Introduction: Progress: Formal and substantive

Anyone who delves deeply into the literatures of theoretical social science must eventually sense that the reach of our disciplines exceeds their grasp.

On the one hand, the concerns that impel people to reflect on social life have unmistakable validity. People are moved to understand the dynamics of war and peace. People yearn to comprehend the causes of economic growth and stagnation. People want to grasp the processes by which human personality is formed. People seek to comprehend the forms taken by social inequality, and their relations to other social conditions and arrangements. People want to understand the origins of deviance, or of the causes of civil upheaval, or of the changes in family structure now sweeping the world. And on and on.

There is nothing mysterious about the reasons for such concerns. They arise directly from the realization that the forces and processes implicated in them *matter* for widely shared human interests. People have no choice but to act in response to some assessments of these forces or remain at their mercy. They are things that people *need to know about* in any reflective effort to make the most of social life. Academic social science may sometimes turn away from such concerns. But the result is only to leave them to others to ponder.

Of course, professional social scientists often *do* strive to address such issues in their work. The problem is that the resulting formulations rarely seem as forceful, or as enduring, as the original concerns. The specialist literatures offer many points of departure for thinking about these questions. But what social scientist would dare to propose a list of succinct, persuasive theoretical answers to them – answers that could be claimed to command wide assent among informed observers?

I scarcely mean that present-day social scientists, individually, have nothing to say about such important issues. The situation is more the opposite: a cacophony of rich but often mutually antipathetic responses. Indeed, on politically charged theoretical issues like those cited above, the doctrines of social scientists often sound suspiciously like abstracted versions of the conflicting prejudices of nonspecialists.

What is worse, nonspecialists who turn to current social science literatures for insight on questions like those noted above are apt to feel that their original concerns have been lost in the intellectual shuffle. State-of-the-art discussion on these and other specialities is often so arcane as to mystify outsiders. And disappointment will be all the more acute should the uninitiated reader stray into the domain of "pure theory" – studies of the basic logic of human capital theory, or rational choice thinking, or network analysis, hermeneutics, ethnomethodology, or any other generic way of knowing social reality. Here one enters a world of theoretical obsessions whose relevance to the concerns of outsiders is apt to seem utterly obscure.

Thus, a tension that forms the central theme of this book. On the one hand, we have an array of questions and concerns whose moral and intellectual legitimacy is hard to miss. On the other, we have specialist literatures whose "answers" rarely seem altogether satisfying in relation to the questions. Indeed, it often seems that what counts as an answer – or as a reasonable effort in that direction – is highly context-dependent. That is, what makes any line of inquiry appear as a promising approach to basic issues – from social stratification, to international conflict, to personality formation – is apt to vary from moment to historical moment, and from one intellectual constituency to another. Such evident transience makes it appear that the theoretical imaginations of social scientists are governed by intellectual tastes far less enduring than the questions they address.

Let us be fair to social science. It is hard to imagine any systematic social inquiry that does not involve theoretical *programs* – extended strategies of inquiry oriented, perhaps quite indirectly, to long-term goals of enlightenment. Such programs, of the utmost interest for this book, range from agendas for the study of specific phenomena to grand designs for scientific inquiry. The trouble is that one can note so many more optimistic departures in these programs than successful arrivals, in the sense of settled conclusions to perennial questions. En route, our programs of inquiry often seem to turn in upon themselves. Instead of registering what any conscientious observer might recognize as *progress* in understanding civil upheaval – or declining productivity, or the origins of deviance, or any number of other widespread concerns – programs of inquiry often devolve into obsessions with issues of concern only to those indoctrinated to the program in question.

To be sure, other disciplines foster intramural debates no less arcane than those pursued by social scientists. Most of us would consider it no more than natural to find communication among specialists in molecular biology or systems analysis or seismology to some degree opaque to the uninitiated.

But there is a difference. In many fields, arcane theoretical debates are ultimately constrained by inputs from empirical inquiry. A space satellite is sent aloft and attains (or fails to attain) its expected orbit; a vaccine is developed that successfully creates immunity in a previously vulnerable population; or (we may someday hope) seismologists learn to predict earthquakes with accuracy. Such outcomes may uphold one theoretical position or another, no matter how obscure the debates en route may have been.

