

PERESTROIKA!

The Raucous Rebellion
in Political Science

EDITED BY

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YALE UNIVERSITY PRESS
New Haven and London

PART I

History, Background, and Perspectives of Perestroika

Part I opens with the original communiqué sent by Perestroika in an untraceable e-mail with the request that recipients circulate it to friends. This is followed by several chapters designed to place the Perestroika movement in comparative, intellectual, and historical perspectives.

Perhaps the best-known Perestroikan and the president of the APSA during much of the discussion that followed the original communiqué, Susanne Hoeber Rudolph, focuses on the central issue that first united Perestroikans: the methodological hegemony of the political science establishment of the time. But Rudolph also notes that the Perestroika movement has evolved into what she describes as a relatively acephalous and loose organizational form for conversation and engagement among the diverse groups within the political science community. Rudolph argues that the conversation has taken this form in part to accommodate the multiple modes of inquiry subsumed under the Perestroika label. For Rudolph, these various modes are united by their opposition to the monopoly claim of the scientific trope in the social sciences. Rudolph finds considerable and consequential heterogeneity as well within the collective "other" targeted by Perestroika adherents. Despite this variety, each side displays internal commonalities that Rudolph summarizes in two clusters of contrasting attributes. Her chapter uses these contrasting features to explore the differences between Perestroikans and their epistemological other.

Catarina Kinnvall provides an important contrast between American and European social science. Her chapter makes the critical point that the methodological hegemony in American political science—a hegemony that stimulated Perestroika in the first place—does not exist in Europe. With no *ancien régime* to topple, there is no need for rebellion. Her chapter begins by elaborating potential differences between Europe and the United States in regard to intellectual tradition. In particular, Kinnvall outlines how political science and international relations might be defined in the American context, and how that definition interacts with the intellectual, cultural, and institutional traditions characteristic of Europe. The second part of Kinnvall's chapter compares the differences between the European and the United States political science

communities in regard to publications, research, curricula, and career opportunities. Highlighted in this section is the higher visibility of political philosophy in teaching and research in European departments, and how the formalized tenure-track system of the United States affects publication patterns in comparison to those of Europe. The chapter ends by drawing general conclusions about how the absence of a European Perestroika debate impacts the profession and the academic status of the discipline.

Theodore J. Lowi brings a historical perspective, comparing the current Perestroika movement with the activities of the Caucasus for New Political Science, which was formed in the late 1960s and early 1970s. Lowi argues that we should let a hundred Perestroikas blossom, but only if Perestroika nurtures a permanent spirit of *glasnost* (enlightened confession), which will produce *sanokritika* (self-criticism) throughout the profession. Lowi argues that a war over proportionality in the *American Political Science Review* and in the APSA won't accomplish anything because there is no elite dominance or conspiracy to vanquish. The best method of analysis, he argues, is a "politics of knowledge": the cultivation of an awareness that dominant fashions in the field are a product of what the state is doing at that time. Building on his 1991 APSA presidential address, Lowi presents two case studies—of the "behavioral revolution" and of the importation of economic rationality into political science—to demonstrate how their hegemony was literally produced by state development.

The chapter by Samuel H. Beer—originally written in response to a request from a graduate student at Berkeley—extends Lowi's historical perspective. Beer attempts to paint an informal sketch of the methods of research fancied by Beer and his Harvard colleagues during the past half century. Beer's chapter might be read as the story of the rise and fall of behavioralism, a pattern contrasted with what Beer concludes is the continued dominance of a historical approach. The thrust of Beer's argument is that history demands our attention as the activity of culture-creating (and -destroying) actors. This capacity precludes the possibility of universally valid laws of political behavior. It does not rule out the discovery of regularities resulting from the impact of situational structure upon intentional motivation. But such generalizations are limited to particular historical contexts. Because political scientists also have this culture-bearing capacity, they can imagine themselves in the positions of their subjects of study and thereby grasp what they do and why they do it. This historical method is commonly called *Verstehen*, or subjective understanding. To emphasize that the subject matter is activity, Beer calls it with regard to explanations of the past "imaginative reenactment" and with regard to predictions "imaginative preenactment." As outstanding examples of political science in this vein he cites Adam Ullmann's studies of Lenin and Stalin, Sam Huntington's survey of the clash of civilizations, and Sidney Verba's analysis of civic voluntarism. This chapter, by a major figure in twentieth-century comparative politics, raises important issues about the nature of science, especially in a comparative context, and provides a lovely segue into the next part of the book, on methodological issues.

