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Author(s): Rudra Sil

Source: *Polity*, Vol. 32, No. 4 (Summer, 2000), pp. 499-531

Published by: Palgrave Macmillan Journals

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The Division of Labor in Social Science Research: Unified Methodology or “Organic Solidarity”?*

Rudra Sil

University of Pennsylvania

Since contending methodological perspectives and different types of research products are founded on irreconcilable philosophical assumptions, the sharp, recurrent debates over social science research methods are likely to be fruitless and counterproductive. This article begins by exposing some of the philosophical assumptions underlying the most recent calls for a unified social science methodology and seeks to help develop a common appreciation of how different kinds of methods and research products advance our understanding of different aspects of social life at different levels of abstraction. Such commonly posited dichotomies as deductivist/inductivist logic, quantitative/qualitative analysis, and nomothetic/idio-graphic research products are shown to obscure significant differences along a continuum of strategies through which context-bound information and analytic constructs are combined to produce interpretations of varying degrees of complexity or generality. Durkheim’s conception of “organic solidarity” in a social “division of labor” serves as a useful metaphor here to capture the complementary roles performed by various research products as well as the trade-offs arising from the strengths and weaknesses of various methodological approaches (ranging from formal and statistical approaches to various case-based and interpretive approaches). Thus, sharp claims regarding the strengths and limitations of particular methods are transformed into elements of an overarching agnostic understanding of the trade-offs and complementarities among these methods. Finally, a distinctive role is identified for an ideal-typical “middle-range” comparative-historical approach in fostering greater communication among a more inclusively defined community of methodologically diverse social scientists.

Rudra Sil is Janice and Julian Bers Assistant Professor of Social Sciences and teaches in the Department of Political Science at the University of Pennsylvania, Philadelphia, PA 19104-6215. He is co-editor of *Beyond Boundaries? Disciplines, Paradigms, and Theoretical Integration in International Studies* (Albany: SUNY Press, 2000) and author of *Managing ‘Modernity’: Work, Community, and Author-*

*The author wishes to gratefully acknowledge comments offered on earlier versions of this article by Andrew Bennett, Jose Cheibub, David Collier, Stephen Hanson, Ian Lustick, Andrew Janos, Dalia Kaye, and three anonymous reviewers. Todor Enev, Eric Lomazoff, and Dani Miodownik provided invaluable research assistance.

ity in Late-Industrializing Japan and Russia (*University of Michigan Press, forthcoming*).

Society becomes more effective in moving in concert, at the same time as each of its elements has more movements that are peculiarly its own.

— Emile Durkheim¹

Recent efforts to establish uniform standards and methods for a unified science of society have led to increasingly acrimonious and divisive exchanges among scholars across social science disciplines, even among those who share the same substantive interests.² The inconclusiveness and mutual exclusivity of epistemological assumptions informing the contending methodological persuasions, however, makes arguments over what constitutes “good” research ultimately unresolvable and possibly counter-productive. Thus, it seems worthwhile to set aside the quest for uniform methodological tenets in favor of greater methodological pluralism, shifting the focus from common rules or standards to a common appreciation of the *trade-offs* involved in pursuing different methods and research products.

In this spirit, this article adapts Emile Durkheim’s notion of “organic solidarity,” linked to the interdependence among individuals in a “complex division of labor,” as a useful metaphor for capturing the quite distinct, yet interdependent, roles played by different research products and methodological approaches in a *scholarly* division of labor. In effect, I contend that the community of social scientists, as long as they share *some* abstract commitment to acquiring a better understanding of aspects of social life, may stand to gain by ignoring hegemonic efforts to establish uniform methodological tenets and by seeking a common understanding of the distinctive advantages (and limitations) of varied approaches designed to investigate different *kinds* of “truths” at different levels of abstraction. Such an understanding can enable greater communication among scholars of starkly different methodological persuasions while fostering greater awareness of their interdependence and perhaps even producing a measure of “organic solidarity” among them.

Below, I begin with a brief discussion of renewed calls for uniform methods for social science and identify some of the unstated philosophical assumptions on which such a unified methodology is to be founded. Section two considers the relationship between analytic constructs and empirical study, noting that “strong” claims made by inductivists and deductivists derive from unverifiable epistemological propositions and that “softer” understandings of inductive and deductive logic can coexist quite easily in practice. The third section notes that while the logics behind

1. Emile Durkheim, *The Division of Labor in Society* (New York: Free Press, 1984 [1933]), 85.

2. See, e.g., the exchanges in *American Political Science Review* 89, 2 (1995): 454-81; *Comparative Social Research* 16 (1997): 1-32; and *American Journal of Sociology* 104, 3 (1998): 722-871.

nomothetic (e.g. statistical) and idiographic (e.g. interpretive) approaches may not be as sharply opposed as frequently assumed, there remain consequential differences along a wide-ranging *continuum* of research strategies, informed by varying epistemologies, objectives, and levels of generality. In section four, Durkheim's understanding of the social division of labor is invoked to capture the complementary roles of different research products, ranging from parsimonious models to richly descriptive narratives. In the following section, this logic is extended to trace the strengths, weaknesses, and trade-offs among specific methodological approaches, ranging from formal modeling and statistical analysis to various kinds of small-n comparisons, case-studies and interpretations; each of these methods is viewed as operating at varying levels of abstraction, offering different *kinds* of intellectual insights that are complementary or combinable. Since the lines of communication across research traditions frequently tend to be blocked or strained, however, the conclusion considers the particular significance of "middle range" comparative-historical approaches in fostering communication and a greater degree of tolerance among scholars partial to different styles of research.

I. A Unified Methodology for the Social Sciences?

Over the past two decades, quite familiar methodological issues have become the subject of increasingly heated debate, resulting in an unprecedented level of polarization within and across social science disciplines. This is in part a result of the gauntlet laid down to positive social science by "post-modernists" who have taken the philosophical skepticism of previous generations of interpretive scholars to new extremes. Their staunchly relativistic epistemology has called into question the scientific status of all social analysis, emphasizing the inherent subjectivism involved in the representation of human activities by human observers trapped in "a web of signification we ourselves have spun."³ Such a view implies that established standards for methodological "rigor" are nothing more than artificial devices for enhancing the status or reputation of certain groups of scholars.⁴ At the other extreme, committed positivists have responded by simply dismissing most interpretive analysis as little more than personal narratives that have no place in the social sciences. They may disagree over the relative merits of deductive and inductive logics, or the reliability of statistical and discursive empirical information; but they remain committed to the idea of uniform, objective criteria for distinguishing and evaluating scientific knowledge. In between, a variety of "soft" positivists and inter-

3. Charles Taylor, "Interpretation and the Sciences of Man," in *Interpretive Social Science: A Second Look*, ed. Paul Rabinow and William Sullivan (Berkeley: University of California Press, 1987), 125.

4. For a critical but balanced discussion of the shared assumptions of varied post-modern approaches, see Pauline Rosenau, *Post-Modernism and the Social Sciences* (Princeton: Princeton University Press, 1992).

pretive theorists have sought to defend an “eclectic messy center,”⁵ while offering middle-range hypotheses, partial explanations or “thick” descriptions, all of which transcend context-bound narratives but fall short of universally applicable causal laws or inferences.

To bring order into this seemingly chaotic state of affairs in the social sciences, some scholars have undertaken new efforts to construct a uniform set of rules that would apply to quantitative and qualitative research as well as to interpretive narratives.⁶ One of the most well-received of these efforts, led by Gary King and his collaborators, has consisted of two related programs for “disciplining” social science disciplines: (a) interpretive and comparative analysis based on qualitative research is driven by the same quasi-experimental logic employed in quantitative research and thus should be subjected to the same rules for designing empirical research and for drawing causal or descriptive inferences;⁷ and (b) all social scientists should deposit their empirical work—whether in the form of raw statistical data or in the form of the field notes representing the discursive data of qualitative researchers—in data banks in order to promote the “norm of replication” that characterizes research in the hard sciences.⁸ These proposals, it is thought, would enable us to maintain the standards of “rigorous” social science without unnecessarily marginalizing qualitative research or interpretive narratives.

Many social scientists, despite some reservations, are justifiably excited: as a result of uniform rules and standards for good research design and scientific inference, qualitative researchers will no longer be dismissed as “soft,” and the scientific status of their craft will be greatly enhanced (especially relative to those employing quantitative methods). These proposals also deserve credit for stimulating more discussion over methodological issues among qualitative researchers and making them more conscious about what kinds of claims can be made, with what levels of confidence, on the basis of what kinds of observations.⁹ What is worth emphasizing, however, is that those championing uniform methods seldom bother to discuss or defend the *epistemological* assumptions behind their definitions and standards of “rigorous” research in all social analysis. King *et al.*, for example, deliberately and explicitly choose to “sidestep” some of the most central and controversial issues in the philosophy of

5. Peter Evans, contribution to “The Role of Theory in Comparative Politics: A Symposium,” *World Politics* 48, 1 (October 1995): 2-3.

6. Of course, efforts to establish the fundamental principles of a unified “science” as a whole can be traced back to Auguste Comte; see his *Positive Philosophy*, 2 vols., trans. H. Martineau (London: Kegan Paul, 1893).

7. Gary King, Robert Keohane and Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton: Princeton University Press, 1994). This point is also made in John Goldthorpe, “Current Issues in Comparative Methodology,” *Comparative Social Research* 16 (1997): 1-26.

8. Gary King, “Replication, Replication,” *Political Science* 28, 3 (September 1995): 444-52.

9. See, e.g., the contributions to “The Quantitative-Qualitative Disputation,” *American Political Science Review* 89, 2 (June 1995): 454-81; and “The Replication Debate,” in *APSA-CP: Newsletter of the APSA Organized Section in Comparative Politics*, 7, 1 (Winter 1996): 5-13.

social science, despite the fact that differences over these issues would produce vastly different kinds of research products aimed at quite different objectives.¹⁰

In fact, the methodological tenets King *et al.* themselves offer are quite clearly predicated on a range of crucial assumptions on such foundational issues as the nature of social reality and the character and limits of social knowledge. King and his collaborators are essentially operating on the basis of an *empiricist* epistemology, one that privileges observable regularities in social life, emphasizes causal effects and descriptive inferences without seeking to understand causal mechanisms, and equates social knowledge with falsifiable law-like propositions concerning relationships between standardized categories of social facts called “variables.” As a result, King *et al.*’s decision to side-step foundational issues helps them to ignore fundamental questions about just how applicable their methodological prescriptions are for a wide range of social scientists who may not share their implicit epistemological assumptions. This is perhaps one reason why the response to the recent calls for a uniform set of methods and standards has been less than enthusiastic, even among many positivist social scientists.

