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Comments welcome

A POX ON PERESTROIKA, A HEX ON HEGEMONY:
TOWARD A CRITICAL POLITICAL SCIENCE

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While the perestroika reform movement that began in 2000 has shaken US political science, the virtual absence of methodological argument on any side means that the dispute is mostly political rather than intellectual. The discipline has been shaken but not stirred. The movement may change the balance of power within the profession, but otherwise leave the practice of political science unchanged. This paper is intended to help move methodological debate, with “methodology” taken in its broad sense of reflection upon the conduct of inquiry (so it also covers epistemology). The existing – now faltering – hegemony (usually identified with rational choice theory and quantitative methods) may be indefensible, but perestroika proposes only an empty pluralism in its place. Perestroika’s heart may be in the right place, but its head needs to catch up. I discuss a critical disciplinary pluralism, not as an alternative program for the discipline, but as a way of making the best of existing political science practice.

Star Wars: Attack of the Clones
To establish the need for this sort of analysis it is first necessary to document the impoverished state of the existing methodological debate. This can be done by taking a look at a symposium of disciplinary stars organized to address the issues raised in the perestroikan critique, published in the June 2002 issue of *PS: Political Science and Politics* under the title of “Shaking Things Up? Thoughts About the Future of Political Science”, based on presentations at the 2001 APSA conference. The stars in question are Russell Hardin, Robert Jervis, Elinor Ostrom, Susanne Rudolph, Marion Smiley, Rogers Smith, and Kristen Renwick Monroe. To summarize radically:

- Hardin, for the hegemons, points to the comparative success of economics in generating policy advice, while providing no evidence that the good advice outweighs the bad. His argument boils down to the fact that because economists agree with each other on the desirability of laissez-faire and people listen to them, economists are important, relevant, and right.1
- Jervis argues against too much specialization; people in different subfields should be aware of what is going on elsewhere so as not to waste time in reinventing the wheel.
- Ostrom recommends that the scope of political science be broadened to cover all human behavior and interactions covered by rules. While mostly orthogonal to the perestroika critique, her program is hegemonic in at least a positivist image of “the development of a more coherent and cumulative body of knowledge” with theory at its core.
- Rudolph proposes “border crossing” inquiry that makes use of multiple approaches in order to come to grips with specific cultures, such as the India she studies.
- Smiley implicitly calls into doubt the problem-driven aspect of the perestroika program by pointing out that “subject matters are “known” in different ways through different methodologies” (p. 198). She concludes that no standards are available to rank methodologies.

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1 When it comes to political science, Hardin’s pick for most important advance is when Downs’s economic theory of democracy ended APSA-sponsored efforts “Toward Responsible Two-Party Government”, to use the title of a landmark 1950 report. This is simply a misrepresentation of disciplinary history. By the time Downs’s book appeared in 1957, these efforts had already been killed by the behavioralists (see Ricci, 1984, pp. 164-6; Seidelman and Harpham, 1985, pp. 155-7).
• Smith wants “to make political science centrally concerned with political questions that command wide interest, to do this work honestly, and to present our results in ways that many can understand” (p. 201).

• Monroe, like Jervis, seeks better communication of findings from one part of the discipline to another. She points to the work of V.O.Key on Southern politics and her own work that recognizes human motivations of self-interest and sociability as exemplars of multi-method inquiry. Monroe advocates pluralism but, unlike Smiley, believes “methodological pluralism is not enough. We need some agreed-upon standards” (p. 204). However, she gives no indication what those standards might be.

Despite their differences, these contributions are in one way very similar. Except perhaps for Ostrom, none opposes pluralism in the discipline. In a surprisingly relativistic conclusion that sits oddly with his praise of economics, even Hardin says that “It is essentially absurd for anyone now to say, for meta-theoretical reasons, that we should go one way rather than another” (p. 185). For new scholars he “would say only do good work and be serious about getting something right” (p. 185). The relativism of Hardin and Smiley would, if accepted, put an immediate stop to methodological debate, and later I will show why relativism can resolve the debate between hegemons and perestroikans, but to the detriment of the discipline. None of the other contributors addresses what we should do with pluralism – beyond the pleas of Monroe and Jervis for better communication of findings across subfields, a very thin kind of engagement.

As if to reinforce pluralism, one other common theme that emerges from the symposium is the degree to which the contributors point to their own work as a model. So Monroe highlights her combining of self-interest and sociability. Ostrom describes an expanded range of questions on which she herself has done brilliant work (particularly on common pool resources) but which if taken seriously would expand the discipline to cover bass fishing (White, 2002, p. 179) and religious rites – indeed, just about everything done by humans. Rudolph essentially describes the sort of area studies work done by “polymaths” such as herself: “My preference for border crossing arises from my interest in a particular kind of comparative politics” (p. 194). Smith states a series of points about politically-relevant inquiry, but his conclusion is not very confident: “Quod erat demonstrandum? Admittedly, not quite. But the above reasoning works for me” (p. 201). In short, asked to reflect upon the shape of the discipline, these distinguished political scientists reflect mainly on, and implicitly advocate, their own work.

