

A Statistician Strikes Out: In Defense of Genuine Methodological Diversity

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Allan Roth, the team statistician...recorded every pitch of every game on a sheet of graph paper and tabulated his data in a cross complexity of techniques....Rickey had hired Roth to supply information to the manager. If Shuba never hit lefthanders' curves, then sit him down against Ken Raffensberger [a left-handed pitcher who threw curveballs]. Dressen [the manager] regarded Roth and his bodies of facts as threats. "I got my own way of figurin'," he said. Dressen soared on intuition and probably feared that figures might wither his expertise (Kahn 1972, 126-127).

There is an old argument in professional baseball about the best way to manage a team to victory. On one side of this debate stand the traditionalists, trusting to their instincts in making decisions about which players to draft and retain and which to put in the game at key moments. On the other side stand the aficionados of "sabermetrics," the highly technical practice of breaking every aspect of a player's performance down into quantifiable components, and making management decisions on the basis of numerical projections and analyses. Each of these two positions leads to very different ways of evaluating players, with traditionalists emphasizing subjective judgments about a player's potential and sabermetricians focusing on measured past performance (Lewis 2003, 30-32). The jury remains out on which of these approaches is the superior one, although the sabermetric approach seems to be gaining popularity with a number of teams.

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Many recent discussions of “methodological diversity” presume that the debate within political science is, in essence, the same as the debate within baseball: numbers or gut instincts? The putatively pluralistic answer offered by scholars like David Laitin (Laitin 2003) and Gary King, Robert Keohane, and Sidney Verba (King, Keohane, and Verba 1994) turns out, on closer examination, to be not particularly pluralistic at all. King, Keohane, and Verba suggest that there is no essential difference between “qualitative” and “quantitative” research, and that therefore all scholars “cannot afford to ignore sources of bias and inefficiency created by methodologically unreflective research designs” (King, Keohane, and Verba 1994, 229). For Laitin, case narratives based on practical experience with a situation can serve to test formal models, provide causal mechanisms, and plot residuals in preparation for future formalization (Laitin 2003, 179). Likewise, statistics and formal modeling have their assigned tasks in the social scientific enterprise; this “tripartite methodology...is the best defense we have against error and the surest hope for valid inference” (*ibid.*: 169). Hence, underlying any surface-level diversity of particular kinds of research is a single, unitary, and uncontestable logic of inquiry.

In statements such as these, we see the partisans of a particular mode of social inquiry—a *statistical-comparative* mode, in which all valid inference is exhausted by the search for cross-case correlations—stepping to the plate and taking critical swings against an emergent alternative position based on dialogue and diversity. These critical swings appear to be calls for tolerance and multiplicity, but upon closer examination, they outline a Procrustean bed into which more interpretive and relational modes of social inquiry—to say nothing of “critical” or *phronetic* notions of social science (Flyvbjerg 2001)—fit uneasily, if at all. This faux diversity rests on three subordinate positions:

1. the assertion, usually never demonstrated, that all social inquiry has or should have the same goals;
2. the notion that social life is a closed system within which constant conjunctions between independent and dependent variables obtain; and
3. the devaluing of open dialogue in favor of closed consensus in the matter of knowledge construction.

All three of these positions depend on a misrepresentation of the current political science debate, which is actually quite different from, and more far-reaching than, the baseball debate about numbers vs. gut instincts. As such, these three critical swings miss the mark.

Methods and Methodologies

In baseball, as in other organized sports, there is only one goal for a manager: winning games. Even teams that are “rebuilding,” or are dumping their high-priced veteran players in favor of lower-salaried rookies, have the objective of winning baseball games at some point in the future firmly in mind. Statistical-comparative methodologists presume that what is true of baseball is equally true of political science, with “valid inference” standing in place of a winning record as the sole arbiter of a successful effort (King, Keohane, and Verba 1994, 34; Laitin 2003, 166). This presumption underpins their professed tolerance for multiple methods in the study of social reality, in that many different techniques (including quantitative statistics, formal modeling, and narrative) are *only* welcome to contribute to the analysis of social life *if* they accept and contribute to this single goal (King, Keohane, and Verba 1994, 56, 75; Laitin 2003, 181, 179). So everyone can play, as long as they agree in advance to play by the same rules.

But these rules, centering on the disclosure of cross-case correlations between independent and dependent variables, leave only a sharply limited space for non-statistical techniques. For example, Laitin's apparently tolerant tripartite scientific method relies on formal models primarily as a way to generate falsifiable hypotheses, which hardly exhausts the modeling enterprise. As for narrative, Laitin reduces its role to, in effect, the provision of local color for spare formal analyses and the factual presentation of how independent and dependent variables are linked, along with the description of unexplained variance in preparation for future systematic analysis (Laitin 2003, 177-179).

