During a fellowship year at Princeton I had the pleasure one evening of being seated at a banquet table right next to a famous philosopher. As we ate dinner, the conversation gradually turned to what kinds of readings we assign our graduate students. Eager to show how well educated I was and how much I insisted that my students be educated, too, I informed my conversation partner that I began my graduate core seminars in comparative politics with a three-week tour through the classics of social theory, moving on to the ups and downs of modernization and dependency theory in the post-war United States, and then proceeding to contemporary debates about institutions, rationality, ideas and their impact on modern political life. His response? ‘No one should read anything published more than ten years ago.’

After catching my breath, I tried valiantly to defend the proposition that graduate students should be acquainted with the ‘classics’ in the field so that they know there is a field and will be able to teach it. His response? ‘Why bother? I promise you that in physics or chemistry nobody is reading anything older than ten years.’ But what about in philosophy, what about Plato and Aristotle? I asked. His response: ‘If you are interested in the history of philosophy, fine, but if you are interested in doing philosophy, then you don’t need to read all that old stuff.’ I’m afraid that I did not do a very good job defending my position in favour of reading the ‘classics’ in a given field. He had clearly engaged in this debate many, many times before, and I was ill-equipped both by training and, obviously, by raw intellect to win this battle. What was self-evident, however, is that he was operating with a particular model of knowledge, which postulates that the purpose of inquiry is accumulation and that any pursuit of knowledge over time
that is not cumulative may be enjoyable but it is not to be taken seriously.

My intuition tells me that I am right, that we should be reading the classics, and as a Weberian I think that Weber should be able to provide us with some guidance on the matter. Even so, in what follows, I do not attempt a full defence of the proposition that we should be reading the classics. I simply try to make comprehensible the empirical observation that we do read them. In doing so, I hope to shed light on some ambiguities in Weber's suggestions to the social scientist and the ambiguous legacy of Weber's writings for North American political science.

My points are simple ones and they are twofold. First, the Princeton philosopher's views were far closer to Weber's own stated views on the subject than I had ever appreciated. Ideal-typical scientific work for Weber is slow and cumulative. We can only thank fate, therefore, that Weber did not follow his own advice.

Second, given that we are still reading Weber one hundred years after he composed both his influential methodological and famous substantive essays (especially The Protestant Ethic), it is logical to ask, what makes a classic? Why do some authors and their work get read over and over again, decades, in fact, after their insights have been surpassed or even refuted? What 'causes' a classic? The answer to this question can be approached in a Weberian fashion. On the one hand, Weber's ideal-type of science and the motivations of the ideal-typical practitioner of science would preclude the existence of 'classics.' Nothing old would ever be read. On the other hand, since Weber's ideal-type of science or scientist exists nowhere empirically, the Weberian method suggests that we can look for the causes of real classics in the deviations from the type. With this point in mind, it makes sense to look for the causes of classics in the non-scientific rationalities that co-constitute actual scientific practice. The point here is not simply to use Weber to understand Weber's continued popularity as a phenomenon - though there is nothing wrong with doing so - but rather to inquire about what causes some bodies of work, including Weber's, to be read with fresh eyes by each new generation of scholars. The evidence from political science suggests that the durability of an author's corpus is not only a function of sound scientific method but is also determined by the ethical motivation for the work itself. If methodology constitutes the formal rationality of social science, moral purpose provides the substantive rationality. One without the other renders the enterprise either purely subjective or meaningless.