Many general theorists, for example, fault the logic of statistical analysis since it discounts the crucial role of deductive reasoning in the process of identifying causal mechanisms and developing social science theories.¹¹ Scholars who systematically apply the comparative method to small-n studies note that many of King *et al.*’s prescriptions simply re-state familiar issues in the language of quantitative analysis while understating the different priorities pursued by qualitative researchers at different stages of research.¹² Others, while concurring that *explanatory* theories might profit from the prescriptions of King *et al.*, note that there are valuable intellectual returns from *other* kinds of equally important qualitative studies—for example, those aimed at generating new typologies or context-sensitive interpretations—that cannot be uniformly subjected to the “norm of replicability” or standard rules of inference.¹³ Still others wonder whether King’s call for replication as a uniform norm reflects a lack of appreciation for the distinctive challenges facing qualitative area specialists doing fieldwork abroad without the benefit of the more reliable and plentiful data, both statistical and discursive, available to other scholars.¹⁴

The problem becomes even more clear in the manner in which King *et al.* attempt to extend their methodological tenets to discipline interpretive analysis.

10. King *et al.*, *Designing Social Inquiry*, 6.

11. See, e.g., Ronald Rogowski, “The Role of Theory and Anomaly in Social-Scientific Research,” *American Political Science Review* 89, 2 (June 1995): 467-70.

12. David Collier, “Translating Quantitative Methods for Qualitative Researchers: The Case of Selection Bias” in *American Political Science Review* 89, 2 (June 1995): 461-66.

13. James Caporaso, “Research Design, Falsification, and the Qualitative-Quantitative Divide,” *American Political Science Review* 89, 2 (June 1995): 457-60.

14. Barry Ames, “Comparative Politics and the Replication Controversy,” *APSA-CP: Newsletter of the APSA Organized Section in Comparative Politics*, 7, 1 (Winter 1996): 8, 11-12.

They justify this attempt on the grounds that even interpretive accounts ultimately consist of propositions designed to bring order to infinitely complex social reality, dismissing as “foolish” any narratives that do not care about “discovering truth.”¹⁵ This view suggests that there exists a consensus about what kinds of statements constitute claims about “truth,” ignoring the significant differences among social scientists over the extent to which different “truths” are accessible to human observers and the extent to which these “truths” can be generalized across contexts. In effect, as King *et al.* seek to blur the distinction between quantitative and qualitative research, they also assume that the epistemological assumptions informing some types of nomothetic analysis apply to all scholarly endeavors that qualify as “social sciences.” Thus, despite their seemingly reasonable claims and seemingly inclusive attitude, King *et al.* end up excluding from their community of “social scientists” anyone who is not attempting to draw causal or descriptive inferences on the basis of their particular (empiricist) understanding of “truth.”

Many widely respected scholars throughout social science disciplines adhere to quite different understandings about the nature of social reality and the character of social knowledge, and their preferred methodological principles vary with the specific ontological and epistemological assumptions they make. Those adhering to a realist philosophy of science, for example, may find internal logical consistency or other abstract principles of evaluation to be the main basis for validating social science research product. In “softer” versions of positivism (on which much comparative-historical research is founded) and non-positivist epistemologies (on which much interpretive analysis is founded), distinctive historical sequences and cultural contexts are valuable in their own right, and if causal connections are posited at all, these are historically specific.¹⁶ If these diverse scholars are ever going to be able to understand and engage each other as members of a single community while epistemological debates remain unresolved, then it may be more useful to evaluate the scholarly endeavors of different social scientists not in terms of adherence to uniform methodological tenets but in terms of the different kinds of contributions they make to our *collective* quest for a broader and deeper understanding of patterns of human activities. The unity of the social sciences would then rest not on uniform adherence to any particular method but on an appreciation of the distinct, complementary, intellectual pay-offs from different kinds of scholarly endeavors, each employing different sorts of information in different ways to produce statements of varying levels of abstracts. To systematically project what this emergent “division of labor” might look like, however, it is first necessary to consider the varied perspec-

15. King *et al.*, *Designing Social Inquiry*, 38.

16. For a discussion of the range of epistemological perspectives undergirding social science research, see Rudra Sil, “Against Epistemological Absolutism: Towards a ‘Pragmatic’ Center?” in *Beyond Boundaries? Disciplines, Paradigms and Theoretical Integration in International Studies*, ed. Rudra Sil and Eileen Doherty (Albany: State University of New York Press, 2000), 145-75.

tives on such problematic issues as the relationship between theoretical generalizations and empirical data (the debates over deductive-inductive logic) and the relationships between categories of facts and the contexts from which they are drawn (the debates over quantitative-qualitative research and nomothetic-ideographic modes of analysis).

II. The Origins of Analytic Constructs: Beyond Deductivism/Inductivism

While many narratives explicitly founded on a relativistic epistemology (for example, hermeneutic exercises or context-bound narratives) do not view the construction or application of analytical constructs as a valid endeavor, most social scientists do have to adopt some sort of epistemological perspective on how the terms, concepts or variables they employ relate to the empirical reality they seek to understand or explain. Virtually all positive social science as well as much interpretive analysis can be grouped into two, diametrically opposed, ideal-typical categories: deductivist or inductivist.

Most deductivists are “theoretical realists”¹⁷ in that, although they do not embrace the full-blown logical positivism of Comte, they do implicitly accept the Kantian view that universal pre-given categories exist independent of the subjective experiences of human beings. Ontology not only has precedence over epistemology, but the epistemology itself presumes that it is possible and necessary to deduce *unobserved* causal mechanisms or pathways as long as the *effects or sequences* they produce are observable. As a result, while empirical falsifiability is important, deductivists emphasize prior causal axioms and the primacy of internal logical consistency. Empirical observations are necessary to social science research, but their primary role is to demonstrate the validity of hypotheses deductively derived from logically consistent formal models; the process of theory-construction itself is viewed as a logical exercise that is distinct from and *prior* to the actual observation of empirical processes. In the end, in a “strong” deductivist approach to social science, “formal, explicit theorizing takes intellectual, if not temporal, precedence over empiricism.”¹⁸

For those partial to inductivist approaches, the purpose of empirical observation is not to lend substance to deductively derived covering laws, but to inductively gen-

17. See George Steinmetz, “Critical Realism and Historical Sociology,” *Comparative Studies in Society and History* 40, 1 (January 1998): 170-86; and Margaret Somers, “‘We’re No Angeles’: Realism, Rational Choice, and Relationality in Social Science,” *American Journal of Sociology* 104, 3 (November 1998): 722-84.

18. Bruce Bueno de Mesquita, “Toward a Scientific Understanding of International Conflict,” *International Studies Quarterly*, 29 (1985): 121-36. This view essentially reflects the position of such philosophers of science as Carl G. Hempel; see his *Philosophy of Natural Science* (Englewood Cliffs: Prentice-Hall, 1966). Deductivism characterizes contemporary rational-choice theory as well as such general theories as structural-functional sociology and neorealist international relations.

erate causal inferences or interpretations on the basis of observed empirical regularities. Inductivists, perhaps more so than their deductivist colleagues, vary in their epistemological assumptions, particularly in regard to the possibility of drawing causal inferences on the basis of observations within or across contexts. Some, inspired by Hume, bring to bear statistical methods to identify compelling law-like regularities across comparable contexts even if causal mechanisms are not in evidence.¹⁹ Others, inspired by Mill, rely on fewer cases and more historically grounded (qualitative) variables in order to draw generalizations ranging from modest partial explanations to macro-causal propositions.²⁰ Less ambitious applications of inductive thinking are even evident in narratives of varying levels of complexity or “thickness” that seek to make sense of actions, patterns, and sequences observed within particular contexts.²¹ Despite the sharp differences between these approaches, however, inductivists can be clearly distinguished from deductivists in that they assign temporal and logical priority to the study of empirical processes and observable patterns, and they view prior deductive theorizing as merely an abstract intellectual exercise with little bearing on concrete social phenomena.

In practice, however, *a priori* commitments to either deductive or inductive analysis represent something akin to definitive claims about whether the chicken or the egg came first. The two perspectives cannot be reconciled except by fiat since they ultimately derive from unverifiable foundational claims about the existence of abstract intellectual constructs and their relationship to concrete social phenomena as observed in different contexts. In the case of deductivism, “internal logical consistency” can become meaningless in the absence of empirical referents; the burden of demonstrating the utility of formal models falls on the shoulders of scholars who must translate logical axioms into concrete hypotheses on the basis of careful empirical observations before the question of external validation is even broached. Thus, the “strong” deductivist proposition that formal theorizing takes intellectual precedence over empiricism becomes nothing more than an arbitrary convention.

At the same time, deductivists are right to note that neither statistical correlations between a few variables, nor generalizations from a few cases, can produce causal propositions free from spurious inferences. The existence of general causal mechanisms, they argue, cannot be inferred from observed events or causal sequences in temporal processes. Moreover, some of the “assumptions” made by inductivists often prove to be central to their inferences; so, often what they are offering are not

19. This is the type of inductivism evident in King *et al.*, *Designing Social Inquiry*. Hume's philosophical empiricism is outlined in David Hume, *An Enquiry Concerning Human Understanding* (La Salle: The Open Court Publishing Company, 1946).

20. See, e.g., Barrington Moore, *The Social Origins of Dictatorship and Democracy* (Boston: Beacon, 1966); and Theda Skocpol, *States and Social Revolutions* (New York: Cambridge University Press, 1979). Mill's inductive logics are outlined in John Stuart Mill, *A System of Logic* (New York: Harper and Row, 1888).

21. On the character and merits of “thick” narratives, see Clifford Geertz, “Thick Description: Towards an Interpretive Theory of Culture,” in his *The Interpretations of Cultures* (New York: Basic Books, 1973), 3-30.

empirically grounded hypotheses but rather claims shaped by prior intellectual constructs smuggled in as assumptions. Such assumptions are not intrinsically different from the axiomatic principles that drive deductive theories since in both cases hypotheses about empirical phenomena appear compelling only on the condition that the initial assumptions are accepted.