CHAPTER I

The Idea The Opening of Debate

Perestroika

[Following is a reprint of the initial communiqué from the individual or group self-named Perestroika, as received by Kristen Monroe. The reprint preserves the original spelling, punctuation, and spacing.]

On the irrelevance of APSA and APSR!

Please Spread this Letter as widely as Possible

Let them know we Exist

To: The Editor of *PS* and *APSR* [*American Political Science Review*]

October 15, 2000

On Globalization of the APSA and APSR: A Political Science Manifesto

Questions to ponder over:

- 1) Why do people like Benedict Anderson and James C. Scott find the APSA and the APSR irrelevant? These are probably the most famous political scientists in the world. They are equally famous abroad and in other disciplines compared to the "stars" of Political Science. (Hey, Hey, Yee. Look at their classic book on literary methodologies.)
- 2) Related to above is the question: Why do a majority of political scientists who do comparative politics ignore APSA and APSR and go to their regional meetings and read regional association journals, e.g., such as those associated with East Asia, Latin America, Hispanic Studies, etc?
- 3) Why does a "coterie" of faculty dominate and control APSA and the editorial board of APSR? I scratch your back, you scratch mine. I give an award to your student from Harvard and you give mine from Duke or Columbia. In short why do the "East Coast Brahmins" control APSA?
- 4) Why are a few men who make poor game-theorists and who cannot for the life-of-me compete with a third grade Economics graduate student, WHY are

these men allowed to represent the diversity of methodologies and areas of the world that APSA "purports" to represent?

5) Why are FAILED Africanists and Economists allowed to dominate a discipline which has a rich history of intellectual contributions from the likes of James Scott, Charles Tilly, Aristide Zolberg, Leonard Binder, Benedict Anderson, R. Bendix, Susanne Rudolph, Theda Skocpol, etc.?

6) Have we learned any lesson from the thousands of pages of research that was funded by APSA in the name of political science to examine the former Soviet Union and make "predictive" models? What happened to those models and why did they fail? How is it that those esteemed colleagues failed to predict the collapse of the Soviet Empire while Sovietologists from Korea, Japan, India and one even from Tanzania could predict the fall of the empire? Are we making the same mistake by ignoring diverse knowledge and methodologies present in the study of Politics?

7) Why isn't [an] APSR subscription made separate from the APSA membership so that APSR becomes truly representative of a "coterie" that rules APSA while the rest of the true political scientists can devote their money to buying the more important regional journals? Either reform the APSR board and have more political historians, area specialists, political-sociologists, and constructivists on the board or let the market decide. You will find a sharp drop in APSR's subscription as soon as APSR is delinked from the membership of APSA.

8) Why are the overwhelming majority of Presidents of APSA or editorial board members of APSR WHITE and MALE? Where are the African-Americans, Hispanics, Women, Gays, Asians—in short, where is the diversity of United States and the world that APSA "pretends" to study—is somebody afraid that APSA will slip out of their hands???

9) Why are all the articles of APSR from the same methodology—statistics or game theory—with a "symbolic" article in *Political Theory* that is often a piece that has been rejected by the journal *Political Theory*. Where is political history, international history, political sociology, interpretive methodology, constructivists, area studies, critical theory and last but not the least—post modernism? Why can't you have 5 per cent of the articles in APSR allocated under the category: incomprehensible. Then just go ahead and publish game theory, statistics and post-modernism under the category.

10) At a time when the free market models of economics are being challenged in IMF [International Monetary Fund] and World Bank, discredited in much of Asia, and protested by numerous groups, why are simple, baby-stuff models of political science being propagated in our discipline? If these pseudo-economists know their math so well, let them present at the University of Chicago's Eco-

nomics Workshop. I assure you every single political science article will be trashed and thrown into the dustbin. Then why are these people allowed to throw their weight around based on undergrad math and stats—an Econ 101. We are in the business of Political Science and not failed Economics.