Weber between Art and Science

Weber's point of departure in 'Science as a Vocation' is much closer to the cumulative model of inquiry than is normally appreciated. After a highly illuminating analysis of the rationalization of German and American academic life that is remarkable for how little has changed since it was written, Weber turns his attention to 'the inward calling' of science. As others have noted, in invoking 'vocation,' Weber's man of science bears remarkable resemblance to the Protestant hero in his study of Western capitalism. The modern scholar works methodically and rationally. Success in science requires above all, Weber tells us, realizing, first, that any significant achievement means specialization. Syntheses across fields are possible but must remain 'highly imperfect.' And whoever lacks the capacity to put on blinders, so to speak, and come up to the idea that the fate of his soul depends upon whether or how he makes the correct conjecture at this passage of this manuscript may as well stay away from science. Weber characterizes the motivation to rational and specialized inquiry as a form of 'intoxication.' Yet even with intoxicated enthusiasm the scholar can never really know whether what he or she has come up with is important or trivial and can never be sure when inspiration will come or what the true sources of inspiration are. For this reason, the scholar can never be certain whether or not he or she will ever distill one important idea in his or her life. In this way, the scholar faces the same sort of uncertainty as the Puritan, though, to be sure, professional obscurity is far more comfortable than eternal damnation.

Weber's emphasis on uncertainty and inspiration leads him to draw the comparison between scientific work and art. Just as with artistic creation, scholarly work requires inspiration. Unlike a work of art, however, which may remain important for hundreds or even thousands of years, modern scientific work 'is chained to the course of progress.' The purpose of art for Weber is 'fulfillment.' The purpose of science, however, is knowledge. Yet knowledge comes only at the price of longevity. In science, each of us knows that what he has accomplished will be antiquated in ten, twenty, or fifty years. That is the fate to which science is subjected; it is the very meaning of scientific work ... Every scientific "fulfillment" raises new "questions"; it asks to be "surpassed" and outdated. Whoever wishes to serve science has to resign himself to this fact.

With these words, Weber appears to place himself squarely in the
camp that views science as a cumulative project. The implications of this view for Weber, and for the rest of us, are potentially profound. Two at least are worth mentioning. First, cumulative knowledge will be specialized knowledge, and specialized knowledge—even if it requires inspiration—will be a slow, incremental, project—to take a phrase from 'Politics as a Vocation,' akin to the 'slow boring of boards.' The purveyor of the broad synthesis will not approximate the scholarly ideal-type as much as the narrower specialist. Second, because all scholarship will be surpassed, it really does not make much sense to speak of a classic in social science, except in a purely historical sense of work that pushed the field forward in either a small or a large way at a specific point in the development of a discipline. So while historians of science may write book after book about Newton and Darwin, physicists and biologists today do not need to read either, and they do not. The same goes for social science. Why bother with Talcott Parsons or Pitirim Sorokin or Paul Lazarsfeld? Or Max Weber, for that matter?

Two pieces of evidence suggest that Weber's ideal-type of science deviated from actual practice, at least in the case of the social sciences. Even if he was right about how science should be practised (the 'ought'), he was dead wrong about how it actually is practised (the 'is'). In the first place, Weber obviously did not practise what he preached. Instead of working with a model of slow accumulation within a well-defined framework, Weber set out to define the framework itself. Even more importantly, however, Weber was the greatest synthesizer of the twentieth century who became best known for his works drawing on multiple disciplines and languages. Although Weber did not take shortcuts in his scholarship, it would be equally wrong to view him as one slowly boring through scholarly boards. His most enduring work remains his comparative studies on the effects of the great world religions on variations in economic ethics and political authority, a project that spanned two thousand years and several language groups. In short, Weber was anything but an intellectual 'organization man.'

Second, if Weber ignored his own advice, subsequent scholars ignored his advice, too. After all, we are still reading Weber's methodological essays one century after they were written, all in the Faustian hope of great scholarly synthesis and historical insight. Furthermore, political scientists continue to read not only Weber but also important scholars from every decade of the twentieth century, and they read him because they believe that the insights they derive from these books and articles make their scholarship better, deeper, more encom-

passing, and therefore of more universal and lasting value than it otherwise would have been. It is the enduring quality of great scholarship in social science that needs to be examined if the deviations from Weber's ideal-typical model of science are to be explained.