Fortunately, it is possible to identify a pragmatic middle ground upon which “softer” versions of the deductivist and inductivist assumptions ultimately converge in the actual conduct of social inquiry. Here, it is instructive to consider Weber’s use of ideal-types in addressing the relationship between empirical research, concept-formation and theoretical inferences. For Weber, ideal-types enable the “analytical accentuation” of certain elements from infinitely complex social phenomena in order to provide an “explanatory understanding” limited to the questions of interest to the investigator.²² The formulation of ideal-types, while intended to provide a framework for Weber’s own historical comparisons, is not intended to be a universal framework on the basis of which one can axiomatically derive *a priori* causal propositions. At the same time, the types do provide a way for self-conscious investigators to develop a broader “explanatory understanding” of certain aspects of social reality as long as they are also able to claim an “interpretive grasp” of the subjective meanings attached by actors being studied. In effect, while Weber’s construction of concepts paves the way for broader inferences, he appreciates the limits of these generalizations given the role played by the investigator’s particular values and interests in the original formulation of concepts and questions.

Weber’s understanding and use of “ideal-type” categories suggests that a reasonable convergence between “soft” versions of deductivism and inductivism is possible if one first distinguishes between heuristic concepts and causal propositions, and then recognizes the dialectical relationship between concepts and observations out of which a range of interpretations, inferences and causal theories can be generated depending on the objectives of the investigator. It is possible for “soft” inductivists to acknowledge that historical or empirical research is never conducted on a *tabula rasa*, that most empirically grounded propositions, including many descriptive narratives, involve some prior question or framework that can enable researchers to limit and order their observations given the infinitely complex relationships among social phenomena even in a single context. At the same time, “soft” deductivists may be able to recognize the difference between the role of full-blown analytic models or axioms in interpreting empirical observations and the role played by *concepts* or conceptual frameworks that serve heuristic purposes and help organize empirical research without anticipating causal explanations.²³ Furthermore, even these pre-

22. Max Weber, *Economy and Society*, 2 vols., ed. Guenther Roth and Claus Wittich (Berkeley: University of California Press, 1978 [1968]), I: 5-9.

23. On this point, see also Victoria Bonnell, “The Uses of Theory, Concepts and Comparison in Historical Sociology,” *Comparative Studies in Society and History* 2, 2 (April 1980): 156-73.

given concepts have to come from somewhere, and the construction of concepts itself requires some basic observational statements based on the typical features of the various actors and units across concrete empirical contexts. And since new cases can lead to questions about how “typical” these features are, even the process of defining cases and identifying concepts becomes in practice a *dialectical* process that involves alternating between deductive and inductive reasoning.

These observations suggest that “soft” deductivists and inductivists of various stripes can agree on a number of things: that concepts enable the translation of context-bound observational statements and allow for meaningful comparisons and broader descriptive and causal inferences; that the construction of meaningful concepts and categories is itself informed by experience, but still constitutes a necessary step prior to the formulation of inferences; and that further applications or refinements of concepts must proceed on the basis of ongoing reassessments of similarities and differences among comparable units of observation. These agreements do not diffuse the core philosophical issues that divide deductivists and inductivists, but we can see that, in practice, soft versions of both camps—and those who are agnostic on the matter—can converge on an abstract formulation of the basic objective: “to develop a system of concepts which combines empirical import with theoretical significance.”²⁴ That is, in terms of their actual research, both deductivists and inductivists will only be taken seriously when they can show that a theoretical proposition or inference both “makes sense” (i.e., has some compelling logic) and says something about the “real world” (i.e., reflects phenomena or sequences in particular empirical contexts).²⁵ As we shall see below, however, such a convergence may be accompanied by a wide *variety* of research strategies depending on what kind of narrative is being developed, about how many distinct social contexts, and at what level of complexity or generality.

III. Cases, Variables and Contexts: Similar Challenges, Varied Responses

Students of methodology frequently posit two distinct understandings of the purpose and limits of empirical analysis. *Nomothetic* analysis represents a search for recurrent patterns reflecting constant relationships among variables that must be defined at a sufficient level of abstraction to permit analysis across a specified range of cases; *idiographic* analysis studies unique historical configurations and attempts to incorporate the subjective meanings that actors themselves attach to their actions

24. Carl Hempel, *Fundamentals of Concept Formation*, as reprinted in *Foundations of the Unity of Science: Towards an International Encyclopedia of Unified Science*, ed. Otto Neurath, Rudolph Carnap and Charles Morris (Chicago: University of Chicago Press, 1970), 730.

25. On this point, see also Jack Goldstone, “Initial Conditions, General Laws, Path Dependence, and Explanation in Historical Sociology,” *American Journal of Sociology* 104 (1998), 830-32.

and experiences within particular historical or social contexts.²⁶ Nomothetic analysis—most obvious in economics, but also dominant in political science and sociology—encompasses formal modeling, statistical analysis for hypothesis-testing, or hypothesis-generating case studies; but in all cases, the focus is on the correlations and relationships among “variables,” abstract categories formed out of similar kinds of “facts” in order to construct general inferences applicable to several empirical contexts within a given domain. Idiographic approaches—best exemplified by cultural anthropology, but also evident in historical narratives in political science and sociology—are thought to focus on the complex factors and processes in historically specific situations that lead to a discernible pattern or sequence; even if the essential facts are termed “variables,” idiographic analysis emphasizes how the particular *interaction* of these “variables” represents a particular combination of actions, sequences or patterns rather than a general model or hypothesis that can be rendered meaningful in other historical or social contexts. The nomothetic-idiographic dichotomy essentially rests on the relationships between intellectual constructs or categories formed by the investigator and the concrete empirical phenomena to which these constructs or categories may be applied.

It is highly significant for the purposes of the argument to follow, however, that the nomothetic-idiographic distinction is neither absolute, nor identical with the distinction between quantitative-qualitative research. It is true that idiographic analyses by definition are likely to be qualitative, given the attention paid to historical patterns and to the meanings assigned by agents to phenomena within contexts; but qualitative information is frequently invoked in nomothetic analysis (as in the case of theoretically driven comparative-historical analysis). Similarly, quantitative studies are variable-oriented (where a few select variables are analyzed over many cases), and as such, fall primarily within the domain of nomothetic analysis; but, in principle, statistical information may be invoked in idiographic analysis to suggest historically specific trends or processes. That is, nomothetic analysis can be based on quantitative, variable-based research or qualitative, case-based research, while idiographic analysis can employ discursive or statistical information as long as it is designed to demonstrate context-specific social processes.²⁷

This simple observation has crucial implications for the fierce methodological debates in contemporary social science: (i) First, are nomothetic and idiographic modes of analysis mutually exclusive categories of social research, corresponding

26. “Context” here is defined in ideal-typical terms as infinitely complex time- and space-bound events and sequences that are practically impossible to represent, let alone explain, in full.

27. Ragin uses “variable-oriented” and “case-oriented” methods as a substitute for quantitative and qualitative methods to suggest that different combinations of cases and variables can produce a middle-ground that combines the advantages of both methods. Yet, he is still addressing a dichotomy *within* nomothetic research, thereby excluding interpretative narratives that are not designed as case studies. Charles Ragin, “Turning the Tables: How Case-Oriented Research Challenges Variable-Oriented Research,” *Comparative Social Research* 16 (1997): 27-42.

to two distinct sets of methodological tenets (in which case, King *et al.*'s prescriptions would apply to quantitative and qualitative nomothetic research, but not to idiographic narratives)? (ii) And, if not, does this suggest a unified logic for all social science research (as King *et al.* would have us believe) or *multiple* logics reflecting a range of significant choices and trade-offs between the poles of nomothetic and idiographic ideal-types?

On the first question, as proponents of uniform methodologies are right to note, quantitative, qualitative and interpretive research involve essentially the *same* challenges in sorting through empirical information, grouping facts together into categories, and ordering those categories, regardless of whether their aim is to produce causal or descriptive accounts. This is certainly not difficult to see when comparing quantitative and qualitative *nomothetic* studies. There are certainly important differences in the way in which "cases" are defined and used by different scholars pursuing different objectives within different research traditions,²⁸ but these differences do not change the fact that, in the absence of control groups in social analysis, both quantitative and qualitative researchers must invoke the logic of the "comparative method" to enable them to select and order their empirical observations so as to shed some new light on other, yet-to-be-defined, comparable cases. More compelling is the fact that most idiographic studies, with the exception of openly subjective context-bound narratives, face essentially the same challenge despite the exclusive attention paid to context-sensitive qualitative information! As King *et al.* rightly argue, ". . . the difference between the amount of complexity in the world and that in the thickest of descriptions is still vastly larger than the difference between the thickest of descriptions and the most abstract quantitative or formal analysis."²⁹ As a result, the process of identifying, ordering and representing "significant" facts in a context-sensitive idiographic analysis is not *fundamentally* different from the way that quantitative or qualitative nomothetic researchers employ analytic constructs to sort out information.

There is, however, a second point to be considered: the *implications* of this observation. The basic challenge of extracting and ordering "significant" facts from infinitely complex social phenomena does *not* logically point to a uniform set of rules for research design since the objectives and the foundational assumptions of scholars still vary substantially and significantly. Even though the *challenge* facing nomothetic and idiographic researchers may be essentially the same, the *responses* to this challenge are dependent on how scholars view the relationship between the contexts they study and abstract categories of analysis such as "cases" and "variables." A large number of nomothetic scholars using qualitative information may indeed profit from King *et al.*'s rules of inference, but many other scholars in the

28. See Charles Ragin, "Cases of 'What is a Case?'" in *What is a Case? Exploring the Foundations of Social Inquiry*, ed. Ragin and Howard Becker (New York: Cambridge University Press, 1992), 8-9

29. King *et al.*, *Designing Social Inquiry*, 43.

social sciences pursue different objectives related to different understandings of “truth” involving different levels of empirical detail presented at different levels of abstraction. This suggests a quite different conclusion than the one suggested by King *et al.*: that there are not one, not two, but *many* different strategies for ordering and comparing facts across contexts, and *each of these strategies involves different purposes, different logics, and different kinds of returns that are all significant and valuable to the collective endeavor of social scientists.*

I suggest that the relationship between “cases,” “variables,” and the contexts from which they are extracted can be better understood along a graded *continuum* rather than simple dichotomies like quantitative-qualitative method or nomothetic-idiographic analysis. As Hempel noted, concept-formation is driven by ideals of “theoretical simplicity and fruitfulness, both of which are surely capable of *gradations*.”³⁰ There may be a unifying logic along these gradations, but it is a highly abstract one analogous to the one employed in Sartori’s “ladder of generality.” In his classic article on concepts, Sartori noted the more cases to which we attempt to apply general categories or concepts, the more the latter have to be “stretched” and the less meaningful and useful they may become in identifying the appropriate empirical referents within the specific context of each case.³¹ Rather than being viewed as an intractable problem, Sartori’s ladder can be adapted here as the logical basis for appreciating the different kinds of intellectual returns—and trade-offs—related to the definition and use of “cases” and “variables” based on a wider or narrower range of discrete empirical contexts.

