Current Criticism of APSA Is Nothing New

Over the several decades of my professional career, complaints about *APSR* and petitions to change the structure of APSA have been perennial features of life among political scientists. To a remarkable extent the agenda of concerns has remained quite constant, focusing on issues of allegedly excessive emphasis on technique and method, the representativeness of the *Review*, and the APSA's governing structures. Over the years the Association has tried to address these concerns, sometimes with a degree of success, but rarely enough to satisfy or silence the critics. In part, the problem lies in the supposed incompatibility of the ostensible polarities; teaching versus research, technique versus substance, real world politics versus theoretically driven research. Of course, these are not either/or choices but questions of degree, and the appropriate balance may vary considerably across both time and the spectra of subject matter and professional career paths. Moreover, though we may give it more lip service than true commitment, the principle of interdependence really does operate on each of these dimensions of our disputes. As that splendid scholar and wise man, V.O. Key, Jr., said in his presidential address 40-odd years ago, "Method without substance may be sterile, but substance without method is only fortuitously substantial" (1958, 967).

One reason these issues do not go away, I think, is the great diversity of motives that attracts people to careers in political science. Some of us wanted to be teachers and, as we had to teach about something, chose a subject we enjoyed, and learned that it was called political science. Others started out to be lawyers and shifted direction along the way. A role model professor may have inspired emulation. Perhaps a fellowship offer was the critical incentive motivating what turned out to be a career choice. A few may have seen academic life as an opportunity to have an impact on the larger world. Such different motives are very likely to be linked to differences regarding what and how to teach and in the enthusiasm they bring to the tasks of research. Accordingly, there is a persistent pluralism of desires regarding what *APSR* should publish and what the Association should promote.

Secondly, there is considerable variation in political science graduate-training programs. A half-century or so ago there were shadings and nuances of difference among doctoral programs, but for the most part everyone took similar courses and acquired the same techniques (such as they were) of research and analysis. The standard graduate curriculum spanned the entire discipline, a feat that was possible because most fields were not very rich in accumulated findings and could be comprehended in relatively short order. But 50 years of steady work has made this manifestly impossible today. One cannot hope to master more than a fraction of the substantive fields encompassed by the discipline, and the enormous growth in sophisticated analytic technique, without which much serious research today simply cannot be done, has made the task of professional preparation vastly more daunting. Inevitably in this rich diversity, departments vary in what they require of students, and consequently a political science Ph.D. from one institution may only marginally resemble the Ph.D. of another.

These differences in motivation and graduate training shape career choices and the satisfactions those choices bring. A humanistically inclined doctoral program is likely to lead to employment in a department where that orientation is welcomed and, more to the point, where it is unlikely to be challenged or redirected in a more technically demanding direction. The obverse is true for those who start out with a high level of skill in formal modeling. Not only are our broad professional orientations too often unchallenged; we spend large chunks of our careers teaching and, perhaps, writing in the much the same way we learned when we were young. Indeed, one norm that is too often missing from the career commitments of political scientists is the idea that they should continue to learn both substantively and technically throughout their professional lives. No one would find it acceptable to teach about Congress as though Sam Rayburn were still in charge, or to disregard the breakup of the USSR. There is no excuse for someone who pretends to be a political scientist to remain ignorant of major theoretical and technical developments in the field.

We cannot require people in midcareer to broaden the scope of their intellectual interests or to improve their grasp of analytic technique. People will not do better work if they are freed from the burden of reading the research portion of *APSR*, or keep up with the discipline merely by skimming the book reviews. Changes in the governing structures and processes of the Association are unlikely to have much effect on the value derived from the hard tasks of investigation and analysis that must inform both teaching and research. The diversity of interests and perspectives that seems sometimes to plague political science will not disappear but continue to express itself in the pluralistic politics that have characterized the profession for these many years. In the end, and in whatever forms win applause in the intellectual arenas, it is the life of the engaged mind that our discipline, like others in the academy, should foster.

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Defeat Narrow-Mindness, not "Hard Science"

Over the past year, Gregory Kasza has emerged as the spokesperson for the Perestroika movement. I count myself among the alienated APSA membership, and thus among the movement's sympathizers. But, is this a false dawn? Is an "ecumenical science" the practical panacea for what ails *APSR*, APSA, and political science in general? I suspect Kasza understates and misrepresents the challenge before us.

