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

John S. Dryzek

Social and Political Theory Program
Research School of Social Sciences
Australian National University
Canberra, ACT 0200
Australia
jdryzek@coombs.anu.edu.au

While the Perestroika reform movement that began in 2000 has shaken US political science, the paucity of productive methodological argument means that the dispute becomes political rather than intellectual. The discipline, like James Bond's vodka martini, 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 (identified with rational choice theory and quantitative methods) may be indefensible, but Perestroika may portend only an empty pluralism in its place. I discuss a critical disciplinary pluralism as a way of making the best of existing political science practice – and redeeming Perestroika's promise.

Space limitations preclude full documentation of the impoverished state of the methodological debate, though a flavor can be gained by 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." One common theme that emerges from the symposium is the degree to which the contributors point to their own work as a model. Asked to reflect upon the shape of the discipline, these distinguished political scientists reflect mainly on, and implicitly advocate, their own work. Strikingly, none of the contributors, even those associated with hegemony, opposes pluralism in the discipline.

Hegemony: The Phantom Menace

In this *PS* symposium and beyond, hegemony receives no explicit defense.¹ 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

¹ Landman (2002) claims to be “rebutting Perestroika”. However, his argument is on behalf of method against an emphasis on substance. Given that Perestroika opposes only methodological hegemony, not method as such, his rebuttal misses the target – and while compelling in its argument for method as systematic inquiry, does not attempt to defend hegemony.

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 (Green and Shapiro, 1994).

Given that rational choice theory and quantitative approaches are not easily reconciled, it is not surprising that no *methodological* defense of hegemony has been launched in response to the Perestroika critique – beyond rhetoric on behalf of science, generalizability of models, systematic empirical knowledge, testable hypotheses, and the like. One of the most thoughtful responses 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 (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 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 probably indicates that it is indefensible. Indeed, *any* integrative program that one might propose for the discipline in its entirety (such as the heroic one offered by Laitin, 2001) is almost certainly doomed. The reason is precisely the discipline's existing pluralism: trying to impose a common program on that pluralism really is like herding cats. And if Perestroikans get their way, the cats will become even harder to herd.

The best argument on behalf of hegemony I can think of is that at least it provides some common focus. 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 (2001) 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 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 Smith² - 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. It would be very hard to muster an argument to the effect that the discipline is stronger in either of these two countries. In the UK, such reform impetus as exists would look to a hegemonic US model of political science (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

What Perestroika favors is a bit harder to pin down than what it opposes, in part because of its 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)

² Colloquy Live, 'The Perestroika Movement in Political Science', *Chronicle of Higher Education*, September 19, 2001. Accessed online at <http://chronicle.com/colloquylive/2001/09/Perestroika/> p. 4.

- Relevance to important political questions and policy issues.³
- 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 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.

Some Perestroikans might believe, with Luke (2002, p. 8) that “Arguing about method is a famously wasteful pursuit in American political science that distracts our attention from concrete political analysis.” Luke argues that the emphasis should be on the political struggle within the discipline: “Who gets what, when, where, and how is an always shifting outcome of network wars... Yet, this is not unseemly, it is often the heart of the matter” (p. 8). As if to drive home the movement’s diversity, Luke is careful to point out that he is speaking “with Perestroika” but not “for Perestroika” (p. 1). This

³ Honig (2002) criticizes the relevance criterion, but in doing so allows that this is one matter on which all Perestroikans agree – herself excepted.

diversity of the movement might seem to fit comfortably with its 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 in methodological terms. 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). I will focus on the first three items in the above list, first, because there appears to be consensus among Perestroikans⁴ that they constitute the methodological core of the movement, and second, because taking them seriously points (I argue) directly to a critical political science beyond Perestroika.

Perestroika’s Empty Pluralism

Perestroika’s advocacy of pluralism requires explanation in light of existing pluralism in the discipline. This pluralism may not extend to the APSR and its imitators, several large Midwestern departments, or the University of Rochester and its doppelgangers, 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.

⁴ With the exception of Honig (see footnote 4) and Luke (2002), who disdains methodology.

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.”⁵ 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. The 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 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?”⁶

⁵ Colloquy Live, ‘The Perestroika Movement in Political Science’, *Chronicle of Higher Education*, September 19, 2001. Accessed online at <http://chronicle.com/colloquylive/2001/09/Perestroika/> p. 3.

⁶ 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.

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: 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, to the detriment of any more widespread conversation, within the discipline or beyond.

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).⁷ Lasswell advocated inquiry that would address the great issues of the age, such as the threat of war, the emergence of a “garrison state”, the undermining of democracy. To Lasswell, the policy sciences were to be “contextual, problem-oriented, multi-method”, requiring the best that political science (and other social sciences) could offer. There is little in Lasswell’s mantra with which the Perestroika could disagree. The contextual aspect is stressed by Sanford Schram (2002, p. x), who after endorsing Perestroika declares “my political science would find its standards of knowledge in asking whether scholarship can

demonstrate its contributions to enriching political discourse in contextualized settings.” (Some of Lasswell’s other proclivities, such as his psycho-analysis 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, in the Yale-based Society for the Policy Sciences, and the journal *Policy Sciences*. But far more prevalent is the narrower case-based work that is the staple of the Policy Studies Organization and its journals. The policy sciences approach remains a minority taste within one subfield, a far cry from Lasswell’s heroic ambitions.