Depending on their objectives and epistemological orientations, scholars must determine whether the object of their analysis is a single context, a single crucial “case,” few cases, or many cases; and, related to this, they must decide just how much to compress the facts extracted from any one context into “variables” measurable across contexts or cases. Given the strengths and weaknesses of a particular matrix of cases and variables, we are left with a wide range of scholarly endeavors to choose from as we progressively widen (or narrow) the range of cases and/or expand (or reduce) the breadth of variables. In some situations, it may be possible to make continuous and related adjustments to the definition of concepts as the domain of possible cases is increased to test the limits of a concept’s utility,³² but, for any given problematique, the attempt to expand the domain of cases still carries the price of “stretching” variables to fit historical or discursive data. In other words, any given matrix of cases and variables—that is, any decision about how many contexts

30. Hempel, *Fundamentals*, 730, emphasis added.

31. See Giovanni Sartori, “Concept Misinformation in Comparative Politics,” *American Political Science Review* 64 (1970): 1033-53; and David Collier and James Mahon, “Conceptual ‘Stretching’ Revisited: Adapting Categories in Comparative Analysis,” *American Political Science Review* 87 (1993): 845-55.

32. This “double-fitting” is outlined in Charles Ragin, *Constructing Social Research: The Unity and Diversity of Method* (Thousand Oaks: Pine Forge Press, 1994).

to study and how much to compress observable data into more generalized categories—has its own strengths and weaknesses and provides different kinds of insights into concrete empirical phenomena. Thus, the decision to employ a particular matrix of cases and variables comes with significant, unavoidable trade-offs that are inherent in the kind of intellectual contribution that results.³³

Expanding this argument, we can even expect intellectual pay-offs from what King *et al.* would consider “poorly” designed research according to the logic of quantitative analysis. Comparative studies selecting on the dependent variable, for example, are dismissed as inherently flawed by King *et al.*,³⁴ although they can make critical contributions by challenging the utility of certain hypotheses, eliminating spurious inferences, suggesting new typologies, or revealing additional variables or causal pathways that were missed by their more “rigorous” colleagues.³⁵ Similarly, there are crucial insights to be gained into the complexities of social processes even from interpretive studies that are so context-driven that they fail to suggest descriptive inferences. Even though such studies would not independently lead to general causal propositions, they would still play a crucial role in uncovering different kinds of facts (or “variables”) and suggesting a more nuanced understanding of a given process or recurrent historical patterns.

The point is not that explicit rules concerning good research design are a hindrance; far from it, such rules can ensure that theoretical claims researchers make are commensurate with the evidence on hand. However, when such rules are extended to suggest uniform criteria for “good” research, they *de facto* privilege certain kinds of research products while closing off a lot of avenues for uncovering unexpected correlations, identifying new variables and their interactions, and tracing alternative pathways at different levels of complexity. If the unit of evaluation in the social sciences is viewed as the community of social scientists rather than the individual investigator,³⁶ then there is room for quite a wide range of research products and methodologies, each operating on different epistemologies and offering different kinds of intellectual returns cast at different levels of abstraction.

IV. The Division of Labor Among Social Science Research Products

I employ Durkheim’s notion of “the division of labor” as an analogy to suggest a

33. For a different view of “trade-offs” among *concepts* satisfying different intellectual criteria, see John Gerring, “What Makes a Concept Good? A Criterial Framework for Understanding Concept Formation in the Social Sciences,” *Polity* 31 (Spring 1999): 366-67.

34. King *et al.*, *Designing Social Inquiry*, 129-37.

35. On this point, see also David Collier and James Mahoney, “Insights and Pitfalls: Selection Bias in Qualitative Research,” *World Politics* (October 1996): 56-89.

36. David Laitin, “Disciplining Political Science,” *American Political Science Review* 89, 2 (June 1995): 456.

more useful way to think about the relationships among competing research traditions and methodological approaches in the social sciences today. For Durkheim, “the division of labor” represented an analytic construct for identifying the distinct roles played by individuals and groups as part of a single social structure. This construct allowed Durkheim to compare the mechanisms through which the members of societies, both simple and complex, were capable of developing a “collective consciousness” linked to the nature of their roles.³⁷ In doing so, Durkheim wanted to challenge the notion—most clearly exemplified in Toennies’s opposition of *Gemeinschaft* (community) and *Gesellschaft* (society)³⁸—that agrarian communities were intrinsically more natural, harmonious and unified compared to the “artificial” relationships and isolating experience of modern industrial society. Durkheim endeavored to show that the differentiation of roles and functions in increasingly complex societies, rather than destroying social solidarity, only transformed the character of that solidarity: whereas “mechanical” solidarity in simple, agrarian communities arose from the common consciousness of individuals with similar roles, complex societies were characterized by “organic” solidarity rooted in the functional interdependence among individuals playing increasingly differentiated roles in the division of labor. This solidarity, however, depended on an emergent “common consciousness,” however abstract, of the growing interdependence of individuals upon each other and on society as a whole.³⁹

In the social sciences, the process of differentiation is evident in the evolution of disciplinary structures over the past two centuries. Increasingly more specialized fields, sub-fields and specialties have emerged to help investigators to gain a more detailed knowledge of a particular set of phenomena and to share their research products with progressively narrower groups of scholars whose specialized training enables them to efficiently grasp the significance of these findings. In addition, distinctive research traditions have emerged to provide common foundational assumptions, vocabularies, research tools, and evaluative criteria, enabling scholars to communicate their findings or claims in a language accessible to others sharing a given tradition. What is lacking, however, are the channels or mechanisms through which the contributions of distinctive research products and methodologies across various disciplines and sub-fields can be simultaneously appreciated as part of an integrated effort.⁴⁰ Instead, acrimonious methodological debates, often reinforcing divisions among sub-fields within and across disciplines, suggest that

37. See Durkheim, *Division of Labor*, 31-87, 229-33.

38. Ferdinand Toennies, *Community and Society (Gemeinschaft und Gesellschaft)*, trans. and ed. Charles P. Loomis (New York: Harper and Row, 1957), 33-40, 76-80.

39. In complex societies, according to Durkheim, “the common consciousness is itself forced to rise above all local diversities, to dominate more the space available, and consequently to become more abstract.” Durkheim, *Division of Labor*, 230.

40. See my “The Questionable Status of Boundaries: The Need for Integration,” in *Beyond Boundaries?* ed. Sil and Doherty, 1-13.

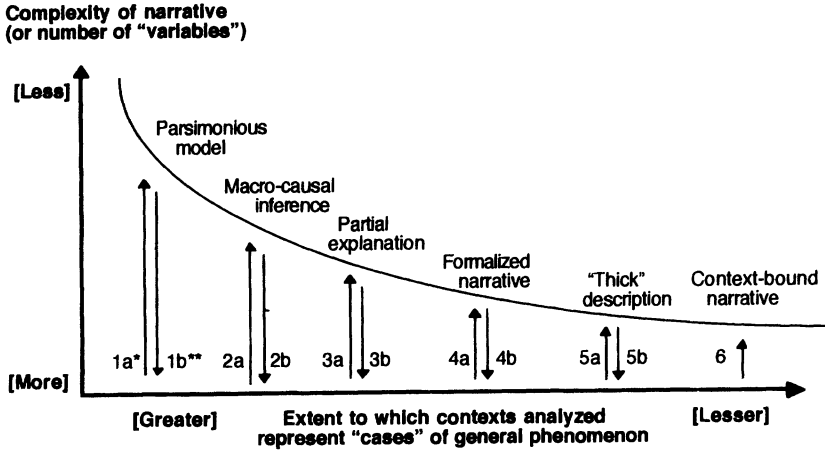
many social scientists are not yet prepared to view their respective contributions and methodologies as *interdependent*, as fulfilling a particular *role* in a collective quest for knowledge by a community of social scientists. In Durkheimian terms, the problem has to do with the prospects for creating a greater sense of solidarity among the differentiated scholars throughout the social sciences. Those who insist that the social sciences be founded on a unified logic and uniform methods are adopting the view of a more *simple* community in which investigators might come to share a “mechanical” solidarity rooted in the similarity of their methods and practices. If, instead, we view social scientists as part of a *complex* division of labor, then whatever solidarity is possible would be of an “organic” type; this would require only a generalized “common consciousness” based on the abstract awareness of the manner in which diverse approaches fulfill diverse, interdependent roles for the social sciences as a whole.

The notion of distinct approaches serving distinct functions in a scholarly “division of labor” is not meant to suggest that scholars need to commit to one or another methodological approach or choose their research projects solely on the grounds of their value to others. Indeed, most social scientists do what they do primarily because they find it intrinsically interesting, and it is often the questions they ask that account for the character and quality of their research. Since, however, methodological debates have such a powerful and divisive impact on the work and lives of social scientists (in terms of hiring, tenure, grants and common civility), it seems worthwhile to consider how the seemingly irreconcilable positions adopted by scholars from different research traditions might be recast as expressions of their particular *roles* within a single scholarly division of labor. Otherwise, social scientists risk becoming fragmented into isolated clusters of researchers incapable of communicating with each other even where their substantive interests converge.

Figure 1 below is designed to represent what a more inclusive division of labor might look like in relation to diverse research products in the social sciences. It draws upon the logic of the aforementioned methodological discussions in order to capture the distinctive kinds of intellectual pay-offs and *trade-offs* from different kinds of research products, depending on how investigators view the relationships between their analytic constructs (such as “variables,” “cases,” and “hypotheses”) and the one, few, or many empirical contexts to which these are supposed to be related.

