

# Beyond Paradigm: Resisting the Assimilation of Phronetic Social Science

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*David Laitin uses Bent Flyvbjerg's Making Social Science Matter as a surrogate manifesto on behalf of the Perestroika movement's campaign for methodological pluralism in political science. After an overview of Perestroika, I note my own vision for the movement, outline the most significant features of Flyvbjerg's call for a revived social science, and provide a critique of Laitin's attempt to assimilate Flyvbjerg's analysis to his own vision for an improved political science. I conclude with a word about the potential of Perestroika to build on Flyvbjerg's insights to promote what I call "post-paradigmatic" social science.*

**Keywords:** *Perestroika; phronesis; political science; praxis*

David Laitin uses Bent Flyvbjerg's *Making Social Science Matter*<sup>1</sup> as a surrogate manifesto for the Perestroika movement's effort to promote methodological pluralism in political science. While I cannot speak for Perestroika, my view is that Flyvbjerg's book does offer an important corrective to the scientistic drift of political science research in recent years. My objection to Laitin is that he refuses to accept the invitation offered by the book to participate in a paradigm shift and instead seeks to assimilate what he likes in the book to his own vision of a revised political science. Because of Laitin's preoccupation with the idea of a unified discipline and his commitment to the prevailing paradigm that emphasizes emulating the natural sciences, his own program fails to overcome the limitations of the prevailing scientistic paradigm.<sup>2</sup>

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Thanks to Fred Block, Brian Caterino, Patrick Thaddeus Jackson, Frances Fox Piven, Joe Soss, Roland Stahl, and Dvora Yanow for helpful comments on an earlier draft of this article.

POLITICS & SOCIETY, Vol. 32 No. 3, September 2004 417-433

DOI: 10.1177/0032329204267292

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In what follows, I provide some background on Perestroika, noting my own vision for the movement. Next, I discuss what I see as the most significant features of Flyvbjerg's call for an revived social science as outlined in *Making Social Science Matter*. Then, I discuss how Laitin's attempt to assimilate Flyvbjerg's analysis to his own vision for an improved political science fails on a number of levels. I conclude with a word about the potential of Perestroika to build on Flyvbjerg's insights to promote what I call "post-paradigmatic" social science.

#### THE PERESTROIKAN POTENTIAL

The Perestroika movement seeks to question what it sees as the hegemonic approach to studying politics in the discipline of Political Science.<sup>3</sup> To be sure, Perestroikans do not always agree on which features of the dominant approach they want to critique. Some focus on the overly abstract nature of much of the research done today, some on the lack of nuance in decontextualized, large-sample empirical studies, others on the inhumaneness of thinking about social relations in causal terms, and still others on the ways in which contemporary social science all too often fails to produce the kind of knowledge that can meaningfully inform social life. Perestroikans do agree, however, on the need for methodological pluralism.

Perestroikans are especially critical of research that assumes political behavior can be predicted according to theories of rationality and that such predictions underwrite the cumulative growth of political knowledge. In addition, participants in the movement have done great work in documenting the exclusionary practices that have made graduate education less hospitable to historical and field research, qualitative case studies, interpretive and critical analysis, and a variety of context-sensitive approaches to the study of politics. Further, a big part of the Perestroikan critique is that the major journals of the field have become preoccupied with publishing research that conforms to overly restrictive scientific assumptions about what constitutes contributions to political knowledge.

I believe that the Perestroika movement is tapping into broader currents of discontent about the dominant approach in political science and the social sciences more generally and is raising fundamental issues of what constitutes good research. I would suggest that the Perestroikan critique also raises the issue of whether there is a dominant paradigm in the field. Many political scientists accept that there is. A case can be made that the dominant paradigm operates according to the following hierarchy of assumptions: (1) political science exists to help promote understanding of the truth about politics; (2) political science research contributes to this quest by adding to the accumulation of an expanding base of objective knowledge about politics; (3) the growth of this knowledge base is contingent upon the building of theory that offers explanations of politics; (4) the building of theory is dependent on the development of universal generalizations regarding the behavior of political actors; (5) the development of a growing body of generaliza-

tions occurs by testing falsifiable, causal hypotheses that demonstrate their success in making predictions; (6) the accumulation of a growing body of predictions about political behavior comes from the study of variables in samples involving large numbers of cases; and (7) this growing body of objective, causal knowledge can be put in service of society, particularly by influencing public policymakers.

Perestroika questions these assumptions and challenges the idea that political science research exists as a unitary enterprise dedicated to the accumulation of an expanding knowledge base of universal, decontextualized generalizations about politics. Perestroika would replace this with a more pluralistic emphasis on allowing for the blossoming of more contextual, contingent, and multiple political truths that involve a greater tie between theory and practice and a greater connection between thought and action in specific settings. Perestroika makes plausible the idea that political science could actually be a very different sort of discipline, one less obsessed with proving it is a “science” and one more connected to providing delimited, contextualized, even local knowledges that might better serve people within specific contexts.