**Hegemony: The Phantom Menace**

In this *PS* symposium and beyond, hegemony receives no explicit defense. In this section I will try to explain why, and attempt to identify the best arguments I think can be made on its behalf. The hegemonic target identified by perestroikans is variously described as hard science, technicism, quantification, and an attempt to impose a “normal science” discipline on political science (see, for example, Kasza, 2000a), revealed in the pages of the American Political Science Review and similar journals (AJPS, ISQ, JOP, PRQ). Rational choice theory is often seen as central to hegemony, but it is not the totality. In fact, any attempt to combine rational choice analysis with quantification immediately encounters a problem of methodological discontinuity. This is for two reasons. First, most quantitative analysis in political science does not test hypotheses
derived from rational choice behavioral assumptions, being quite eclectic in these terms (indeed, often scorned by rational choice theorists for exactly this failing). Second, rational choice theorists rarely subject their predictions to quantitative test. If they did, they might find (like their counterparts in economics) that the $R^2$ that can be squeezed out of their models is small by the standards that quantitative political scientists are used to. Economists are not worried by this fact because their paradigm is unchallenged within their discipline. Small percentages of variance explained are not allowed to undermine the legitimacy of the basic neoclassical microeconomic approach, for the only empirical question is the comparative weight of different variables that the paradigm identifies as important, not their overall success in explaining variance. Turning back to political science, when rational choice theorists do put their deductions to explanatory use, the findings are often disappointing (as Green and Shapiro, 1994, have made clear).

Given that rational choice theory and quantitative approaches are not easily unified or even reconciled at the methodological level, it is not surprising that no methodological defense of hegemony has been launched in response to the perestroika critique. Rhetoric on behalf of science, generalizability of models, systematic empirical knowledge, testable hypotheses, and the like does not count here. Indeed, the contrast between the pervasiveness of such rhetoric in the informal deliberations that go on within (some) departments and its scarcity in response to the perestroika critique is striking.

One of the most thoughtful responses to that critique to date is the editorial by Ada Finifter (2000) defending the APSR, at least, against the critics. But Finifter’s is not a methodological defense of hegemony. The substance of her argument is that what is published in the APSR represents the best of what is submitted, such that if perestroikans do not like what is published, they ought to submit more of their own work to it (see also Jervis, 2000). She approaches (but does not quite reach) a methodological defense of hegemony at only two points. The first (p. viii) is where she argues that, in contrast to what the critics aver, the APSR does not ignore articles that deal with “…great political issues. Rather the intent is to obtain more systematic and reliable information about them”. She does not explicitly equate “systematic and reliable” with “deductive and quantitative”, but goes on to point out that the critics should not infer that articles containing statistics or mathematical symbols cannot “deal with great political issues”. The second (p. ix) is to argue that “the article that appeals to a broad scholarly audience of political scientists may be a chimera” because the discipline is so fractured. This implicitly replies to Kasza’s (2000b) conjecture that articles with a broad appeal get sent to a broad set of referees, while articles with narrow appeal get sent to a narrow set of referees. Given that consensus is more likely among the latter, Kasza avers, the APSR discriminates in favor of articles with narrow appeal to specialists. Kasza’s conjecture is consistent with the facts of what is published in the APSR, but so is Finifter’s second point. I see no way to resolve their difference that would not violate the confidentiality of the APSR’s reviewing process. However, Kasza’s argument would not explain the absence of highly specialized articles outside the APSR’s standard fare of rational choice, statistics, and Straussian political theory.

The fact that hegemony has received no methodological defense is probably a good indicator of the fact that it is indefensible – at least to the degree hegemony involves rational-choice-theoretic and statistical analysis forcing out other approaches. However,
such forcing out is not the only conceivable route to disciplinary unification of the sort that “hegemony” connotes. An alternative agenda is proposed by David Laitin (2001), who wants to integrate the various parts of the discipline through reference to the best existing practice in a variety of fields. Laitin’s basic argument is that political science has an underlying intellectual coherence beneath its current institutional chaos (perestroikans believe almost the exact opposite: that there ought to be intellectual pluralism despite the current institutional uniformity). In Laitin’s view, political theory deploys and develops the canon in order to identify the big questions that need to be asked, comparative politics generates evidence about how those concerns are addressed in different political systems, and political institutions (as a field) examines the operation of institutions as they either promote or impede values derived from the canon. And public policy attempts to identify manipulable factors that affect desired outcomes.

While Laitin presents this division of labor as capturing “a coherence that most political scientists feel but rarely articulate” (p. 7), his is either a normative or procrustean exercise that does not capture much or most of what is actually done in his subfields. For example, public policy does not, contra Laitin, “address policy problems relying on the substantive knowledge and methodological skills developed in the non-applied part of the discipline” (p. 7). Its practitioners find that the “non-applied” discipline has not bequeathed much that is useful to them, such that they must carry out their own inquiries about (say) the determinants and consequences of policy. Political theory for its part is as much analytical as canonical, and (like other subfields) is often caught up in disputes that outsiders would regard as arcane. Its relationship to the rest of political science is multifaceted and contentious; and empirical work can be brought to bear on political theory questions, rather than the vice versa that Laitin proposes. There is a huge gap between concepts as they appear in political theory and in comparative politics. Contrast, for example, the richness of the ways “democracy” is conceptualized in political theory with its impoverished treatment by comparative politics scholars, almost all of whom work with a minimalist or electoralist Schumpeterian model that almost all political theorists reject. It would take a lot of work to bridge this gap.