This diagram essentially posits that the wide range of research products in social science disciplines are all capable of capturing different kinds or aspects of “truth” cast at different levels of generality, with each research product founded on quite distinct epistemological assumptions about how social phenomena can be grasped, represented, and compared by investigators. In all but the last type of research product (the context-bound narrative), it is possible for a social scientist to view his or her empirical investigation as either being shaped by a pre-existing model or theoretical discourse, or as informing new constructs, conceptual frameworks or hypotheses. Hence, the alternating arrows (1a-1b through 5a-5b) are

Figure 1.
A More Inclusive "Division of Labor"
(Selected Ideal-Typical Research Products)



* a = "inductively" generated constructs or interpretations inferred from empirical observation
 ** b = "deductively" generated concepts and propositions applied to empirical contexts

designed to capture an idealized relationship between the research product and the empirical analysis as construed by the investigator, with the "a" and "b" lines in each case respectively signifying an *ideal-typical* distinction between inductivist and deductivist understandings of how analytic constructs relate to concrete contexts.

The curvilinear shape of the graph is intended to capture the fact that the level of complexity or parsimony in a given proposition or narrative, and the extent to which it can be rendered meaningful across increasingly diverse contexts, both approach infinity along the vertical and horizontal axes respectively. At the idiographic extreme, we find context-bound narratives that are consciously designed to represent as much of that reality as possible without consideration of theoretical implications (6). At the nomothetic extreme are general theoretical statements (whether laws or causal inferences) that are designed to be operationalized and applied across a wide range of contexts, because the complex information in those contexts would be coded and consolidated into a few, select "variables" (1). The two poles thus represent the diametrically opposed ideals of parsimony and complexity. Neither may be attainable in practice, but the *pursuit* of these ideals has quite different implications for the design, conduct and outcome of social research, despite the fact that all investigators must cope with the challenge of selecting and ordering information from infinitely complex social realities.

In between the extremes, there is quite a wide range of ideal-typical research

products (lines 2 through 5), ranging from macro-causal explanations (that lack the parsimony of an elegant causal model but offer inductively generated explanatory propositions) and partial explanations (that are not universally applicable, but still suggest conjunctural factors that account for part of the variance in outcomes) to formalized narratives (that focus on a single context, but use an analytic framework to suggest a singular explanation and enable some comparison) and “thick” descriptions (that are designed to provide an interpretive understanding of a particular phenomenon or sequence but without suggesting explanatory logics or enabling systematic comparison).

The relationships (and trade-offs) among these ideal-typical research products can be better understood by considering separately the distinct choices represented by the horizontal and vertical axes in Figure 1. The horizontal axis represents a continuum defined in terms of whether the empirical *contexts* a scholar studies are viewed as “cases” within the domain of an investigation. As we proceed across from lines 1 through 6, the investigator is progressively more preoccupied with gaining a deeper understanding of the dynamics within fewer and fewer units of observation. At the extremes, the number of contexts being analyzed in a study can serve as an approximation of the attitude of the investigator towards the relationship between abstract analytic constructs and concrete contexts; parsimonious models, for example, are tested frequently over a large number of cases, while context-bound narratives tend to be in a language designed specifically to represent unique social patterns or historical sequences. But, in between (lines 2 through 5), it is significant that the actual number of units studied does *not* correspond to a particular understanding regarding how the empirical contexts are related to abstract constructs; for example, it is quite possible that one scholar will employ only two or three contexts as comparable cases to form more abstract causal propositions, while another may examine eight or ten contexts, but only for the purpose of highlighting interesting contrasts across equally “thick” narratives.⁴¹ Thus, what ultimately differentiates the choices along the horizontal axis is not the actual numbers, but the objective and attitude of the investigator towards the unit of observation.

The vertical axis reflects a related but distinct dimension for contrasting research products: the extent to which observed processes or facts are compressed into fewer “variables” for the purpose of identifying and evaluating strong correlations or recurrent patterns across time and space. The fewer the number of variables analyzed over a given number of cases, the more parsimonious the model becomes, although this will be at the expense of detailed empirical knowledge; the greater the number of vari-

41. One might, for example, juxtapose eight or ten context-sensitive narratives, with concepts only providing a (causally insignificant) framework for highlighting distinctive features; on the other hand, one may also rely only on three or four cases to generate or test much more abstract propositions or models. This point is also pertinent to the discussion below of the relative merits of different kinds of small-n comparative studies.

ables considered, the greater the chance that the interaction among these variables may approximate the complexity of narratives in any given empirical context, although this will be at the expense of parsimony. Thus, inferences generated by a multi-variate statistical analysis would appear slightly lower on the graph than those generated by a bi-variate one, but the distinction between the two would be relatively small when considered alongside more historically grounded partial explanations or thick descriptions. But, again, it is not so much the actual number of variables, but the extent to which the coding process takes observable facts out of particular contexts or cases in order to render them comparable to facts taken out of other contexts for the purpose of generating or testing progressively more stylized propositions.

Figure 1 thus represents the combined result of the two-dimensional choices and trade-offs scholars make along a continuum of research products, each accentuating different aspects of social phenomena at different levels of abstraction. The extent to which a spatio-temporally singular context is treated as a “case” of a more general phenomenon, and the manner in which contextual information is compressed into variables, result in a variety of research products, each with its distinct intellectual pay-offs and corresponding limitations.⁴² The following section seeks to relate the location of various research products in the division of labor to the role of specific methodologies in order to identify the trade-offs and interdependence among the latter.

V. Trade-Offs and Interdependence Among Selected Methods

The research products in Figure 1 above represent ideal-types and do not necessarily share any one-to-one correspondence with any particular method. A given method (whether formal modeling, statistical analysis, small-n comparison, case-study, or interpretation) may be used in different ways in the development of quite different research products; conversely, for any given research product, it is possible to “triangulate” a variety of methods at different stages of the process.⁴³ Nevertheless,

42. Excluded from this division of labor are research products that would result if a scholar were to keep increasing the value along both the horizontal and vertical axes in Figure 1. As the number of contexts and level of detail both increase, the “cases” would essentially become independent studies of separate contexts while the number of “variables” would increase to the point that they are indistinguishable from observable “facts.”

43. It is possible, for example, to employ a parsimonious deductive game theoretic model to identify causal mechanisms, while using qualitative studies to examine how well the logic of the model works in comparable cases. Similarly, a single-country study may establish a large-n analysis to establish regional variations while seeking to develop a historically specific explanation for that variation. Examples of these two approaches to triangulation include David Laitin, *Identity in Formation: The Russian Speaking Populations in the Near Abroad* (Ithaca: Cornell University Press, 1998); and Robert Putnam, *Making Democracy Work: Civic Traditions in Modern Italy* (Princeton: Princeton University Press, 1993). See also the discussion in Sidney Tarrow, “Bridging the Quantitative-Qualitative Divide in Political Science,” *American Political Science Review* 89 (June 1995): 471-74.

there are elective affinities between the *position* occupied by different research products in Figure 1 and the objectives and assumptions of particular methodologies. That is, the logic and value of a particular methodological approach can also be understood in terms of degrees of parsimony or contextual embeddedness of a proposition.

At one end lie formal modelers and proponents of large-*n* statistical methods who, despite their differences over the ontological and epistemological status of causal mechanisms, are seeking to parsimoniously capture general relationships among variables through (respectively) causal models and probabilistic inferences applicable to a large number of contexts (line *l* in Figure 1). At the other end, we find context-bound interpretations designed to produce descriptive or evocative narratives; whatever ideas may guide the selection and presentation of observations, the result is intended to provide some sense of the social processes as experienced and understood by actors within time- and space-bound contexts (6). In between are narratives of varying degrees of complexity and theoretical ambition, encompassing stylized interpretations that are context-sensitive but suggestive of some implicit comparisons and contrasts (5), case-studies designed to explore or generate broader hypotheses (4), and a range of small-*n* studies aimed at uncovering or supporting explanatory propositions, partial explanations and historical patterns (2-4).

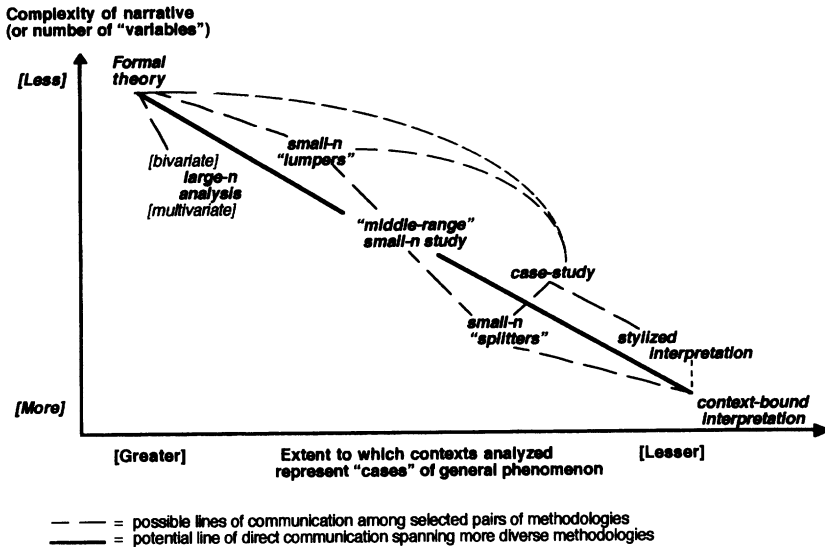
As in the case of the various research products discussed above, the strong positions taken by proponents of various methods can be reinterpreted as elements of a more agnostic appraisal of the trade-offs (and interdependence) among diverse approaches within the division of labor. Figure 2 captures the roles and comparative advantages of selected methodologies along the same kind of continuum noted above in Figure 1.

Formal modeling

Proponents of formal modeling typically view parsimony as an ideal that is too readily sacrificed in historically grounded narratives and misapplied by quantitative researchers. They take for granted that pre-existing causal forces account for much of human activity, and they therefore emphasize general theoretical models in the form of logically consistent covering laws from which causal accounts of diverse outcomes may be deduced. Inferences drawn from observable patterns in social life are therefore subordinated to the position of providing partial external validation to these axioms or models.⁴⁴ When formal modelers do turn to empirical research for external validation, they tend to prefer quantitative research (*lb* in Figure 1) given

44. On the value of general models and theoretical constructs, see James Coleman, *Foundations of Social Theory* (Cambridge: Harvard/Belnap Press, 1990); Rogowski, "The Role of Theory and Anomaly"; and Edgar Kiser and Michael Hechter, "The Role of General Theory in Comparative-Historical Sociology," *American Journal of Sociology* (1991) 97: 1-30.