Lastly,

11) When are you going to offer the APSA presidency to Benedict Anderson or Charles Tilly or Richard Falk or Susanne Rudolph or Ari Zolberg or James C. Scott or Theda Skocpol, who are more representative of our discipline than the "coterie" that runs APSA?

I hope this anonymous letter leads to a dismantling of the Orwellian system that we have in APSA and that we will see a true Perestroika in the discipline.

Mr. Perestroika

CHAPTER 2

Perestroika and Its Other

Susanne Hoeber Rudolph

On October 15, 2000, an anonymous political scientist calling himself, herself, or themselves Mr. Perestroika sent an immoderate e-mail message to a small set of colleagues. It excoriated the APSA and its flagship journal, the *American Political Science Review* (APSR), for irrelevance, technicism, statistical obsession, third-class economics, domination by East Coast white males, and oligarchical practices: "Why are all the articles from APSR from the same methodology—statistics or game theory—with a 'symbolic' article in political theory . . . Where is political history, international history, political sociology, interpretive methodology, constructivism, area studies, critical theory and last but not least—post modernism?"¹

This e-mail message evoked an astonishing response in a profession in which prudence and gravitas are admired. The message seemed to articulate the suppressed dissatisfactions of a significant portion of the profession. Much of the early e-mail that inundated the apparently astonished sender came from senior faculty who enthusiastically embraced one or another part of the message, and encouraged moves to send challenging letters to PS: *Political Science and Politics*. But the seniors of our profession also set about cleaning up the message's syntax and leaching out its angry tone. Soon they signed on to more sober versions of Mr. Perestroika's critique² in order to mobilize a large constituency. A widely signed letter to PS by Rogers Smith, a theorist then at Yale, supported the manifesto: "Why do the APSR and other professional fora seem so intensively focused on technical methods, at the expense of the great, substantive political questions that actually intrigue many APSA members, as well as broader intellectual audiences? Though some recipients may have felt uncomfortable with the anonymous authorship and the highly polemical tone of this post, nevertheless an astonishing number of scholars, from all ranks of the profession, felt impelled to announce that they, too, shared these profound dissatisfactions with the status quo."³ Richard Betts, a professor of international relations at Columbia, concurred: "The work published in the journal comes almost exclusively from those who do quantitative research and formal modeling, and very disproportionately from just one of the four prin-

cipal subfields of the discipline (American Politics)." The Perestroika list serve grew into a casual community of discontent, a horizontal acapaluous gathering of like-minded people. Critics asked, Who are you? Whom do you represent? To whom are you accountable? The attempts by some Perestroikans to turn the list serve into a formal organization were resisted. It remained as it had begun, a forum for loosely structured conversation.

The nature of the discontents that surfaced was by no means homogeneous. Perestroika came to include theorists of very different stripes. Some feared for the death of norms at the hands of a militant positivism; others resisted the monopoly claims of scientifically certified objective truth. The Perestroika conversation harbored liberal arts faculty and teachers of undergraduates who sought vivid, engaging texts that address large social and political questions. The parsimonious abstractions of the formalists struck them as devoid of meaning. The conversation spoke for a problem-driven rather than a method-driven discipline and deplored the tendency to reward technical virtuosity for its own sake. Political ethnographers and (old) institutionalists who thought that context mattered protested the one-size-fits-all claim of rational choicers. Communitarians damned the possessive individualism of methodological individualists. Bombs-and-bullets international relations practitioners defended case studies; interpretivists defended participant observation and semiotics; historical institutionalists and area scholars defended variety and difference in face of the homogeneity claims of large *n* studies.

The anonymity of the original Perestroika, who continued to manage the list serve, was a source of controversy. Did it not compromise the transparency and accountability of the enterprise? Or should the choice of anonymity be read as indicative of the chances a young political scientist took when he or she spoke truth to epistemic power? Perestroika's continued anonymity turned Perestroika into a kind of Rorschach test, amenable to varied interpretations by the many streams of dissent. Anonymity prevented the identity of any particular Mr. or Ms. Perestroika from defining the movement.