Laitin’s model promises intellectual coherence while allowing diversity in the practice of inquiry. The whole enterprise is driven by concerns generated by the political theory canon. I would be more inclined to see the problems that the discipline needs to solve as being generated by history and political context (mediated by all kinds of social and political forces). This is why, for example, ethnic conflict and nationalism become such major problems for the discipline in the 1990s. Even if there are enduring questions generated by the canon as interrogated by political theorists, their interpretation and relative weight depends a lot on the historical context.

The best argument on behalf of hegemony I can think of is that at least it provides some common focus for the discipline. When rational choice theorists and other “hegemons” look at the discipline they see not a republic that they rule, but rather a plethora of principalities undertaking all manner of inquiries – the “organizational chaos” of which Laitin speaks. But the chaos (or, more neutrally, plurality) is actually intellectual as well as organizational. Hegemony at least gives us a center of sorts to struggle over – even if that center is internally incoherent. In this sense the current state of the discipline is an identifiable center plus considerable pluralism. Now, what would happen were perestroika to be successful to the degree it demoted that center to just one
approach (or, really, several approaches) among the many that make up the discipline? This question is actually quite easy to answer because we have several available models of political science – or, rather, political studies, to use the terminology favored by perestroikans such as Rudolph (2002) and Smith\(^2\) - elsewhere in the world to look at. A pluralistic political studies with minority interests in statistical methods and rational choice theory captures quite well the state of the discipline in the United Kingdom and Australia. While this is not the place to engage in a “state of the discipline” survey for each country, I think it would be very hard to muster an argument to the effect that the discipline is stronger in either of these two countries. That weakness is not just a function of the sheer comparative size of the American discipline. As a test of this proposition, I would ask of any US political scientist (perestroikan or otherwise) if they have ever advised one of their best undergraduate students to pursue a PhD in the UK or Australia on the grounds that they would find a more congenial intellectual climate where their interests would be better served than in the US. (Sending political philosophers to Oxford doesn’t count.) The reverse, of course, happens all the time. In the UK, such reform impetus as exists would look to a hegemonic US model of political science (see Dowding, 2001).

The claim that hegemony at least gives us something to contest may be the best argument that can be made on its behalf, but it is not an adequate one (which perhaps explains why hegemons themselves do not make it). The argument makes sense only as a holding action against disciplinary fragmentation, with no claim that it will yield intellectual progress, a discipline with greater problem-solving capacity. Can perestroika deliver in these terms?

The Perestroikan Alternative

Perestroika opposes the hegemony of rational choice theoretic and statistical approaches in the APSR and similar journals, the dominance of such approaches in key research departments, and hierarchy in the American Political Science Association that perpetuates such approaches. What perestroika favors is a bit harder to pin down, in part because of its self-proclaimed diversity as a movement. A search of key perestroika documents, letters (especially to *PS: Political Science and Politics*), and published interviews with perestroika luminaries produces the following list:

- Pluralism in approaches to the subject matter of politics.
- Problem-driven research (as opposed to method-driven research)
- Relevance to important political questions and policy issues.
- Area studies
- Political studies rather than political science
- Political philosophy
- Interdisciplinary inquiry
- Reform of the APSA to make it more open and democratic.
- Reform of the editorial practices of the APSR.
- Reform of departmental hiring and promotion practices.

The last three of these points are organizational rather than intellectual. The first seven points have methodological aspects that merit close scrutiny. Now, one problem here is that the points as stated cover quite a diverse range of orientations, not all of them interested in one another (for example, political philosophers generally have little interest in the atheoretical bent of area studies). And at least one – interdisciplinary inquiry – could be shared by hegemony; rational choice theory began as interdisciplinary inquiry involving economics and political science. The perestroika movement resists any stronger statement of its methodological commitments on the grounds that it encompasses a diverse range of scholars and substantive concerns. This might seem to fit comfortably with the proclaimed commitment to intellectual pluralism, or what Kasza (2001) calls “ecumenical science.” I will now argue that a gesture in the direction of pluralism is inadequate without sustained attention to what pluralism can and ought to mean. I am not proposing that perestroika construct some counter-hegemony. Rather, I take my bearings from Mary Parker Follett (1918, p. 10): “The pluralists have pointed out diversity, but no pluralist has yet answered satisfactorily the question to which we must find an answer – what is to be done with this diversity?” (quoted in Schlosberg, 1999, p. 53).

Perestroika’s Empty Pluralism

Perestroika’s advocacy of pluralism requires explanation in light of the fact that there is already substantial pluralism in the discipline. This pluralism may not extend to the APSR and its imitators, several large midwestern departments, or the University of Rochester, but it is alive (if not necessarily well) just about everywhere else. A look through the program for the annual APSA meeting reveals a remarkable range of topics and approaches. Organized sections of APSA and new journals in ever more specialized subfields proliferate. In Almond’s (1990) lament for the lost hegemony of what he calls “the broad cafeteria of the center”, we are increasingly sitting at “separate tables” as the discipline fragments into ever finer subdivisions.