In the study of social life, few programs of inquiry can claim such vindication. Although social science produces many "findings," one must strain to identify what could legitimately be called social science "discoveries," or empirical observations by any name that decisively settle theoretical controversies. Such decisive results require a measure of agreement on the *significance* of empirical observations that appears much scarcer in our disciplines than in natural science.

All of this is hardly for want of attempts to invent, and reinvent, durable structures for interpreting empirical material. On the contrary, social science could shame Detroit with the regularity of its claims for theoretical "breakthroughs," "new syntheses," "reorientations," and, above all, "revolutions." But the very rapidity of such changes illustrates a key contention of this work – that ways of interpreting and ascribing significance to empirical material are enormously vulnerable to the shifting winds of theoretical fashion. Thus the empirical "findings" that strike professional social scientists as being full of significance at one point may presently appear as nonfindings or even embarrassments to those who follow. Without a more stable theoretical context, the interest they hold is as volatile as the price of speculative issues on a stock exchange.

Consider the succession of theoretical visions that have preoccupied social science in the English-speaking world in the second half of the twentieth century: structural-functionalism; behaviorism in its many forms; network analysis; game theory; symbolic interactionism; and countless varieties of Marxism, "structuralism," and hermeneutics, to note just a few of the more prominent. And within the subdisciplines focusing on organizations, international conflict, development economics, public policy, religion, political upheaval, deviance, the family, or the like, one could identify many other theoretical twists, most of them equally short-lived.

Contemplating the passing array of such innovations, one cannot shake the impression that they reflect nothing other than a constantly varying intellectual *taste*. And insofar as such transitoriness represents the *only* pattern shaping theoretical change in our disciplines, compelling answers to enduring human concerns about social, political, or economic life would appear to be a utopian prospect. The long-term results of our work might then be better characterized in terms reserved for pure fashion: always changing but never improving.

True, proponents of each new theoretical wrinkle are apt to claim that their favored vision will be different. *Our* new framework, they will insist, finally captures the fundamental realities of social life. It at last focuses on the "core concepts," "basic processes," "deep structures," or the like that represent the royal road to any and all meaningful understanding. Thus it finally puts our enterprise on the proper analytical footing and, by so doing, sets the stage for authentic, enduring progress.

Such claims increasingly generate a sense of déjà vu. The half-life of new theoretical projects in social science, it would appear, is considerably shorter than that of volatile radioactive substances. The "progress" that they achieve, it becomes increasingly clear, often registers as such only from within the worldview that theoretical enthusiasts create for themselves. Once the social context supporting that vision shifts, one suspects, the "fundamental" status of its concepts or findings, the progressive lustre of its accomplishments, are bound to fade.

And in light of such transience, is anyone safe in imagining that the theoretical preoccupations of today – including one's own – will prove more enduring than earlier ones? Can we reasonably expect that the next fifty years of our intellectual history will describe clearer lines of intellectual progress than the last? Or will future developments in our disciplines simply resemble what we are familiar with to date: a succession of short-lived visions, each satisfying a specific and ephemeral theoretical taste?

What *would* constitute authentic intellectual progress, then? Obviously, any understanding of this slippery notion has to identify, not so much a quality inherent in any particular idea, but rather a *relationship* among ideas. What marks any idea as progressive, in other words, is something about where it leaves us in relation to where we started. For the purposes of this book, an idea embodies progress when it can be shown to be a necessary stepping-stone to understandings of value to subsequent analysts. Ideas may be progressive, in this view, even where those whose thinking depends on them are unaware of their role. The notion that life-forms do not generate spontaneously – as flies were once thought to do from decaying flesh – may not be a salient concern to life scientists today. Nevertheless, I would count the rejection of the spontaneous-generation model a progressive step in the history of today's life sciences; for that rejection helped to constitute necessary premises for subsequent lines of thought.

Were this the only criterion of progressive status, however, nearly every program of inquiry could claim to exhibit it. For every such program develops its own intellectual agenda – including its own standards of accomplishment and strategies for pursuing such accomplishments. And every intellectual program succeeds, at least to a degree, in pursuing its own agenda; each can point to accomplishments registered strictly in its own terms. That much can be claimed as readily for now abandoned and apparently irrelevant intellectual systems – say, Scholastic philosophy or Stalinist economics – as for the flourishing intellectual traditions underlying theory and practice, say, in today's life sciences.