Such a political science would therefore have very different standards as to what counts as meaningful political knowledge. It would, for instance, be less interested in studying such things as “development” or “modernization” in the abstract as objects of inquiry on their own, as when economics becomes the study of “the market” as opposed to the examination of the variety of markets. Instead of focusing solely on “development” or “modernization” per se, political science would be more about studying change in particular countries or using concepts like development or modernization in contextually sensitive ways to compare change in different countries.

This alternative political science would also be less preoccupied with perfecting method or pursuing research strictly for knowledge’s own sake. As Rogers Smith has underscored, “knowledge does not have a sake”; all knowledge is tied to serving particular values.<sup>4</sup> Therefore, this new political science would not be one that is dedicated to replacing one method with another. Instead, such a discipline, if that word is still appropriate, would encourage scholars to draw on a wide variety of methods from a diversity of theoretical perspectives, combining theory and empirical work in different and creative ways, all in dialogue with political actors in specific contexts. Problem-driven research would replace method-driven research.<sup>5</sup>

My own version of Perestroika would build on this problem-driven, contextually sensitive approach to enable people on the bottom working in dialogue with social researchers to challenge power. My Perestroikan-inspired political science would be open to allowing ongoing political struggle to serve as the context for deciding what methods will be used in what ways to address which problems. This new dialogic political science would not find its standards for credible scholarship in arcane vocabularies and insular methods that are removed from local

contexts and seem objective but that contain their own agendas. Instead, my political science would find its standards of knowledge in asking whether scholarship can demonstrate its contributions to enriching political discourse in contextualized settings.

Such a new political science would, however, recognize the risks associated with connecting to ongoing politics. It would guard against losing its critical capacity for the sake of achieving relevance. It would retain its critical capacity while in dialogue with ongoing political struggle, providing therefore a powerful “critical connectedness”—what Charles Lemert has called “global methods.”<sup>6</sup> It would however be less interested than the old political science in serving the state with objective knowledge. It would forego the dream of scientific grandeur that aims to produce socially useful, decontextualized, objective knowledge, independent of politics.

Nothing of course springs full grown from the head of Zeus, and it is critically important to note that the potential of Perestroika has always been manifest in selected efforts in social science, if in recent decades more at the margins and most frequently outside the disciplines in interdisciplinary work and “applied” fields. I use the word “applied” hesitantly however since it reinforces the hegemonic perspective of a particular sort of epistemic privilege that assumes theory precedes action, that research is top-down in that first we study things as they exist objectively in truth and then we “apply” those understandings grounded in theory from above down to the real world of practice. This is to privilege decontextualized, universal knowledge over situated knowledges and only ends up reinforcing the idea that the social sciences need to ape the natural sciences in the pursuit of scientifically tested and validated generalizations about reality. Instead, throughout the relatively short one hundred or so years of modern social science, there have always been practitioners of this craft who have been animated by alternative understandings of the kind of knowledge that social science can meaningfully produce. These practitioners have sought not just to criticize the Olympian perspective of the top-down hegemonic approach, and not just propose alternatives, but to demonstrate them convincingly in their own work. These researchers can be found across the social sciences, employing a diversity of methodologies, and studying a variety of topics. They situate their studies in the world of action, they insist on framing their work in terms of its relevance to ongoing human struggles and concerns, and they let their work emerge from the bottom-up with the hope of not producing universal truths, but poignantly relevant forms of knowledge that can help inform the human condition as it is experienced, fought over, and changed by the very same people being studied. A few examples are in order.

James Scott’s writings, for instance, from *The Moral Economy of the Peasant* to *Seeing Like a State* have looked at the world of power from the perspective of those on the bottom.<sup>7</sup> He has in *Seeing Like a State* demonstrated quite convincingly that the bottom-up perspective not only affords a different view but one that

is more attuned to the needs of people in contextualized settings. A similar perspective is offered in the politically poignant analyses of Cynthia Enloe in such books as *Bananas, Beaches and Bases* and *The Morning After: Sexual Politics at the End of the Cold War*.<sup>8</sup> Enloe highlights the gendered character of international relations in a world of superpowerdom and demonstrates in telling ways its particularly devastating consequences for women. Like Scott, she illustrates in her work that a bottom-up perspective produces a situated knowledge that can inform ongoing efforts to engage political power and produce political and social change.

The work of Frances Fox Piven and Richard A. Cloward represents an especially noteworthy example for my particular version of what Perestroika can be bringing to a revived political science. From *Regulating the Poor* to *Why Americans Don't Vote*, their research grows from the bottom-up informed by ongoing political struggle, seeking to theorize and strategize what is needed to feed back into those specific struggles.<sup>9</sup> Like Enloe and Scott, Piven and Cloward employ a number of case studies, as in *Poor People's Movements*, to tease out helpful lessons for those working to challenge power. Sometimes their theorizing was employed to inform a specific struggle but also to offer more general understandings that could be applied beyond that struggle.<sup>10</sup> Enloe, Scott, and Piven and Cloward are not alone; there are many other instances of such work scattered around the margins of political science and in other fields.<sup>11</sup>

In fact, there is a rich tradition of several decades now leading to a growing number of studies in what is popularly called Participatory Action Research (PAR).<sup>12</sup> The PAR approach emphasizes being in alliance with those being studied so as to overcome the unquestioned assumptions and privileges associated with some people studying other people. Compelling examples here include Chester Hartman's 1974 *Yerba Buena*, which grows out of his activism working with tenants to resist their displacement in the face of land-grabbing developers, and William Foote Whyte and Kathleen King Whyte's 1988 *Making Mondragon: The Growth and Dynamics of the Worker Cooperative Complex*.<sup>13</sup> The Whytes' analysis richly details how their research grows out of and effectively feeds back into the struggles the Mondragon community is caught up in as its members seek to find a way to make socialism in a capitalist world sustainable. Within political science more than say sociology, this sort of work is still marginal, making the Perestroika challenge all the more necessary.

