The Perestroikan Challenge to Social Science

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Political science faces a challenge from a "Mr. Perestroika," who decries the hegemony of formal and statistical analysis in the discipline. Although not connected with this movement, Bent Flyvbjerg makes the best case for a renewed dominance for qualitative and case study work throughout the social sciences. This article challenges Flyvbjerg's call for a phronetic *as opposed to an* epistemic *discipline. It challenges as well the unqualified call for pluralism advocated by many in the perestroika movement. It offers instead an integrated tripartite method in which narrative, statistics, and formal modeling fill in a scientific frame.*

Keywords: *phronesis; Bent Flyvbjerg; narrative; philosophy of social science; Mr. Perestroika*

The specter of an insurgency haunts political science. Under the leadership of a "Mr. Perestroika," a wide group of political scientists has abandoned the project of a scientific discipline.[1] It would be convenient to write off this quasi-coordinated attack on the scientific turn in the study of society, calling its proponents Luddites. Indeed, their abhorrence of all things mathematical—and their typical but useless conflation of statistical and formal reasoning—reveals a fear of the modern. It would be equally convenient to write off this attack due to lack of any manifesto offering an alternative view of the discipline. Mostly we hear a desire for pluralism rather than a defense of best practices. But I think it would be prudent to

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respond, to defend what may well be a Sisyphean project in seeking a science of social life.

While there is no intellectual manifesto that lays down the gauntlet, a recently published book by Bent Flyvbjerg captures many of the core themes in Mr. Perestroika's insurgency.[2] And thus this book offers an intellectual target for those who wish to confront the perestroikan challenge intellectually.[3] For in this clever, succinct, and readable book, Flyvbjerg portrays the quest for a *social* science as quixotic at best and self-defeating at worst. The social world, he argues, is sufficiently different from the natural world that any hopes for a Galilean conquest over the unknown in social science will forever remain unrealized.[4] Social scientists, in order to sidestep the scorn that is regularly heaped on them by natural scientists who recognize the scientific limits to the study of humans, should cultivate their own turf by making reasonable judgments about the social world, based on a realistic view of power and sensitivity as to how that power is exerted. Relying on Aristotle's categorization, Flyvbjerg dubs this methodology *phronesis*. Social scientists can succeed doing phronesis, Flyvbjerg confidently asserts, because we write and read careful case studies that provide to us an expert's feel for how, in a particular context, our political interventions can bring social betterment.

This is a viewpoint to be taken seriously. Flyvbjerg has conducted well-conceived fieldwork in Denmark and has long been an astute commentator on urban planning and popular participation in social planning. Furthermore, *Making Social Science Matter* has received excellent notices from some of the leading social scientists in the world, including Clifford Geertz, Steven Lukes, and Pierre Bourdieu. Finally, the arguments in the book resonate with parallel points articulated by political science perestroikans, who have yet to be seriously confronted with intellectual arguments.

My response to Flyvbjerg and the challenge he presents to the scientific aspirations of many political scientists proceeds in stages. First, I challenge Flyvbjerg's stylized facts purportedly showing the failure of what he calls "epistemic" social science. Since Flyvbjerg presents these facts to motivate his study, it is important to establish that the premise of the book—constructed from these stylized facts—stands on weak foundations. Second, I challenge Flyvbjerg's portrayals of both context (which he claims is not subject to analysis) and science (for which he sets a standard that many research programs in the natural sciences could not meet). It is important to challenge these views because Flybvjerg argues that the irreducibility of social context makes a predictive science of the social impossible. I can then show that Flyvbjerg's claims for the greater intellectual payoff for phronesis, because of his mistaken views on context and science, need to be radically circumscribed. Third, I discuss phronesis at work, first in a discussion of Flyvbjerg's use of that method in his field research on urban planning in Aalborg, Denmark, and then in a discussion of the work by Stanley Tambiah on ethnic war in Sri Lanka. In both cases, I argue, the work would have much greater scientific value if placed within what I have dubbed the *tripartite method of comparative research*—a method that integrates narrative (much of what Flyvbjerg calls phronesis), statistics, and formal modeling.[5] Fourth, I discuss the contributions that phronesis makes in scientific explanation, showing why it has equal stature to statistics and formal modeling in the tripartite method. Finally, in the conclusion, and in defense of the tripartite method as a standard, I discuss (in reference to claims by one of Mr. Perestroika's defenders) the limits of methodological pluralism.

THE PREMISE OF FLYVBJERG'S BOOK

Flyvbjerg introduces his brief with three examples. Astonishingly, they all work to undermine his entire argument. The opening example is that of the now infamous contribution by NYU physicist Alan Sokal to the journal *Social Text*. Sokal's "contribution" was a hoax. He purposefully submitted what he conceived of as postmodern gobbledygook. Yet it sailed through *Social Text*'s peer review as a serious critique of science. Flyvbjerg offers this example, and the controversy that occurred in the wake of Sokal's publication, as inter alia an "exposé of . . . social science." But why, the reader might ask, would social science get implicated in this scandal? *Social Text* has no pretensions to science. More important, in large part because of a cult of science in leading social science journals such as *American Political Science Review*, *Econometrica*, and *American Journal of Sociology*, it is doubtful that a physicist could get an article of that sort past peer reviewers. Reviewers would want to assure themselves that the data set was available and subject to review, the theory was clearly articulated, and the findings were linked closely to theory and data. Sokal chose *Social Text* precisely because members of its editorial board had ridiculed the notion of scientific objectivity.

The second example, immediately on the heels of the presentation of the Sokal hoax, concerns the study of human sexual practices conducted by scholars working at the National Opinion Research Center (NORC). Flyvbjerg delights in quoting *The Economist*'s humorous put down of this study (and later on he uses an equally clever one-liner from *The Economist* to write off the entire profession of economics). Flyvbjerg also cites a more serious attack on the statistical methods employed in this study, written by a population geneticist, and a rather limp defense of those methods by the authors in response. This is evidence for Flyvbjerg that natural scientists hold social scientists in contempt. Social scientists, he concludes, should not even try to imitate the scientific method with fancy statistics and impressive regressions. Rather, if they sang a tune that they in fact could hold, they would no longer have opprobrium heaped on them by their natural science colleagues.