Thus it is essential to distinguish between *formal* and *substantive* progress. Every theoretical system in our disciplines registers formal progress, simply by pursuing those intellectual directions that it sets for itself. The question is, do these strictly "local" accomplishments matter in any way to the concerns of the broad public of "outsiders" to the theory? The ability to make such a difference amounts to what I term *substantive progress* – the development of analytical tools that subsequent thinkers "cannot afford to do without," regardless of their identification (or lack of it) with the theoretical program that gave rise to them.

In focusing on this distinction between formal and substantive progress, I am embracing a distinctive (and anything but uncontroversial) theoretical position. No view of theoretical success or failure, I hold, makes sense without some vision of the ultimate interests guiding inquiry. By my lights, those interests have to be identified with the challenges and strains of social living itself – with action dilemmas experienced by the widest potential intellectual constituencies. These concerns, which include those noted at the beginning of this Introduction, are at once theoretical and practical. When understandings arise that provide a better grip in dealing with such issues, it is no exaggeration to say that they leave the overall state of social understanding improved. To say the least, not all formal accomplishments registered within programs of inquiry can lay claim to such status. But when such broadly shared analytical interests are served, one can speak of substantive progress.

Judging substantive progress is obviously an enormously interpretive business. I do not for a moment pretend that even the most scrupulous observers could easily agree on specific instances. But the principle that I invoke should nevertheless be clear. Expansion of the fund of insights that the widest constituencies of analysts *need to know* amounts to substantive progress.

To expect this much of any idea – any generalization, any finding, any analytical concept or strategy, and so on – may seem like a tall order. It is. Yet one can point to cases where it is amply fulfilled – though more readily outside of social science than within. Ideas on the role played by tectonic plates in the production of earthquakes, for example, are foundational for seismology today – including both "pure" theory and the practical mobilization of theory, as in efforts to predict earthquakes. It may well be that today's working seismologists have only the foggiest idea of the intellectual context out of which prevailing theories arose. Perhaps, for example, the alternative formulations that had to be rejected in order to embrace these ideas have long since been forgotten by today's researchers. But today's concepts appear to be things that any analyst *needs to know* who wants to understand and deal with earthquakes and related movements of the earth's crust.

Is it possible to identify such substantively progressive ideas in social science? Possible, I think, but anything but easy. A key problem is the difficulty of agreeing on what constitutes what "any analyst" would or would not "need to know" about any subject. One reason for these difficulties is the volatility of the intellectual sex appeal surrounding the formal claims of programs of inquiry as they flash across the intellectual stage. For a time, enthusiasts of every ascendant mind-set are apt to tout their distinctive analytical achievements as "steps ahead" sub specie aeternitatis. The question is, which of such claims can legitimately promise to retain their force in any longer historical assessment? Judgments of such staying power are a key aim of this work.

In the attempt to make such judgments, I seek to approach each theoretical program considered here from the standpoint of a theoretical "outsider" – someone who begins with no special stake in the short-term appeal that momentarily surrounds every ascendant theory. And in doing so, I try to make my language reflect the attitude I adopt. When I invoke the editorial "we," I mean to express the view of a distinterested theoretical "outsider." For each analytical accomplishment claimed by enthusiasts of any particular theoretical program, I want to ask "What's in it for us?" – with the "us" understood as referring to the broadest community of those seeking to understand and deal with social, economic, and political life.

My concern here is with *theoretical* knowledge, as distinct from the *techniques* of inquiry. The distinction is crucial for this book. In the strictly technical aspects of our work – indeed, as in the technical side of art, music, and literature – progress is unmistakable. By almost any standard, today's means for assembling, organizing, and analyzing relevant information are far more effective than those of earlier periods – indeed, even than those of a decade ago.

By technical improvements, I mean much more than just increased sophistication in computerized data management and statistical analysis, important as these things are. I also have in mind such things as compilations of comparative ethnographic data, techniques for transcribing and analyzing conversations, methods of content analysis, and a host of other ways of bringing analytical attention to bear on relevant data.