#### THE PERSTROIKAN POTENTIAL . . . IS TO BE FOUND IN *PHRONESIS*

A clarion call for more of the work described above is Bent Flyvbjerg's *Making Social Science Matter*. It builds on the kinds of work mentioned above to explicitly develop an alternative to the hegemonic approach to social science. While this book was not written expressly and specifically for Perestroika, it does indeed directly address the kind of efforts that Perestroikans are promoting.

Flyvbjerg's book makes an impassioned argument about what is wrong with the social sciences today and provides a detailed list of examples of how its alternative social science is already possible and already happening.

Flyvbjerg's alternative social science is dedicated to enhancing *phronesis*. For Flyvbjerg, *phronesis* is what the social sciences has to offer that the natural sciences cannot with their emphasis on *episteme* and *techné*. This Aristotelian tripartite distinction is critical for Flyvbjerg in highlighting the comparative advantage of social science. *Phronesis* is, as Aristotle termed it, akin to practical wisdom that comes from an intimate familiarity with the contingencies and uncertainties of various forms of social practice embedded in complex social settings. *Episteme* is knowledge that is abstract and universal; *techné* would be the know-how associated with practicing a craft.

Given their subject matter, the natural sciences are better at testing hypotheses to demonstrate abstract principles and law-like relationships, while the social sciences are better at producing situated knowledges about how to understand and act in contextualized settings. While the natural sciences excel at conducting decontextualized experiments to understand abstract and generalizable law-like relationships, the social sciences can conduct case studies involving field research that produces intimate knowledge of localized understandings of subjective human relationships. Flyvbjerg supports a multimethodological approach and has no objection to using quantitative data but his emphasis is on producing research that can help develop *phronesis* by increasing understanding in specific contexts rather than questing after the ghost of an abstract knowledge of law-like processes. The main reason that Flyvbjerg emphasizes this alternative approach is that human beings are conscious subjects that, on the one hand, cannot be studied like inanimate objects and who, on the other hand, can learn and adapt to their situations once given access to the research conducted about them. Flyvbjerg, therefore, very much wants social scientists to revise their standards for acceptable research methodologies, reincorporating context-sensitive research, such as case studies that help social actors learn to appreciate the complexities of social relations and practice various social crafts more effectively.

Flyvbjerg sees contemporary social science as hopelessly lost when it emulates the quest for theory-driven abstract knowledge of universal rationality that characterizes the natural sciences. His critique resonates with the recent book by Stephen Toulmin, *Return to Reason*, that makes a similar argument on a broader historical scale stretching from Descartes to Kant, to the modern university and contemporary social science.<sup>14</sup> Flyvbjerg adds that there is no symmetry between natural and social science because the real interpretive problems of the natural sciences are compounded by what Anthony Giddens called the "double hermeneutic" of the social sciences. By virtue of its distinctively human subject matter, the social scientists inevitably are people who offer interpretations of other people's interpretations. And the people being studied always have the potential to include the social scientists' interpretations in their own, creating an ever-changing sub-

ject matter and requiring a dialogic relationship between the people doing the studying and the people being studied. For Flyvbjerg, this situation unavoidably means that there can be no theory for social science in the sense of time-tested theories of a static social reality.

As a result, argues Flyvbjerg, the social sciences should not seek to emulate the natural sciences. In such a comparison, the social sciences will always fare very poorly, being seen as inferiors incapable of producing knowledge based on tested theories that can evince prediction of the worlds they study. Instead, Flyvbjerg feels that the social sciences are better equipped to contribute to the practical wisdom of *phronesis*. The social sciences can distinctively produce the kind of knowledge that grows out of intimate familiarity with practice in contextualized settings. These are local knowledges, even tacit knowledges, that cannot be taught a priori but which grow from the bottom up, emerging out of practice. Add a sense of praxis, seeking the ability to push for change, leaven it with an appreciation of the ineliminable presence of power, and this phronetic social science can help people in ongoing political struggle question the relationships of knowledge and power and thereby work to produce change.<sup>15</sup>

Flyvbjerg creatively uses Aristotle, Nietzsche, Foucault, Bourdieu, and others to make his own distinctive argument for an alternative social science. He fuses an Aristotelian concern for *phronesis* with a Marxist concern for *praxis*, adding in a Foucauldian critique of Habermas's preoccupation with consensus to demonstrate that a phronetic social science that can offer a praxis worth pursuing is one that would work within any contextualized setting to challenge power, especially as it is articulated in discourse. Flyvbjerg's phronetic social science is open to using a plurality of research methods to help people challenge power more effectively.