Figure 2.
Interdependence and Communication Among Different Methods
(in Relation to Various Nomothetic and Idiographic Research Products)



the common preference for standard concepts and uniform coding methods across the largest possible universe of cases. In principle, however small-n comparisons, critical case-studies and formalized interpretations (i.e. 2b-4b) can also help enhance the plausibility of a model.⁴⁵ What this suggests is that formal modelers are in a position to communicate directly with quantitative researchers and (somewhat less directly) with a range of qualitative researchers, prompting them to consider the extent to which statistical regularities or discursive information can be compressed and ordered to permit systematic generalizations across contexts. This also suggests that formal modelers can potentially provide a common analytic language for translating and comparing different kinds of empirical research, thereby, enabling better communication among quantitative and qualitative researchers concerned with similar substantive questions.

This does not, however, mean that formal models can provide a unified founda-

45. For examples of formal models employed in conjunction with statistical data, comparative studies, and interpretive narratives, see, respectively, Bruce Bueno de Mesquita, *The War Trap* (New Haven: Yale University Press, 1981); Laitin, *Identity in Formation*; and R. Bates, A. Greif, M. Levi, J. Rosenthal, and B. Weingast, *Analytic Narratives* (Princeton: Princeton University Press, 1998).

tion for social science as some programmatic statements have suggested.⁴⁶ For one, many social scientists neither share the deductivist assumptions of formal modelers, nor agree on an alternative foundation for their different styles of empirical research. In particular, as qualitative information gets progressively more embedded in historical and cultural contexts (5-6), the distance between contextual information and universal categories strains the lines of meaningful communication between interpretivists and formal modelers. More generally, in the absence of empirical referents, formal models become little more than a set of games, matrices and mathematical exercises that are far removed from actual historical contexts or social processes even where rich narratives might be juxtaposed alongside the formal models from which the explanations are derived.⁴⁷ Thus, despite their emphasis on unobservable causal laws and on the priority accorded to analytic constructs over empirical analysis, formal modelers must ultimately rely on some sort of empirical analysis to make their axioms relevant. Acknowledging this fundamental interdependence without demoting empirical analysis to second-class citizenship is one of the more fundamental, and more readily practiced, features of a methodological division of labor.

Large-n statistical analysis

Most quantitative analysis also operates at a great distance from discrete historical contexts and employs constructs (variables) that can be operationalized and measured across the largest possible population of cases (large-n). The propositions they offer, however, are in the form of probabilistic inferences based on statistical regularities rather than deterministic laws based on internal logical consistency. Proponents of large-n, quantitative analysis place a high value on the explicit rules that are uniformly applied to the definition, observation and measurement of variables over many cases. Since the same mathematical operation is performed on the same variables in each case, large-n methods are deemed to be less prone to human bias or error; and to the extent that bias and error affect all empirical analysis, these can be systematically reduced through public, replicable procedures that are hard to define in qualitative research. Moreover, statistical analysis enables scholars to estab-

46. E.g., David Lalman, Joe Oppenheimer, and Piotr Swistak, "Formal Rational Choice Theory: A Cumulative Science of Politics" in *Political Science: The State of the Discipline II*, ed. Ada Finifter (Washington D.C.: American Political Science Association, 1993); and Kiser and Hechter, "The Role of General Theory."

47. Thus, in recent efforts to bridge formal rational-choice analysis and interpretive work (e.g., in Bates *et al.*, *Analytic Narratives*), much of the explanatory weight still rests on the formal model, with the narrative relegated to the role of providing contextual information. As a result, there is little possibility for appreciating the independent effects or interactions of historical factors insofar as these suggest alternatives to the causal accounts deduced from original models. This point is developed in Rudra Sil, "The Foundations of Eclecticism: The Epistemological Status of Agency, Culture, and Structure in Social Theory," *Journal of Theoretical Politics* 12, 3 (July 2000): 353-87.

lish the frequency distribution of effects over a large number of representative cases, thereby providing probabilistic estimates thought to be more reliable than deterministic general propositions offered by formal modelers or sweeping macro-historical explanations.⁴⁸ Because of these features, large-n statistical analysis, its epistemology notwithstanding, is frequently viewed by formal modelers as a reliable (even desirable) means for providing external validation to general models.⁴⁹ At the same time, from the perspective of qualitative researchers, statistical anomalies and previously unsuspected correlations can help identify recurrent historical patterns and point to new puzzles, hypotheses or interpretations to guide case-based investigations.

These advantages of large-n analysis, however, are not without trade-offs. For one, the advantages of probabilistic inferences based on the frequency distribution of effects also become liabilities when it comes to causal *mechanisms*. Statistical correlations may support or suggest explanatory propositions, but since quantitative researchers do not concern themselves with causal mechanisms and pathways, even the most sophisticated multivariate statistical techniques betray a “tendency toward abstract, and sometimes vacuous, generalizations.”⁵⁰ Moreover, as more idiographic qualitative researchers will note, statistical inferences are not directly able to make sense of actual social processes within a given historical context (the “black box” problem); the advantage gained by focusing on variables in large-n statistical analysis comes at the expense of a deep understanding of how particular beliefs, practices, personalities and social structures combine to shape outcomes or sequences.⁵¹ In addition, variable-based statistical approaches are vulnerable to “Galton’s problem,” the non-independence of cases resulting from the common effects of external or pre-existing factors; while it is possible for a self-conscious scholar to become aware of these effects, the search for probabilistic assessments based on pre-selected independent variables means that the presence of common exogenous factors can be easily missed.⁵² These limitations suggest a need to recognize the basic interdependence within a scholarly division of labor between quantitative analysis and other approaches more concerned with causal mechanisms and context-specific actions and sequences.

48. The reliability of the logic behind statistical methods is emphasized in Goldthorpe, “Current Issues”; King *et al.*, *Designing Social Inquiry*; and Stanley Lieberman, *Making It Count: The Improvement of Social Research and Theory* (Berkeley: University of California Press, 1985).

49. John Goldthorpe, “The Quantitative Analysis of Large-Scale Data Sets and Rational Action Theory: For a Sociological Alliance,” *European Sociological Review* 12 (1996): 109-26

50. Charles Ragin, *The Comparative Method: Moving Beyond Qualitative and Quantitative Strategies* (Berkeley: University of California Press, 1987), 69.

51. See also Rueschemeyer’s discussion of this problem in relation to quantitative studies of democracy in Dietrich Rueschemeyer, “Different Methods—Contradictory Results? Research on Development and Democracy,” in *Issues and Alternatives in Comparative Social Research*, ed. Charles Ragin (Leiden: E. J. Brill, 1991), 26-28.

52. Eugene Hammel, “The Comparative Method in Anthropological Perspective,” *Comparative Studies in Society and History* 22 (1980): 145-55.

Context-bound interpretation

At the other end of the continuum, the attention to the complex character of time- and space-bound phenomena is most pronounced in strictly idiographic *context-bound* interpretations (6). Such narratives are typically founded on a more relativistic epistemology in which generalizations across contexts are deemed impossible given the incommensurability of the meanings actors attach to phenomena they experience. The interpretivist would still need to make important decisions about how to represent an infinitely complex social world in a narrative of limited length; however, to the extent that pre-existing ideas and categories may influence the narrative, these are seen as societal discourses (rather than scientific constructs) mediated by the subjective consciousness of the interpreter. This limits the primary aim of context-bound interpretation to the translation, however incomplete and subjective, of the meanings actors ascribe to their actions, environments and experiences within their social contexts.⁵³

Nevertheless, such accounts do claim a place within social science disciplines. Even if their proponents themselves decline to view their work as “scientific,” they usually do share with their positivist colleagues at least an abstract commitment to uncovering *some* aspect of social phenomena, however subjective the process. Even context-bound efforts at uncovering hidden meanings do involve crucial insights into the relative significance of particular actions, beliefs, or events vis-à-vis each other. More importantly, context-bound interpretations are ultimately informed by a set of detailed empirical observations, some of which are likely to coincide with observations relevant to more nomothetically oriented case analysis. At various stages of their research, formal modelers, quantitative researchers and small-n comparativists may all find that the detailed sequences and patterns in such interpretive narratives may help refine the operationalization of, and causal weight attached to, a particular variable for a given case. For this reason alone it is worthwhile to include context-bound interpretation as a valid and potentially useful approach within the social science division of labor.

The problem is that the gulf separating the most relativistic idiographic account and formal or statistical nomothetic analysis is a formidable one, marked by sharply differing epistemological orientations towards the validity of analytic constructs. Thus, unless context-bound interpretive accounts are transformed into more formalized narratives (which may be invoked in case-studies or small-n comparisons), or unless general models are represented as stories about particular contexts, it is difficult to imagine how the insights contained in a purely idiographic narrative can

53. This is why there is no “6b” line in Figure 1. Translations might provide some intuitive grasp of the subjective experiences of actors, but they would not be in a language designed to facilitate comparison to the experiences of actors in other contexts. See Taylor, “Interpretation”; and Paul Ricoeur, “The Model of the Text: Meaningful Action Considered as Text,” *Social Research* 38 (1971): 529-55.

be made meaningful to the abstract propositions suggested by a formal model or statistical inference.⁵⁴ Hence, the methods corresponding to the wide-ranging space in between these extremes (2 through 5) not only have their own particular strengths and weaknesses, but they also serve as links in enabling communication between the extremes (1 and 6).

Case-studies and Stylized Interpretations

Unlike context-bound interpretations (6), the insights contained in case-studies (4) and stylized interpretations (5) are framed in such a way as to permit at least implicit comparisons across contexts. Case studies and stylized interpretive studies may be distinguished from each other according to the objectives and epistemological orientation of the investigator: the former are typically designed to support nomothetic endeavors and encompass what Lijphart describes as the “hypothesis-generating” (4a) as well as “theory-confirming” or “theory-infirming” types (4b) of case analysis; the latter reflect an idiographic commitment and correspond more closely to Lijphart’s “atheoretical” and “interpretive” case-studies (5a or 5b).⁵⁵ As the distinction between nomothetic and idiographic analyses is actually one of degrees, however, I consider case-studies and stylized interpretations in tandem here to examine how they complement formal or statistical methods.

Case studies and stylized interpretations offer valuable insights and information about how persons, beliefs, practices, events, and social relations combine to produce particular historical sequences or recurrent patterns, a feat that cannot be duplicated by variable-based approaches. A survey or coded interview data involving a large sample of workers, for example, may enable large-scale comparisons of levels of job alienation or industrial conflict, but would offer little insight into the nature of daily work life or the complex relationships among different categories of workers and managers on the shopfloor.⁵⁶ Case-studies in nomothetic analysis are frequently designed to systematically examine the applicability of pre-established theories, particularly through the analysis of “must fit” cases that may be invoked to

54. Some scholars have recently suggested that rich narratives can complement general models. The understandings of “narrative” and “interpretation” embraced in these works, however, reflect a preference for more stylized or “thick” interpretations rather than the context-bound accounts discussed here. See Bates *et al.*, *Analytic Narratives*, 10-14; and Edgar Kiser, “The Revival of Narrative in Historical Sociology: What Rational Choice Theory Can Contribute,” *Politics and Society* 24 (September 1996): 249-71.