These successes count to the enduring credit of our disciplines. They demonstrate both the intellectual virtuosity and the practical utility of social science. But technique is not theory. Today we have statistical and survey techniques far superior to Emile Durkheim's for analyzing such things as suicide, crime, and divorce. But definitive judgment of Durkheim's broad doctrines – say, on the relationship between moral authority and deviance – is more elusive. I do not mean by this that Durkheim's position is forgotten, or should be; on the contrary, it has shown far more staying power than most theoretical doctrines in our discipline. I simply mean that informed thinkers continue to disagree as to how right Durkheim was about key theoretical points – for example, the relative importance of moral authority versus other forces in ensuring compliance with normative standards.

When I speak of theoretical work, I mean analytical ideas of applicability extending beyond any single case: not just the causes of a single strike, but those of a wave of strikes or a category of similar strikes; not just an account of the social forces underlying Hitler's rise to power, but an analysis of shifts from pluralist to extremist politics in a variety of settings. I have in mind both representations of particular slices of empirical material that are given in theoretical terms – for example, comparative analyses of suicide rates – and also the conceptual and strategic rationales that frame such investigations. All such heterogeneous intellectual productions form part of our effort to make theoretical sense of the social world.

Strictly one-of-a-kind, idiographic investigations unquestionably and legitimately command our interest. The effort to understand the chain of events that brought Hitler to power, after all, has a claim on our imaginations quite independent of any parallels to similar processes elsewhere. Moreover, many utterly nontheoretical investigations lead to insight of much practical value – for example, by charting the spread of an infectious disease or showing how to reach voters susceptible to particular electoral appeals.

But few students of social life, I suspect, can altogether resist what one might call the *theoretical yearning* – the temptation to draw from analyses of specific situations implications for the understanding of others. What does an analysis of the transmission of AIDS in a particular population suggest about the spread of the same disease elsewhere or about that of other diseases altogether? Like many another yearning, the appetite for theoretical inquiry is hard to suppress, once aroused. Most of us cannot resist the temptation to consider what implications processes observed in one setting may have for understanding material from other domains.

Note something distinctive about theoretical as distinct from strictly idiographic work. The intellectual appeal of theoretical work depends enormously on the promise of the larger, unrealized structure of enlightenment that it implies. The perceived virtues of theoretical work, in other words, lie not just in the light it sheds on a particular case but also in our assessment of the larger intellectual enterprise which it supposedly helps to further. It depends, in other words, on shared perceptions of where our broader enterprise is going – and of where it has been.

Thus the intense interest surrounding work successfully portrayed as "paving the way" for new and compelling forms of enlightenment – work construed as "breaking new ground" or "opening the path" to "new vistas" of theoretical understanding. The first functionalist account of urban graft, or the first feminist account of the rise of modern science, or the first network account of job markets thus generate keen excitement. The attraction stems not just from what such studies have to say about the specific materials reported in them but from the broad sense of intellectual direction that they convey. Even work that strikes outsiders as utterly arcane or obscure – and sometimes especially work of this kind – may assume intense theoretical interest, if only its enthusiasts see it in this light.

And thus the extraordinary premium, in theoretical work, placed on the proclamation of "core concepts," "basic processes," "deep structures," and the like. Any theoretical view whose proponents succeed in convincing the scholarly public that it focuses attention on issues somehow logically or strategically *prior* to other concerns is bound to reap great success.

The trouble with all this is hardly that such "deep structures" are not there to be discerned, but that the study of social life admits of so many of them. What appear as the most fundamental of considerations at one intellectual moment, or to one intellectual constituency, may appear as irrelevancies and distractions elsewhere. The conviction that a particular insight represents an indispensable step in some ordained progression of expanding enlightenment is an indispensable ingredient in the constitution of theoretical fashion. But it is hard to point to many such convictions that have endured.

Or, to put matters a little differently: The possibilities of theoretical abstraction in our fields are infinite. There is simply no logical limit to the theoretical agendas that could conceivably serve to animate our work – and, in so doing, form bases for judgments of strictly formal progress. The question is, which of such formal achievements will impress the intellectual public, at any one moment, as consequential or worthy of attention? The difference between compelling, widely adopted theoretical programs and others lies in the ability to command a sense of *meaningfulness* – to convince intellectual "consumers" that the aspects of social life on which they focus are ones that matter, that deserve our attention. And much of this ability turns on our perception of the directions of theoretical movement or progress. Hence the vast energies devoted to portraying each bit of theoretical work as an essential step in some far-reaching process of progressive enlightenment.