Yet, what is most exciting is that Flyvbjerg not only goes beyond critique to offer a positive program; he demonstrates it in detail, pointing to a rich variety of contemporary work from that of Bourdieu, to Robert Bellah, to his own work. Flyvbjerg's research spanned fifteen years and focused on a major redevelopment project initiated by the Danish city of Aalborg where Flyvbjerg continues to teach urban planning. His research on the project evolved over time, becoming more phronetic as he came to appreciate how social science could make real contributions to the ongoing dialogue over the city's redevelopment efforts. First, Flyvbjerg was annoyed that decision makers rejected the relevance of studies about education elsewhere and he came to be concerned with power. Without saying so, he evidently took to heart the idea that he had to work harder to produce research that, even while it challenged power, demonstrated its grasp of the Aalborg context. In the process, power relations were challenged in a very public way that helped reinstitute democratic deliberation in the halls of city government. The framing of the development agenda was successfully revised without "going native" to include more grassroots concerns; an ongoing dialogue with participants in the redevelopment process was richly elaborated; and social sci-

ence research that gave up an interest in proving grand theories became critical to a very robust discourse on urban planning. As a result, the Aalborg planning project gained increased visibility as a successful project that went out of its way to democratize its decision making in part by allowing social science research to help keep it honest, open, and collaborative.

#### THE LAST STAND OF THE "SCIENTIFIC FRAME"

Laitin's argument is fundamentally contradictory because, in spite of his many criticisms of Flyvbjerg, he finds the idea of a social science dedicated to help develop *phronesis* to be an important one. Yet, he adds: "But the point here is that while Flyvbjerg's notion of *phronesis* may have some important role to play in the professionalization of social practitioners, it must be combined with statistical and formal analysis if the goal is valid social knowledge."<sup>16</sup> Laitin concludes his critique with a plea that we should all accept the need to work within what he calls a "scientific frame," which he suggests means focusing research on explaining causality. Evidently, Laitin's understanding of *phronesis* makes it a form of knowledge that can be developed within a "scientific frame" and is therefore in that sense not really distinctive from *episteme*.

Yet, the kind of epistemic knowledge that Laitin suggests is the primary concern of a "scientific frame" focuses on explanation of causal relationships according to abstract principles; whereas the *phronesis* that Flyvbjerg seeks to develop is a form of knowledge that focuses not on explaining causal relationships according to abstract principles but instead is geared toward understanding the meaning and significance of human relationships situated in specific contexts. Laitin's approach to social knowledge is more from the Olympian perspective of the outsider who is trying to explain what happened according to objective causal forces; while Flyvbjerg's approach is more bottom-up developed by an insider within a situated context and seeking to produce meaning that local actors in that context can use effectively. Toulmin's distinction between abstract rationality and situated reasoning is appropriate here. Disciplining the bottom-up phronetic research so that it fits into the "scientific frame" that focuses on explaining causality would be undermining the ability of such research to help develop situated reasoning in order to get more abstract rationality.

Laitin misses the contradiction because he assumes research dedicated to aiding in the development of *phronesis* can be made consonant with research designed to build epistemic knowledge. Yet, these are distinctly different forms of knowledge and one cannot be assimilated to the other. Both abstract rationality and situated reasoning have their contributions to knowledge. But assimilating the latter to the former will not help us have both. It is as if Laitin was forced by the power of Flyvbjerg's argument to concede the importance for social science to be related to *phronesis* but he is reluctant or unable to see how this involves produc-

ing a different kind of knowledge than his preferred scientific form of knowing. The result is a mixed-up model of social science research.

Laitin starts his dismissal of Flyvbjerg's argument for a phronetic social science by winning some battles that are basically unrelated to determining the outcome of the larger struggle. Laitin complains about how Flyvbjerg handles secondary issues such as whether experts rely on intuitive knowledge and whether context can be studied analytically. While I am sure that there is a large literature debating these issues, and Flyvbjerg's take on each of them may not be definitive, Laitin's complaints neither diminish the importance of thinking about a phronetic social science nor help underwrite Laitin's alternative model for social science.

Laitin then turns to his own model. He combines three different registers for producing social knowledge: formal modeling of abstract principles and law-like relationships, statistical analysis of such models across large numbers of cases, and compelling interpretive narratives that highlight causal relationships so as to suggest how statistical findings help clarify case studies.

Laitin's revised model includes a place for *phronesis* as valuable only when combined with statistical and formal analysis of abstract principles and law-like relationships. Laitin insists on this combined approach because he fears the dangers of an "isolated" *phronesis* that is not supplemented with the counterintuitive findings that come from more scientific testing across large numbers of cases. Laitin points to the phronetic research of Stanley Tambiah on violence in Sri Lanka to note how it could be improved if it was leavened with considerations that come from examining statistical studies on the causes of violence in such settings.

