CONTENTS

Preface ix

CHAPTER I Analytic Tools for Social and Political Research 1

CHAPTER II A General Frame: Social Action 27

CHAPTER III Knowledge 40

CHAPTER IV Norms 64

CHAPTER V Preferences 87

CHAPTER VI Emotions 107

CHAPTER VII “The Human Group” Revisited 123

CHAPTER VIII Midpoint 135

CHAPTER IX Aggregations 152

CHAPTER X Collective Action 168

CHAPTER XI Power and Cooperation 183

CHAPTER XII Institutions 204

CHAPTER XIII Social Identities 228

CHAPTER XIV Macrocontexts 243

CHAPTER XV Cultural Explanations 265

CHAPTER XVI Conclusion: Usable Theory? 286

References 301

Index 325
This volume is built on a few basic premises. A first set concerns the state of social and political theory. The social sciences are not rich in full-scale empirical theories—or even tested complex hypotheses—that are ready to be taken off the shelf and put to satisfactory use in explanation or prediction. We do, however, have a significant and growing stock of focused theory frames, which offer conceptualizations of problems as well as reasoned identifications of the factors likely to be of causal relevance. In contrast to fully developed theories, these frames do not contain ordered sets of empirical hypotheses capable of explanation or prediction; nor do they allow the derivation of such hypotheses. But they can guide the creation of theoretical hypotheses in the course of social and political research, propositions that are suitable for making sense of the empirical evidence. Focused theory frames are the most important of the analytic tools discussed in this volume.

The second set of premises helped shape the substance of the book. I opted for action theory as a framework for the ordering and integration of the analytic tools considered. This was a pragmatic choice. It was a tentative, suspendable, and reversible option, certainly not a commitment to the ontological presuppositions of different meta theories and even less with them. However, I chose to take a stab at recovering action theory before adopting it as an integrative baseline for the discussion of varied analytic tools.

Action theory, I argue, has to be "recovered" in three ways. It has to be freed from the abstract complexities that engulfed it in Parsons’s work. It has to be enriched through recognition of the importance of the “internal” dimensions of action—the importance of different degrees of knowledge and ignorance, of variations in preferences, of contrasts in normative orientations, and of the role of emotions. This recognition could build on the models of action developed by Weber, Mead, and Parsons, as well as on more recent sophisticated versions of rational choice theory. Finally, action theory has to be oriented toward causal arguments. It has to be saved from the radical skepticism about explanatory theory that for many follows from the complexities of the subjective space of action, a skepticism that seems further confirmed by the sparse theoretical accomplishments of political science and sociology.

CONCLUSION: USABLE THEORY?

Why are theoretical propositions, capable of explanation, difficult to come by in the social sciences? The outlines of an answer are well known. Experimental isolation of causal effects is rarely feasible. At the same time, human motivation and behavior seem shaped by an endless number of factors, some blocking or counterbalancing others, some possibly substituting for each other as they bring about similar effects.

The internal dimensions of action induce skepticism above all because this subjective space is not easily understood with any certainty. Even introspection has to deal with ambivalence, confusion, and obsfuscation. Moreover, the subjective side of action requires in any case interpretation as well as rendition into the analysts’ language; and much can get lost in such translations. Paradoxically, skepticism about delving into the subjective space of action also arises precisely because so many of its elusive features seem to have great causative power—think of various forms of ignorance, the fear or the readiness to break norms others respect, or acting in heated emotion.

In addition, “the facts of the situation” often become relevant only as they are filtered through subjective understandings. That means that the same “objective” givens are in their effects dependent on the understanding, desires, and normative views that people in varied positions bring to them. This can limit subtly—yet in its effects drastically—the conditions under which a given hypothesis holds, under which for instance we can claim that personal loyalties and aversions can be controlled in an organization by bureaucratic rules and sanctions. That is obvious if a hypothesis extends across very different historical and cultural contexts; but similar differences within our own world may have the same effect, constraining the validity of our theoretical propositions or leaving their scope uncertain.

1 Consider this judgment of Douglass North (1990, 111): “There is nothing the matter with the rational actor paradigm that could not be cured by a healthy awareness of the complexity of human motivation and the problems that arise from information processing. Social scientists would then understand not only why institutions exist, but also how they influence outcomes” (cited by Moe 2005, 223).