This example also works against Flyvbjerg's argument. The inference of low esteem toward social science in general coming from a single review of a particular work in social science is unjustified. Making an inference from a single case, an ambiguous one at that, is logically unjustified. This suggests that Flyvbjerg has little concern for valid inference, something that should make supporters of his phronetic alternative nervous. His inference is not only invalid; it is wrong, and on two counts. First, Flyvbjerg ignores the intriguing collaborations between biologists and social scientific game theorists in the past two decades that have created new knowledge in fields closely related to the scientific critic of the NORC study.[6] Natural scientists who have worked in productive collaboration with social scientists would hardly hold social scientists as a species in similar contempt as did the reviewer of the NORC book.

His inference is wrong on a second count. The book appeared when the AIDS epidemic was first spreading. Many in the press were reporting linear and ghastly projections of the spread of the disease based on briefings from medical professionals. The NORC team, relying on its scientific finding that there are in America, especially among homosexuals, closed networks of sexual practice, predicted that the growth curve would flatten, and the disease would continue to eat away within segmented sexual communities. The NORC researchers could not offer a precise prediction of how many would incur AIDS, but their research on sexual practice entailed an observable implication, which turned out to be true. This does not prove their methods to be impeccable. Indeed, one could well point to the methodological problem not only in the NORC study but also in the entire genre of studies that postulate causal sequences from cross-sectional survey data. But the NORC team's correct analysis that AIDS would not spread generally through the American population adds confidence that they were accurately portraying American sexual networks.[7] In sum, Flyvbjerg's use of the NORC example as evidence that natural scientists hold in ridicule all forms of scientific activity in the study of the social world is unconvincing.

Flyvbjerg's third example, a study of human learning conducted by Hubert and Stuart Dreyfus, serves as a leitmotif for the entire book. The Dreyfuses conducted an experiment in which subjects were asked to observe videotapes and then evaluate the competence of paramedics, made up of one expert and five novices who were all engaged in cardiopulmonary resuscitation (CPR) to victims of heart failure. The experimental subjects included people with three levels of expertise: experienced paramedics, students learning to become paramedics, and life-saving instructors. The experimental results showed that experienced paramedics, but not the other two sets of observers, could consistently and correctly pick out the expert practitioner of CPR. Those subjects who are novices, or so Flyvbjerg's preferred interpretation goes, were attuned to the question of who was best following the rules of CPR; meanwhile, the expert subjects were less interested in the rules. They were looking for the single practitioner who had an eye for context and knew which rules could be waived to save the largest number of victims.

One of the study's authors (Stuart Dreyfus) offered the following insight to make sense of the finding. He was a mathematician and a chess aficionado. For a long time, he believed that if he could solve all the necessary algorithms, he would become a master. To his chagrin, mathematical logic took him only so far. Those with an expert's "feel" for the chessboard were able to defeat him, and very often these people had no education in higher mathematics. Only those with a feel for the chessboard (often honed by playing "fast chess" rather than studying algorithms), Stuart Dreyfus observed, could become masters. The lesson for social science that Flyvbjerg draws from the experiment and from of the chess anecdote is that in the complex world of human beings, no algorithm will correctly predict action; rather an expert's feel for the context will bring a better grasp of what is likely to occur. Only experts who have worked and lived in the social world (in the same way as chess players having developed skills through practice) will be able to know how best, in the experimental case, to choose a paramedic if they were in need of one.

One could criticize the chess analogy (and by implication the inference) by pointing out that it is increasingly dated, as supercomputers are becoming chess masters with rule-based algorithms. But there are two far more disconcerting things about the use of the Dreyfus's study as a justification for phronesis. First, there is another interpretation of this study, never considered, that undermines the thesis of the book. From what was presented (as admitted by Flyvbjerg),[8] the Dreyfus's used a rather standard scientific procedure common in experimental psychology to make a discovery concerning human cognition. The experimenters learned from their controlled environment (certainly not a case study!) that there are different levels of competence in the human learning process, with implications on what it entails to become an expert. This seems to me to be an advertisement for the scientific method in gaining new psychological knowledge rather than an invitation to jump the scientific ship. Second, there is overwhelming support in controlled experiments that statistical models outperform expert clinical intuitions in diagnosing human disease.[9] Here is a case where natural scientists would put Flyvbjerg up for ridicule for not examining whether a finding he liked was sufficiently robust to work in other experimental settings.

In sum, looking at the Sokal example, the NORC sex study, and the Dreyfus's study as compelling reasons to abjure the scientific method in social science, Flyvbjerg's attempt to create a sense of scientific failure through the use of telling examples is manifestly unsuccessful.

THREE MISUNDERSTANDINGS

Flyvbjerg is adamant that methodological admonitions urging students to study society scientifically are mired in misunderstandings about the social world. But he is guilty of some grievous misunderstandings himself.

1. What Is Context?

"Context" plays a leading role in many tracts purporting to show the limitations of scientific procedures for the study of society, and in Flyvbjerg's book as well.[10] But Flyvbjerg never actually defines it. His method, we are told, is sensitive to context, whereas science is not. Humans are always sensitive to context but not computers. Therefore, people are better judges of complex situations that are heavily influenced by context than are computers. This judgment rests on a grievous overstatement. *Context* comes to us from the Latin *contextus*, meaning a connection of words. In English, this has come to mean, among other related things, "the parts of discourse that surround a word or passage and throw light on its meaning" (*Webster's Ninth New Collegiate Dictionary*). If this is what *context* means, surely computers have been programmed to use surrounding words to throw light on a particular word's meaning. Search engines allow us in our investigation of a particular concept to specify words before and after this concept is used. This procedure helps throw light on a particular concept's meaning.

Of course, Flyvbjerg means more than word connections. Indeed, *contextus* is closely related to the Latin *contexere*, "to weave." Here, context implies a skein of interwoven factors. But to say humans are good at capturing context is hardly a justification for phronesis. For one purpose of social science is to disentangle such skeins in order to trace the effects of its separate strands, or to examine the impact of particular interactions among strands. Appealing to context is merely to say that we have not yet discovered the various factors or the interaction of factors that produced outcomes of significance. Science *is* sensitive to context if sensitivity means the desire to analyze it, to break it down to its separate strands, and to hypothesize how the woven strands influence the course of social events. Ultimately, one's hypotheses about the implications of various contextual strands will demand statistical verification with interaction terms and flexible functional forms. Appealing to context is therefore a cop out; analyzing it and verifying our analytical judgments about it are what social scientists ought to be doing.