At the very time Lasswell wanted the discipline to 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 an immediate aim – 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, and impressionistic work with a systematic search for reliable knowledge based on study of the actual behavior of political actors. To their critics, it was about securing respectability within the US university system (Ricci, 1984), appealing to funding sources, or depoliticization in the early cold war.

The contemporary hegemony is constituted in part by the commitment to quantification that is one legacy of the behavioral revolution. (As David Easton (2001)

⁷ Prior examples might include 19th century moral science and some of the efforts of Charles Merriam.

notes, behavioralists could in the 1950s complain that they were unfairly excluded from the APSR. As late as 1956, 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, 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, the US university system has always demanded the trappings of science, which proved inconsistent with any more practical emphasis on political problems or the great conversation of democratic development. To Seidelman and Harpham, it is the recalcitrance of the political system that reform scientists wanted to improve. 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 constitutive of the discipline from the beginning, not a feature that arrived with behavioralism or rational choice theory.

In this light, Lasswell’s failure to reorient the discipline to make it relevant to the great questions of political life is just one of a string of failures. But closer examination of the reasons for this particular failure is 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. 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. Lasswell’s

policy sciences required individual policy scientists to be as superhuman as Lasswell himself, to have 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 was the best device for integrating fragmentary bits of knowledge held by individuals. 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 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; not deep intellectual pluralism of the sort we observe in political science. But Hayek and Popper are right that *some* mechanism for integration is needed. And this highlights what is currently missing from Perestroika's pluralist program.

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.

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?"

Toward a Critical Political Science

Let me start from Perestroika's stated commitment to problem-driven inquiry. Problems are aspects of the world that need explaining or remedying. Not just any problems will do; rational choice theorists are justifiably criticized for the narrowness with which they define problems, and their consequent lack of relevance to significant political problems.⁸ 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). 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 cope with

⁸ But see Landman (2002) for an account of the degree to which systematic comparative inquiry (both large-n and case study) has produced findings relevant to important issues of development and democracy.

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 so defined 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 critique of rational choice theory's problem-solving narrowness.

Our search for critical standards 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.'

In terms of who does the defining, if it is only the practitioners of a particular research tradition, 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 political relevance. The more defensible answer is that problems get defined in a social process in which both political scientists and other political actors participate. 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. Such social processes can be distorted by money, power, and ideology. Defensible problem definition, and so a defensible discipline, can only exist if such distortions are recognized,

criticized, and counteracted. This critical commitment is not a matter of preference: it is a matter of cognitive rationality. Elsewhere I have developed a more elaborate argument on how principles of communicative rationality can be brought to bear here (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 (Habermas, 1984). But whether or not one accepts this particular criterion, *some* critical normative standard is necessary.

Such a critical commitment is especially important when ideological hegemony distorts the disciplinary agenda. As Oren (2002) demonstrates, American political science has been powerfully shaped by shifting enemies and friends in US foreign policy. In the Cold War, 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 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). 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 dubious democracies.

A putatively rational and progressive discipline can, then, be judged in terms of the adequacy of its plurality of research traditions in relation to 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. But this only goes so far: the imagery here is of a set of 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?

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 all the way down that the shortcomings or indeed strengths of particular approaches can be exposed. Approaches can emerge strengthened as well as weakened by such encounters, which constitute 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. It is best not to think of rational choice theory as explanatory theory; rather, rational choice theory shows 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 by alternative forms of rationality. Such alternatives can only be revealed by a research tradition capable of recognizing both the 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 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 better equipped to address at least one of the great political issues of our age: the increasing marketization and so individuation of social and political life (Offe, 1987). The combination of rational choice and critical theory can elucidate the consequences of this individuation, and to investigate institutional and other means for curbing it. Mere tolerance 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, critical engagement needs to go all the way down.

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 bedeviling cultural explanation will also undermine any combination with rational choice. The combination 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 lacks any mechanism to explain the 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. But this encounter has at least highlighted a deficiency in cultural explanation that its practitioners must rectify.

In a different arena, Berejikian (1992) shows that critical engagement need not be 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. 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 involves 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. This intellectual engagement encompasses structure, rational agency, and ideology.

Rules of Engagement

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

First, *engagement across traditions means that you need research traditions to begin with*; 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.

Postmodernists interested in destabilization of rigid understandings might 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 critics 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. So if realism, liberalism, and other traditions did not exist, critical metatheorists of international relations would surely have to invent them. The implication of my first point is not that all approaches to inquiry need “hardening” into research traditions with explicit 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 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 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. The exemplary work of Skocpol (1979) does not simply test a theory of revolution in a comparison of cases. It also develops that theory in the context of 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.*

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 requires the ultra-formalization that makes reading the APSR like trying to read the phone book upside down (as one of my colleagues 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 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. 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 under anarchy, and open themselves to a world of reflection and intersubjective understanding.

Third, *engagement means standards beyond those internal to particular research traditions*, such that an 'anything goes' pluralism cannot stand. Most straightforwardly, the standard in question is success when it comes to resolving socially-determined problems (see above). Particular pieces of research can be evaluated in these terms, but more consequential when it comes to cultivating the discipline is the record 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. The need to assess over time and different contexts means we should not be overly quick in discarding a tradition. It might prove amenable to resuscitation if it can find the right partner; or indeed, 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.

The general point here is that it is reflective disciplinary practice that should be decisive when it comes to comparison of research traditions and their comparison; standards themselves come out of contests and debates across different traditions of inquiry. Accordingly, it makes sense to speak of the problematics of engagement, rather than any eternal rules of engagement.

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.

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