There are, in fact, a number of issues in the discussion of Tambiah's study of the Sri Lankan civil war that highlight critical problems in Laitin's approach.<sup>17</sup> While Laitin acknowledges that Tambiah's study is a careful one with rich empirical evidence, he calls Tambiah's argument into question largely on the basis of statistical analyses of the relationship between linguistic grievances and civil wars. Laitin here seems ignorant of the idea of a statistical outlier. While looking at a universe of countries, there may indeed be no statistical relationship, as Laitin suggests, between linguistic grievances and civil wars. Yet, that does not preclude the fact that in any particular country in the sample, there may indeed be a relationship.

Therefore, Laitin prematurely drops the hypothesis of linguistic grievance as the initiating cause of the violence in Sri Lanka and uses the statistical analysis as the basis to hunt for other explanations. Yet, it could well be that evidence of the sort that suggested a relationship was staring him right in the face right there in Sri Lanka. Laitin does pursue a number of questions about why the violence grew in Sri Lanka but in reflex-like fashion he allows the lack of correlation between linguistic grievances and violence across countries to frame his hunt for a compelling explanation as to why the civil war developed in Sri Lanka. Laitin does this because he is so keen to show the indispensable value in a "scientific frame" (i.e., grounding the hunt for causal explanations in the particular case in statistical anal-

yses across cases). In the process, Laitin allows the cross-national statistical causal analysis to prematurely negate Tambiah's careful study of the lived experience of people in Sri Lanka. The general overrides the particular irrespective of whether linguistic, religious, regional, or whatever other specific factors lay behind the Sri Lankan civil war. It is hard to resist the conclusion that this sort of reasoning is fundamentally flawed.

Laitin's use of the "scientific frame" in reconsidering the Sri Lankan case becomes its own exclusionary practice preventing us from finding plausible contextual explanations if they are not first suggested by decontextualized statistical work. It strikes me as a higher form of ignorance of an almost willful sort. Why mortgage the contextually sensitive explanations to what systematic statistical analysis tells us is only generally, but perhaps not particularly, true? Laitin is offering us an interesting way in which to tie large-*n* statistical research to case studies, and perhaps vice versa, but the way he concludes the exercise as suggesting that his reconsideration of the Sri Lankan case proves the indispensability of the "scientific frame" for contextual analysis overlooks how the exercise shows the opposite: using the "scientific frame" as Laitin does in this case shows that it can foreclose more relevant sources of knowledge and better explanations.

This becomes clear when Laitin considers a series of anomalies in the timing of different events to "prove" that Tambiah's account of Sri Lankan violence is wrong. But as he should know, any social science account—including those based on his own holy trinity—is necessarily incomplete and questions can be raised about its explanatory power. Therefore, Laitin makes a logical error by proceeding to say that because one can ask questions about Tambiah's more phronetic-like interpretation, then it obviously shows the weakness of *phronesis*. Yet, Laitin makes this claim without offering a rival interpretation of the sources of the distinctively bloody Sri Lankan civil war. While he claims that this exercise demonstrates the deficiencies of "isolated" *phronesis*, it actually proves the dangers of limiting the hunt for contextual explanations to what decontextualized statistics suggest are general causes. In the end, Laitin's approach gives us no additional leverage on the historical events *and* shows his disregard for Tambiah's rootedness in the lived experience of Sri Lankan people.

#### FROM THE BOTTOM UP: THE MORAL ECONOMY OF METHODOLOGICAL PLURALISM

Actually, a different version of Laitin's argument that is less dismissive of *phronesis* and less insistent on the indispensability of a "scientific frame" could make Laitin's exercise more fruitful. One can distinguish between soft and hard versions of his position. In the soft version, narratives can learn from statistics and formal modeling and vice versa. This appeals to the methodological pluralism of many Perestroikans since we also think there can be learning across these different approaches. But in the hard version of Laitin's argument—especially stated towards the end of the entire article—we hear the gatekeeping voice of the tenure

and recruitment committee that wants to cut off “unproductive” lines of inquiry. When Laitin is speaking in this voice, he would deny Tambiah a senior appointment because he failed to look at the cross-national statistics on language and civil war. Jim Scott and Cynthia Enloe might be denied tenure if Laitin’s views were hegemonic.

Scott’s *Seeing Like a State* provides an especially good example here.<sup>18</sup> He does not ground his research in a “scientific frame.” Scott details a number of cases of what he calls “high modernist” state-centered thinking of an overly rationalistic sort that lead to excessively ambitious projects that are imposed on indigenous populations in ways that do great violence to them. Yet, the extra value of Scott’s object of inquiry is that “high-modernist” state-centered thinking is the real-world equivalent of social science’s “scientific frame.” To critique one is to critique the other. And to offer an alternative to “high modernist” thinking is to begin to create the basis for a phronetic social science. Therefore, Scott’s book does important double duty, simultaneously critiquing “high modernist” state-centered thinking and doing so by way of illustrating and helping make possible an alternative social science.