2. What Is Science?

Science for Flyvbjerg must meet an ideal or else it is not science. It is portrayed as the activity that can "generate ultimate, unequivocally verified knowledge" yielding some "final truth."[11] Hardly anyone in the natural sciences would hold such a view. Nor would mathematicians, who mostly rearrange symbols consistent with axioms rather than pursue a final truth. Most scientists see their findings as provisional, contingent, and subject to replication and rejection.

Oddly, of the several criteria for science elucidated in *Making Social Science Matter*, the only one Flyvbjerg insists social scientists cannot achieve is that of prediction. Yet this is the only criterion for which Flyvbjerg provides no "philosophy of science" cites. He just asserts it to be a necessary component of science.

This criterion is the most demanding of all, and many fields that are widely respected as scientific (e.g., population biology, evolution, and geology) would quickly fail this test. But if what is meant by prediction is the ability of scholars in the field to make reasonably good probability estimates of individual behavior under laboratory conditions or in well-defined activities (e.g., voting), then several branches of social science can meet such criteria. Social scientists, for example, have long been able to make reasonable predictions of how any individual will vote knowing a few facts about his or her socioeconomic background, age, and education.

Stating ideal criteria for science—and writing off those fields that do not meet these criteria as a breeding ground for phronesis—represents a bimodal approach to scientific categorization. It is better to evaluate research environments as a continuous variable measuring the extent that they approach commonly accepted scientific standards, with the notion that doing better in meeting such criteria dominates doing worse. Instead of some unreachable ideal as the criterion of science, I propose a notion of a scientific frame. To the extent to which a community of scholars is concerned about such things as uncertain (ex ante) conclusions, public procedures, careful measurement, rules of inference, and rewards for replication, that community has adopted a scientific frame.

I also propose that within the scientific frame, a tripartite methodology that includes narrative (the essential component to phronesis), formal, and statistical analyses is the best defense we have against error and the surest hope for valid inference. To the extent that a community has adopted a scientific frame and relies on a tripartite method, it will be in a better condition to make good judgments. The problem with good judgment resting only on one leg of the tripartite method (exemplified in Flyvbjerg's rendition of phronesis) is that it is hard to know if one's judgment is wrong. The scientific frame buttressed with the tripartite method—as I will illustrate in a subsequent section—has ample procedures for figuring out if our best judgments are misplaced.

3. For What Is Phronesis Valuable?

Flyvbjerg is ambiguous about the goals to be maximized in social science. He seems to move the goal post. On one hand, he points to social scientists seeking to make valid causal inferences about the social world. He criticizes them for the inevitability of their failures. But in his alternative model, that of phronesis, his goal is to give students in professional schools useful knowledge, helping them to make a better world. Here I am sympathetic with Flyvbjerg's brief. For professional training of policy analysts and politicians, it would seem useful to focus on normative questions (what kind of life do we want to lead?), experience to get a feel for the practical, and case studies (what kind of world did my predecessors face, and how well did they do?), with somewhat less emphasis on making valid causal inferences about how certain outcomes were reached. For Ph.D. training, the balance would need to be reversed. 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.

PHRONESIS AT WORK

Flyvbjerg summarizes his politically engaged and ultimately successful research on city planning in Aalborg, Denmark, as an example of the potential for phronesis. He reports that a city-planning initiative in Aalborg was captured by downtown businessmen who had a vision of super profits that would come with shoppers who arrived from long distances in their automobiles. They sacrificed the interests of local pedestrians and bicyclists, whose interests were subverted in the plan for roadways into the downtown center. Leaving the ivory tower of intellectual debate, Flyvbjerg confronted local power with phronetic knowledge, acquired through painstaking penetration of the particularities of a single city. Armed with a deep understanding of all backroom deals, his several public appearances parried the slander heaped on him. More important, he presented his data in a way that the public could appreciate. He was thus able to turn the tide away from business control over planning back to the interests of the pedestrians and bicyclists. The citizens of Aalborg were rewarded with democratic debate based on phronetic intervention and an outcome closer to their own preferences.

The smoking gun in Flyvbjerg's investigation was that the Chamber of Industry and Commerce in Aalborg had preferred access to the technical committee of the City Council. Through this preferred access, the chamber's point of view, in which the only route to commercial survival is in attracting customers from far away arriving by car, became the "rational" one in terms of how the future was to be determined. Flyvbjerg sees this as confirming the "basic Nietzschean insight [that] 'interpretation is itself a means of becoming master of something.' " Flyvbjerg concludes, now basing his notion of power on an extended analysis not only of Aristotle and Nietzsche but also of Habermas, Bourdieu, and Foucault, that "the interpretation which has the stronger power base [namely, that of the Chamber of Industry and Commerce] becomes Aalborg's truth."[12]

There appears to be something tautological about this finding. The only way one knows the strength of the chamber's power base is the degree to which it was able to make its position hegemonic. This is hardly a finding about the effects of power on the setting of interpretive frames.[13] For that, we would need to know what resources translate most efficiently into the victory of hegemonic interpretations. We would further need to know the mechanisms (bribes, implicit or explicit threats to leave to other cities, campaign contributions) by which certain resources are expended to secure preferred interpretations. We would need to know how far people can be moved from their ideal points on a policy spectrum by power such as that held by the chamber. And if power is being exerted merely because those who are without it are afraid to defy those with it (and therefore, the exertion of power is not directly observable), we would need to know about the off-the-path beliefs of those without power so that they would be induced into quiescence. Pointing out that power rules is hardly an explanation for its influence, and the two chapters on power give us little handle on its prospects and merits under different well-specified conditions.

To be sure, Flyvbjerg wants phronesis to answer a range of questions. The question of how power is used to create rationality is but one. He also wants social science to answer normative questions about what is desirable and what ought to be done. And he wants social science to help prepare professional students "to help them achieve real practical experience."[14] I have no quarrel at all with the promotion of normative and professional pursuits, but the promotion and quality of such pursuits stand outside of the question of whether for a certain range of questions the scientific frame is appropriate for study of the social. Furthermore, in his discussion of power, Flyvbjerg trespasses onto the zone of science (seeking to identify the causes of the chamber's influence) without playing by its rules. It is phronesis inappropriately applied.

THE DANGER OF ISOLATED PHRONESIS

Nothing calls out more strongly for "social science that matters" than that of civil wars in the post–World War II world. In the course of the past half-century, there has been a slow, steady, incessant outbreak of new civil wars throughout the world. New wars break out at a faster rate than they get settled, such that the number of active civil wars and the percentage of countries experiencing civil wars increased steadily from 1945 to 1999. In the last half-century, there have been more than 16 million deaths as a result of 122 distinct civil wars. Many of these wars have cost the lives of far more than one thousand people, the minimum necessary to be included in Michigan-inspired data sets.[15] In this category stands Sri Lanka, where more than sixty thousand people have been killed in a war pitting Tamil separatists against the majority Sinhalese government. A social science that could help reduce the devastation of civil wars would matter a great deal.