Despite the disparate streams constituting Perestroika, there was agreement that the APSR had been hijacked and monopolized by practitioners of rational choice and formal modelers using statistical tests and mathematical proofs. The dissidents saw political science as an open-ended category, not an objective one independent of history, ideology, or culture. They saw the discipline's form as the result of a social process in which living agents constituted meanings and models and standards. Lead actors in this social process were the APSR and the four regional political science journals, not faceless entities but living organisms motivated by editors and their collaborators. The journals were seen as abusing their certifying role, their capacity to decide for the profession what constituted quality political science, by drawing the boundaries too parochially. Their role as gatekeepers for the profession had consequences for the quality and orientation of teaching and research. When an editor under the spell of the scientific paradigm wrote a would-be contributor, "I'm returning your ms. because it is unsuitable for a political science journal," she was

disclosing not a self-evident truth but a socially constructed, contestable judgment. Homogeneous, narrow journals reduce the space in which political scientists can ask questions, pursue knowledge, find and retain jobs, and get promoted. The dissidents wanted a more inclusive view of what counts as political science.

Perestroika did not write on a blank slate. The pervasive presentism of political science obscures the fact that challenges to a prevailing behaviorist hegemony had already been raised in the late 1960s by the Caucus for a New Political Science. However, the crucial issues for the APSA dissidents of 1968–69 had been different from the issues Perestroikans raised at the turn of the millennium. The revolt of the late 1960s had merged the questions of *demographic* representation (race and gender) with questions of *political* representation (opposition to the Vietnam War). The movement that started in 2000 is more about what we might call *epistemological* representation.

The dissidents of 1968–69 felt that American political scientists had been too slow in recognizing the failings of the Johnson administration in the Vietnam War. Resistance to the war fueled resistance to racism and sexism. “Mississippi summer,” student activists, and civil rights protests and freedom marches helped radicalize women. Race and gender moved to the top of the country’s and the APSA’s agendas. The movement of 1968–69 had more of what Robert Jervis, in his chapter in this volume, indicts Perestroika for not having—ideological concern and commitment. It is striking that the prospect of a war against Iraq, not much more popular in academia than the Vietnam War (but without the incendiary stimulant of the draft), played no significant role in the Perestroika movement. Participants in the conversation accepted one of Perestroika’s rare personal interventions, a decision not to let objections to the Iraq war crosscut the resistance to epistemological hegemony.

The movement of 1968–69 did have a recessive epistemological dimension. Its spokespersons, like those of 2000, felt that political science had become indifferent to the great social issues of the day. It was increasingly fixated on technique and methodological precision. David Easton’s 1968 APSA presidential speech called for an end to the so-called behavioral revolution and for the profession to address the pressing issues of the era, such as race, poverty, and gender: “Substance must precede technique. If one must be sacrificed for the other—and this need not always be so—it is more important to be relevant and meaningful for contemporary urgent social problems than to be sophisticated in the tools of investigation. For the aphorism of science, that it is better to be wrong than vague, post-behavioralism would substitute a new dictum, that it is better to be vague than non-relevantly precise.”⁴

While the epistemological discontents of the mid- to late 1960s were directed against empiricism and those of 2000 were directed against formalism, they converged in their protest against technicism and impersonality in the pursuit of knowledge. Both opposed the dehumanization that accompanied academic Fordism. The Berkeley students made the warning on the eighty-column, thirteen-row IBM punchcard—Do Not Fold, Staple, or Mutilate—

their battle cry. The card was central to the rationalization of university procedures as well as to the methodologies of the behavioral revolution. This student battle cry expressed alienation from the normative anesthesia of academic objectivity, even while protesting institutional surveillance and regimentation. There are echoes of such alienation in the Perestroika e-mail conversation’s repudiation of uncontextualized inquiry and formalistic methodology.