2 Jim Mahoney related this point—familiar since the French moralists, Nietzsche, and Freud—to the facts of biological evolution: “Human consciousness only dimly perceives the sources of action. Our minds evolved not to understand why we act the way we do. Rather, the stories our consciousness tells us about why we act in certain ways may have very little to do with the real reasons why we act in certain ways” (personal communication).
The complexity and variability of the subjective dimensions of action are a major challenge to theoretical explanation and prediction. Yet their variability is, as we have seen, reduced by the fact that this internal space of action is to some extent molded by broader social conditions. This makes cognition, preferences, normative orientations, emotions, and their effects more predictable. And so do the relatively stable social structures, small and large, into which cognition, norms, and preferences congeal as the result of sustained interaction and symbolic mediation.

Yet a similar variability reappears, if to a lesser extent, on the levels of meso- and macrostructures and -processes. If, for instance, close-knit groups with their power both to constrain and to enable are significant causal agents, it is clear that their form and the specific mechanisms they activate vary considerably in detail. The same is true of states, to add an example of an even more complex composite causal agent. Causal mechanisms grounded in groups or states take on a great variety of forms, and they vary in the conditions that activate them as well as in the outcomes they create. That means that in exploring specific causal outcomes, we have to pay close attention to the exact features of the face-to-face groups and the states in question as well as to the contexts in which they operate. And for the same reason we have to take care not to extend the scope conditions of our hypotheses loosely.

It is this individual and social variability and the different ways in which such variation is constrained that explain why focused theory frames are as helpful as they are. Focused theory frames combine clearly formulated research problems with arguments about which broad factors are likely to be causally relevant and which kinds of causal processes may account for expected outcomes. As these theoretical ideas are not sufficiently specific to predict outcomes or to explain results empirically identified, theory frames fall short of the explanatory and predictive power expected of ideal theories. Yet they can advance further hypothesis development. Their grounding in comparable past research and its interpretation not only may suggest explanatory ideas; it also can increase our confidence that a causal interpretation of numerical associations or historical sequences makes sense and does not mistakenly seize on spurious correlations and strings of unrelated events.

This leads to a second conclusion: we can often come to more specific explanations if we are ready to develop new, sufficiently specific explanatory hypotheses in the course of empirical research. It is particularly to that undertaking that this volume offers analytic tools. For instance, workers' groups may resist or be quite willing to cooperate with management. Past theory frames lead us to expect that the groups' effect on cooperation will be the stronger, the more cohesive a group. Yet the very direction of this group effect is likely to depend on such conditions as the overall relations of trust and mistrust between management and employees, on the particular relations between workers and lower management, or on job security and the role of competition among group members.

What can such theory work in the course of local research rely on? There is likely to be the initial theory frame, built on a review of relevant past research and theory. The causal factors it points to can guide research but do not yield specific predictions. If it offers causal mechanism hypotheses, these may derive from similar research projects; but they gain special power if they have been used in explanations of a variety of substantively different empirical patterns. Small group research, to keep with the example chosen, has yielded a number of causal mechanism ideas regarding the constraining and enabling of members in a large variety of settings. The role of other factors, potentially significant in the research site at hand, must be assessed with multiple observations in the particular setting and, if possible, in other, comparable sites as well. That may include assessments of the relative strengths of factors working in opposite directions, such as the effects of competition among workers in an environment of high unemployment vs. the strength of group coherence. In the famous Hawthorne experiments, the combination of these two mechanisms may have led to strong, group-enforced output restrictions (Roethlisberger and Dickson 1939).

The bottom line of this brief restatement of the difficulties and the prospects of social and political theory development is clear and fairly simple: causal explanation, based on general (though not unbounded!) theoretical statements, is a difficult but not an unattainable goal. It is critically facilitated by the continuous improvement of focused theory frames, and by the willingness and ability of researchers to engage in theory building "on the ground," in the course of empirical research.

Analytic Routines and Pitfalls

If single-minded behaviorism, which shut out any consideration of subjective reasoning and motivation, has failed, and if crude attributions of insight and motives across all areas of social life are clearly inadequate, exploring the subjective dimension of action seems a promising strategy. The quartet of chapters discussing this internal space of action made it clear that the complexities of knowledge and ignorance, of normative orientations, of variations in preferences, and of emotional response do not preclude causal analysis. Not only are their results helpful in interpreting empirical findings; they yield hypotheses about the causal relevance of cognition, preferences, norms, and emotions, both in their own right
tics from the actions of the collectivity. For many issues, this procedure simply ignores the better part of the social and political analysis of collectivities. At the same time, I contrasted this fallacy of simplistic aggregation by an observer with institutionally grounded aggregations (such as the aggregation of many expressions of preference through the market mechanism or the political results of voting outcomes) that play a critical role in the life of large collectivities.