55. Using Eckstein’s categories, these correspond approximately to “heuristic” case-study (4a), “crucial case analysis” or “plausibility probes” (4b), idiographic-configurative analysis (5a) and disciplined-configurative (5b). See Arend Lijphart, “Comparative Politics and the Comparative Method,” *American Political Science Review* 65 (1971): 682-93; and Harry Eckstein, “Case Study and Theory in Political Science,” in *Handbook of Political Science*, ed. Fred Greenstein and Nelson Posby (Reading: Addison-Wesley, 1975), 96-116.

56. Anthony Orum, Joe Feagin, and Gideon Sjoberg, “Introduction: The Nature of Case Study,” in *A Case for the Case Study*, ed. Feagin, Orum and Sjoberg (Chapel Hill: University of North Carolina Press, 1991), 22.

refine or demonstrate existing models or propositions (4b).⁵⁷ Similarly, despite the idiographic commitments of most narrativists, the vocabulary or explanatory logics of existing theoretical models or hypotheses could certainly provide the basis for the prioritization of “facts” in stylized interpretations (5b).⁵⁸

At the same time, detailed studies of single contexts, albeit lacking the quasi-experimental control of statistical analysis, may enable scholars to “extract new ideas at close range,” pointing to original hypotheses and new research agendas.⁵⁹ Case studies, for example, can yield a preliminary set of concepts and hypotheses that can be subsequently refined through new cases studies in a “building block” approach, or through the introduction of heuristic assumptions in the form of “imaginary experiments” (4a).⁶⁰ The rich data in stylized interpretations, although not conducive to the formation of broad analytic concepts, may be invoked to demonstrate how particular experiences or ideals are understood in a given context, while prompting more theoretically ambitious colleagues to treat these understandings as causally significant factors (5a).⁶¹ In these various ways, both case studies and single-context interpretations serve a critical function in the division of labor: they both provide “the detailed and rich data [that] permit the analyst to develop a solid empirical basis for specific concepts and generalizations.”⁶²

There remains a question, however, as to just how significant a contribution case-studies or stylized interpretations can *independently* make to the cumulation of theoretical and empirical knowledge across contexts. In testing theories or applying models, studies of single contexts are much less definitive than multivariate statistical analysis since, as critics of case-study methods note, even “small measure-

57. See, e.g., the application of modernization theory in Neil Smelser, *Social Change in the Industrial Revolution* (Chicago: University of Chicago Press, 1959), and of rational-choice theory in Dennis Chong, *Collective Action and the Civil Rights Movement* (Chicago: University of Chicago Press, 1991).

58. As Kiser (“The Revival of Narratives,” 251-52) notes, this is especially true for rational-choice theorists who share with many interpretivists key assumptions about agency, temporality and pathways. However, there is also a danger that the richness of the narratives maybe sacrificed as their content is adapted to conform to models derived from a given set of axioms and motivational assumptions. This tendency is evident in Bates *et al.*, *Analytic Narratives*.

59. David Collier, “Data, Field Work and Extracting New Ideas at Close Range,” *APSA-CP* (Winter 1999): 4. Collier cites the classic example of the widely used concept of “corporatism” which emerged out of research on a single country; see Philippe Schmitter, *Interest Conflict and Political Change in Brazil* (Stanford: Stanford University Press, 1971).

60. On the “building block” approach, see Howard S. Becker, “Social Observation and Case Studies,” *International Encyclopedia of the Social Sciences*, 11 (1968): 232-38. On “imaginary experiments,” see Max Weber, “Basic Sociological Terms,” in *Understanding and Social Inquiry*, ed. Fred Dallmayr and Thomas McCarthy (Notre Dame: University of Notre Dame Press, 1977), 44.

61. The impact of Clifford Geertz’s work (e.g., the essays in *The Interpretation of Cultures*) on political science and sociology is telling in this regard. For a more recent example of this approach, see Frederic Schaffer, *Democracy in Translation: Understanding Politics in an Unfamiliar Culture* (Ithaca: Cornell University Press, 1998).

62. Orum *et al.*, “The Nature of Case Study,” 7.

ment errors in a particular case can lead to results that are flatly preposterous.⁶³ In terms of independently producing new conceptual frameworks and original hypotheses, studies of single contexts are especially vulnerable to the inductivist fallacy; while investigators may consider hypotheticals or speculate about the significance of various factors in other cases, the absence of controlled comparisons severely circumscribes the applicability of the concepts and propositions to be extracted from a single case study or interpretation. These limitations do not generally erode the value of such approaches, but they do reflect unavoidable trade-offs that accompany the necessary search for information pertaining to social processes within given contexts.

Small-n “lumpers” and “splitters”

Given the kinds of limitations present at the two ends of the spectrum in Figure 2, small-n studies may be an effective means for combining some of the complexity of case studies with some degree of quasi-experimental control through the comparative method. While small-n comparison was previously considered by many to be a valuable preliminary step in establishing the plausibility of existing hypotheses prior to designing an elaborate large-n test,⁶⁴ most contemporary comparativists see small-n comparison as an important approach in its own right, valuable in different ways at all stages of empirical research and theory-construction.⁶⁵ Since the gulf separating case-studies or stylized interpretations from formal models or statistical analysis is substantial, however, it is not surprising that there is significant variation in the design and purpose of small-n research. It is thus worthwhile to examine more carefully the different trade-offs faced by “splitters,” who emphasize the distinctive features of historical cases, and “lumpers,” who rely on controlled comparisons to demonstrate the viability of pre-existing theoretical propositions (2b) or to infer causally significant configurations (2a).⁶⁶

“Splitters” are small-n comparativists who employ what Skocpol and Somers have termed a “contrast of contexts” strategy.⁶⁷ This approach is designed to be attentive to historical particularities in tracing the emergence of comparable phe-

63. Bueno de Mesquita, “Toward a Scientific Understanding,” 127.

64. Lijphart, “Comparative Politics,” *passim*.

65. David Collier, “Building a Disciplined, Rigorous Center in Comparative Politics,” *APSA-CP: Newsletter of the APSA Organized Section in Comparative Politics* 10, 2 (Summer 1999), 1.

66. The lumpers/splitter distinction is adapted from the methodological discussion in David and Ruth Collier, *Shaping the Political Arena: Critical Junctures, the Labor Movement and Regime Dynamics in Latin America* (Princeton: Princeton University Press, 1991), 1-32; it also corresponds to the opposition of “universalizing comparisons” and “individualizing comparisons” in Charles Tilly, *Big Structures, Large Processes, Huge Comparisons* (New York: Russell Sage, 1984), 81.

67. Theda Skocpol and Margaret Somers, “The Uses of Comparative History in Macrosocial Inquiry,” *Comparative Studies in Society and History* 2, 2 (April 1980): 174-97.

nomena, relying generally on the logic of Mill's "method of difference" but heeding Mill's cautions about the reliability of such methods for generating causal explanations. In some cases, the concepts referring to historical outcomes may be general enough to permit contrasts across distinct causal sequences or pathways, but the explanatory concepts are not general enough to permit broader descriptive or causal inferences since even the units of comparison are not taken for granted.⁶⁸ Other small-n splitters may not concern themselves with causal connections at all, but nevertheless employ just enough standard categories and concepts so as to enable the investigator to demonstrate unexpected contrasts across apparently similar historical contexts. In effect, small-n splitters end up producing a series of case-studies (where causal connections are posited) or stylized interpretations (where they are absent), each couched in a language that permits detailed concrete comparisons but not abstract propositions.⁶⁹

Thus, within the broader division of labor, the trade-offs of contrast-oriented small-n comparison ends up approximating those evident in case-studies and stylized interpretations (4-5), albeit the simultaneous consideration of several cases puts the splitters in a position to more effectively challenge general theories or causal inferences. While their modest theoretical pretensions keep them from getting trapped by the inductivist fallacy or from having to distort historical facts in search of general theory, their emphasis on coherent narratives is what ultimately frustrates small-n lumpers and more general theorists. As Skocpol and Somers note, their ". . . descriptive holism precludes the development of explanatory arguments, even when these are implicitly present, crying to be drawn out of the comparative-historical materials."⁷⁰ As in the case of single-case analyses or stylized interpretations, the richness of history and the complexity of social processes comes at the expense of theoretical significance or causal knowledge.

Small-n lumpers (2) join the splitters in opposing the ahistorical character of deductive models and statistical analysis; unlike the splitters, however, they seek to apply controlled comparisons modeled after Mill's *joint* methods of agreement and differences or his method of concomitant variation to develop or defend theoretically significant propositions. The approaches of small-n lumpers include what Skocpol and Somers have termed the "parallel demonstration" and "macro-causal" types of comparison. The former is designed to interpret a few select cases according to an existing system of concepts and propositions to demonstrate how well historical processes in several cases "fit" the causal pathways suggested in an existing theoret-

68. This is the approach favored in Charles Tilly, "Means and Ends of Comparison in Macrosociology," *Comparative Social Research* 16 (1997); 43-53.

69. This is evident in the comparative approach adopted in Clifford Geertz, *Islam Observed: Religious Development in Morocco and Indonesia* (Chicago: University of Chicago Press, 1971); Reinhard Bendix, *Kings or People: Power and the Mandate to Rule* (Berkeley: University of California Press, 1978); and Liah Greenfield, *Nationalism: Five Roads to Modernity* (Cambridge: Harvard University Press, 1992).

70. Skocpol and Somers, "The Uses of Comparative History," 193.

ical model.⁷¹ When small-n studies are used in this manner, however, there is a danger that Mill's method of agreement may be privileged over the method of differences; as a result, each of the individual case-studies would be interpreted in a way that none seriously challenges a pre-given theoretical premise except perhaps at the margins.⁷² In this event, small-n "parallel demonstrations" might become no different from a collection of theory-confirming or -infirming case studies (4b), but without the benefits of a "must fit" crucial-case and with a much greater danger of selection bias. As a result, large-n statistical analyses (1b) and the analysis of a crucial "must fit" case (4b) may better serve the purpose of testing propositions than can lumpers of this variety although the latter can suggest more refinements to hypotheses.