Partisans of the statistical-comparative approach also have a difficult time appreciating non-statistical modes of inquiry in their own terms. For example, Laitin propounds a highly selective reading of Bent Flyvbjerg's call for a more phronetic social science as being merely a call for sustained case narratives, rather than a call for social science to contribute "to the reflexive analysis and discussion of values and interests" (Flyvbjerg 2001, 3). Laitin seems to regard the goal of disclosing cross-case correlations to be self-evidently equivalent to the notion of "valid inference," and devotes no space in his article to *justifying* this goal; hence it is not surprising that he devotes no space to engaging different specifications of the goals of social inquiry. Similarly, King, Keohane, and Verba's discussion of Clifford Geertz's anthropological point that one cannot understand the meaning of an action without immersing oneself in the local situation (Geertz 1973, 6-7) reduces the issues involved to the question of whether particular social actions are correlated in a systematic fashion (King, Keohane, and Verba 1994, 38-40). Again, there is little or no *justification* provided for this position.

But unlike in baseball, not everyone in political science actually has the same goals. To continue the metaphor, some people are less interested in managing a team to victory by looking

for the predictors of overall team success than they are in studying how the social structure of baseball—the rules which are, by necessity, taken for granted by managers engaged in the act of managing a team—came to be the way that they are (Gould 2003; Markovits and Hellerman 2001). Others are more interested in tracing the links between baseball and other elements of public culture (Seidel 1988). These are different kinds of questions. Looking for well-verified correlations between factors, explaining how domains of social life congeal and disperse, and tracing meaningful relations according to an abstract specification of what is important about a situation are *qualitatively different* analytical exercises. All are empirical (rather than normative) questions, and all demand a relatively rigorous application of theoretical precepts to masses of information; in this broader (Weberian) sense, they are all “scientific” questions (Weber 1949). These three questions should not be shoehorned into a single conception of social inquiry.

I have not chosen these three questions at random. Each is an example of the kind of question that would be asked by Laitin’s three components of science if we considered these three as *methodologies* rather than simply as *methods*. The distinction is critical: methods are techniques for gathering and gaining access to bits of data, whereas methodologies are “grounded...in the history of political or social scientific thought and/or in related epistemological-ontological assumptions taken up in the philosophy of (social) science and embedded in the research process” (Schwartz-Shea and Yanow 2002, 459-460; see also Shotter 1993). As such, methodological considerations are more basic than questions of method, “for once a methodology is adopted, the choice of methods becomes merely a tactical matter” (Waltz 1979, 13). In a sense, methodology (often called simply “theory” in older works) *constructs* an analytical world out of the veritable infinity of data characteristic of any particular situation, by grounding “a frame of reference that fixes the order and relevance of the facts” in specific

ontological and epistemological considerations. In David Easton's pithy formulation, "A fact is a particular ordering of reality in terms of a theoretical interest," and methodological considerations are never absent from concrete empirical research (Easton [1953] 1971, 53).

Although never absent, methodological considerations can be more or less explicit; in Laitin's statement, the methodology is almost completely implicit, and as such is asserted rather than demonstrated. But from his criticism of Tambiah's account of the Sri Lankan civil war (Laitin 2003, 173-175) it is apparent that Laitin's preferred methodology is a *statistical-comparative* one, seeking to identify independent variables that are efficient predictors of outcomes across cases. This seemingly innocuous position actually supervenes on a variety of contentious metaphysical assumptions, including the stability of entities and the uniformity of causal effects, that Andrew Abbott has gathered up under the heading of "general linear reality" (Abbott 1988).

But there are other alternatives. For example, take the second component of Laitin's tripartite approach to science: formal modeling. What many methodologists fail to take seriously is that models are quintessentially *interpretive* in character, participating in a rather different exercise than that advanced by statistical-comparative methodology. Models sort data into relevant categories, with relevance specified by the model itself, and provide a baseline from which to render phenomena comprehensible. Empirical findings cannot "falsify" the model, which is ideal-typical rather than descriptive; rather, discrepant information provides an opportunity to further calibrate the model, so that the former discrepancies become newly comprehensible (Hardin 1995, 91-100).² Also, the truth criterion for a formal model is its logical

² In this light, the position that disconfirming or discrepant evidence should lead to a reformulated model, which is sometimes claimed as an innovative methodological position (e.g. Bates et al. 1998, 16), appears less radical and more

soundness, and not the correspondence between its theoretical terms and the empirical world. Hence a model can be logically true but practically useless—a situation that does not arise when operating in a statistical-comparative methodology (Waltz 1979, 71-72).