Finally, a few comments are due on the desirable form of analytic achievements. While I am not at all prepared to give up on the goals of integrated empirical theories capable of explanation and prediction (though perhaps in limited and partial form only), I believe that the better part of the analytic accomplishments of sociology and political science consists of focused theory frames. Though these frames fall short in explanatory and predictive power, they share a number of important features (see chapter 1):

- building on past research, they identify intriguing and important research problems as well as major factors of causal relevance;
- they often come in interrelated clusters illuminating common concerns;
- they can guide further theory development in the course of empirical research, and they are judged by how useful they are in such theory-building endeavors;
- in particular, they offer relevant context for hypothesis development;
- they provide an analytic background guiding the interpretation of research findings;
- they indicate directions for plausible theoretical generalization from empirical findings; and
- they are open to revision in light of research results.

As they focus on causally relevant factors for a well-defined problem, theory frames are good places to assemble causal mechanism hypotheses. Ultimately there is no satisfactory explanation unless we can offer causal mechanisms that account for an outcome.

Causal mechanisms come in quite different forms, with some arising from interpretation of frequency correlations, others emerging from the study of diverse, qualitatively identified event sequences that suggest causation. Causal mechanism hypotheses may focus on large or minute consequences and are often more or less isolated from each other. Often they will explain only one aspect of an outcome and remain in this sense partial. Thus strong group support may explain why people hold on to unpopular convictions; but it may not explain the substance of those convictions. Nevertheless, the systematic pursuit of causal mechanisms holds great promise, not least for a closer integration between empirical research and theoretical analysis.

Even if we look toward focused theory frames and causal mechanisms as the more accomplished and desirable analytic tools, it is quite important that we not denigrate lesser achievements. Thus many of us are familiar with more or less improvised lists of factors that seem causally relevant for outcomes of interest. Such preliminary estimates are, of course, the raw material out of which—after the inspection of diverse evidence about possible causal sequences and frequency associations—causal mechanisms and well-reasoned theory frames are formulated.

Another important analytic achievement consists of the identification of common or even universal problems that find different solutions in different social contexts. This was a central objective of the comparative political sociology of Reinhard Bendix (1964, 1978). We encountered similar issues when discussing the social control of expertise in different societal contexts (see chapter 3). Othet's have made this a central objective in comparative research on education (Schriewer 1999).

Ideally, the problems identified in such research turn out to be "generative problems," suggesting further and further questions as contrasting solutions are being pursued in different contexts. The fundamental problem of the social control of expertise is such a generative problem (see chapter 3). It derives from the asymmetry of knowledge between professionals and their clients in conjunction with the fact that their relation involves matters of great interest to the clients as well as to the wider community. But the control of expertise will take very different forms in different professions. The initial problem formulation suggests that it is likely to vary with the composition of their clientele, its knowledge and power resources, with the moral relevance the community ascribes to a profession's work, with the predictability of the results of experts' efforts, and even with the availability of empirical evidence about the success or failure of their work and advice. These and similar differences make different modes of control more or less effective—control by the market, control by state regulation and supervision, collective self-control by a profession's organization, and control of employed experts by other employed experts. A generative problem of this kind leaves us with different sequences of questions, each of which illuminates a variant of the original issue, using a set of interrelated critical variables.

A last example from a virtually inexhaustible list of "minor" analytic accomplishments is the identification of conceptual equivalences across different social and cultural contexts. It involves the careful translation of indigenous descriptions of various practices—for instance, of varieties of legal or medical work—into a common conceptual language. This achievement requires painstaking comparative research. It obviously links up with
the need for distinctive concept formation suitable for specific research questions and preliminary hypotheses. Searching for conceptual equivalences may also lead back to the detection of widespread problems and contrasting responses to them, as well as to the identification of factors of causal relevance in focused theory frames. But the construction of conceptual equivalences is a worthwhile effort in itself. Leaving these questions unresolved constitutes one reason why detailed comparison across countries with different institutions is so difficult. It may also explain why sociological analysis too often either remains confined to one's own country or resorts to a more abstract discussion of societies in general.