Kasza claims that a reorientation of political science, away from "method-driven" inquiry towards a more inclusive "problem-driven research," will invigorate political science by reintegrating the study of human beings. But, which "problems" are we supposed to address? Kasza's disdain for the "hard scientific fetish for theory's sake" drives an artificial wedge between theory and empiricism. Eschewing theory in favor of empiricism belies a false dichotomy. From the perspective of reflectivism, positivist theory and empiricism are but two sides of the same coin. Problem-driven research and methodological pluralism can just as easily succumb to the kind of "technism" that concerns Kasza.

In disputing "hard science" Kasza falls short of an indictment of positivism, the ultimate source of the Kuhnian normal science. Kasza's criticisms hark back to debates of the 1960s where traditionalists tried to resist scientific approaches. The real nemesis is not "hard science" but a narrow view of what constitutes knowledge.

Before we "defeat the hegemonic project" of "hard science," I feel we must not forget that, epistemologically speaking, not much separates it from its softer counterpart aside from methods. The notion that a division of labor between deductive theorists and empiricists is the root cause of our angst must be questioned. Theory and problem-driven research are not antithetical or mutually exclusive. Theory and the study of substantive problems are not antithetical. We should take care not to throw the baby out with the bath water. Theories are crucial, since they define the world we live in, and more crucially they may exclude contending views and policies.

Our profession is not plagued with too much theory. Rather, in recent decades the rational choice paradigm has enjoyed too much pride of place. The result has been the relative impoverishment of political science as other theoretical perspectives have been marginalized. Serious consequences follow. The privileging and predominance of one theoretical perspective conditions the problems we perceive and how we phrase our research questions. Quantitative methodology may conflate the problems associated with the dominance of rational choice, but it is far from clear that a focus on substantive issues coupled with qualitative methodology would actually resuscitate political science. In fact, it is every bit as likely that graduate students trained to employ qualitative methodologies might be socialized to emulate the rationalists.

Consider that in recent years some "hard science" advocates have made incursions into the qualitative world. Gary King, Robert O. Keohane, and Sydney Verba (1994) claim that in essence there is no difference in terms of research design fundamentals; that the same logic of causal inference applies to both quantitative and qualitative research. Others have taken the rational choice revolution into the realm of macro concerns and historical questions in the form of *Analytic Narratives* (see Bates et al. 1998), ostensibly to answer the critique that rational choice lacks empirical support (Green and Shapiro 1994). I am not too concerned that rational choice theorists ignore substantive issues. I have no doubt that recent events will prompt great interest among scholars on the topic of terrorism, for instance. My concern is that rational choice now aims to venture out of its traditional deductive-quantitative research mode and incorporate qualitative methods and case studies with a view to generalizing.

In short, what Kasza calls "technism" is a symptom, not a disease. Habermas once wrote of the "cognitive interests" that shape knowledge claims, and in this context the technical interest in control and prediction come to mind. Qualitative methods are just as susceptible to "technism" as quantitative and deductive research due to a shared commitment to an empiricist epistemology. Thus, methodological pluralism is not a panacea. The issue is whether we encourage our graduate students to think for themselves. Do we believe that, freed of the chains of "hard science," emancipated graduate students would transcend the deductive-nomological or inductive-statistical paradigm? Will the result be scholarship that starts to address real life political problems in the way Kasza hopes? This requires at the very least a rudimentary background in the philosophy of science and epistemology.

So, what is the nature of the problem? First, let me recommend Steve Smith's "Positivism and Beyond" (1996) for a more sophisticated synopsis than I could hope to offer. However, if I were to venture to paraphrase, a commitment to synthetic "falsification" as the basis of scientific knowledge makes us (and *APSR*'s editorial board) reluctant to accept the validity of what appears to be "soft science." "Hard science" does more than analyze "facts" out of context, rendering them abstractions. Moreover, it assumes that "facts" and "values" are distinct, and that "facts" are theoretically neutral. Inductive empiricism entailing qualitative methods is not all that different, while at a distinct disadvantage in terms of what our profession rewards. The dilemma for students entertaining ideas about studying the reality of "social facts" is that they may find that they have difficulty convincing a dissertation committee of the validity of their constructs. Whereas the quantitative researcher may rationalize or black-box significant "unexplained variance," the qualitative scholar will be held to a higher standard. Whereas the quantitative researcher may get by on prediction (i.e., correlation), qualitative researchers implicitly raise the bar by trying to explain. This, in part, accounts for the prevalence and banality of much quantitative research in prestigious journals like *APSR*.