Stanley Tambiah, a world authority on Buddhism, has sought to understand the sources of violence in Sri Lanka from a perspective that Flyvbjerg would clearly agree as phronetic. Tambiah was impelled to study this conflict from a deep normative desire to make his homeland once again an island of peace. He accumulated materials related to the conflict and wrote scholarly books on it and on a related set of deadly conflicts. But he was continuously engaged with authorities in Sri Lanka, with the international press that all-too-often systematically misrepresented the conflict, and with Sri Lankans around the world equally interested in ending a human tragedy. He examined the particular cultural and historical context of the dispute, and all his writings exhibit deep understanding of the local situation, a full recognition of the sources of local power, and a clear desire to alter the terrible curse of interethnic relations that seems Sri Lanka's fate.

Tambiah was at first revolted by but not engaged in Sri Lanka's troubles. In 1956, he had brought a student team with him to investigate a peasant resettlement program in the country's Eastern Province. But the project was interrupted by the first ethnic riots to take place since independence in 1948. These riots occurred when an oppressive language law was being debated in parliament. The majority Sinhalese population was from independence pressing for their language to become the medium of instruction, ultimately through the university curriculum. The language law of 1956, popularly known as the "Sinhala Only Act," promised to make Sinhalese the sole official language of the island within twenty-four hours. Tambiah, then teaching at the University of Ceylon, was immediately disenchanted and felt that he must emigrate. He felt he could not advance professionally if he were compelled to teach in Sinhalese (he is a native Tamil speaker, and English was the medium of instruction throughout his education). Furthermore, the quality of university education would, in his judgment, plummet were it to be cut off from Western scientific literature, a likely prospect were the medium of instruction to become Sinhalese. With ethnic tensions already evident on his home island, he moved his research site to Thailand.[16]

It was twenty-seven years later that he felt "compelled to take up the issues in Sri Lanka concerning ethnic conflict, ethnonationalism, and political violence."[17] A pogrom in 1983, leveled against the middle-class Tamil community in Colombo, in which ministers of the state were implicated, in his words, "fractured two halves of [his] identity as a Sri Lankan and as a Tamil." He wrote *Sri Lanka: Ethnic Fratricide and the Dismantling of Democracy* to find his "way out of a depression and to cope with a personal need to make some sense of that tragedy, which was the beginning of worse things to come."[18] In the preface to that book, he acknowledges that it is not a "distanced academic treatise" but more an "engaged political tract." His goal, he writes, is "not only to understand the Sri Lankan problem but also to change it; it intends to be a historical and sociological reading which necessarily suggests a course of political action."[19] One might say that the 1983 pogrom moved Tambiah from epistemic to phronetic social science.

In his subsequent work on Sri Lanka, he was never far from contemporary politics, asking such phronetic questions (asked in Flyvbjerg)[20] as where are we going and what should be done. He took his theoretical work on Buddhism (conducted in Thailand) to address a compelling concern to all those interested in a peaceful Sri Lanka: how could a religion that advocates nonviolence become the breeding ground for anti-Tamil pogroms? That his answer, published as *Buddhism Betrayed?*[21] was banned in Sri Lanka (and its author accused of being a terrorist) showed that he was speaking truth to power.[22]

Tambiah's accounts of the sources of the Sri Lankan civil war reflect deep concern and careful judgment. He weaves together the social, economic, religious, and political themes in a way that shows mastery of the material. He puts special emphasis on the "Sinhala Only Act." "That," he has noted, "is the beginning of the feeling among Tamils that they were discriminated against by the majority."[23] Tambiah recognizes that those Tamil youths, planning for professional employment and therefore most threatened by the language policy, were not themselves involved in the riots subsequent to the language act. The worst violence occurred in the peasant-populated settlement schemes in the Eastern Province. Tambiah therefore provides a holistic contextual account and writes that, "If one wonders what could be the relationship between the official language controversy and the ethnic violence . . . the answer is . . . the language issue was also becoming interwoven with the government's policy of peasant resettlement."[24]

Sensitive to Flyvbjerg's phronetic concern that researchers address the issue in regard to any policy of "Who gains, and who loses, by which mechanisms of power?"[25] Tambiah analyzes the winning coalition. Politicized Buddhists who espoused racialist doctrines calling for extermination of Tamils organized this coalition. These Buddhists were able to attract into their program rural elites, teachers, indigenous doctors, traders, merchants, and all those educated in Sinhalese who were threatened by the English-speaking elites in the capital. As for the 1983 riots in which up to two thousand people were killed, Tambiah writes, "those who stood to gain [the] most were, firstly, middle-level Sinhala entrepreneurs, businessmen, and white-collar workers, and secondly, the urban poor, mainly through looting."[26]

Tambiah's analysis is fair minded and judicious. But what kind of truth comes from his phronetic engagement, one not combined with the statistical and formal methods? Consider first some statistical data that put a wrinkle in Tambiah's account. A cross-sectional analysis with "civil war" as the dependent variable shows that high levels of linguistic grievance are not predictors of civil war. In fact, controlling for GDP, in most model specifications there is a negative sign, suggesting that higher levels of linguistic grievance are associated with a lower susceptibility to civil war. Although the idea is counterintuitive, the statistical models open the possibility that the oppressive Sinhalese language laws might have ameliorated the violence (triggered by the settlement schemes) rather than exacerbated it.[27]

The first-cut statistical test of the effects of interethnic oppression on the linguistic front raises a host of new questions, previously unasked.[28] Why, if Tamils were most threatened by the language policy, did the Sinhalese initiate most of the rioting in Colombo in both 1956 and 1958, with virtually no Tamil violence aimed at Sinhalese until 1975? Why should there have been post-language law riots that were initiated by Sinhalese, inasmuch as they got the law they wanted? Why did the most horrifyingly fatal riots (those of 1981 and 1983) and the consequent full-scale Tamil rebellion occur after Tamil was accorded nearly equal status as Sinhalese in Sri Lankan law? Or finally, why did the language issue disappear from public debate in inverse proportion to the level of escalation of violence on the island?