In this light, one position that cannot hold is that more disciplinary pluralism will mean more politically relevant, problem-driven research. Rogers Smith states that “the ultimate objective is to create a political science that speaks clearly and accessibly to substantive important questions about politics. The problem is that there is, for good reasons, no consensus about just what such questions and such work really amount to. So, as a practical matter, we have to seek in the first instance to create space in the discipline for a greater variety of kinds of political inquiry.” But if particular pieces of political science do not already “speak clearly and accessibly to substantive important questions about politics”, more pluralism will make not the slightest bit of difference. In the words of Nike, if this is what you want, “Just Do It!”

Actually some people are already doing it. My own field of environmental politics is pretty much defined by its concern with some major political problems. Almost invariably, people enter this field because they are environmentalists, not because they have a particular theory or method they want to try out. None of this research is published in the APSR or its imitators, and much of it is not well understood by the rest of the discipline, at least if Laitin’s (2001, pp. 9-10) comment about environmental politics is anything to go by. He says it provides the discipline only with an independent variable

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that can affect the real stuff of politics – for example, when it comes to “How have ecological issues transformed political parties?” (p. 10) The real questions of environmental politics take exactly the opposite form: for example, “how do party systems promote or impede the resolution of ecological problems?”  

There is no logical connection from the degree to which the discipline is pluralistic to the degree that it produces problem-driven research. Susanne Rudolph avers that “A wish to have broader answers drives you to multiple methodologies” (quoted in Miller, 2001, p. 2). This may well be true for particular pieces of research, but it is not true when it comes to the shape of the discipline as a whole, because multiple methodologies lead to multiple answers, not to broader ones. The only conceivable connection is probabilistic: that is, the more kinds of research are done, then purely by chance the greater the likelihood that at least some of them will be problem-driven and/or speak to important political questions. But exactly the opposite result is also plausible: more pluralism means more specialization means narrower framing of questions in the terms set by particular research traditions, as opposed to any more public or widespread conversation, within the discipline or beyond it.

Lessons from our History

The connection between multiple methodologies and problem-driven research has been made at least once before in the discipline’s history, in the policy sciences idea first proposed by Harold Lasswell in the late 1940s (see especially Lasswell, 1951). Lasswell advocated policy sciences that would address the great political issues of the age, such as the threat of total war, the dangerous emergence of a “garrison state”, the undermining of democracy. To Lasswell, it was something of a mantra that the policy sciences were to be “contextual, problem-oriented, multi-method”, requiring the best that political science (and other social sciences) could offer. I think there is very little in Lasswell’s mantra, or in his broader ambition for the policy sciences, with which the perestroikan could disagree. (Some of Lasswell’s other proclivities, such as his psycho-analyzation of political pathologies and advocacy of propaganda to save democracy from itself, would of course be more controversial.) We would, then, do well to ask what happened to the policy sciences movement, and why. The movement is still alive, institutionally in the Yale-based Society for the Policy Sciences, also in the journal Policy Sciences. But while the Lasswellian approach is at least in my judgment intellectually head and shoulders above the kind of atheoretical or theoretically eclectic, case-based policy studies that are the staple of the Policy Studies Organization and its journals, it is the latter which have attracted greater numbers within the political science discipline. The policy sciences approach remains a minority taste within one subfield, a far cry indeed from Lasswell’s heroic ambitions.

Ironically, at the very time Lasswell was advocating that the discipline take on the great issues of the age in contextual and multi-method fashion, political science began a revolution that took it in a very different direction (which, paradoxical as ever, Lasswell himself also supported). Behavioralism had a scientific and positivist self-image, also favoring quantitative methods. Policy relevance was not part of its immediate aim —

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4 Equally irksome is the frequent comment that the environment is just a “policy area”. Political ecologists might reply that politics is just a particularly problematic subsystem of the global ecological system.

5 Prior examples might include 19th century moral science and some of the efforts of Charles Merriam.
though that would supposedly come in time as reliable scientific findings accumulated. The reasons for behavioralism’s success have been well-documented, if not always agreed upon (see Farr, 1995). To the behavioralists themselves, it was a matter of replacing legalistic, formalistic, impressionistic, and arid kinds of analysis with a systematic search for sound and reliable knowledge about politics based on study of the actual behavior of political actors. To their critics, it was always about securing respectability within the US university system (Ricci, 1984), appealing to funding sources, or depoliticization of the discipline in the early cold war.

The contemporary hegemony that Perestroika opposes is constituted in part by the commitment to quantification that is one legacy of the behavioral revolution. (As David Easton (2001) notes, behavioralists could in the 1950s complain with some justification that they were unfairly excluded from the APSR. As late as 1956, APSR editor Harvey Mansfield, Snr. was hostile to behavioralism, a situation remedied only by his replacement.) But it was not behavioralism that first prevented the discipline from speaking “clearly and accessibly to substantive important questions about politics”, in Rogers Smith’s words. As Ricci (1984) and Seidelman and Harpham (1985) point out in their disciplinary histories, the tragedy of American political science since its very inception has been its failure as a “reform science” that would be taken seriously in the political system. The reasons for this failure are varied; to Ricci, it is the fact that the US university system has always demanded the trappings of science, which proved inconsistent with any more practical emphasis on political problems or joining the great conversation of democratic development. To Seidelman and Harpham, it is the recalcitrance of the political system that reform scientists wanted to improve. At any rate, such discipline history should give us pause for thought. The American science of politics was founded in the late 19th century. Its failure to speak “clearly and accessibly to substantive important questions about politics” is actually constitutive of the discipline from the beginning, not a contingent feature that arrived with behavioralism or rational choice theory.