Simply put, there are classics in social science. Some books and articles endure. This empirical observation raises the question, why is this the case? The most obvious hypothesis that comes to mind is that in the social sciences we do not in fact work with a cumulative model of knowledge. A rival hypothesis is that we do operate with a cumulative model of knowledge but have made precious little progress in answering any question of social-scientific import. For this reason the questions authors of classics raise are of enduring value and continue to stimulate the scholarly imaginations of subsequent generations. In this view, classics are defined by questions, not by answers or sound scientific procedure. But even these observations, as fundamental as they may sound, still leave open the question of what makes a classic a classic. Why do we read Weber a hundred years after the fact? I have no definitive answer to this question, but several plausible candidates come to mind. In what follows I turn to these questions by way of assessing the role of objectivity in political science.

Objectivity and the Classics of Modern Political Science

If we treat Weber's statement on the rationalization of science from 'Science as a Vocation' as a hypothesis, and if we add onto that the complex treatment of objectivity in his methodological essays, we should expect that in fact there are few enduring classics in the field. Furthermore, following the logic inherent in the Weberian ideal-typical model of science, those few works that do endure should be characterized by exceptionally self-conscious use of concepts and methods, so that scholars may still use them as points of departure in their own work. To what extent is this true? How much attention do the classics of social science devote to questions of method and objectivity in social analysis? In what follows I approach these questions with examples from comparative politics, the subfield of political science whose practitioners have been most influenced by Weber's work.

The consensual classics in comparative politics, such as the work of Reinhard Bendix, Barrington Moore, and Samuel Huntington, all embark upon contextualized comparisons using ideal-typical taxonomies of regime types and carefully selected cases in order to distill carefully
bounded generalizations. On the one hand, all of these works reside intellectually within a well-defined framework, primarily as scholarly responses to Parsonian modernization theory. Although these are ambitious, synthetic works, each of these scholars pushed comparative political history forward by chipping away at the edifice of evolutionary social theory. Bendix's *Nation-Building and Citizenship* and *Kings or People* are both designed to highlight the particular experience of the West compared to other parts of the world, in order to illustrate enduring patterns of political authority. Barrington Moore's *Social Origins of Dictatorship and Democracy* illustrates how different class coalitions in the pre-modern world produce systematically different regime types in the modern world. Samuel Huntington’s *Political Order in Changing Societies* shows how social change may produce as much political strain as it does democracy.

It is important to note, however, that while each of these books is methodologically very self-conscious in its use of concepts, the authors do not devote an inordinate amount of time to definitional issues. Barrington Moore begins his *Social Origins* without any elaborate discussion of the concepts of class or democracy, even though his book is about the relationship between the two. Furthermore, the first three cases in *Social Origins* – England, France, and the United States – all share similar values on their independent and dependent variables. In sum, Moore illustrates the 'same' case – bourgeois dominance leading to representative government – three times before turning to cases where other classes retain the 'lions' share of power. No attempt is made to justify the case selection except to say that these were three powerful and independent states. Not only does Moore eschew any conceptual or methodological discussion at the outset of his work, he also refuses to generalize. Instead he dives directly into the histories of England, France, and the United States, and returns to questions of generalization only after all of the evidence has been presented, at the end of the (long) book.

Moving from Harvard to Berkeley, even Reinhard Bendix, a scholar known much more for his commitment to conceptual clarity than causal explanation, is remembered by his graduate students to have issued injunctions against over-conceptualization and needless theorization: they were summed up in his quip about the Weberian conceptual predisposition by quoting the supposed first line of a German train schedule: *Unter Eisenbahn ist zu verstehen* ... (By train we mean ...) And on theory, when asked by a graduate student about the absence of a

'Theoretical chapter' from Toqueville’s *Ancien Regime*, Bendix is reported to have said, 'Aristocrats do not write long methodological introductions,' meaning, among other things, that the theory was embedded in the narrative. Also in Berkeley, Chalmers Johnson, the scholar who invented the notion of the capitalist developmental state in his path-breaking – and by now classic – book on industrial policy in Japan, was heard more than once to warn young job candidates against 'blowing up an outhouse with a howitzer.'