The tripartite scientific method helps to address these questions. The cross-sectional statistical data show that the holistic context of an interwoven linguistic and settlement grievance was not like two final straws on a camel's back. Rather, these two issues could well have had polar opposite impacts on the Tamil community.[29] To analyze why, it is useful to model linguistic grievances and to show what each party's best response would be to the probable action of others. From such a model, taking into account preferences of the parties over a range of possible outcomes, it turns out that those most aggrieved by the act were students and teachers. The aggrieved, given their payoff schedule, would gain more from bureaucratic bargaining than they would from guerrilla attacks. From this model, one can comprehend the logic by which the language laws temporarily concentrated Tamil opposition onto the bureaucratic field and politicized rather than militarized the ethnic conflict.

This same model gains plausibility because it helps make sense of another conundrum, namely, that while the Sinhala Only Act had broad public support, its implementation was almost nonexistent. In fact, Sinhala civil servants had every interest in undermining the implementation of a law that would diminish the value of the primary skill—competence in English—that earned them their positions. These bureaucrats wrote careful annual reports on the efforts to implement Sinhala hegemony and in so doing perpetually delayed its fulfillment.

In this research, statistical results put previous narratives under critical scrutiny. A formal model captured the strategic core of the politics of language in Sri Lanka. Thus, through a combination of statistics and formal modeling, one is now compelled to rethink the relationship of the Sinhala Only Act to the Sri Lankan civil war. But explanation does not stop there. There is a third component to scientific explanation. Complementing the statistical and formal approaches is a return to narrative to see if the case would be illuminated rather than obscured by the statistical and formal models.[30] Suppose it were the case that a return to narrative showed again that language grievances drove Tamils into guerrilla camps and into violent confrontation with the state. This knowledge would compel the statistical analyst to specify anew the interaction terms that seemed important in the narrative. If this should turn out successful, the Sri Lankan narrative would have helped yield a more powerful general statistical model. Similarly, formalists would be compelled to rethink the preferences of the actors or the structure of their interactions. Again, the goal would be for a general model of language grievance that could capture the effects of oppressive language laws for political action.

In this case, however, the statistical and formal models helped construct a new and more coherent narrative, one that has not (in my search through the literature) elsewhere been told. Facts that had been obscure in the Tambiah narrative can now be highlighted. For one, those educated Tamils who did not emigrate (as did Tambiah) mostly appealed their cases in the various governmental ministries to ensure their professional advance and the security of their civil service appointments. Second, as noted, the law was consistently subverted by Sinhalese government bureaucrats. More stunning is the fact that previous narratives ignored the crucial sequencing of the violence in 1956 in the face of the passage of the Sinhala Only Act. It was the Sinhalese who struck first in a violent manner but not the Tamils. A more coherent narrative (one which shows that the Tamils did not respond to the act with violence) can be told when there is knowledge that the coefficient relating language grievance to violence is negative! That this narrative has not yet been constructed is in part due to the hegemonic vision among experts that the language issue played at least some role in driving ethnic conflict into ethnic warfare.

The methodological lesson here is that serious social analysis requires a scientific frame, and this frame encompasses all three elements of the tripartite method. Sensitive observers saw oppression in the 1950s and civil war in the 1980s and naturally linked the two in a causal chain. In the absence of a data set including many countries, some with linguistic oppression but most without, it is impossible to ascertain whether one particular factor was ameliorating or exacerbating. Tambiah imagined a positive coefficient linking levels of linguistic grievance to the likelihood of ethnic fratricide, and he therefore viewed Sri Lanka as a case confirming his theory of ethnic warfare. But if he had pictured a negative coefficient as his model, he would have been pushed to ask why Sri Lanka was the exception, having both language grievance and violence. The narrative demands of the question "How did the linguistic grievance play into the set of grievances that led to ethnic war?" are quite different from the one asking, "How come, despite linguistic grievances, Sri Lanka experienced a civil war?" In some cases, a powerful narrative would force a respecification of statistical models that had initially challenged the narrative's causal chain. Here, the statistical findings induced a narrative that shed new light on an old case.

As this example of civil war violence in Sri Lanka shows, it is the interaction of statistical, formal, and narrative work that fills the scientific frame. It helps illustrate why Flyvbjerg's attempt to separate out phronesis (as a kind of narrative) from its statistical and formal complements is radically incomplete and subject to uncontrolled bias. The stark distinction that Flyvbjerg draws between phronesis and the epistemic obscures the productive complementarity of narrative, statistics, and formal analysis in social science.

THE TRIPARTITE METHOD IN PRACTICE

But what, it might be asked especially by those who accept Flyvbjerg's plea for phronesis, is the positive scientific role for narrative within a tripartite method? Is my tripartite method merely giving lip service to narrative, while the technological giants of formal and statistical models wash away all its value? My answer is no. I see narrative as a co-equal to the statistical and formal elements of the tripartite method, playing three roles. First, narrative provides plausibility tests of all formal models, helping us to assess whether a game theoretic model actually represents a set of real-world cases. Connecting a plausible narrative with a formal model is a difficult and subtle task; doing it successfully adds plausibility to a formal model.

An exemplary use of this narrative tool is that by Robert Bates, who applied a reputation model (based on the chain-store paradox) to account for the dynamics of the rise and fall of the coffee cartel. It is often the case that formal models, absent narratives, lead researchers astray. The chain-store paradox is no exception. This formal model explains the rationality of large stores cultivating reputations for underpricing new competition, even if it means selling at a loss until the upstart store goes bankrupt. The model can be appropriated elegantly to show how large countries leading primary product cartels can drive out of the international market those smaller countries seeking to lower prices to gain market share. In applying this model to the coffee cartel, Bates found that although the model was internally consistent and powerful, he could not narrate the historical sequencing of the cartel based on the moves of the reputation game. Brazil was insufficiently powerful to serve as chain-store leader. Thus, the narrative compelled him to rethink the strategic logic and to apply a different analytic tool. It turned out that a spatial model of coalition formation within the largest purchasing country (the United States)—in which a cold war logic provided American support for high-priced imports—explained how a dispersed set of sellers could maintain an oligopoly price as long as they did and why it fell apart when the cold war waned. The narrative did not prove the reputation model wrong; rather it showed that it was the inappropriate representation of the strategic situation that faced the coffee exporting countries.[31] Elegant formal models standing alone are inadequate; they need to be supplemented by narrative to show that the real world is represented in the models. Thus, narrative adds plausibility to formal models.