Seen in this way, the use of a model has more in common with the act of interpreting a text than it does with the act of testing a hypothesis. The point is to make connections plain and comprehensible, rather than to look for law-like patterns of correlation. This is particularly true of models that make presumptions about the contents of individual minds, such as the rational-choice accounts of human behavior often implicitly equated with “formal models” (e.g. Laitin 2003, 176-178). Such models have an unfalsifiable hermeneutic core, a “model of man” which guides and grounds the effort to explicate a plethora of concrete situations; this core can never be subjected to falsification in the course of an investigation, since it serves as the centerpiece of the analytical apparatus generating the findings in the first place (Moon 1975). But the same is true of all forms of modeling, including systems models that make no presumptions about the contents of individual minds: the exercise of applying a model to a case or set of cases is a particularly disciplined form of interpretation rather than an exercise in falsification.

Likewise, Laitin’s third component—narrative—can be easily understood as a methodology rather than as a method. Setting aside for a moment the merely descriptive aspects of Laitin’s notion of narrative, we are left with a focus on causal mechanisms. But Laitin’s declaration that to focus on mechanisms means to “link independent and dependent variables...basically showing how favorable conditions from a statistical sense translate into outcomes” (Laitin 2003, 176-177) ignores a substantial body of recent *relational* work on

conventional—at least, more conventional for a *methodology* of modeling, rather than for a *method* of modeling serving as an adjunct to a statistical-comparative methodology.

mechanisms that proceeds in a very different direction. The central thrust of this work is that causal mechanisms are qualitatively different than intervening variables linking inputs and outcomes, but exercise their impact in unique configurations. The goal of relational analysis is to show how a number of robust mechanisms come together in a particular case so as to produce a unique outcome. What replicates across cases, then, is *not* a systematic correlation between inputs and outputs, but particular causal mechanisms like brokerage and certification (McAdam, Tarrow, and Tilly 2001, 29-34, 142-148; see also Tilly 1995). The role of narrative when it comes to these mechanisms is not simply to trace linkages between factors, but to demonstrate how concrete outcomes are produced through concatenations of these mechanisms and processes. Methodologically, this is a relational approach to the study of social life, privileging mechanisms and processes rather than the putatively rational decisions of self-propelled actors or the homogenous effects of independent causal factors (Emirbayer 1997; Jackson and Nexon 2002; Tilly 2002, 73-75).

By suggesting that Laitin's three "methods" are better thought of as three divergent methodologies, I do not mean to dismiss any of the three as *prima facie* invalid; nor do I mean to suggest that Laitin's effort to subsume interpretive and relational techniques under a statistical-comparative methodology is necessarily inappropriate. But it is incumbent on Laitin to *argue for* his preferred methodology, instead of simply *assuming* its transcendental validity and recommending that a "procedure's flowerbed should no longer be cultivated within the discipline in which it was originally seeded" if it does not advance his preferred goals (Laitin 2003, 179). This is a familiar strategy among partisans of statistical-comparative methodology, who frequently declare their methodological orientation instead of arguing for it—as though that orientation were simply and self-evidently equivalent to "social science" per se (King, Keohane,

and Verba 1994, 7).

Where statistical-comparative partisans value only a diversity of *methods*, we could instead prize *methodological* diversity. In order to really address this issue, statistical-comparative partisans would have to begin by acknowledging that not every social researcher wants to develop winning strategies for a baseball team.

Open and Closed Systems

In a standard nine-inning baseball game, a team must send men to the plate a minimum of 25 times.³ Each team plays 162 games during the regular season, for a minimum total of 4,050 plate appearances per season. And there are 30 major league baseball teams, so that an annual season generates a minimum of 121,500 plate appearances—a “large n” by almost anyone’s standards. But what makes baseball an ideal situation for statistical-comparative analysis is not merely that a lot of data is generated, or even that the data is readily quantifiable into on-base percentages and batting averages and the like; many parts of social life generate such volumes of numerical data. Rather, what makes baseball amenable to statistical-comparative analysis is that the data is generated by repeated actions taking place within a very stable system of rules that set boundaries on acceptable play but do not uniquely determine outcomes. Baseball’s numbers are *meaningful*, and have been so for over a century—ever since the pitcher’s mound was moved