As Brian Cateino points out elsewhere in this volume, there is a long genealogy of such epistemological conflicts, many of which were fought out in the *Methoden Streit* that preoccupied German academics at the end of the nineteenth century. Max Weber’s essays on methodology represented an attempt to reconcile the claims of the *Natur Wissenschaften* and the *Geistes Wissenschaften*, an opposition renewed and lamented after World War II by the novelist and scientist C. P. Snow in his *Two Cultures*. Weber conceded that the social sciences, like the natural, could be construed as subject to regularity if not law, a view staunchly resisted by those who imagined that the human sciences grasped only the unique and the particular. Contrary to the *Geistes Wissenschaftler*, who asserted that a lawful universe had to be firmly rejected because it was a determined universe (foreshadowing the structure versus agency argument of the 1990s), Weber argued for the compatibility of regularity with agency. The implicit determinism of a lawful universe could be modified by the responsible, choosing individual precisely because he or she understood its regularities.

The heterogeneity of the constituency for Perestroika has its counterpart on the opposite side. The unabashedly nonempirical nature of formal modelers who invoke mathematical proofs is in marked contrast with the empiricism of many statistical approaches. But when methodological disputation breaks out, advocates of these tendencies can unite. They fancy they have common objectives because they have common opponents. Can we specify the attributes respectively of Perestroikans and their epistemological “other”? I’ve made myself two lists, each of which represents an ideal type of one of the contending perspectives. It is an ideal type in the conventional sense that while all the attributes that characterize the ideal type do not appear in real world instantiation, the ideal type provides criteria that help us analyze and distinguish real world complexity. My categories are not strictly isomorphic. But even when they overlap, each provides a contrasting formulation.

“Scientific” Mode of Inquiry	“Interpretive” Mode of Inquiry
certainty	skepticism; contingency
patrimony	thick description
cumulative knowledge	nonlinear succession of paradigms
causality	meaning
singularity of truth	multiplicity of truth
universal/homogeneous	contextual/heterogeneous
objective knowledge	subjective knowledge

The left-hand list, which I label "Scientific," unites what I have noted are rather disparate tendencies, the deductive formalism of rational choice with the more inductive empiricism of statistical approaches. It is the model of science that unites them even when their practice divides them. The heterogeneity of the right-hand list, which I label good "Interpretive," was summed up in an introductory paragraph of this chapter. The disparate tendencies are united by a lack of enthusiasm for the monopoly claims of the scientific trope.⁵

The first dichotomy, certainty versus skepticism/contingency, reflects the fact that the world and its phenomena appear with varying degrees of clarity and reality to different observers, yielding fluctuating levels of confidence that truth can be ascertained. Descartes and Montaigne had different ontological needs. For optimists about truth attainment the enabling forces are methodologies and research rules drawn from the sciences (replicability, statistical adequacy, impersonality of the observer), whose sharpening is meant to conquer the contextualized subjectivities that are said to bias observers and/or to solve puzzles and assess interrelationships that are said to be too complex to handle with ordinary language. The interpretivist, by contrast, is likely, *Rashomon*-like, to present multiple takes on the truth or to make more modest claims about the level of confidence with which something can be known. Views differ more sharply among interpretivists than among scientists on what counts as evidence or what constitutes a valid argument. The interpretivist, like the scientist, lives among demanding research conventions designed to discipline the researcher's subjectivity. But while the disciplining of the subjectivity of the scientist lies in the correct selection and processing of evidence or in the theoretical adequacy of formal representation, the disciplining of the interpretivist's subjectivity arises from self-consciousness and reflexivity reinforced by the epistemic community. The interpretivist is invited to apply a sociology of knowledge to her own case, to consider how power or social status or cultural categories shape her vision. Instead of invoking a veil of ignorance to erase those characteristics that mark her existence, she summons a magnifying glass to reveal them more clearly.