Scott queries a number of cases of a very diverse sort from Le Corbusier’s “principles of urbanism” as the basis for planning the modernization of Paris to Lenin’s approach to a vanguard party for a communist revolution, to Soviet collectivization of agriculture, to Nyerere’s 1973 decree for the militaristic planned “villagization” of Tanzania, to the Westernized approach to agricultural development of the Third World more generally. Scott does not survey these cases to build a data set to produce a correlation and develop a “scientific frame” that can inform the hunt for explanations in any one case. Scott does want the cases to learn from each other and he from them. As diverse as they are, they all point to their shared problem of “seeing like a state”—from the top-down, high-modernist perspective committed to a grand vision of universal truths that can be applied without attention to the particularities of context. Scott counters high-modernist rationality not with *phronesis* but its cousin *mētis*—still another Greek term referencing knowledge derived from practical experience. He effectively highlights how the high-modernist approach to imposing change marginalizes important local forms of *mētis* in ways that have long-run negative consequences for the people on the bottom.

There are several features to Scott’s mode of analysis that are at odds with Laitin’s model. First, he uses cases to think more generally rather than the other way around. Second, he does not mortgage his explanations of any one case, let alone all of them, to a statistical analysis across cases. Third, to the extent that he looks across cases for commonalities, it is not strictly for causes that can be inferred from correlations. Fourth, Scott’s interest is less in what caused what to happen as to what can be done to do it differently in ways that are more consistent with how people think they ought to live.

Scott's work therefore points to yet another issue with the Laitin "scientific frame": its fixation on causal explanation. Some scholars might argue that social science is primarily about finding causation; some others might agree but suggest that a variety of methods, including nonstatistical ones, need to be employed; and still others might want to debate whether explaining causation exhausts the goal of social science. *Seeing Like a State* helps us appreciate that to explain causality, we need to understand meaning. While planners using various models might be able to predict traffic patterns with a rerouting of city streets, they may be hard put to understand what the rerouting means to those who have to travel it. So they fail to anticipate how people respond to the new routes. An appreciation of context-sensitive knowledge helps us develop more understanding of what things mean to people. And that I would argue is an important part of helping people decide what to do about the problems they confront.

A phronetic social science is arguably, therefore, about more than getting robust causal explanations that can be effectively applied in specific contexts. It is also about understanding what is important to people in those contexts as a way to gain a deeper understanding of social processes.<sup>19</sup> Yet Laitin's scientism forces him to downgrade the importance of the experiential, the local, the things that have not been put into statistics or formal models. It must be said that Laitin is not alone since political science is distinctive in how far it has gone in emphasizing this type of research at the expense of other forms of empirical work. In neighboring disciplines such as sociology, it is now widely recognized that ethnographic studies bring into the conversation processes that have previously escaped statistical analysis. But Laitin's strong position precludes this type of understanding; as a result, he blindfolds political scientists since with his model, they would not be allowed to emphasize the local, the peculiar, the anomalous. Therefore, contrary to Laitin's claims, the insistence on the indispensability of the "scientific frame" would lead to a discipline that would be even more univocal than the one we have now. The existing scientific consensus would be even more consolidated around explaining causality through easily measurable variables at the expense of understanding meaning.

Therefore, while Laitin's revised model seems on the surface to be dedicated to promoting more practical wisdom, it ends up questing after abstract universal knowledge. Laitin mistakenly thinks he is strengthening research's ability to aid in the development of *phronesis* by requiring that the interpretation of case studies make use of causal explanations that have been tested statistically. Yet, given that this confuses the nature of *phronesis*, it results in a kind of hybrid research that at best only helps grow scientific knowledge of abstract causal laws. This, I would argue, is inevitable since the situated reasoning that would be derived from phronetic research is not likely to make use of the causal modeling that Laitin incorrectly assumes it needs.

Yet, the fact that Laitin reduces phronetic research to an issue of narrative interpretations tellingly suggests that his confusion runs deeper than I have so far

allowed. First, he ignores that the phronetic research that people like Flyvbjerg are promoting involves more than spinning interpretive tales about what happened in a particular situation. For instance, Flyvbjerg's phronetic research is multi-methodological, involves more than simply the question of deciding on interpretation, and is not limited to producing explanatory interpretations. The phronetic research that Flyvbjerg is proposing is research that can do much more than offer interpretive narratives; it would supply contextualized information in a variety of forms, from numbers to narratives and everything in between, all in service of enhancing local actors' ability to address the issues they are confronting.

Laitin reduces phronetic research to the development of narrative interpretation and he then subordinates these narratives to the task of explaining causality. The fallacious thinking that comes of this is amply demonstrated in Laitin's examples. The exemplary case of his tripartite model is Randall Stone's *Lending Credibility*, which analyzes the effect of International Monetary Fund (IMF) conditionality programs on economic performance in the post-Soviet states.<sup>20</sup> Laitin praises this work because the case studies confirm the model and the narrative helps stitch the cases to the model. It is at this point that Laitin is glaringly inverting what he promised for his tripartite model. Contextually sensitive work is in service of building bigger and better decontextualized causal generalizations rather than the other way around. And throughout his entire discussion of this work, Laitin emphasizes how the analysis winnows away possible causal explanations for the adoption of IMF policies. Stone is lauded for isolating what causal processes, or "mechanisms," explain IMF structural adjustment policies working in some countries and not others. How such causal explanations will help local actors better deal with their financial problems on their own terms is never explained. A top-down managerial, in this case IMF, perspective is favored over a bottom-up recipient perspective on how to cope in a globalizing political economy. Explanation crowds out understanding. Objective truth trumps subjective meaning. Situated reasoning is sacrificed on the altar of abstract rationality.