I cannot offer any quick-fix remedies. I simply highlight that if the Perestroika movement aspires to more than a palace coup, then there is need for a more profound diagnosis of what ails our profession. This should be a debate that transcends methods, and confronts the root causes of conformity. A tall order for sure, but we need to train future generations in the interim. Graduate programs should reorient their methods courses to encompass a broadened concern for epistemology.

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To their credit, some programs do this. Others simply provide students with a copy of Imre Lakatos' defense of Kuhnian falsification, and leave it at that. Others skip the formality altogether, and jump right into regression models. This is a grievous disservice to graduate students.

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Great Political Issues

In defense of the accusation that the APSR does not address "great political issues" Ada Finifter wrote: "Yes, many of our articles use advanced statistical techniques and mathematical modeling, but not at the expense of dealing with the great political issues. Rather, the intent is to obtain more systematic and reliable knowledge about them."

She then gave a long list of great political issues dealt with by *APSR* under her tenure. As a political theorist interested in great political issues and concerned about being marginalized to the point of obsolescence, I found none of this reassuring. She suggests that great political issues should be studied systematically and objectively, which means empirically and mathematically. Accompanying this approach to great political issues was the sense that these methodological constraints are "advanced" methods that show a progress beyond the unempirical and nonmathematical study of politics characteristic of, say, Machiavelli's writings or of the Federalist Papers.

The question is, what does it mean to deal with or to confront great political issues? For Finifter to deal with great political issues means to obtain systematic and reliable knowledge about them through statistical techniques and mathematical modeling. Statistical knowledge is quantified empirical knowledge and mathematical models try to create an order from the chaos of data. Let us begin with the attempt to establish a mathematical science of politics just as modern physics has established a mathematical science of the motion of physical bodies. Physics could only attempt to mathematize motion on the basis that the bodies in motion are not conditioned by anything other than physical laws of nature. Man, however, is conditioned by human laws or conventions. A citizen of America is not the same as a citizen of Iran, or a citizen of ancient Rome. The tastes, morals, and behavior conditioned by respective societies are radically different. Because man is a mixed body, there can be no mathematical science of politics.

Politics is not an empirical science anymore than it is a mathematical science. If empirical studies are to avoid being completely aimless, they must be directed by certain criteria of relevancy. The criteria of relevancy, like empiricism itself, cannot be empirically established; the criteria are established by man's political awareness or by his opinion about the character of the just society. Great political issues concern the nature of the just society. When Publius wonders whether America should be a small or a large republic, he reflects on the ends of government, as he knows that size matters and that one cannot have the same way of life in a Greek polis as in a modern state. Confronting great political issues means confronting the ends of man, which means that political philosophy is the core of political science. Publius did not need the kind of empirical data gathered by statisticians and he certainly did not need mathematical models. He needed political experience and political thought and he needed to deepen his experience and thought by reading political history and political philosophy, especially Locke and the illustrious Montesquieu. When the works of great political thinkers who confronted great political issues are no longer read and when we use a method and language that cannot illuminate their thoughts or even enter them, then we can be sure that something has gone wrong in the profession.

The empiricist or mathematician might claim that the search for human ends is futile and only a matter of subjectivity or history. If this is true it does not prove that empirical and mathematical political science are objective, but rather that political science is not possible. The statistician or mathematician is faced with two possibilities: either their work has no relevance or its relevance is determined by great political issues, subjective or not. Even if one were to arrive at the conclusion that there is no just society by nature, one could only do so by thinking about all the political possibilities and their claims to justice, from the freedoms of modern liberal societies to the duties of ancient republics. The empirical and mathematical study of politics turns away from these comprehensive questions, less from the spirit of true objectivity than from hiding. The empirical and mathematical political scientist does not have to give an account of him- or herself as a human being and may even support certain political beliefs under the guise of fact finding, even though those facts exist in a politically relevant context that needs to be examined. Confronting great political issues means questioning one's own society in order to see both its pros and cons, and that means evaluating liberal democracy. To turn a blind eye towards reflection on the ends of man and of politics is not to avoid subjectivity but to lose oneself in irretrievable obscurity about ends. Exactness and competence in narrow matters does nothing to alleviate this obscurity. Political philosophy is defensible as a necessary and important part of the profession, as it articulates those ends around which regimes form, and it subjects them to scrutiny. Anything less is not to deal with great political issues.