In this light, Lasswell’s failure to reorient the discipline in order to make it relevant to the great questions of political life is just one of a string of failures. But I think a closer examination of the reasons for this particular failure are quite instructive if perestroikans want to do better. Of course, explaining why something did not happen is much more challenging than explaining something that did happen. Perhaps the behavioral revolution got in the way. Perhaps Lasswell’s warnings about the garrison state could be seen as politically suspect in the cold war context. But the real reason may be that the intellectual demands of his approach were massive. Under behavioralism, one could learn a technique and apply it; under the policy sciences, techniques would only ever be parts of the puzzle. Lasswell’s policy sciences program required individual policy scientists to be as superhuman as Lasswell himself, to have a detailed grasp of a wide range of social scientific (and other) approaches.

**What is to be Done with Diversity?**

If such knowledge is beyond the capacity of one individual, the obvious alternative is to try to increase capacity by involving several or many individuals. Attempts have often been made to coordinate such efforts hierarchically and bureaucratically; this works for relatively simple problems, but not for more complex
problems (as Nelson 1977 pointed out long ago, this can get us to the moon, but not solve the problems of the ghetto). F.A. von Hayek (1979) argued that the market could be justified as the best device for integrating fragmentary bits of knowledge held by individuals with limited personal capacity. Karl Popper (1963) made a similar kind of argument about the capacities of liberal open societies (and against their authoritarian competitors) to make use of diffuse bits of knowledge in policy making. These models of the market and liberal democracy cannot be applied directly to the contemporary practice of social science. Popper’s argument for the comparative rationality of liberal democracy was itself derived from his account of an ideal (natural) scientific community, with policy reforms analogous to scientific experiments. However, pluralism for Popper meant only different vantage points from which to criticize particular policy reforms; his approach does not easily handle deep intellectual pluralism of the sort we observe in political science, but which he was reluctant to recognize as an aspect of scientific practice. But I think Hayek and Popper are on the right track at least in the sense that some mechanism for integration is needed. And this highlights what is currently missing from perestroika’s pluralist program. Mere pluralism and toleration across different approaches is not enough.

As it stands, perestroika advocates pluralism, but does not say what is to be done with this pluralism, or assumes (with no specification of a mechanism) that pluralism will somehow lead to a more politically engaged and relevant discipline. Without such analysis, pluralism becomes relativism in which there are no critical standards to make good (contextual) decisions across competing approaches to the study of politics. Such relativism will not disturb the hegemons; recall that Finifter (2000) and Jervis (2000) argue that the APSR is already open to all parts of our fragmented discipline equally, it is simply a matter of some people choosing to make more use of it.

The perestroikan principle of political relevance could serve as a critical standard – but I have not seen it deployed in this fashion, except occasionally to condemn the approaches favored by the hegemons. Shortly I will show how this standard can be deployed in much more productive and positive fashion.

In the absence of critical standards, the danger is that uncritical standards emerge. For example, a wing of British political studies long prized access to politicians and disparaged publication in competitive refereed outlets. Those on top were those with the best access (and/or the best High Table chat). Alternatively, public profile – the ability to get in the media – could substitute. Or an argument could be assessed on the basis of the institutional affiliation of who is making it, not the substance of the points made.

My conclusion to this point is that hegemony in political science is indefensible but that as it stands the perestroikan advocacy of an uncritical disciplinary pluralism will not produce anything much better. The perestroikans have not answered Mary Parker Follett’s question: “what is to be done with this diversity?” Before turning to what I think the discipline should do with pluralism, let me note one danger that exists to the extent such methodological reflection is shunned.

Perestroika Minus Methodology Equals Aristocratic Counter-Revolution

Mr. and Ms. Perestroika are famously anonymous. But perestroika has its luminaries, many of whom are full professors at Ivy League institutions or the University of Chicago. Yale University is a perestroikan hotbed. Rogers Smith, identified as a
“leader” of perestroika (he replies that he is “in no sense a spokesman for the perestroika movement, only a participant in it”) is ex-Yale, now University of Pennsylvania. The movement immediately produced two APSA presidents, Theda Skocpol of Harvard and Susanne Rudolph of Chicago. Organizationally, the movement knocked on the APSA door; finding nobody at home, it walked straight in. The new Perspectives on Politics journal which, presidential protests to the contrary notwithstanding, is taking shape mostly to respond to the perestroika critique, is to be edited by Princeton’s Jennifer Hochschild (one of the signatories of the perestroikan letter to PS, Political Science and Politics, December 2000). Rogers Smith protests that “the vision of a single core of Perestroikans taking over the discipline as a well-coordinated cadre is a wrong one.”

Perhaps so; but one can see why Smith is worried about this interpretation.

Such individuals have hardly experienced careers stunted by hegemony; indeed, they are prominent members of some of the most highly-rated departments of political science in the United States, and authors of books with top university presses. A cynic here might think that perestroika therefore represents a push by this cadre to secure the remaining parts of the discipline they do not control: notably, the APSR and the formal offices of the APSA. Sven Steinmo (2000) expressed surprise on joining the APSA Council on a reformist platform that “the APSA Council was not made up of a clique of white males from elite East Coast universities. On the contrary, the Council represents a diverse set of political scientists from different parts of the country and different types of schools.” No hegemony there. Such suspicions of aristocratic counter-revolution may be unfounded, but they are understandable to the extent perestroika lacks any methodological component.