The dichotomy between parsimony and thick description highlights the fact that communicative styles and form play different roles for the two sides. Parsimony is the correlate of a deductive mode of inquiry unencumbered by mere facticities. It is a feature more of formal modeling than of quantitative investigations. When the scholar as scientist can assume motivations, he is released from the burden of investigating them. With the benefit of making allegedly self-evident initial assumptions, he is released from the challenging task of convincing his readers of the validity of the warrants that enable him to reason deductively about how the world works. Thick description stands in the service of inductive social science. But Clifford Geertz's suggestive phrase evokes something more than an accumulation of facticities. Through his metaphors and narratives the scholar advances a theory as well as constructing a reality.

Another dichotomy contrasts a vision of a cumulative social science to a vision of a nonlinear series of distinctive paradigms, what Stephen J. Gould calls punctuated equilibria. Historians and philosophers of science differ as to whether knowledge is cumulative. Cumulation is a proposition not only born of the optimism of scientists but nurtured by the enlightenment rationalism that imagined a continuously improving human condition. Thomas Kuhn's nonlinear, nonprogressive view of the scientific enterprise holds that successive paradigms displace each other when researchers pile up enough anomalies to discredit a regnant paradigm and generate a better theory.

The theory of cumulative knowledge in political science poses special problems for the field of political theory. The notion that a recent "breakthrough" renders previous knowledge obsolete doesn't carry much conviction in a field that takes ancient thinkers as seriously as or more seriously than contemporary ones. Those who enroll under the banner of such organized sections of the APSA as Normative Theory, History of Political Thought, and Political Philosophy and Theory treat as valid knowledge texts whose provenance well predates the rise of modern science in the seventeenth century.

Political theory is a field ready to entertain the thought that many good ideas have cycled through once or twice before. Jürgen Habermas is not necessarily thought to represent the product of a cumulative process that goes beyond Plato or Georg Wilhelm Friedrich Hegel, rendering them obsolete or irrelevant. Such cumulation of knowledge as exists among theorists leads to a redefinition of problems in light of new historical situations, refinement of arguments, and clarification and systematization of known positions. But it cannot resolve disputes arising from heterogeneous priors. It is difficult to find good reasons or valid grounds for eliminating players. For example, the theory of natural law, often dismissed, has a way of circling back, as the preoccupation with human rights makes clear. In a chapter in this volume David Latinists, "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. . . . There can be no hope of cumulation if we insist that all methods, and all procedures, must be protected." Such a judgment relies on the possibility of consensus about what is valid knowledge or "best practice," a consensus that is nowhere in sight. That doesn't mean we live in a standardless universe. It means that different epistemes, different communities of inquiry, develop different standards by which to judge "validity."

The arguments on the two sides of the next dichotomy, causality versus meaning, are not very neatly distributed between the sides. Political scientists on both sides of the line are concerned with explaining outcomes—what determines the advantage incumbents enjoy in congressional races, what moves God to treat the just unjustly, what enabled England to defeat Spain, what makes citizens obey. Even as the interpretivists do not ignore cause, the team for science often finds it revealing to address questions of cause via inquiring into meaning structures.

Despite the overlap, different modes of inquiry produce different types of knowledge. She who has a knack for mathematical reasoning, has training in statistical methods, and is committed to a neo-Newtonian episteme is likely to focus on the objectivist outer world of cause rather than the subjectivist world of meaning. She will look for projects that nurture and employ her special capacities, and find the episteme that focuses on cause. By contrast, he who has mastered Tamil and Hegel will look for projects that use the human capital he has come to possess. Language is a tool that reveals the way the people of a culture perceive it and construct it; it leans to the side of meaning. Hence the alleged trend toward requiring "methods" courses more and language courses less is consequential for the balance between cause and meaning as intellectual goals of our profession.

The next dichotomy, singularity of truth versus multiplicity of truths, is a relative of the dichotomy certainty versus contingency. It functions sluggishly in distinguishing the scientists from the interpretivists. Theory and international relations scholars who have participated in the Perestroika conversation include spirits for whom multiplicity of truth is an unacceptable possibility, a child of relativism, postmodernism, and other undesirable tendencies. Nevertheless, the category has a certain viability. Interpretivists are more likely to imagine a world of multiple truths. Their preference for difference, distinctions, heterogeneity—to be explored later in this chapter—disposes them more favorably toward multiple truths.