Some readers will no doubt find this judgment excessively harsh. But how else are we to account for certain unmistakable features of theoretical communication in our disciplines? Everyone recognizes the standard incantations, at the beginnings of books and articles, invoking supposedly unimpeachable sources of theoretical meaning. In sociology, these claims are apt to take the form of insistence that the intellectual problems one is addressing in fact go back to Marx, Weber, and/or Durkheim. Other disciplines will invoke their own totems of theoretical authenticity – from Adam Smith or Schumpeter to Machiavelli, Murdoch, Aristotle, or Burke. And in concluding our works, of course, we reemphasize the theoretical "centrality" of the questions to which we have sought to "contribute." We insist that "more research is necessary" to illuminate these questions fully – thus inviting the sort of continued attention that would imply that our own contribution represents a step ahead in some common pursuit. If the directions of intellectual movement were indeed self-evident, such breast-beating would hardly be necessary.

Thus the key preoccupation of this book: To what extent do the often transitory preoccupations of theoretical social science generate insights with the potential to outlive the context of their origins? When, if ever, do the accomplishments registered by theoretical programs in their own terms include insights potentially constituting substantive progress? Do professional social scientists today in any meaningful sense know *more* than their intellectual ancestors a generation or a century ago? Is it accordingly reasonable for present-day social scientists to seek – as I suspect we all do – to create in their work a "contribution" to some relatively enduring, larger structure of enlightenment?

I believe that such questions are even more important than we generally acknowledge. If one's aim is to create insight whose value can be reckoned only from within a single theoretical project, the irrelevance of the results to "outsiders" should be of no concern. But for anyone with more far-reaching aspirations for his or her work, it becomes necessary to raise the pressing question: Why should anyone on the outside *care* about these intellectual exertions? What reason is there to believe that any particular insight from any particular program will matter to future thinkers, once the short-term glamour of the program has worn off? What prospect is there that any such insight might achieve the status of *reliable means* to *enduring analytical ends* – and by that token constitute an authentic contribution to social science wisdom, a manifestation of substantive progress? Such questions, taken seriously, impose a rigorous constraint on any approach to social inquiry.

Can we identify any theoretical insights from the social science literature that meet such demanding criteria? Questions of this kind, it seems to me, trigger self-deprecating chuckles in social scientists' off-the-record discussions. The very discomfort evidenced by such reactions, I suspect, may account for the scarcity of systematic attention to the issues involved.

Yet there is really no need to shy away from these questions. Indeed, there are certain advantages to posing them at this stage in our intellectual history. For by now we have seen enough theoretical doctrines come and go to grow wary of the more extravagant claims made for their enduring accomplishments. We ought to be able to mine our own meandering intellectual history for insights into the long-term prospects for theoretical understanding of social life.

In pursuing such aims, I want to avoid focusing exclusively on the formal claims of theoretical doctrines – claims to identify the unique and indispensable "deep structures" or "core concepts" of the subject matter, for example. Such claims typically amount to exhortations on the value of particular forms of insight over others. Often it is asserted that one or another set of such assumptions is the only viable long-term basis for successful elaboration of social understanding – which is to say, the only hope of authentic intellectual progress.

For the purposes of this book, I start with quite a different assumption. Any number of conceptual systems or research strategies, I want to argue, could in principle serve to organize the work of social inquiry – and produce results that embody what I have termed formal progress. The question is, for any particular theoretical program, *is there any reason to believe that the insights so generated would long engage the theoretical yearnings of social scientists?* In short, have we any reason to believe that the knowledge yielded by any theoretical approach will continue to serve the analytical needs of future generations?

To answer such questions, one must concentrate on what might be called the *intellectual ecology* of theoretical work – that is, the empirical realities of shifting theoretical taste. What is it about particular doctrines, at particular historical moments, that makes them seem so compelling? And what has changed when (often only a little later) they appear to have lost all meaning? To what extent are the salient theoretical "victories" of any particular approach meaningful only in the terms of the doctrine