Weber also viewed methodological reflection in purely instrumental terms, as the means to better empirical research, and not an end in itself. As he notes in his criticism of Meyer (in a rare moment of agreement),

Methodology can only bring us reflective understanding of the means which have demonstrated their value in practice by raising them to the level of explicit consciousness; it is no more the precondition for fruitful intellectual work than the knowledge of anatomy is the precondition for 'correct' walking. Indeed, just as the person who attempted to govern his mode of walking continuously by anatomical knowledge would be in danger of stumbling, so the professional scholar who attempted to determine the aims of his own research extrinsically on the basis of methodological reflections would be in danger of falling into the same difficulties.

For Weber the key point of objectivity is that once a particular 'problem setting' has been discerned, then, and only then, should the analysis of the data be 'an end in itself. It will discontinue assessing the value of individual facts in terms of their relationships to ultimate value-ends. Indeed it will lose its awareness of its ultimate rootedness in the values-ideas in general.' But what kind of practical advice is this? How does one go about ensuring that facts are not assessed in terms of 'ultimate value ideas'? How one accomplishes such a result in comparative historical work is something that Weber does not address specifically in his methodological essays. But he does provide us with important clues. Maintaining objectivity for Weber and the great scholars of postwar comparative politics meant not only conceptual clarity (which obviously mattered to Weber a great deal – otherwise why bother with *Economy and Society* at all?) and maximizing inferential leverage through careful case selection (which Weber also cared deeply about – otherwise why would he have immersed himself in the histories of so many 'exotic' religions and cultures), but it also required cultivating a keen sense of historical irony. The Puritan did not intend to
create capitalism, and the capitalist did not intend to create the iron cage of modernity, but that is what happened. The 'irrationality' of religious belief justified and contributed to the creation of modern capitalist rationality, which in turn created a new kind of irrationality - the 'polar night of icy darkness' of bureaucratic politics and economy.17

A keen sense of irony also informs the classics of postwar comparative politics, because ethically 'good' results (democracy for Moore or stability for Huntington) could often come from ethically 'bad' antecedents (war, repression, violence, Leninism). At the same time, Bendix warned us, movements that cast off the moral restraints of liberalism in the developing world could arise in response to the successful liberalism of the West.

In yet another classic of postwar political science, Ralf Dahrendorf argued in *Society and Democracy in Germany* that Hitler paved the way for postwar German democracy by destroying the pre-modern social order that constituted the foundation of German authoritarianism.18 The painful irony for Dahrendorf is that if the aristocratic opposition to Hitler (which Dahrendorf labels 'counterrevolutionary') had succeeded in one of their assassination attempts, the future of democracy would have been far less certain than in a defeated Nazi Germany.

This sense of irony is to be found at the very core of Weber's thinking on social science and is summed up in his notion of 'unintended consequences.' Irony is what permits the social scientist to distance him- or herself from the moral valence of root causes being examined or macro-political outcomes being accounted for and to evaluate both in a dispassionate way. One reason that we continue to read the classics in social science is that they provide us with object lessons in the use of irony for obtaining scholarly objectivity.

So it appears at first glance that Weber's injunctions about objectivity have informed much of modern political science and account to a significant degree for why classics become classics. Each of the books mentioned above deployed concepts self-consciously and carefully; each is meticulous in its selection of cases (Moore, in fact, to the point where he chose the Asian cases of Japan and China for illustrating Fascism and Communism rather than the German and Russian cases, which he knew far better); each is able to achieve scholarly distance from causes or outcomes that may be potentially displeasing.