The second role for narrative is to provide mechanisms linking independent and dependent variables in statistical analyses. It is quite common in social science to find explanatory power in macro-variables such as gross domestic product per capita, or democracy, or ethnic linguistic fractionization (a dispersion index giving the probability that an individual randomly matched with another in his or her country will be of the same ethnic group). The problem is that such social facts as GDP are more like facilitating conditions than causal forces. They do not have the capacity to alter values on a dependent variable. It is therefore difficult to assess what it means for it to be causal for some outcome, such as democracy or civil war. As Elster has taught us, we need to link independent and dependent variables with mechanisms, basically showing how favorable conditions from a statistical sense translate into outcomes.[32]

For example, Przeworski et al. show a statistical link between parliamentary rule and stable democracy (everything else held to mean value). This means that parliamentary democracies are more robust against economic shocks than are presidential systems.[33] But this finding requires a mechanism to give it causal weight. This has led Przeworski (and other collaborators) to examine exogenous shocks, in a narrative mode, to figure out which of the scores of mechanisms listed in the literature are actually causal. One early conjecture was that in parliamentary systems, governments face no-confidence votes and are likely to fall. But here, the government not the democracy is challenged. Since presidents have fixed terms, and there is no institution with the constitutional authority to vote the president out of office for weak performance, in a presidential system an exogenous shock is likely to invite the army to compel the president to leave office. When this occurs, not only the government but also the democratic regime falls. Here, the no-confidence weapon is the mechanism (found through narrative but then complemented with a formal model and statistical tests) that gives the original statistical finding causal weight. This conjecture remains provisional. While one of the papers to emerge from this search for mechanisms emphasizes statistical tests and formal proofs,[34] the narrative mode was the source of insight into mechanisms.

In providing plausibility to formal models and mechanisms for statistical models, it is sometimes the case that the role of narrative gets obscured in the final presentation of scientific work. Consider an exemplary model of the tripartite method, Randall Stone's *Lending Credibility*.[35] This study assesses the impact of International Monetary Fund (IMF) conditionality programs on economic performance in the post-Soviet states. Numerous earlier studies found only mixed results. Sometimes the IMF impact was positive and other times negative. Thus, the accepted view that the IMF was no nostrum for structural maladjustment. Through careful (one might say phronetic) investigations, Stone figured out that the IMF succeeded only where its threats (to cut the country off from further loans) were credible. For large countries of great strategic value to the United States, however, such threats were not credible, as these countries knew that they would be bailed out by the United States if they defaulted. Stone therefore creates a model of credibility that predicted where the IMF would have success, and his statistical tests confirmed the observable implications of the theory. As expected, strategically important countries were punished more often, but their punishment periods were shorter. Also, they were less likely to keep inflation under control and less likely as well to attract foreign investment.

Stone's narratives helped him develop the formal model that was then put to statistical test. The very success of the model meant, however, that there were few surprises or new causal conjectures in the chapters that told narratives of particular countries that received IMF support. One might say that the findings of the narrative were already eaten up by the formal model and statistical tests. The four chapters narrating the model in the cases of Russia, Ukraine, Poland, and Bulgaria therefore fell flat. This was not because narrative was not important; rather it was because the findings from narratives fed into statistical model specifications such that there was little new to add in the ultimate telling of the country-level stories.

Mechanisms, Stone's work illustrates, are in some cases no more than underspecified intervening variables. To the extent that a narrative provides the appropriate mechanism, it is incumbent on the researcher to specify the values on that mechanism and run the statistical model again with a new variable. If the mechanism-turned-variable fails in a significance test, it should give us pause as to whether it really was the causal link between the independent and dependent variable. But if it proves significant statistically, and it gets built into a formal model, adding it to the narrative will make it appear that the narrative is secondary. In fact, the narrative was the source for the correctly specified causal mechanism.

Suppose, however, that there are several mechanisms linking a set of values on right-hand-side variables to a specific value on a dependent variable. The favorable right-hand-side conditions might be thought of as opening a set of separate pathways toward the same value on the dependent variable. In such cases, all of the mechanisms could fail statistical tests even if properly specified because none could account for more than a small subset of the observations.[36] A statistician might respond by saying that the mechanisms were not properly specified because the conditions under which they were conjectured to operate ("a small subset of the observations") were not adequately operationalized in the statistical model via interactions, non-linearities, and so on. Even if there were not enough data for signal to overwhelm noise at conventional levels of statistical significance, Bayesians have developed methods to squeeze significance even when faced with "degrees of freedom" problems. But as pathways multiply, these techniques get increasingly tenuous. Under such conditions, narrative would need to stand alone, and rules of narrative coherence and completeness would help to decide whether the causal structure was as theorized. Here, narrative would be providing a more apparent value added than in the case where there was a single mechanism that linked right- and left-hand-side variables.

But even in the case where there is a single mechanism, one that holds up to statistical scrutiny, narrative plays a third role, and this through the analysis of residuals. Never in social science is all variance explained, and even in powerful models, the amount that we are able to explain is often paltry. Narrative, by giving a more complete picture of a social process, fills in where statistical and formal models are incomplete. In the case of Stone's narratives, we learn in Poland that Finance Minister Leszek Balcerowicz was more committed to showing the credibility of Poland's reform to international capital than was the IMF. In the narrative, part of the causal weight goes to the charismatic and technical mastery of

Balcerowicz over politicians on both the left and right sides of the political spectrum. We have few tools to model formally or test statistically the role of charismatic leadership in the fostering of reform. Yet in this case it may well have had causal weight, especially because Stone reveals that Poland was quite important strategically to the West, and this should have made its leaders more likely to defy the IMF and to inflate the currency.[37] Examination of the residuals through narratives plants the seeds for future work that can better specify and model causal factors that carried weight in the narratives but were absent in the statistical and formal models.

There are thus three scientific roles entrusted to narrative in social science. First, they provide plausibility tests of formal models. Second, they provide mechanisms that link statistically significant facilitating conditions to outcomes. And third, through the plotting of residuals, they plant the seeds for future specifications of variables that have not yet been successfully modeled. In no sense is the phronetic part of the scientific enterprise a marginal one.