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Interpretive, Qualitative, and Quantitative

While I am in complete agreement with the views expressed by Professor Gregory J. Kasza in his letter in the June 2001 *PS* concerning *APSR* and the dominant view of what constitutes knowledge of the political and social world, I write in an attempt to bring some background to one point Kasza raises.

In an effort to distinguish field and other non-"quantitative" research methods (such as participant observation and ethnographic studies, and semiotic, ethnomethodological, metaphor, narrative, category, and other analyses) from those informed by positivist philosophical presuppositions, many scholars have adopted the term "interpretive," used by Professor Finifter in her statement. (I am guilty of this myself, including in the recently published book, *Conducting Interpretive Policy Analysis* [Sage, 2000].)

The use of this term draws on the extensive writings of (mostly) continental philosophers from the late 1800s through the 1970s–80s, who wrote in phenomenology, hermeneutics, and some-critical theory veins of argument. It has also been extended to include American schools of thought and analytic methods, such as symbolic interaction and ethnomethodology. As an umbrella term for the presuppositions of these and other approaches, it has come into use since the late 1970s and 1980s, in political science through articles published in *Policy Sciences*, and in the social sciences more broadly through two collections of essays edited by Paul Rabinow and William Sullivan, *Interpretive Social Science* (Berkeley: University of California), first published in 1979.

In addition to its solid philosophical grounding, the term allows us to avoid the unfortunate nomenclature of "qualitative" research methods. As against quantitative methods, this language implies that those engaged in research that does not rest on statistical modes of analysis do not count—and that those who analyze their data in the form of numbers, rather than words, do so outside of a values context. The distinction developed when the use of survey research and statistical analyses surged. As these methods were clearly quantitative in nature, linguistic logic brought qualitative into use to distinguish survey and statistical methods from field-based research.

Today, we can see that the distinction is not only misleading, but that it occludes more significant differences. Interpretive philosophies and their attendant research methods put human meaning-making—both as process and as substance—at the core of their explanatory and analytic focus. If methods were taught in the context of epistemological and ontological issues—that is, if they were taught with an emphasis on method*ology*, rather than, in most cases, just as a set of (largely statistical) tools and techniques—these differences would likely be perceived much more clearly.

Interpretive does not mean "impressionistic." Interpretive methods are as scientific—as systematic—as those informed by positivist presuppositions. Because they are based on a different understanding of human acts, including matters of control, they cannot be "lined out" in as stepwise a fashion as the so-called "scientific method." Yet they, too, depend on sustained observation over time. Professor Kasza is right on another point, too: Most research methods books used in political science courses portray interpretive (or qualitative) methods as unscientific, as my colleague Peri Schwartz-Shea and I found recently in reading and analyzing 14 methods textbooks ("'Reading' 'methods' 'texts,'" *Political Research Quarterly*, forthcoming).

This will continue to be the case as long as interpretive methods are judged against the criteria of positivist presuppositions—assessed in terms such as validity and reliability, which have developed out of positivist epistemological and ontological ideas. When more political scientists grasp the methodological–philosophical differences underlying the various methods used across the broad range of our fields of inquiry, and expand the teaching of "methods" beyond statistics, interpretive methods may come to be judged against the criteria of interpretive presuppositions. In that event, the discipline will no longer be able to portray these methods as unsystematic or unscientific.

The advantage of interpretive is its emphasis on the philosophical grounding of those methods. It unfortunately implies, however, that quantitative research entails no interpretation on the part of the researcher. As many so-called qualitative researchers now use those methods in keeping with positivist presuppositions, the continued use of that language seems unhelpful. We might well shift to the language of positivist methods and interpretive methods. Until another set of terms emerges, I suggest that we continue to use interpretive, albeit with a fuller understanding of its meaning.

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The Trivialization of Political Science

The recent criticisms of *APSR* (letters to the editor, *PS*, December 2000 and June 2001) stem, I believe, from *APSR*'s behavioral orientation, which causes authors to present trivial findings. At the end of *APSR* articles, we often have to ask, so what?