The dichotomy universal versus contextual runs parallel with the dichotomy homogeneous versus heterogeneous. Scientists reaching for lawlike generalizations formulate commonalities. Political scientists under the influence of economic models tend to posit a common motivation driving all action, uninflected by historical, cultural, or moral variation. Interpretivists like to emphasize distinguishing features. Their modes of inquiry privilege difference and heterogeneity not only as a principle of observation but also as an aesthetic and a value.

The concept of globalization provides a vehicle for observing these dichotomies in practice. While globalization can claim expounders on both the scientific and the interpretivist sides of the line, the concept resides most comfortably with universalizing/homogenizing phenomena. Globalization is, among other things, a theory of history, a theory that features convergence and the erasure of difference. It has in common with its predecessor, modernization theory, the not-so-hidden implication that convergence entails cultural, economic, and political assimilation of the other to the Atlantic world as mediated by the Washington consensus.

On the homogeneity-heterogeneity continuum the logical academic counterparts to globalization are area studies.⁶ Area studies were crafted during the cold war to stockpile area experts who could guide American policy makers through the exotic linguistic and cultural byways of the other. But the unofficial ideology of area studies was always more subversive. It provided an arena in which difference could be not only explored but celebrated. Area

studies worked within an episteme that was more Burkean than Lockean, an episteme in which the heterogeneity that was erased by Lockean liberal universalism was valued. And area studies were unabashedly interdisciplinary against the prevailing currents that held the division of academic labor and specialization in high esteem. Unlike American studies, a species of area studies that could build on students' familiarity with their own language, history, and culture, area studies had to provide a wide array of foundational knowledge in order to lay the groundwork for theoretically and empirically informed interpretation. They had to be interdisciplinary.

The debate on globalization is one way the dichotomy between universal and particular has played itself out. Another way is via the debate, especially heated in comparative politics, between large *n* studies and case studies. Those committed to the scientific trope tend to argue that large *n* studies are the only way to do comparative politics. As David Latin notes in a chapter in this volume, "Theoretical work going back to Harry Eckstein sets constraints on what a particular case study can show," or, more provocatively, "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."

Large *n* studies and single case studies contribute differently to knowledge. Large *n* studies adopt the rhetorical conventions of science by formulating their findings in terms of general laws.⁷ But, as Max Weber reminded us, the more general the law, the less likely is it to explain a particular case.⁸ Yet the social sciences often want to explain particular cases rather than universal trends. Exploring why Vichy France supported the Nazis is a different but not inferior project to exploring the general social or historical variables that correlate with authoritarian rule. The deliberately unparliamentous case study provides a more complex and multifaceted image of causality than the large *n* generalization that has been thinned out to fit the commonalities of multiple cases. Large *n* studies can generate hypotheses for comparative inquiry by suggesting probable associations—for example, that democracy is most likely to persist in wealthy countries with per capita incomes above a certain threshold and less likely to persist as income falls.⁹ The case study, by contrast, can be especially valuable when it deviates from the regnant generalizations by providing a conspicuous and generative anomaly. For example, democracy in India persists despite one of the lowest per capita incomes in the world. Why? The deviant case requires a new line of explanation.

Finally, the dichotomy objectivity versus subjectivity highlights the fact that *who* writes political science is relevant to *what* is written. Scholars who view themselves as scientists believe they can produce objective knowledge because they are unmarked by their location in culture and history. Scientists speak as omniscient observers. The trope of science obscures the situated nature of the knowledge they produce. History's winners and losers affirm different truths. Nationalist histories record popular mobilization differently than do imperial histories. Colonial subjects understand the world differently

than do colonial masters. Their subjective knowledge distinguishes their scholarship, not only what they select but also how they interpret.

The schema I have presented sums up some of the differences that distinguish Perestroika from their imagined other. The debate is not new, and is not likely to be resolved soon. The contrasts provide parameters within which social scientists can fashion the different identities that suit their passions and their skills. Consistent with the skeptical perspective I assign to Perestroika, I can hardly claim the absolute validity of one side of the schema, but I can and do claim a preference for those modes of inquiry that suit the problems I deal with and the skills I deploy. A comprehensive political science will be a self-conscious discipline, a discipline that recognizes and accepts the role difference plays in scholarship but whose reflexivity helps its differently situated members to see common ground.