CONCLUSION

The Aristotelian division between *episteme* and *phronesis*, as applied by Flyvbjerg, maps well onto recent methodological debates within political science, as evidenced in Mr. Perestroika's assault on the disciplinary hegemons, between rational choice and qualitative research. Like Flyvbjerg in regard to epistemic science, supporters of qualitative research equivocate about the long-term prospects of rational choice modeling in the social sciences. But at minimum, Mr. Perestroika's acolytes call for methodological pluralism.[38] The approach taken to science in this article, while carrying no brief against pluralism, entails a caution against a pluralism that sees formal and statistical research as only two of a thousand flowers that should be permitted to bloom.

The caution is to insist that if theoretical logic or scientific evidence finds a theory or procedure to be fallacious, that procedure's flowerbed should no longer be cultivated within the discipline in which it was originally seeded. There can be no hope of cumulation if we insist that all methods, and all procedures, must be protected. A few examples of unjustified pluralism follow. Consider first the method of case selection in comparative politics. It was once considered by the community of comparativists a useful exercise to choose a set of cases that had the same interesting outcome (for example, modernization breakdowns) to learn what causes it. Subsequent work in the methods field called this procedure "selecting on the dependent variable" and showed why it will ultimately lead to faulty inferences about causation.[39]

Similarly, many statistically oriented scholars in the field of international relations relied on logistic regressions to analyze binary time-series data on whether there was an outbreak of war in a given year. This procedure was found to lead, at least in some cases, to invalid inferences. The authors who report the bias show that this problem can be corrected with a set of dummy variables tapping unmeasured state dependence in the data (e.g., the longer a spell of peace, the less likely a war, ceteris paribus).[40] It would be a scientific travesty should one group of international relations specialists demand that statistical modelers who do not correct for serial dependence have a right to continue as they were doing, simply because there is a long tradition in cross-sectional work that has in the past ignored problems of time dependence.

A final example: comparativists who do qualitative case studies have no claim to disciplinary recognition by virtue of the fact that examination of a single case is a time-honored procedure in their field. Theoretical work going back to Eckstein sets constraints on what a particular case can show.[41] More recent methodological work, exemplified in the text by King, Keohane, and Verba, gives a road map on how a study of a single country can be transformed into a high-*n* research design, thereby increasing the study's scientific leverage.[42] There can be no argument based on tradition justifying the minimization of leverage. New work in comparative politics must, if it is to gain respect in the wider discipline, adjust methodologically to take into account scientific advances. Pluralism without updating is not science.

This point is doubly important when fields get defined by positions in grand debates and protected by tradition. It would be a warping of the scientific frame if we built into the charter of any department of political science that there had to be an expert in "realism," or in "South Asia," or in "democracy," or in "qualitative methods." Of course, advertising for jobs by area of specialization is crucial, especially if a department seeks broad disciplinary coverage. But institutionalizing slots for particular specialties is a threat to scientific progress. Consider a document from seventeenth-century Spain in which the University of Barcelona appealed to the king's audience for the right to sidestep interference in its affairs by the Council of Castile, which had stipulated that the department of philosophy have three professors who held to Thomist views and three who did not.[43] Three centuries later, it appears quaint that a philosophy department should be divided along those lines. But the implications of such royal charters are dangerous. When any academic field consecrates a debate by giving interlocutors on both sides permanent representation, the result can only be resistance to innovation. A scientific frame would lead us to expect that certain fields will become defunct, certain debates dead, and certain methods antiquated. A pluralism that shelters defunct practitioners cannot be scientifically justified.

Flyvbjerg at his most generous is calling for pluralism but giving pride of place to an alternate methodology for the social sciences, going back to Aristotle's recommendations. But rather than accepting an alternate methodology, this article asks that we all work inside a scientific frame. Within that frame, we ought to maximize inter alia openness of procedures, internal coherence of argument, good measurement of variables, increasing attempts to unravel context, assiduous concern for valid causal inferences, and rewards for replication. Along with formal and statistical analysis, narratively based case studies (as one element in the procedures Flyvbjerg recommends as phronesis) play a crucial role in filling in this frame[44]; but there is nothing to be gained in advertising a program that does not insist on the best approximation to science as the data and our abilities will allow.

NOTES

1. "Mr. Perestroika" is the pen name of an anonymous insurgent within the political science discipline. His movement began with an e-mail sent to friends and colleagues in October 2000, but it spread like a bush telegraph, precipitating a mass mobilization within the discipline against the practices of the American Political Science Association and its lead journal, the *American Political Science Review.* The ferment first received attention in an article in the *New York Times* (5 November 2000), followed by coverage in the *Chronicle for Higher Education* (17 November 2000). Movement members remain active in seeking to alter the discipline organizationally and intellectually.

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

3. In his review of this book for the *American Political Science Review* (March 2002), Stephen White emphasizes that Flyvbjerg's book ought to serve as the foil for a fully articulated anti–Mr. Perestroika response.

4. Flyvbjerg equivocates throughout the book on the question of whether scientific work has any merit in the study of the social world. On one hand, he writes, "it is . . . not meaningful to speak of 'theory' in the study of social phenomena, at least not in the sense that 'theory' is used in natural science" (p. 25). On the other hand, he acknowledges the value of "attempts at formal generalization, for such attempts are essential and effective means of scientific development" (p. 76). Despite these occasional nods to the value of a social science (see also formulations on pp. 49 and 87), his major theme is that "we must drop the fruitless efforts to emulate natural science's success in producing cumulative and predictive theory" (p. 166). He does not provide evidence on the degree to which natural science research meets his standards.

5. This tripartite approach recommended here is more fully developed in David D. Laitin, "Comparative Politics: The State of the Subdiscipline," in *Political Science: The State of the Discipline* edited by Ira Katznelson and Helen Milner (New York: Norton, 2002).

6. I know of three prominent contributions along these lines. Robert Axelrod (political science) has collaborated with William D. Hamilton (biology), "The Evolution of Cooperation," *Science* (1981) New Series, (March 27) 211 (4489): 1390-96. Reinhard Selten (economics) has collaborated with Peter Hammerstein (biology) in "Game Theory and Evolutionary Biology," in *Handbook of Game Theory*, edited by Robert J. Aumann and Sergiu Hart (Amsterdam, the Netherlands: Elsevier, 1994), vol. 2, 929-93. Robert Boyd (anthropology) has collaborated with Peter J. Richerson (ecology) in *Culture and the Evolutionary Process* (Chicago: University of Chicago Press, 1985).

7. See a summary discussion on this matter at http://www.cdc.gov/mmwr/preview/mmwrhtml/00001277.htm. The team reported, while acknowledging validity problems due to the sensitivity of the questions, that "most Americans appear to be at relatively low

risk of infection with HIV-1 and other STDs from sexual exposures." See also Edward O. Laumann et al., *The Social Organization of Sexuality* (Chicago: University of Chicago Press, 1994), 546-47.