**Toward a Critical Political Science**

Any alternative program for the discipline is almost certainly doomed to failure. The reason is precisely the discipline’s existing pluralism: trying to impose a common program on that pluralism really would be like herding cats. And if perestroikans get their way, the cats will become even harder to herd, perhaps hardly worth herding. So any cat-herding manual I might propose would rightly merit dismissal.

Let me start instead from Perestroika’s stated commitment to problem-driven inquiry. Problems are aspects of the world that need explaining or remedying. Now, not just any old problems will do; rational choice theorists and other hegemons are rightly criticized for the narrowness with which they define problems, and the consequent lack of relevance of their inquiries to significant, real-world political problems. A progressive and defensible discipline is one whose capacity to address significant problems increases with time (I will address the question of how to recognize problem significance in a moment). On this I think perestroikans would agree, at least to the extent that political science needs to progress from a currently unsatisfactory position in these terms. Given political complexity and intractability, we need all the help we can get from a variety of research traditions. Having a range of effective research traditions at our disposal is conducive to the progress of political science, conceptualized as a growing capacity to

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cope with contingency in the problems faced by the discipline (Dryzek, 1986, pp. 315-7; Dryzek, 1990, pp. 205-8). My only quarrel with the perestroikans here is that their pluralism with no critical standards will not produce progress defined in these terms because it is unable to make judgements across research traditions in terms of the quality of their contributions to problem solving. This inability stifles the perestroikan critique of rational choice theory’s problem-solving narrowness. The critique either becomes rhetorical, or it is specified that the quarrel is mainly with that tradition’s hegemony, not its existence.

Our search for critical standards by which to assess problem-solving effectiveness can begin by looking closely at the circumstances of problem definition. It is against such problems that progress ought to be assessed, but these problems do not have a brute existence, let alone weight. As Stephen White (2000, p. 744) puts it, ‘problems do not fall like apples from a tree into the lap of an entity called “society.”’ There is always the question of how a problem is defined and who in society does the defining. What is crucial here is to understand that this normative issue now must be seen as a matter on which political scientists will always have to take a position.’ There are, then, two issues here. First, who does the defining? Secondly, how does the process take place?

In terms of the “who”, if it is only the practitioners of a particular research tradition, then the result is scholasticism. If it is only political scientists more generally, that is more defensible on the social scientific dimension, but it would still fall short on the political relevance dimension. Thus the more defensible answer is that problems get defined in a social process in which political scientists participate, but other political actors participate too. It is against this set of (changing) problems that the progress and rationality of political science as a discipline ought to be addressed.

The conditions of this broad social process of problem definition are crucial. Given the way the world is, social processes of problem definition are distorted by money, power, and ideology. Defensible problem definition, and so a defensible discipline, can only exist to the extent such distortions are recognized, criticized, and counteracted. This critical commitment is not a matter of preference: it is a matter of cognitive rationality. We cannot have a rational discipline without it. Elsewhere I have developed a more elaborate argument on how principles of communicative rationality can be brought to bear in such a process (Dryzek, 1990, pp. 209-13). Communicative rationality is the degree to which communication oriented to reciprocal understanding is free from domination, deception, self-deception, manipulation, and strategizing (see also Habermas, 1984).

Such a critical commitment is especially important when ideological hegemony distorts the disciplinary agenda. It is relatively easy now to look back at the Cold War in these terms. Funding priorities for area studies were largely determined by who might need to be subverted or invaded. Research programs with a hint of class analysis about them were downgraded in favor of those with an ontological individualism (so, for example, in voting studies, the University of Michigan’s psychological approach forced out the social determinism of Lazarsfeld’s Columbia school in the 1950s). It is much harder but more important to step back and criticize the ideological distortions that have come in the wake of September 11, 2001. The malfunction of the presidential election system in the previous year is now harder to describe as relegating the United States to the ranks of the world’s more questionable democracies. The dominance of the more
regressive parts of the oil industry in making US energy/security policy becomes harder to investigate. In international relations, classifications that place North Korea and Iran but not Saudi Arabia and Egypt in the ‘dangerous’ pile are more easily challenged outside the US than within it.

To this point I have argued that a putatively rational and progressive discipline can be judged in terms of the adequacy of its plurality of research traditions in relation to a set of problems defined in social processes that transcend the discipline’s boundaries and reach into the larger polity. The circumstances of problem definition must themselves be subject to critical scrutiny on the part of political scientists and others. But this only goes so far: the imagery here is of a set of candidates (research traditions) that periodically get judged according to changing problem sets. This begs the question of what we do as political scientists to get our research traditions in better shape. That is, how ought we to cultivate our discipline?

Despite his procrustean tendencies (surely to be resisted in a pluralistic discipline) I believe Laitin (2001) is on the right track in at least one way: the key is to make the best of what we’ve got, rather than propose something different. What we have is pluralism with localized hegemony (the localities identified earlier: the APSR and its imitators, large midwestern departments, Rochester and its doppel-gangers). How, then, are we to make the best of pluralism?

