously for an objective (or universalistic) moral realm.[61] Because it partakes of this objective world, it is open to argument. Political philosophy begins, but does not end, with a statement of faith. Thus, to grant political philosophy greater prominence in political science does not mean that we shall be opening up the field to endless bickering over "values." It means, rather, that we shall be forced to articulate publicly our own views, and that these views may then be subjected to philosophical analysis, as is now being done in the field of public health.

Conclusions

As the field of political science has become, over the past decade, increasingly methodologically self-aware, it is high time for the field to become philosophically self-aware. This should enhance the quality of political science research as well as the relevance of that work to the world beyond the academy.

Edward Hallett Carr, writing on the eve of World War II, presented the issue as a confrontation between realism and utopia.

> In the field of thought, [realism] places its emphasis on the acceptance of facts and on the analysis of their causes and consequences. It tends to depreciate the role of purpose and to maintain, explicitly or implicitly, that the function of thinking is to study a sequence of events which it is powerless to influence or to alter. In the field of action, realism tends to emphasise the irresistible strength of existing tendencies, and to insist that the highest wisdom lies in accepting, and adapting oneself to, these forces and these tendencies. Such an attitude, though advocated in the name of "objective" thought, may no doubt be carried to a point where it results in the sterilization of thought and the negation of action. But there is a stage where realism is the necessary corrective to the exuberance of utopianism, just as in other periods utopianism must be invoked to counteract the barrenness of realism. Immature thought is predominantly purposive and utopian. Thought which

61. Even communitarian, or avowed "relativistic," moral arguments are themselves general ("objective") in character. The writer claims—implicitly—that it would be better for all concerned, or at least for most, if right and wrong are understood within specific spheres rather than through some (Kantian or other) universalist assumptions. See Michael Sandel, *Liberalism and the Limits of Justice* (Cambridge: Cambridge University Press, 1982); Michael Walzer, *Spheres of Justice* (New York: Basic Books, 1984). This itself is of course a universal claim. Indeed, the only way to abjure moral objectivity and universality (we are using these terms more or less interchangeably) is to refuse to engage in any sort of moral philosophy—to say, in effect, "This is what I believe and I offer no general reasons for this belief."

> rejects purpose altogether is the thought of old age. Mature thought combines purpose with observation and analysis. Utopia and reality are thus the two facets of political science. Sound political thought and sound political life will be found only where both have their place.[62]

Of course, it is difficult to say exactly *how* norms should be brought into greater contact with empirical inquiry. As Hirschman observes, "Morality is not something like pollution abatement that can be secured by slightly modifying the design of a policy proposal." Yet, it belongs there nonetheless, and it can get there—to continue Hirschman's train of thought—"only if the social scientists are morally alive and make themselves vulnerable to moral concerns."[63]

Empirical study in the social sciences is meaningless if it has no normative import. It simply does not matter. Empirical study is misleading if its normative import is present, but ambiguous. It matters, or may matter, but we do not know how. Likewise, a normative argument without empirical support may be rhetorically persuasive or logically persuasive, but it will not have demonstrated anything about the world out there. It has no empirical ballast. Good social science must integrate both elements; it must be empirically grounded, and it must be relevant to human concerns.

In this essay we have advocated a normatively informed political science that seeks to subject questions of value to empirical test. Good social science, we have tried to show, involves a marriage of science and social importance—science *and* society. This means a more forthright and self-conscious—and, one hopes, a more sophisticated—embrace of political philosophy, which may be looked upon as providing the ground for empirical endeavor. Having a normative orientation means that a study is about things that matter, or ought to matter, to a broader public. It is relevant. But social relevance need not impugn science. These two elements must be made to work together.

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62. Edward Hallett Carr, *The Twenty Years' Crisis, 1919–1939: An Introduction to the Study of International Relations* (New York: Harper & Row, [1939] 1964), 10.

63. Hirschman, "Morality and the Social Sciences," 31.