**Pragmatic Rules of Thumb**

Throughout this volume, I have offered some pragmatic suggestions for theory construction, complementing the reflections on useful analytic elements of theory, whether they are routine or not, helpful tools, or warnings about pitfalls. I begin this brief review with the need to unlock the implicit theoretical knowledge without which we would not be able to conduct our lives as well as we do. All intelligent actors in social life are in effect social theorists with a great deal of analytic knowledge. This, on the one hand, complicates the task of theoretical generalization because all social behavior is knowledge-dependent and thus variable across social positions and historical contexts. But it also means that within a given sociocultural context and knowledge horizon people know a great deal about how social life works. This knowledge typically remains implicit, as illustrated by the fact that we use the grammar of our language correctly even if we are unable to write down its rules; but it can often be made available.

A first and fairly obvious tack of making implicit knowledge explicit is to focus on puzzles in social patterns or event sequences with which one is thoroughly familiar. A number of possible explanations will, in the light of that familiarity, be quickly disposed of as inadequate. Others can be weighed and assessed if things are explored only a little further. Focusing

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3 Most teaching of theory in the social sciences is not particularly helpful in unlocking this implicit knowledge. Oriented toward efficient instruction, it offers capsules of past theories in a well-ordered scholastic mode, in effect separating everyday life experience from thinking about social and political theory. Yet the best way to activate the implicit knowledge is regular attempts to solve puzzles derived from the students' social environment. In this volume I have resorted to another device—avoiding too conventional an arrangement of topics and seeking out unexpected and sometimes perhaps startling connections between themes. A less predictable assembly of ideas may be more effective in jogging the reader's memory and imagination.

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on explanatory ideas that transcend the immediate case may yield initial ideas about the extension or limitations of the scope of their applicability. It may also generate hypothetical ideas of what in other circumstances can lead to similar outcomes, and what factors might counterbalance causal mechanisms that seem important in the situation at hand. A skeleton of theoretical ideas, then, will emerge from reflection on patterns that are well-known if we merely focus away from descriptive detail and toward analytic generalization. This is, of course, an exact parallel to what the textbooks declare to be the proper function of case studies—the development of hypotheses.

As in case studies, much may be gained through a move from one well-known pattern or event sequence to another; for it is only when we go beyond a single case that we can observe the effects of factors whose presence or absence is invariant within the first case. The rich recent literature on case studies may be helpful in prompting the right moves in such preliminary and tentative assessments of causal factors involved in well-understood patterns (Brady and Collier 2004; George and Bennett 2005; Gerring 2007).

A similar tack involves well-understood patterns that are familiar not from one's own experience but from the literature. Several of Max Weber's ideal types can serve as examples. Many of these may, as noted earlier, be read as embryonic theories—for instance, the pure type of bureaucracy is claimed to be the most effective tool for large-scale administrative tasks, or formally rational law is considered most suitable for, and supportive of, early commercial capitalism. Here the task is to specify the detailed hypotheses, which are often left implicit—about what helps increase productivity, makes corporate action possible, ensures long-term predictability, and the like. The large later literature on formal organizations or the more compact body of work on Weber's views on law and capitalism (e.g., Rheinstein 1954; Trubek 1972; Ewing 1987) can greatly help in improving the results of such theory explication; but the main point is the learning and fun involved in doing it oneself.

A greater challenge to the theorist's inventiveness is posed by contradictions between underspecified causal mechanisms. I have commented on several trivially contradictory ideas, predicting in one case that people would decline to participate in a common cause because so many do so already, or because very few others are as yet involved (see chapter 1). In another context I have sought to resolve the apparent contradiction between the sour-grapes metaphor for giving up on an ambition because it is hard to satisfy (see chapter 5). As many causal mechanism hypotheses have their origins in commonplace views, such contradictions are not at all infrequent; but
these ideas need some reconciliation—most typically with specifications that make a difference—before they can even tentatively be deployed as causal mechanism hypotheses.

Inspiration for new ideas has many different sources. One of the most important sources for new ideas is building on analogies. The long-established analogy between social formations and organic bodies has left analogical thinking with a bad reputation. This is partly due to the fact that such analogies were pushed to literalist extremes, as in inquiries about which social phenomenon corresponds to brain or stomach rather than whether both organic bodies and social formations exhibit significant equilibrium tendencies. More important, both opponents and advocates of arguments by analogy have often failed to distinguish between the invention of ideas and their testing. When used by many, analogical arguments were often wrongly taken as proven. Yet if we separate the contexts of invention and of validation, judicious use of analogy can be very helpful in theory development.