A critical pluralism necessitates engagement across research traditions, not just mere tolerance of different approaches, and not just communication of findings (as advocated by Jervis, 2002 and Monroe, 2002). It is only in their engagement with one another that the shortcomings or indeed strengths of particular approaches and styles of inquiry can be exposed. Approaches can emerge strengthened as well as weakened by such encounters, which are actually an alternative to evaluation of an approach against some absolute standard of adequacy. Consider rational choice theory. When Green and Shapiro (1994) apply absolute standards of explanatory adequacy to the approach they find it falls far short. Their critique is correct but unimportant when viewed in light of the need for engagement across traditions; Green and Shapiro offer no alternative tradition with which rational choice theory might engage.

Green and Shapiro and their critics alike miss the point of rational choice analysis. It is best not to think of rational choice theory as explanatory theory; rather, rational choice theory shows in great detail what would happen if political actors behaved according to its behavioral precepts of strategically rational maximization (Johnson, 1991). The predictions generally involve disorder; underprovision of public goods, universal nonvoting, manipulation of legislative processes, arbitrariness and instability in collective choice, domination of policy making by well-organized minorities, rent-seeking by office-holders, Pareto suboptimality in public policy, and so forth (as Barry and Hardin, 1982, summarize, “rational man and irrational society”). The trick, then, is to figure out ways in which the behavioral proclivities that yield such dire results can be curbed or prevented by making them subject to alternative forms of rationality. Such alternative forms and their consequences can only be revealed by a research tradition capable of recognizing both the sort of individual motivation assumed by rational choice theorists and alternative wellsprings of human action. I believe the one best equipped is critical theory, which has a place for strategic rationality, but also develops at great length an alternative kind of communicative rationality (Schiemann, 2000). This particular
engagement removes the positivist self-misunderstanding of rational choice theory and reveals its critical potential (Dryzek, 1992). Thus strengthened, rational choice theory becomes much better equipped to address at least one of the great political issues of our age: the increasing marketization and so individuation of ever more areas of social and political life (Offe, 1987). The combination of rational choice theory and critical theory can be deployed to elucidate the consequences of this individuation, and to investigate institutional and other means for curbing it. But note that mere tolerance, “live and let live” pluralism across research traditions could not produce this outcome. Nor could simple communication of “findings” from one subfield to another. Nor could unremitting condemnation of the Green and Shapiro sort. Rather, the happy outcome requires a critical engagement of two research traditions.

Johnson’s (2002) cautionary tale of the encounter of culturalist (“interpretive”) and rational choice traditions shows why such engagement must be critical. He argues that an unrecognized conceptual problem bedevilling cultural explanation will also undermine any combination with rational choice analysis. The combination of these two traditions ought to be fruitful because “given that symbolic forms have force, the [rational choice] models help us to conceptualize more precisely why strategic actors seek to deploy them for political advantage” (p. 234). Unfortunately, Johnson argues, cultural analysis has a conceptual problem in its lack of any mechanism to explain the independent causal force of symbolic factors. Thus analyses such as the Bates, Figueiredo, and Weingast (1998) study of state transitions “ultimately reduce symbolic action, and hence culture, to strategic considerations” (Johnson, 2002, p. 243). What starts as engaged pluralism turns into rational choice reductionism. One moral that Johnson does not draw is that this encounter has at least highlighted a deficiency in cultural explanation that its practitioners now need to rectify.

In a different arena, Berejikian (1992) shows that critical engagement can occur without being hobbled by conceptual problems. Drawing on macrohistorical structural (Skocpol, 1979) and microeconomic (Popkin, 1979) explanations of revolutionary peasant action, he begins by showing that an adequate structural account requires microfoundations in a model of individual peasant agency. But structuralism is not thereby reduced to individual rational action. Rational choice explanation itself fails because it cannot explain why peasants sometimes take the huge risks that revolutionary action demands (as opposed to joining radical organizations, which is easy to explain through reference to selective incentives). Berejikian then defines a “social frame” as the “perception by individuals that participation in the existing social-structural arrangement means either gains or losses” (p. 652). Behavioral decision theory tells us that individuals will take on much greater risks if they believe the status quo is a situation of loss from some reference point. The key task of revolutionary leadership is therefore to convince peasants of a losses frame. Thus social structure does have causal force, but it is mediated by the perceptions of it that revolutionaries can propagate. Within this frame, individual peasant decisions about whether or not to revolt are decisive. The intellectual engagement now encompasses structure, rational agency, and ideology.

**Rules of Engagement**

Hazards of the kind Johnson has identified notwithstanding, without these sorts of productive encounters across research traditions there is no discipline as an intellectual
entity. All that remains is an organizational entity that is not worth contesting in intellectual terms, a mere holding tank. Let me try to draw some concrete implications from this seemingly innocuous starting point.

First, engagement across traditions means that you need research traditions to begin with; that is, traditions of inquiry with identifiable ontologies, theories, and methods. This requirement immediately causes problems for kinds of inquiry with little conceptual content; descriptive policy studies might fall into this category, as would journalistic political commentary, and some area studies work. However, area studies is redeemable to the extent it can engage research traditions, of which more shortly.