Yet this is only part of the story, for at the end of the day, each of these books has been subject, more or less, to empirical attack, and yet each of them continues to be widely read. Weber's work on the relationship of Protestantism to capitalism has withstood over a century of withering empirical attack. The same holds for the decades of criticism endured by Moore's *Social Origins*. After almost forty years, there is hardly anything left in Barrington Moore's book that has not been criticized, repeatedly. His selective use of evidence and historiography in particular stands out. Indeed, when rereading the volume, one is struck by the footnotes and how the same sources are cited over and over again, suggesting to even the non-specialist that there were certain books that 'helped' Moore make his case. In an important essay on the use of historiography in comparative politics, Ian Lustick goes so far as to say that Moore was guilty of selection bias in his use of historiography.19 Each major issue in the historiography of France or Japan or China contains a great debate, and Moore generally accepts the side in the debate that supports his interpretive needs. This same criticism, as Lustick notes, could be mounted against virtually every major work in comparative politics in the postwar era, and against Weber's own 'The Spirit of Capitalism' for that matter. Note that the point here is not that there are empirical errors in great works but that most authors of the 'classics' in comparative politics do not easily pass a strict version of an 'objectivity' test.

Why then, if not on grounds of scholarly objectivity, do these books continue to be read? I suggest they continue to be read not only because of their methodological rigour but also because of the moral valence of the questions they pose and the answers they provide. It is important to recall that Weber had nothing against the motivation for empirical research deriving from normative concerns. Weber merely held that competing values could not be adjudicated by rational inquiry. Empirical inquiry could inform us about the costs and benefits of pursuing conflicting values in society, but it could not adjudicate which values were the right ones. But once this point was understood, the scholar was free to allow his or her values to motivate the study of a subject.

Let us once again consider Moore in this regard. Moore's volume is actually about many things. In the context of modernization theory it was understood as a book about the multiple paths into the modern world. As a critique of modernization theory, however, it is also an impassioned plea for the importance of violence in each society's pathway from traditional to modern society. It is worth recalling that the book's first chapter, on England, carries the subtitle 'The Contribution of Violence to Gradualism.' The subsequent chapter on France high-
lights the importance of the terror in ensuring a decisive break with the pre-modern order. And, in the following chapter, Moore’s much neglected case study of the United States, the Civil War is portrayed as the ‘real’ American revolution without which the United States would have been left with a society divided between a highly industrialized North and a slave owning ‘Junker’-like South much more akin to the Kaiserreich than to England. In short, the road to democracy, or any viable institutional order for Moore, was paved with blood, lots of blood. In the context of when it was published, in 1966, it is easy to see the implication for Moore’s American readers: who were they to deny the Viennese their own revolution, when it appeared that the precondition of their own modernization was something, if not identical to the carnage of the American Civil War, then at least in the same family of political events? Moore’s sense of irony allowed his book to be both ‘objective’ (in that there was at least some group of historians who would agree with each and every one of his interpretive points, even if none would agree on all) and morally charged at the same time. And it was this combination that helped transform the book from a comparative study of lord and peasant in the modern world to a classic of comparative politics. Similar observations could be made about virtually any book in modern social science that acquires the status of a ‘classic.’

Of course, as is so often the case with Weber, rereading Weber’s essay on objectivity in the social sciences illustrates that Weber understood this point very well. Indeed, within Weber’s own work the evidence is quite convincing that he believed objective analysis could occur only after the substantive end of the inquiry, the value orientation of the scholar, had been clearly identified. As he states in “Objectivity,” “These viewpoints are necessary in order to engage in an empirical science of concrete reality which seeks to understand the cultural significance of individual events in their contemporary manifestations and the causes of their being historically so and not otherwise.”

This much is fairly easy to accept. Ultimate ends motivate objective analysis in a two-stage process where the first (the motivation) does not contaminate the second (the analysis). But even Weber was uncertain whether the two could so easily be quarantined from each other. In “Objectivity” in Social Science,’ after many pages of dense prose, in the deservedly famous final paragraph Weber begins by reiterating the importance of the fact–value distinction for practical research by imploring us to remember that once the purpose of the study has been established, we can ‘discontinue assessing the value of individual facts in terms of their relationships to ultimate value-ideas.’ He ends the thought with a semi-biblical ‘And it is well that it should be so.’ At that point, however, Weber provides us with the most important caveat in his thinking on social science methodology.