³ A team can send only 25 men to the plate during a game if a) they are the home team; b) the visiting team faces only three batters per inning for a total of seven innings, and faces four batters—only one of whom scores—during one and only one inning; and c) the home team holds the visiting team scoreless for nine innings. [A team can face only three batters per inning either by i) retiring the side in order or ii) retiring the side in such a way that no one scores and no one is left on base, which would involve a combination of double and triple plays, successful pick-off attempts, and runners caught when attempting to steal a base.] As this is tremendously unlikely to happen even *once* during the regular season, actual plate appearance numbers are, obviously, *much* higher, raising the population size even more. Thanks to Charles Tilly and Peter Howard for reminding me of these issues.

back to its present distance from home plate (Gould 2003, 152-153). Baseball thus constitutes an arena in which major factors are effectively fixed and small variations among players, ballparks, and strategies of play can be effectively correlated with measurable outcomes.

In other words, baseball approximates a closed system: a system of action that is relatively isolated from external influences and features essential individuals interacting in restricted ways (Bhaskar 1975, 75-78). Present in situations like those in a laboratory, a closed system permits the formulation and testing of statistical-comparative hypotheses with relative ease, as the relevant boundary conditions are truly parametric and experimenters can consequently work to isolate the impact of minute variations while holding most other factors constant.

But it remains an open question whether or not social life *as a whole* constitutes a closed system. Most philosophers of science and social theorists who have taken up the issue argue that it is better to think of social life as an open system, within which causation is not always marked by systematic cross-case correlation. As such, generalization cannot take place at the level of systematic connections between inputs and outputs, but must take place elsewhere—either at the level of causal mechanisms and processes, or at the level of innate dispositional essences of entities (Bhaskar 1998; Giddens 1984; Wendt 1999). If social life is not as approximately closed as baseball is, then statistical-comparative techniques may not be the most appropriate ones for investigating it.

In fact, although baseball, like most organized sports, is deliberately produced as an approximately closed system through the actions of a bureaucracy dedicated to preserving the integrity of the game, this closure remains only approximate. Economic, environmental, and pharmacological factors continually intervene to change the game in various ways over time. The

perpetuation of the sport as a relatively closed system of action—like the perpetuation of any set of social boundaries—takes (practical, discursive) work (Neumann 1999, 35-37; Tilly 1998, 67-70). It might be a useful *pragmatic* assumption to simply posit the game as a closed system, particularly if one is trying to solve a particular set of problems and manage a team to victory, but this assumption should not be reified into a description of how the system “really is” (Easton [1953] 1971, 128-129, 291-292; Parsons 1954, 216-217). In addition, such a methodological commitment closes off several avenues of inquiry—in particular, a critical examination of the rules governing the game, and of the processes and mechanisms that came together so as to produce the particular arrangement of rules presently in force. Whether these trade-offs are justified is a complex matter, worthy of extended discussion rather than dismissal through silence.

In particular, any case for methodologically presuming that social life constitutes a closed system needs to confront two related issues. First of all, precisely isolating the boundaries of a social system is empirically problematic, even for relatively closed systems like organized baseball. Commentators continue to debate whether alterations in the height of the pitcher’s mound and the size of the strike zone, or the introduction of a “wild card” playoff spot, have irrevocably altered the game (Boswell 2004; Gould 2003, 304-310). Recently implemented revenue-sharing measures hope to address the imbalances between rich, large-market teams and their poorer, small-market brethren (Pennington 2003). It is far from simple to adjudicate just how significant these changes are. The basic conceptual problem is that “‘obeying a rule’ is a practice,” as is determining whether some particular rule is essential to an activity or not; the best that can be done is to advance an argument based on a sense of what the game is all about (Wittgenstein 1953, §202, 562-568). Formal specifications of rules never exhaust the activity

that they supposedly govern, which means that there is always room to contest any particular specification of the rules—even for baseball (Flyvbjerg 2001, 42-45).

The problem becomes even more acute when we consider social arrangements in which the rules of the game are themselves an object of contestation. The play of the game of baseball does not directly involve revisions of the rules, but many other areas of social life—legislative and judicial processes, social movement activism, international diplomacy, and the like—*do* directly involve such an ongoing contestation of the rules. In fact, I would venture to say that most of the phenomena of interest to social scientists involve this kind of practical endogeneity, where the activities under investigation have at least the potential to modify the boundaries of the phenomenon. But this does not mean that there can be no systematic study of these phenomena. Nor does it mean that there can be no causal conclusions about these phenomena, unless we follow Laitin and other statistical-comparative partisans in restricting causality and the “zone of science” to the search for invariant laws in a closed system (Laitin 2003, 171).