Notes

This chapter is based on a paper prepared for Panel 7-1, "Perestroika: Undisciplined, Unpunished," at the American Political Science Association annual meeting on August 31, 2001, in San Francisco.

1. Mr. Perestroika, e-mail titled "To the Editor, PS and APSA, On Globalization of the APSA: A Political Science Manifesto," posted on the Perestroika list serve, October 26, 2000.

2. Richard Betts, e-mail posted on the Perestroika list serve, October 27, 2000. Letters were written by Greg Kasza, Richard Betts, David Pion-Berlin, and Rogers Smith. Perestroika signed on to one or another of the letters.

3. Rogers Smith to Robert Hauck, deputy director, American Political Science Association. Circulated on the Perestroika list serve on October 30, 2000.

4. *American Political Science Review*, January 1969.

5. A debate about the appropriate language to designate these categories attended David Collier's initiative to form a new organized section of the APSA on qualitative methods, with some scholars arguing that "interpretive methods" was a stronger formulation. David Collier, personal communication to the author, December 30, 2002.

6. For a recent discussion see Ali Mirsepassi, Amrita Basu, and Frederick Weaver, eds., *Localizing Knowledge in a Globalizing World: Recasting the Area Studies Debate* (Syracuse, NY: Syracuse University Press, 2003).

7. For a sympathetic discussion of large n studies in comparative politics see Barbara Geddes, "The Great Transformation in the Study of Politics in Developing Countries," in *Political Science: The State of the Discipline*, ed. Ira Katznelson and Helen V. Milner (New York: W. W. Norton, 2002) 342-70.

8. Max Weber, "Objectivity" in *Social Science and Social Policy*, in *Max Weber on the Methodology of the Social Sciences*, trans. and ed. Edward Shils and Henry A. Finch (Glencoe, IL: Free Press, 1949) 49-112.

9. Adam Przeworski, Michael E. Alvarez, Jose Antonio Chelbub, and Fernando Limongi, *Democracy and Development: Political Institutions and Wellbeing in the World, 1950-1990* (Cambridge: Cambridge University Press, 2000).

CHAPTER 3

Not Here, Not Now!

The Absence of a European Perestroika Movement

Catarina Kinnvall

The Perestroika movement has a European precedent in the "post-autistic economics" movement that erupted in June 2000 at the Sorbonne in Paris. This "post-autistic" movement was soon to spread across much of continental Europe as well as making inroads into the United Kingdom with the publishing of an open letter by twenty-seven Cambridge University economics Ph.D. students.¹ The movement was a reaction against the mathematicization of economics as a discipline, which, it was argued, had resulted in an "autistic science with no relation to real life."² The attack concentrated on the extent to which a derivative of neoclassical economics—rational choice—had taken on a hegemonic role in research, publication, and teaching, constituting the main obstacle to greater plurality in the field. However, despite this post-autistic economics movement, there has largely been an absence of a European Perestroika debate in the fields of political science and international relations (IR).³ This is not to say that there has been a lack of debates concerning the development of the discipline and the social sciences in general. Rather it is to argue that such debates have been devoid of most of the components characterizing the American debate, as there has been no overarching hegemonic perspective to rebel against. This absence of hegemony has affected the kinds of debates taking place in Europe as compared to the United States, as without a hegemonic perspective there has been little cause for revolution. In this chapter I discuss some reasons why this may be the case.

First, I elaborate some potential differences in intellectual tradition. Early American social scientists defined their intellectual projects in a society without rigidly structured scientific institutions and without strong and deeply rooted traditions of sociophilosophical discourse. In comparison, their European contemporaries tended to be much more deeply rooted in the institutional structures of science and society. I show how intellectual, institutional, and political relations in Europe were characterized by partly similar and partly different problems than those facing the United States, one of which has been their particular preoccupation with the state. Included in this overview is a general discussion of what may constitute an American definition of