Further solidifying the idea that Laitin is really primarily interested in a top-down perspective designed to explain causality are his concluding examples that are offered to illustrate what he means when he calls for pluralism in science. These examples make clear that what he is offering us is a pluralism of technique rather than an appreciation of diverse approaches. By the time Laitin gets to his conclusion, we are once again back with that old *bete noire* of the "isolated" case study. We are exhorted to beware of the dangers of case studies "selecting on the dependent variable" and leading us into making faulty inferences about causation. The gloves come off and it is clear that it is all about better models that prove causation. This would be *phronesis* in name only. Laitin at the end is thoroughly committed to the dominant scientific paradigm, and he makes it clear that we should be in pursuit of abstract rationality at the expense of situated reasoning.

Laitin invokes *Designing Social Inquiry*, by Gary King, Robert Keohane, and Sidney Verba to suggest that case studies need to be assimilated to the scientific

paradigm if they are to produce good social science.<sup>21</sup> This work, like Laitin's argument here, misses the point made by Flyvbjerg and others. King, Keohane, and Verba unfortunately see case studies as just another form of positivistic research that needs to be structured according to the same design logic to ensure that such research is able to isolate causality. Yet, as Ann Chih Lin has effectively demonstrated, there are different types of ethnographic, field, and qualitative research, some that are more positivistic in their orientation in that they are designed to produce qualitative data that can test hypotheses about objective causal processes while others are more interpretive in their orientation and are designed to heighten our understanding of the subjective experiences of the people being studied.<sup>22</sup>

Therefore, contrary to King, Keohane, and Verba, not all case study research can be assimilated to the positivistic paradigm. Flyvbjerg's phronetic social science would emphasize the kinds of case studies whose purpose and significance would be mangled by attempts to structure them to ensure they can isolate causality. Laitin's use of King, Keohane, and Verba to insist that all case studies must be structured according to a positivistic logic to isolate causality simply misses the central point of Flyvbjerg's argument for a social science that can produce *phronesis*. Phronetic social science is not about isolating causality. It is about promoting appreciation and understanding of situated knowledge to decide what is happening, why, and what should be done about it.

#### BEYOND PARADIGM

Laitin's contradictions aside, Flyvbjerg's phronetic social science is a credible alternative to the dominant form of social science research. But I do not think it is constructive to say that Flyvbjerg's approach represents a new paradigm. This is because I am increasingly convinced that social science is ideally better seen as post-paradigmatic rather than as organized by one paradigm or another. For me, the idea of paradigm has no relevance to social science except as its own form of mimicry. Paradigmatic research is what natural scientists do. Social science for the reasons provided in this essay ideally should not be seen as amenable to being organized paradigmatically in any strict sense of the term.<sup>23</sup>

The idea of a phronetic social science helps us think about how social science is already post-paradigmatic, allowing for a variety of approaches to co-exist so as to maximize the types of knowledge we can create and utilize. What we need to be doing more of is embracing that diversity especially in its capacity to help people live better. From this perspective, to use Toulmin's terminology again, we do not want to simply replace an emphasis on abstract rationality with an exclusive focus on situated reason. Instead, we want to redress the imbalance and allow for studies that enhance our ability to develop situated reason to have equal footing with those that encourage us to think in terms of abstract rationality and its related principles about generalizable relationships, causal or otherwise. Now, that would be

a real pluralism for social science, not one of a diversity of techniques, but of a diversity of knowledges, not assimilated to an exclusive model with a “scientific frame,” but one where different models of good research that each emphasized the best route to their distinctive form of knowledge. Then we would have a real pluralism in political science. I wonder what David Laitin would say about that.

## NOTES

1. Bent Flyvbjerg, *Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again* (New York: Cambridge University Press, 2001).

2. On David Laitin’s interest in “disciplining” Political Science so that all political scientists are obligated to work within what Laitin calls a “scientific frame,” see David D. Laitin, “Disciplining Political Science,” *American Political Science Review* 89 (June 1995): 454-56.

3. I review these issues in detail in Sanford F. Schram, “Return to Politics: Perestroika and Postparadigmatic Political Science,” *Political Theory* 31 (December 2003): 835-51.

4. See Rogers M. Smith, “Should We Make Political Science More of a Science or More about Politics?” *PS* 35 (2002): 199-201.

5. Ian Shapiro, “Problems, Methods, and Theories in the Study of Politics, or What’s Wrong with Political Science and What To Do about It,” *Political Theory* 30 (August 2002): 596-619.

6. Charles Lemert, *Social Things* (Lanham, MD: Rowman & Littlefield, 2001), 176-206.

7. James C. Scott, *The Moral Economy of the Peasant: Rebellion and Subsistence in Southeast Asia* (New Haven, CT: Yale University Press, 1990), and *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed* (New Haven, CT: Yale University Press, 1998).

8 Cynthia Enloe, *Bananas, Beaches and Bases: Making Feminist Sense of International Politics* (Berkeley: University of California Press, 1990), and *The Morning After: Sexual Politics at the End of the Cold War* (Berkeley: University of California Press, 1993).