In using analogies we must keep an eye on critical differences as well as similarities. Thus social ranking in face-to-face groups highlights a number of features of more inclusive systems of inequality; but it misleads profoundly if we do not remain aware that what is at stake in many small groups are just differences in liking and appreciation rather than inequality in the necessities of life and long-term life chances.

The utility of analogical thinking derives in large part from a move that has even wider applicability—the attempt to see a given set of issues in a new light by linking it to phenomena not routinely associated with them. Thus one can break out of the constraints of commonplace ideas about family life by seeing families in the same frame as the dynamics of ephemeral student discussion groups, or by relating decision making about having children to the broad economic problems of choice under constraints of scarcity.

When we explore a new set of issues, it is often useful to have devices for ordering ideas. Resorting to a simple two-by-two classification of conditions or outcomes is so common that some wits have claimed that social scientists respond to any serious problem by making the sign of the cross. Yet while the utility of even the most straightforward ordering of ideas is not to be dismissed, one may reap great insights by paying special attention when a master deviates from such simple conceptualizations of a problem. Which intuitions (or explicit reasonings) stand, for instance, behind Weber's threefold typology of authority based on rational-legal, charismatic, and traditional legitimation? If we cross-categorize the complex defining features of these three types, how many cells would define the underlying "property space" (Barton 1955)? Why are some category cells neglected or considered empty? By answering these questions we may come either to find fault with Weber's categorization or to uncover powerful intuitions.

Weber presents this same threefold typology with a large claim for its utility as a theory frame. The theoretical orientations it implies are to open a more powerful understanding of systems of rule than the ancient typology of the one, the few, and the many as rulers. Weber makes it clear that the three pure types involve more than contrasting versions of sheer legitimation; they also include the distinctive organizational machineries that are associated with each pattern of legitimating ideas. Again, by reconstructing the reasoning behind the overall claim, we can learn a great deal about one of the most influential theory frames of modern social science.

Most attempts to create an effective theory frame will not have the large sweep of Weber's reorientation of the study of systems of rule. Most are based on a careful review of the recently accumulated research experience. Our own efforts will gain if we both understand the purpose of routine reviews of the literature introducing most research reports and identify the rationales of agenda-transforming theory frames.

When we turn to lesser forms of analytic accomplishments than theories and theory frames, the distinction between elements of theory building and pragmatic advice for theory construction becomes less and less clear. Preliminary and intuitively plausible lists of causally relevant factors are indeed a humble form of theoretical accomplishment. Yet they are the raw material for developing causal mechanism hypotheses as well as orienting theory frames, though they have to be subjected to searching examination in diverse attempts at explanation of empirical findings.

We have repeatedly seen that a good understanding of different types of concepts—distinguishing relational from classificatory, operational from nominal, and distinctively identifying from essentialist concepts—is of critical help in generating hypotheses. Thus we have seen that unrecognized tautologies and wasted opportunities for formulating and exploring substantive hypotheses can result from a failure to differentiate sharply between concepts defining causal conditions and those defining outcomes. (An illustration used was overlapping definitions of civil societies and democratization; see chapter 1.) Of special importance is the insight that adjustments of hypotheses almost invariably require modifications of the

* Two recent publications give suggestions for stimulation and discovery of ideas: Abbott (2004) and Snyder (2007). Abbott offers a whole book on "heuristics," going back in part to older traditions of rhetoric. One great suggestion is to review the tensions and controversies between major metatheories and fundamental methodological positions as a source of ideas. Snyder introduces an intriguing set of interviews with major authors in comparative politics with an essay of reflections on what made their work vibrant and successful, and what might help students in the same endeavor.
concepts used. This is at odds with the widely held notion that conceptual clarity requires conceptual stability. At the level of orienting theory frames, this inescapable interrelation between conceptual change and innovative hypothesis formation entails an even more irritating paradox. It is precisely in the most important areas of inquiry, for instance in arguments about institutions and institutionalization, that we find a great variety of central concepts and the most intense contests about promising conceptualizations, an inevitable condition that strikes many as a simple case of conceptual confusion.

The quality of focused theory frames depends critically on the judicious choice of the central concepts. Concepts that articulate well with the envisioned causal and structural hypotheses are the secret of success of the best theory frames. Max Weber’s triad of rational-legal, charismatic, and traditional systems of domination illustrates this very well.