A recent article, Vol. 95, No. 2, June 2001, "Why Are Japanese Judges So Conservative in Politically Charged Cases?, " may illustrate this point. Based upon some decisions in so-called "headline-grabbing disputes," the authors (J. Mark Ramseyer and Eric B. Rasmussen) conclude that Japanese judges are conservative so that they may more readily ascend the pay scale and receive better assignments. However, an older school of political science (which I represent) doubts that the political words used in the article, "conservative," "leftist," and "politically salient," among others, can be treated in this superficial way.

This trivialization process misses the really interesting questions about the Japanese judiciary, including: to what extent does it facilitate or reduce corruption?; how trustworthy and efficient is it?; to what extent do the public and/or business community consider its decisions to be legitimate? In my own 1998 book, *The Mysteries of Development: Studies Using Political Elasticity Theory* (University Press of America), which was summarized in *PS*,

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September 2000, I examine Japan in an effort to deal with the question of why corruption devastates poor countries but not rich countries?

A 2001 book by Alex Kerr, *Dogs and Demons: Tales from the Dark Side of Japan* (Hill & Wang), suggests that Japan has become a highly corrupt country. Yet, it remains a very wealthy country, with a high quality of life. We could use some scholarly assistance in dealing with this paradox. In this regard, some case studies might have been useful, as are found in the Spring 2000 issue of *Japan Quarterly*, having to do with police corruption. Unfortunately, *APSR* is decreasingly relevant in dealing with such interesting political questions.

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Who Publishes? More than Rice and McCormick Believe

James M. McCormick and Tom W. Rice's "Graduate Training and Research Productivity in the 1990s: A Look at Who Publishes," in the September 2001 *PS*, takes on an important task in rating the success of political science departments in training their graduate students to publish. The measure McCormick and Rice use to do so, however, is highly biased, leading to dubious results. That the bias is common—indeed, almost universal—in ratings of political science departments makes McCormick and Rice's compliance with this trend more troubling.

The problem is their choice of five journals as the measure of "who publishes": *APSR, AJPS, JoP, Political Research Quarterly,* and *Polity*. The initial problem is the exclusion of book publication. For some parts of our discipline, especially international relations and comparative politics, book publication is as important as article publication, if not more so. Many a tenure case turns on the fact and quality of the candidate's book(s), with articles secondary. Rating any sort of publication record in our field—in this case, departments' graduates—on the basis of article but not book publication unjustifiably discriminates against those who simply have bigger arguments to make, and so make them in books. One might even argue that such bigger arguments may be more important.

It is not unreasonably difficult to include book with article publication in a ratings system. For instance, the June 1999 *PS* carried a rating of academic presses in political science. Why not add to the data sets a count of who published books with, say, the top 10 or 20 academic presses? There would, of course, have to be some kind of multiplier factor: A book carries the work of several articles—I would argue about four on average—but any sort of inclusion would be better than the blithe ignorance of book publication common in most ratings systems, including McCormick and Rice's.

The second problem is the choice of journals to include in the rating. McCormick and Rice argue that their choice—APSA's flagship journal plus those of the four regional associations—is appropriate because "each journal is broad-gauged in its coverage," and so, together, "these outlets present a cross-section of the discipline." This is simply wrong. There are vast swaths of the discipline—especially in the IR, comparative, political theory, and public administration and public policy communities—who rarely look at any of these journals, and who instead publish their work in excellent journals that are typically ignored in political science publication ratings. Of the five journals McCormick and Rice examine, *APSR* and *AJPS* are indeed leading journals, but *JoP* is a bit overrated, and *PRQ* and *Polity* are second-tier journals at best.

How might journals be more appropriately chosen? One simple and already-studied way of measuring which are the most important journals is to consider citation rates. *Journal Citation Reports on the Web* (<http://jcr.isihost.com>) provides data on the number of citations of a large range of journals that might be considered in selecting journals for a rating system. One way to identify the journals with the most important articles is to consider the average citation rate of each article in a journal. I estimated this for the year 2000 by dividing the total number of citations of each journal in that year by the total number of articles published in that year. Since *JCR Web* publishes data on IR journals in a separate category from its Political Science category, I consulted both in doing my mini-study. The most-cited journals in the field according to these criteria were:

Journal	Citations per Article Published in 2000
Public Opinion Quarterly	78.6
APSR	74.5
World Politics	52.4
International Organization	50.7
Journal of Conflict Resolution	41.0
International Security	38.5
AJPS	35.4
New Left Review	20.3
JoP	20.2
Political Science Quarterly	19.5
International Studies Quarterly	19.0

According to these data, the clear top four journals in political science, in terms of serious scholarly attention attracted, are *POQ*, *APSR*, *World Politics*, and *IO*. If one includes the second cluster of journals to rate the top seven, the ones to add are *JCR*, *IS*, and *AJPS*. *Political Research Quarterly* and *Polity* are simply not on the charts, attracting 5.0 and 5.5 citations per article published in 2000.