8. Flyvbjerg, *Making Social Science Matter*, fn. 19, p. 22.

9. For an extensive review of this literature, see Robyn M. Dawes, David Faust, and Paul E. Meehl, "Clinical versus Actuarial Judgment," *Science*, New Series, 243, no. 4899 (31 March 1989): 1668-74.

10. "Context is central to understanding what social science is and can be" (p. 9). There are nineteen other references to this term in the index. Chapter 4 is called "Context Counts."

11. Flyvbjerg, *Making Social Science Matter*, 139.

12. Ibid., 153.

13. In fairness to Flyvbjerg, there is more explicit attention to mechanisms in his full study, *Rationality and Power: Democracy in Practice* (Chicago: University of Chicago Press, 1998).

14. Flyvbjerg, *Making Social Science Matter*, 145, 72.

15. James Fearon and David Laitin, "Ethnicity, Insurgency, and Civil War," *American Political Science Review* 97, no. 1 (2003).

16. Stanley Tambiah, "Continuity, Integration and Expanding Horizons" (interview with Mariza Peirano), Série Antropologia, no. 230, Departamento de Antropologia, Universidade de Brasília, 1997, 8-10.

17. Ibid., 14.

18. Ibid., 26.

19. Stanley Tambiah, *Sri Lanka: Ethnic Fratricide and the Dismantling of Democracy* (Chicago: University of Chicago Press, 1986), ix.

20. Flyvbjerg, *Making Social Science Matter*, 145.

21. Stanley Tambiah, *Buddhism Betrayed?* (Chicago: University of Chicago Press, 1992).

22. Tambiah, "Continuity," 26-27.

23. Ibid., 9.

24. Tambiah, *Buddhism Betrayed?* 57 and 47.

25. Flyvbjerg, *Making Social Science Matter*, 145.

26. Tambiah, *Sri Lanka: Ethnic Fratricide*, 20-27; Stanley Tambiah, *Leveling Crowds* (Berkeley: University of California Press, 1996), 100.

27. These points are developed in David D. Laitin, "Language Conflict and Violence," *Archives Européennes de Sociologie* 41, no. 1 (1997): 97-137. The subsequent discussion draws on that article, without use of quotation marks.

28. The results would be more compelling if the effect sizes were properly analyzed so that something could be said about substantive significance of the negative relationship between language oppression and violence. The point here, however, is not to infer a negative relationship supported by the data but to wonder why there was no strong positive relationship, as standard theories of grievance had led us to expect.

29. James Fearon and I address the issue of why settlement schemes more likely yield guerrilla action in "Sons of the Soil," unpublished manuscript.

30. Many in the narrative tradition claim that narratives ought to be formalized. It may well be that game trees and narrative both are formal models but perform complementary tasks in scientific explanation. If this were the case, the terms referring to the tripartite agenda would require adjustment.

31. Robert Bates, "The International Coffee Organization: An International Institution," in *Analytic Narratives*, edited by Robert Bates et al. (Princeton, NJ: Princeton Uni-

versity Press, 1998). The references for the chain-store paradox are Reinhard Selten, "The Chain-Store Paradox," *Theory and Decision* 9 (1978): 127-59; and David Kreps and Robert Wilson, "Reputation and Imperfect Information," *Journal of Economic Theory* 27 (1982): 253-79.

32. Jon Elster, "A plea for mechanisms," in *Social Mechanisms: An Analytical Approach to Social Theory*, edited by Peter Hedstrøm and Richard Swedberg (Cambridge: Cambridge University Press, 1998), 45-73. Techniques to assess causal mechanisms without use of narrative include experiments and recently developed random-matching models in statistics. See Judea Pearl, *Causality* (Cambridge: Cambridge University Press, 2000), for explications of these techniques. I remain skeptical that either the experimental or statistical innovations will supplant narrative in helping to uncover the causal mechanisms linking values on independent variables to values on dependent variables.

33. Adam Przeworski et al., *Democracy and Development: Political Institutions and Well-Being in the World 1950-1990* (Cambridge: Cambridge University Press, 2000). Random-matching techniques (see Pearl, *Causality*) allow us to avoid the unrealistic assumption that other independent variables have comparable values in parliamentary and presidential regimes.

34. José Antonio Cheibub, Adam Przeworski, and Sebastian Saiegh, "Government Coalitions and Legislative Effectiveness under Presidentialism and Parliamentarism" (unpublished paper, 2002).

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

36. I believe this is at least part of what Elster is suggesting with his view that mechanisms will almost never reveal statistically significant relationships.

37. Stone, *Lending Credibility*, 99.

38. See John J. Mearsheimer, "Methodological Parochialism vs. Methodological Pluralism" (paper presented at Panel 7-1, "Perestroika: Undisciplined, Unpunished," APSA meetings, San Francisco, 31 August 2001).

39. Barbara Geddes, "How the Cases You Choose Affect the Answers You Get: Selection Bias in Comparative Politics," *Political Analysis* 2 (1990): 131-50.

40. Nathaniel Beck, Jonathan N. Katz, and Richard Tucker, "Taking Time Seriously: Time-Series-Cross-Section Analysis with a Binary Dependent Variable," *American Journal of Political Science* 42, no. 4 (1998): 1260-88.

41. Harry Eckstein, "Case Study and Theory in Political Science," in *Handbook of Political Science*, vol. 1, edited by Fred Greenstein and Nelson Polsby (Reading, MA: Addison-Wesley, 1975).

42. Gary King, Robert Keohane, and Sidney Verba, *Designing Social Inquiry* (Princeton, NJ: Princeton University Press, 1994).

43. Case number 064, in the year 1681, from a data set of 617 documents from seventeenth- and eighteenth-century Spain, housed at the Rare Book Division of the Princeton University Library.

44. This argument applies to my promotion of the tripartite method. In response to a critic of their article cited earlier, Dawes, Faust, and Meehl point out that although the results are not conclusive, clinical predictions appear to be better if researchers rely on statistical models only and ignore clinical judgments by experts. See Benjamin Kleinmuntz et al., *Science*, New Series, 247, no. 4939 (1990): 146-47. Should it be demonstrated that narrative judgments add no explanatory or predictive value in political science (which I doubt would occur), it would be in defiance of the scientific frame to continue insisting on the tripartite method.

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