9. Frances Fox Piven and Richard A. Cloward, *Regulating the Poor: The Functions of Public Welfare* (New York: Vintage Books, 1971), and *Poor People’s Movements: Why They Succeed, How They Fail* (New York: Vintage Books, 1977).

10. I examine the special case of Frances Fox Piven and Richard Cloward in Sanford F. Schram, *Praxis for the Poor: Piven and Cloward and the Future of Social Science in Social Welfare* (New York: New York University Press, 2002).

11. I provide a detailed examination of the virtues of a bottom-up approach as applied to the study of social welfare policy in Sanford F. Schram, *Words of Welfare: The Poverty of Social Science and the Social Science of Poverty* (Minneapolis: University of Minnesota Press, 1995). For a good recent example of bottom-up analysis, see Vijay Prashad, *Keeping Up with the Dow Joneses: Debt, Prison, Workfare* (Boston: South End, 2003).

12. For a primer on participatory action research, see William Foote Whyte, ed., *Participatory Action Research* (Newbury Park, CA: Sage, 1990). For a review of participatory action research as practiced by feminist researchers in particular, see Shulamit Reinharz, *Feminist Methods in Social Research* (New York: Oxford University Press, 1992). Also, see Patti Lather, “Feminist Perspectives on Empowering Research Methodologies,” *Women Studies International Forum* 11 (1988): 569-81.

13. Chester Hartman, *Yerba Buena: Land Grab and Community Resistance in San Francisco* (San Francisco: Glide Publications, 1974). William Foote Whyte and Kathleen

King Whyte, *Making Mondragon: The Growth and Dynamics of the Worker Cooperative Complex* (Ithaca, NY: Cornell University Press, 1988). For an analysis that highlights years ahead of time the relevance of *Making Mondragon* for the Perestroika movement in political science, see Kenneth Hoover's review of the book in the March 1990 issue of *American Political Science Review*. Hoover notes that as early as 1943 after writing his classic *Street Corner Society: The Social Structure of an Italian Slum* (Chicago: The University of Chicago Press, 1943), William Foote Whyte challenged the Political Science profession in the pages of the *American Political Science Review* to "take an interest in politics" and "establish themselves as participant observers." Hoover notes that Whyte implored political scientists to do this so that they could develop a practical knowledge of politics and its relationship to social structures. Kenneth R. Hoover, "Making Mondragon: The Growth and Dynamics of the Worker Cooperative Complex by William Foote Whyte and Kathleen King Whyte," *American Political Science Review* 84 (March 1990): 351-52.

14. Stephen Toulmin, *Return to Reason* (Berkeley: University of California Press, 2002).

15. For the argument that a phronetic social science can contribute to what I have called "radical incrementalism" or the idea that praxis involves promoting change for the least advantaged by exploiting the possibilities in current political arrangements, see Schram, *Praxis for the Poor*, 109-35.

16. David D. Laitin, "The Perestroikan Challenge to Social Science," *Politics & Society* 31 (March 2003): 170.

17. See Laitin, "Perestroikan Challenge to Social Science," 171-75, where he discusses Stanley Tambiah, *Sri Lanka: Ethnic Fratricide and the Dismantling of Democracy* (Chicago: University of Chicago Press, 1986).

18. David Laitin treats *Seeing Like a State* as an example of "undisciplined" history because it fails to conform to the "scientific frame," it does not follow consistent criteria for appraising evidence, and it lacks a comparative analysis that would make its claims about state intervention persuasive. See David Laitin, "Review of *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed*, By James C. Scott (New Haven, Yale University Press, 1998)," *Journal of Interdisciplinary History* 30 (Summer 1999): 177-179.

19. See Dvora Yanow, *How Does a Policy Mean? Interpreting Policy and Organizational Actions* (Washington, DC: Georgetown University Press, 1996).

20. Randall Stone, *Lending Credibility* (Princeton, NJ: Princeton University Press, 2002).

21. Gary King, Robert O. Keohane, and Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton, NJ: Princeton University Press, 1994).

22. Ann Chih Lin, "Bridging Positivist and Interpretivist Approaches to Qualitative Methods," *Policy Studies Journal* 26 (Spring 1998): 162-80.

23. Bent Flyvbjerg uses the term "non-paradigmatic" to describe phronetic social science. I use "post" as opposed to "non" but not as a chronological marker. The "post" need not be "after," or following in time. And post need not be "anti" as in against. Post can mean to carry on but differently. In addition, while I agree that ideally paradigm has no relevance to social science, the reality is that paradigm is what most social scientists mistakenly think they are involved in. So a post-paradigmatic social science would be one where we finally start to appreciate better how we were never really working in a paradigm in the sense that Thomas Kuhn used to describe the natural sciences when he first coined the term. Last, while social science ideally is nonparadigmatic, the power politics involved in creating the hegemony of the rational choice theorists in political science today has made it the equivalent of a paradigm, even if an inappropriately and coercively imposed

one. Post-paradigmatic political science represents the aspiration to move beyond a situation where such a hegemonic approach is imposed on the discipline.

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