As before, my purpose is not to simply dismiss the statistical-comparative position. To the contrary, I would like to see it spelled out more explicitly. I think that someone could do the field a great service by making explicit arguments on behalf of considering social life as a whole to be a closed system in which constant conjunctions of variables obtain on a regular basis. Among other things, such arguments would serve as useful foils for those of us who disagree.

Dialogue versus Synthesis

Indeed, it is this absence of explicit arguments that constitutes the greatest failing of statistical-comparative partisans: they generally neglect to provide *grounds* for their positions. Instead, we are confronted with a plethora of assertions about the character of “science,” and

implicit presumptions about the nature of social reality that calls for such a science. This is a very unfortunate way to respond to calls for open dialogue about these basic issues—calls such as that consistently issued by the “perestroika” movement within American political science. There is, by design, no perestroikan “manifesto offering an alternative view of the discipline,” (Laitin 2003, 163) because the perestroikan alternative is (in my reading, at least) based on dialogue rather than consensus. The alternative to the present dominance in the field of statistical-comparative methodology is not the dominance of some rival methodology, but instead a sustained dialogue about the social world among practitioners of rival methodologies.

The need for such a dialogue, as far as I am concerned, rests on what Max Weber identified many years ago as the “irresolvable conflict” between “different value-orderings of the world” (Weber [1917/1919] 1994, 16-17). Different practitioners of social inquiry necessarily approach the world with very different value-orderings, and regard different aspects of particular phenomena to be of interest. But Weber’s solution is not to declare some methodologies and their encoded value-orientations categorically invalid; instead, his solution is to demand that each researcher make her or his presuppositions explicit, and that each researcher implement her or his project in a rigorous, logically consistent manner (Weber 1949, 80-84). Weber argues that this will produce insights that can be appreciated even if cultural values (and their associated methodologies) change (Weber 1949, 58-59). But what it will *not* lead to is a field-wide consensus on fundamental value-orderings, because systematic empirical enquiry cannot itself definitively answer questions about such things (Weber [1917/1919] 1994, 19-20).

Obviously, appreciating the insights generated by a different set of value-orderings will be made much easier if authors explicitly spell out what their value-orderings *are*. In practice, particular authors cast their lots with particular methodologies, at least for the purpose of

particular projects; nothing in my Weberian stance militates against that. A commitment to dialogue at the level of the field should not be mistaken for a demand that every scholar deploy multiple methodologies in a single piece of empirical research. Indeed, the field is probably well served by individual (and perhaps even departmental) methodological specialization—as long as a sustained dialogue with scholars and scholarship stemming from different methodological traditions accompanies this specialization. Laitin’s caricature of an engaged pluralism (Lapid 2003) as involving the protection of “defunct practitioners” from challenges mis-states the case; the purpose of a dialogue is not to entrench the participants further into their separate camps, but to encourage discussion of issues from multiple perspectives (Laitin 2003, 180). Faced with these two alternatives—the statistical-comparative resolution of these fundamental philosophical issues by fiat, and the call for sustained dialogue about those issues advanced by many in the perestroika movement—we should ask ourselves: which path points towards the kind of social science that we want?⁴

The Post-game Wrap-up

David Laitin has staked out and presented a position shared by many in contemporary social science: that all methods of inquiry should take their places in the division of labor established by a firm commitment to a statistical-comparative methodology. In this way they can all triangulate on the essential character of social reality and help us to better understand and affect it. Laitin is certainly one of the most sophisticated practitioners of this kind of social

⁴ For an exploration of how these issues are played out on the curricular level in political science Ph.D. departments in the United States, and an argument that instituting a philosophy of social science requirement would go a long way towards producing a condition of engaged pluralism, see Schwartz-Shea 2003.

science; his empirical work provides numerous examples of one way of making case narratives, formal models, and statistical hypothesis-testing work together. But in this particular trip to the plate, Laitin has failed to reach base safely. He swings through the first pitch tossed by many advocates of methodological diversity—the notion that there are diverse goals of social inquiry—by simply presuming that every social theorist has the same aims. He fouls off the second pitch—the notion that social life is, or might be, an open system—by failing to confront the issue squarely. And he misses the third pitch—the call for dialogue rather than a too-hasty synthesis—by trying to refute a straw man argument about the protection of defunct perspectives. Hence:

*there is no joy in Stats-ville —
mighty Laitin has struck out.*⁵

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⁵ Adapted from “Casey at the Bat,” by Ernest L. Thayer. In the poem Casey lets the first two pitches go by without taking a swing, while Laitin does swing at all three. But it’s still a swinging strikeout, like that of the mythical Casey.

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