But there comes a moment when the atmosphere changes. The significance of the unreflectively utilized viewpoints becomes uncertain and the road is lost in the twilight. The light of the great cultural problems moves on. Then too science prepares to change its standpoint and its analytical apparatus and to view the streams of events from the heights of thought. It follows those stars which alone are able to give meaning and direction to its labors.22

These lines have been read any number of ways by students of Weber’s thought. In terms of this essay, however, they suggest that Weber was deeply conflicted about a cumulative model of knowledge for the social sciences and about his ideal-type of scientific inquiry. Whereas objectivity required a strict separation of fact from value in the course of inquiry, inquiry itself and external circumstance itself could undermine the very distinction between the two by rendering our concepts meaningless. At that point, we need to review and renew our conceptual apparatus in light of concerns that seem more relevant. That relevance, as determined by the underlying moral motivation and significance of the work, is what makes a classic a classic and accounts, to some extent at any rate, for why we continue to read them.

In this respect, it is useful to recall that the classics of ancient Greece themselves went unread for over one thousand years. Only when circumstances seemed to give them renewed meaning did Renaissance Europeans turn to them. They read Aristotle and Plato because these thinkers spoke to the problems of the age. This suggests that, contrary to the cumulative model of knowledge, with which Weber obviously did not completely agree and was in any case undercut by his understanding that external circumstances may render concepts irrelevant, in the social sciences we return to books written long ago not because they deal with human problems that are eternal – that there are few such problems accounts for the fact that all scholarship appears at some point in time to be obsolete – but because they address problems that eternally recur. For the social scientist, the concerns that Weber raised and addressed in his methodological essays recur time and again, and for that reason his work on the subject is still profitably read.
NOTES

3 Ibid.
4 Ibid.
5 Ibid., 13.
6 Ibid.
7 This is admittedly only one way of reading this passage, albeit one that if accurate contains profound implications for Weber’s work as a whole. Alternatively, one could argue that the cited passages from Science as a Vocation should be read in a Popperian mode as a discussion of the ‘logic of discovery’ in science, rather than a discussion of the problem of refuting or verifying theories. Without engaging in a lengthy defence of the proposition that Weber supports a ‘cumulative growth’ model of science, it is enough at this point simply to acknowledge the plausibility of alternative readings of a frequently ambiguous text.
12 ‘Does not the exclusion of the smaller Western democratic states produce a certain antipeasant bias throughout the whole book? To this objection there is, I think, an impersonal answer. This study concentrates on certain important stages in a prolonged social process which has worked itself out in several countries. As part of this process new social arrangements have grown up ... which have made certain countries political leaders at different points in time during the first half of the twentieth century ... The fact that the smaller countries depend economically and politically on big and powerful ones means that the decisive causes of their politics lie outside their own boundaries. Therefore a general statement about the historical preconditions of democracy or authoritarianism covering small countries as well as large would very likely be so broad as to be abstractly platitudinous.’ Moore, Social Origins, xiii.
13 I am grateful to Laurence McFalls for pointing out an alternative reading of this quip. It is possibly a commentary on the importance of social position in the production of classics. It is true that a disproportionate number of great works have been produced by wealthy outsiders who make their intellectual homes only at the edges of the academy.
16 Ibid., 112.
20 A further attribute of classics is one that does not easily fit into this framework, yet surely counts as one of the non-scientific rationalities that is co-constitutive of actual scientific practice. I refer here to the esthetic quality of a given author’s work. It is no accident that many of the classics of social science, such as Moore’s Social Origins and Huntington’s Political Order, are beautifully written. Weber himself noted Ranke’s artistic genius even as he complained that the historian offered neither original ideas nor new facts. Where the logic of the esthetic appeal in the social science breaks down completely and ironically, is in the case of Weber’s own writings. Even his most well-known works, such as The Protestant Ethic and Politics as a Vocation, are essentially impenetrable.
21 Weber, Methodology, 96.
22 Ibid., 112.