Two final pragmatic rules of thumb for theory construction derive from the particular difficulties of identifying theoretically useful empirical regularities at the microlevel of individual action. It is a fundamental, yet not often recognized insight that the emergence of social structures large and small and their “feedback” effects constrain the variability of individual insight and motivation. This leads to two suggestions: First, structuralist approaches may have a chance to bypass some of the complexities of the subjective space of action. And second, in the search for explanations it may be useful to pay special attention to mesolevel factors.

Structuralist approaches can gain an advantage in two ways (see chapter 8). Some theories, such as the late Peter Blau’s work as well as contemporary network theory, leave aside subjective consciousness, confine themselves to structural regularities such as the heterogeneity of membership in a social formation, and relate them to, say, intermarriage rates. This is a limited advantage, but one that does yield interesting results. The second form of the structuralist strategy just focuses on the ways in which more inclusive social structures shape individual attitudes and actions.1

1 Beyond that, it is often possible to come to conclusions at the meso- and macrolevel from microlevel assumptions that are roughly right, rather than right in all particulars. Often the behavior of a simple majority or even a significant minority of individual actors is sufficient to permit predictions about social movements or other causal factors involved in revolutions (Skocpol 1979; Goldstone 2003).

Similar constellations offer a remedy for a drawback of probabilistic hypotheses—that they normally cannot predict single or a small number of cases. This is the case if causal conditions and outcomes occur at the same level of analysis. However, if they are located at different levels—if we consider, for instance, a certain degree of popular support or opposition as a partial positive or negative causal condition for a system of rule or a cluster of policies (see, e.g., Brooks and Manza 2007)—this handicap of probabilistic hypotheses vanishes.

The second structuralist route to theory formation brings us to the pragmatic rule of thumb emphasizing the special causal significance of meso-level social structures and developments (chapters 14 and 15). These are constrained and shaped by more encompassing social phenomena over which they typically have little influence, and they are at the same time causally relevant for microphenomena, which on their part can rarely shape such mesopatterns as formal organizations, associations, or political parties. The meso-level of social life, then, stands in a doubly asymmetrical causal relation to the macro- and microlevels. This gives it a critical place in many attempts at causal explanation. If a study is confined to a given country or to countries that share important similarities relevant for the investigation, meso-level factors acquire great relevance because macrofactors are implicitly held constant. Moreover, meso-factors such as parties, unions, and other interest organizations may be decisive even in macrocomparative research if the macrodifferences across countries that were considered prove to make little difference for the issues at hand.

A Final Note

I have sought to focus my discussion on a variety of analytic tools as well as on a number of pragmatic suggestions. There is a large repertoire of analytic tools that can be used independent of diverse philosophical and metatheoretical positions one may adopt. Most of the analytic tools discussed are relatively neutral vis-à-vis different grand theory frames, such as those focused on power and conflict, on values and legitimation, on purposive rationality, or on cultural templates, and associated with such names as Bates, Bourdieu, Foucault, Giddens, Gramsci, Parsons, or Weingast.

The starting point of my deliberation was the disappointing achievements of empirical social and political theory when measured against the ambitions of a social science modeled after the natural sciences. If there is one point in the ideal of a unity of all science I prefer to hold on to, it is that the fundamentals of valid explanation and conditional prediction are the same across all fields of inquiry. This applies with particular force also to the differences in methodological approach in areas of social science that allow greater or lesser use of quantification than others. Yet while the fundamental standards of validity are the same, it is hard to claim that—with the possible exception of economics—specific areas and modes of social inquiry show significantly greater theoretical achievement than others. If this estimate is roughly correct, one cannot easily point to strategies that promise greater theoretical advance than others.
What, then, emerges from our considerations as reasonable prospects of theoretical advance in the social sciences? The future of empirical social and political theory does not look dismal, even if there is no single grand remedy for the very real difficulties. For the foreseeable future, theory frames with increasing, though still limited theoretical power will probably be the major carriers of advance.

Over time, the interaction among theoretical orientations, empirical evidence, and theory work in the course of empirical research not only can yield plausible and even sturdy local results; but it is also likely to contribute to theoretical generalizations, be that in the form of richer theory frames, by way of specific tested theoretical propositions (including prominently causal mechanism hypotheses), or even in some cases resulting in integrated sets of theorems deserving the label “theory” without qualification. This differs from the old (and false) hope that an accumulation of empirical findings will eventually amount to theoretical knowledge. There are too many directions of possible generalization for that hope to be reasonable. But the guidance of revisable focused theory frames makes reasonable “analytic induction” possible.

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