"Macro-causal" lumpers are neither fascinated by the particular histories of the cases they study (as splitters tend to be), nor committed to the defense or exposition of a general theory (as "parallel demonstration" lumpers tend to be). Rather, macro-causal lumpers select cases for their collective value in inferring initial causal conditions (but, significantly, not mechanisms) from the presence and absence of historical configurations where outcomes vary. That is, they tend to be far more deliberate in applying Mill's joint methods of agreement and differences to uncover the necessary causal combinations leading to general categories of outcomes (such as democracy or revolution). While macro-causal comparativists and large-n quantitative researchers share an empiricist skepticism with regard to the status of unobserved causal mechanisms, the former still emphasize the importance of studying history (at the expense of parsimony or theoretical elegance); thus, rather than general models or probabilistic assertions, they prefer to identify effects of historical configurations in order to develop falsifiable propositions about the latter's causal significance in comparable contexts. Thus, macro-causal approaches appear to be more effective than other small-n comparativists in employing controlled comparisons to combine historically specific accounts into falsifiable propositions about the effects of particular initial conditions.⁷³

The advantages claimed by small-n lumpers, however, come at a price. For one, despite their attention to variations in historical patterns, they risk being too casual in the handling of complex historical records, taking for granted certain historical facts while ignoring alternative interpretations and distorting other facts in the inter-

71. Skocpol and Somers ("The Uses of Comparative History") cite S. N. Eisenstadt, *The Political Systems of Empires* (New York: Free Press, 1963) and Jeffrey Paige, *Agrarian Revolution: Social Movements and Export Agriculture in the Underdeveloped World* (New York: Free Press, 1975), as examples.

72. This was the case with comparative studies of "modernization" in the 1950s-70s that took for granted the assumptions of evolutionary change and structural-functionalism in interpreting differences among countries in terms of degrees of "modernization." See, e.g., Marion Levy, "Contrasting Factors in the Modernization of China and Japan," *Economic Development and Cultural Change* 2 (1953); and Alex Inkeles and David Smith, *Becoming Modern: Individual Change in Six Countries* (Cambridge: Harvard University Press, 1974).

73. Skocpol and Somers, "The Uses of Comparative History," passim.

est of comparability.⁷⁴ Moreover, in the case of macro-causal comparative approaches, the focus on the effects of initial causal conditions does not prevent emergent theoretical propositions from falling prey to the familiar problem of “too many variables and not enough cases” as they seek to code information and draw inferences based on a large number of potentially relevant historical factors and an even larger number of possible causal combinations.⁷⁵ While the first problem has more to do with the scholar’s own self-conscious use of materials, the latter problem is less simple to deal with. Lijphart has offered a strategy for selecting more similar cases and redefining variables so as to reduce the number and “property space” of variables;⁷⁶ Ragin seeks to expand the number of cases while compressing variables through a Boolean truth table to record the presence or absence of a factor across cases;⁷⁷ and Savolainen has advocated the use of Mill’s method of difference for the limited purpose of alternative causal explanations.⁷⁸ These strategies, however, do not break new ground. Lijphart’s and Ragin’s suggestions do not clarify how to avoid the “small-n, many-v problem” when comparing complex historical configurations that many small-n researchers focus on; and they do not address the challenge of coding and compressing data concerning factors that can be present in *degrees* (e.g., the degree of political stability). Moreover, as Lieberman notes, attempts to invoke the method of difference to eliminate single causes still yields deterministic propositions that involve spurious inferences since the frequency distribution of effects is not considered.⁷⁹

In the end, the different problems of small-n research are better understood neither as fatal flaws nor as challenges to be solved by increasing cases, invoking formal models, or improving historical research, but as unavoidable *trade-offs* that accompany the distinctive intellectual gains produced by each of the small-n approaches. While one might concur with Goldthorpe that Millian logic, Boolean algebra and other techniques cannot overcome the problem of “too many variables and not enough cases,”⁸⁰ it is also important to recognize that small-n comparative approaches have their own particular advantages that would be lost if they were charged with adding progressively more information to their analyses.

74. Michael Burawoy, “Two Methods in Search of Science: Skocpol versus Trotsky,” *Theory and Society* 18 (1989): 759-805; and Ian Lustick, “History, Historiography, and Political Science: Multiple Historical Records and the Problem of Selection Bias,” *American Political Science Review* 90, 3 (September 1996): 605-18.

75. See Goldthorpe, “Current Issues,” 8; Stanley Lieberman, “Small N’s and Big Conclusions,” in *What Is A Case?* ed. Ragin and Becker, 105-18.

76. Lijphart, “Comparative Politics,” *passim*.

77. Ragin, *The Comparative Method*; and his *Constructing Social Research*.

78. Jukka Savolainen, “The Rationality of Drawing Big Conclusions Based on Small Samples: In Defense of Mill’s Methods,” *Social Forces* 72 (1994): 1217-24.

79. Stanley Lieberman, “More on the Uneasy Case for Using Mill-Type Methods in Small-N Comparative Studies,” *Social Forces* 72 (1994): 1227.

80. Goldthorpe, “Current Issues,” 8.

VI. Toward “Organic Solidarity”: Middle-Range Comparative-History As Mediator?

The remaining category of small-n comparative study, “middle-range” historical comparisons (3), represents an ideal-typical center between small-n lumpers and splitters, and hence between nomothetic quests for parsimony and idiographic representations of complexity. The reason I conclude with this loosely defined category is neither to identify a new set of trade-offs, nor to confer upon it a privileged position linked to any inherent methodological superiority, but rather to emphasize its distinctive *practical* significance within the division of labor outlined above.

Middle-range comparative-historical studies may come in a variety of forms—some examining historical conjunctures in search of partial explanations for recurrent patterns or path-dependent accounts of variations across comparable cases (3a), others probing the relevance of existing theoretical constructs or contesting more general hypotheses (3b). What ultimately distinguishes this middle-range comparative-history, however, is its epistemological agnosticism. That is, middle-range comparative-history is founded on a healthy skepticism with regard to logical positivist assumptions about universal causal mechanisms and the empiricist assumptions about the meaningfulness of probabilistic inferences; at the same time, in contrast to the stark relativism of many interpretivists, there is still an openness to the possibility of causal connections, whether observable or not, that shape similar and divergent historical pathways across comparable contexts.⁸¹ While this epistemological center may not be associated with specific methodological injunctions, this type of middle-range small-n comparison may be identified in terms of what it is not: middle-range comparative-historical studies neither takes for granted pre-existing theoretical categories or propositions as is often the case with formal models, “parallel demonstration” comparisons and theory-confirming or -infirmiting case studies (1b-4b), nor seeks to infer general causal explanations from observed relationships between particular outcomes and historical configurations (2a), nor merely juxtaposes semi-formalized narratives to highlight the uniqueness of contexts (4a-5a).

The resulting research product is likely to be sufficiently modest in terms of its theoretical claims so as to avoid the charges of spurious causal inference frequently lodged against macro-causal comparativists. It is also likely to be sufficiently attentive to the complexity of historical processes and the variety of historical records so as to avoid the charges of distorting “history” or ignoring “context.” At the same

81. While this position is comparable to the “critical realism” or “relational realism” advocated in Steinmetz (“Critical Realism”) and Somers (“We’re No Angels”), my defense of this “center” rests not on its value as a unifying foundation for social science, but rather on its potential function in diffusing the sharp, multi-faceted oppositions seen in contemporary methodological debates. See Sil, “Against Epistemological Absolutism.”

time, it is sufficiently framed in terms of broad concepts and variables so as help translate the complex factors and configurations identified in case-studies and context-specific narratives (4, 5) into more general concepts within broader theoretical frameworks and models (1, 2). In effect, middle-range comparative-historical studies treat the units of comparison simultaneously as independent contexts and comparable cases of a broader phenomenon, while relying on historically situated comparisons to generate partial explanations of historical outcomes and contingent hypotheses about broader categories of phenomena.⁸²

However difficult it might be to specifically identify or consciously design such a middle-range comparative study, the essential point here is that the very *attempt* to do so paves the way for improving the communication between—and illuminating the complementarities among—the other intellectual endeavors identified within a more flexible division of labor. Some level of communication and mutual respect may exist among those close to either the nomothetic or the idiographic extremes (that is, among formal modelers and quantitative researchers at one end, and among those conducting case- or context-specific empirical research at the other). As Figure 2 demonstrates, however, the communication links can get strained in crossing the rather wide gap in the middle. Small-n comparative studies, taken collectively, might be able to plug this gap effectively, but the significant differences in objectives and epistemologies among (nomothetic) lumpers and (idiographic) splitters still leave a substantial void. This is why the epistemological agnosticism identified with middle-range small-n comparativists puts them in a position to simultaneously appreciate the importance of historical complexity and context alongside the theoretical questions and scientific inferences drawn by their more nomothetically oriented colleagues. As a result, middle-range small-n comparative-historical studies, given their location on the continuum, are in a position to facilitate greater communication not only between splitters and lumpers, but by extension, also among those partial to more nomothetic and more idiographic modes of analysis. This mediating role does not accord middle-range comparative studies any special intellectual status, but it does highlight its *practical* value in enabling more meaningful communication among a wide range of other approaches generally thought to produce incommensurable research products.

The purpose of this article has not been to either elevate or diminish the status of any particular method or research product, or to discount the significance of methodological debates in general. Rather, the aim has been, first, to highlight the distinctive intellectual pay-offs of diverse approaches that are often obscured by the

82. See, e.g., the approach taken in my own comparative work, *Managing 'Modernity': Work, Community, and Authority in Late-Industrializing Japan and Russia* (Ann Arbor: University of Michigan Press, forthcoming).

stark oppositions invoked in such debates; and second, to specify how different kinds of research products and methodologies complement each other by generating particular *kinds* of insights into a given problem. This does not require that we discount the significance of the range of issues over which practitioners of social science disagree; in fact, their differences can reflect quite profound and meaningful commitments to competing epistemological positions. Given the inconclusive character of philosophical debate, however, practical necessity dictates that scholars learn to appreciate the contribution of those whose research products and methodological tools are founded on alternative objectives and priorities. A common awareness of the trade-offs—and, thus, interdependence—among particular research products and methodologies, can transform the “mechanical solidarity” scholars now exhibit *within* research traditions into the “organic solidarity” so sorely needed among the community of social scientists as a whole.