Postmodernists who argue that they are interested in destabilization of rigid understandings might immediately object here that they have no interest in creating a research tradition of a more disciplined kind. This seems to be a particular problem for “critical” metatheorists in international relations; see their responses to Keohane’s (1988) demand that the IR crits produce a research program of their own. For example, Ashley and Walker (1990, p. 398) scorn “paradigmatic conceits” that block the thought necessary to cope with an international system full of paradox. But even postmodernists of this sort require something to destabilize, and that something can be found in research traditions. That is, if realism, liberalism, and other traditions did not exist, critical metatheorists of international relations would surely have to invent them, for their own existence is parasitic on these traditions. The implication of my first point is not that all approaches to inquiry need “hardening” by their practitioners into research traditions with explicit and well-defined ontology, theories, and methods. Such hardening may work against engagement by making different traditions look like monoliths that can only compete, losing the subtle variations that may create points for dialogue (Reus-Smit, 2002). But they do need hardening to the point where they are able to engage other research traditions.

The other holdouts against well-defined research traditions might be area specialists, but even here it is not hard to see how the case knowledge that area studies generates can be put to comparative use not just in testing theories (King, Keohane and Verba, 1994) but, more significantly, generating and refining theories. Thus the exemplary work of Skocpol (1979) does not simply test a theory of revolution in the context of comparison of three main cases of social revolution (plus several other negative cases where revolution did not happen), it develops that theory in the context of the comparative case analysis. While violating positivist precepts about deductive theory development followed by empirical test, Skocpol demonstrates just how productive engagement between theory and area studies all the way down can be.

Second, engagement requires mutual intelligibility across research traditions. Propositions and findings need to be stated in a form intelligible to practitioners of other traditions. One of the failings of rational choice theory is the predilection of some of its practitioners for complicated mathematics. The important findings of rational choice theory can however be stated in simple terms in plain language. Only the esoterica actually requires the ultra-formalization that makes reading the APSR like trying to read the phone book upside down (as my colleague Gerry Mackie puts it). Words as well as mathematical symbols can become unintelligible, and one does not have to read too far into critical theory or postmodernism to find plenty of examples.
The toughest issue here is not one of clear language (though that is surely important). Mutual intelligibility can be blocked to the degree different traditions have theoretical terms that are not easily translated and compete as central explanatory factors. Yet examples I have already adduced show that the causal force of culture can be reconciled with rational choice theory’s emphasis on calculation; and that structural explanation and ideology can be interpreted in light of behavioral decision theory when it comes to peasant revolution. Flexibility in theoretical commitments here is crucial. For example, the encounter I described above between rational choice theory and critical theory can only be productive to the extent that rational choice theorists are prepared to allow that their behavioral assumption is contingent rather than absolute. Similarly, Ashley’s (1984) identification of the critical potential in realist theories of international relations requires realists to let go of their assumption that states necessarily maximize relative power in a situation of anarchy, and open themselves to a world of reflection, communication, and intersubjective understanding.

Third, engagement means standards beyond those internal to particular research traditions, such that an ‘anything goes’ pluralism cannot stand. (Monroe, 2002 advocates such standards but does not say anything about what they might be, applying only a medical metaphor as to why we need them. In medicine, the standard is contribution to human health; though Monroe fails to note that even there, the definition of “health” is controversial.) Most straightforwardly, the standard in question is success when it comes to resolving socially-determined problems of the sort I have already discussed. Particular pieces of research can be applauded or condemned in these terms, but more consequential when it comes to cultivating the shape of the discipline is the success or failure of a research tradition over time as it confronts series of problems. Engagement means that we can also assess felicitous (or indeed disastrous) combinations of research traditions in light of the critical problem-solving standard. My earlier discussions of rational choice’s engagements with, respectively, critical theory and cultural explanation suggests that we have enough material to begin assessing the results of these engagements (Johnson’s, 2002, assessment of the latter is of course negative). The need to assess over time and different contexts means we should not be overly quick in condemning a particular tradition to the garbage can. It might prove amenable to resuscitation if it can find the right partner; or indeed, it might fortuitously find itself able to speak to a new set of problems that arise. The latter perhaps explains the resuscitation of political culture inquiry in the 1990s, now under the “social capital” heading. Such analysis spoke directly to American anxieties about social disintegration amid increasing prosperity.

Here, it is easy to slide into the kind of language used by philosophers of social science for assessing research programs or traditions. This is dangerous because of the history of misuse of such philosophy in political science. Such misuse comes in at least two waves. The first is the use of logical positivism to justify behavioralism, which ignored the lack of deductive theory in the behavioral program, not to mention the discrediting of positivism among philosophers. The second came with those who believed that the lesson of Thomas Kuhn was the need for a discipline-defining paradigm – which in practice could only mean imposing an arbitrary conformity. Reflective practitioners eventually rejected these attempts to legislate for the discipline. The general point here is that it is reflective disciplinary practice that should be decisive when it comes to standards to apply to research traditions and their comparison; standards themselves
come out of contests and debates across different traditions of inquiry (Dryzek and Leonard, 1988, p. 1258). This does not rule out intelligent use of the language of the philosophy of social science.

**Conclusion**

In the wake of perestroika, the only defensible political science is pluralistic. But the discipline has to be critically pluralistic in two ways not recognized by perestroikans. The first is in adopting a critical and ultimately democratic orientation toward the political circumstances in which problems get defined and weighted. The second is in critical engagement across traditions of inquiry, whose results can be assessed in terms of this changing problem set. Mere toleration and ecumenicalism will not do.

**References**


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