How much more interesting a universe of scholarship is opened up if one thinks about these journals! *POQ* attracts more scholarly attention than any other journal, publishing work with a different style than *APSR*. Considering how many people pay attention, perhaps the work it carries should garner more respect. *World Politics*, for years the best journal in IR and comparative, has recently focused more on publishing work in comparative politics, becoming the clearly outstanding journal for a wide range of scholarship in that field, much more methodologically diverse than the monomaniacally quantitative *APSR* and *AJPS*. *International Organization* long ago transcended its name to become a leading—at the moment,

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probably the leading—journal in international relations.

While *IO* and *IS* publish mostly work using qualitative methodologies, including (especially in *IO*) work that dares to challenge the positivist mainstream, *JCR* publishes overwhelmingly quantitative and formal analysis, providing balanced representation of the vibrant IR community. Does it not make sense to ensure the IR and comparative are fairly represented in any system of rating departments? Including *JoP* would ensure that the overall system was not tilted too far away from that plurality of the field that studies American politics. The comparative subfield might be better represented by dropping an IR journal and replacing it with the less-cited but well-respected Comparative Politics (15.3 citations per article published in 2000).

Considering *JoP*'s twin in this particular rating, *New Left Review*, offers opportunity to reflect on another issue. As Robert W. Cox wrote two decades ago, "Theory is always for someone and for some purpose." All mainstream political science accepts more or less as given the current political–economic system—in the U.S. and globally—dominated by capitalism and the institutions of advanced industrial democracies. It is unquestionably important that some political science research question that assumption, and much of it does, from the points of view of various feminimisms, postmodernisms, leftisms and other schools. Research that does so is generally shut out of the "mainstream" journals, however, so it migrates to others—most prominently, according to the data above, *New Left Review*, but also to other outlets such as *Signs*.

If we value our academic freedom, we ought to value more highly in our ratings systems the work of those who most test its limits and succeed, despite being ignored by the mainstream. Indeed, for the same reason, the best ratings system might well depart from the citation index to compile a top 10 list of journals including Political Theory (11.3 citations per article), the leading journal in political theory: That community is the smallest, so citation rates will naturally be lower, but an appropriate rating system should surely include the work of those whose contribution is to suggest most of the new ideas the rest of us study. Finally, public administration and public policy are also important parts of the discipline—critically so for those of us who care about the relevance of political science to real-world problems—so a rating system should also consider inclusion of journals like *Administration and Society* or *Public Administration Review*.

There are obviously many factors to consider in deciding how to rate the quality of journals and departments. I have suggested only a few. The main point is that, given the growing consensus in the field and in APSA itself that *APSR* publishes too narrow a slice of the best research in political science[1]—a criticism that also applies to *AJPS* and *JoP*—any rating system based primarily to the obsolete "top three" rule of thumb is simply inadequate.

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Note

1. For a report on the state of the APSR and efforts to reform it, see Sven Steinmo, "Perestroika/Glasnost and "Taking Back the APSR," paper written for APSA Annual Meeting, San Francisco, August-September 2001.

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McCormick and Rice Make Serious Omission

In reading the multiple attempts to quantify scholarly productivity of departments and individuals—in particular, McCormick and Rice's "Graduate Training and Research Productivity in the 1990s" from the September 2001 *PS*—I am struck by the absence of scholarship's most common, influential, and fungible product: books. This omission not only ignores the substance of many of our most important contributions to knowledge, but it further biases the analysis towards sub-fields and methodologies that lend themselves to brevity and serialization. Speaking as a graduate of one of the top-ranked institutions by various indices, neither I nor any (to the best of my knowledge) member of my graduating class nor any member of my thesis committee has ever published (or is likely to) in *APSR*. Yet among us, with major university presses, we have produced dozens of books (as well as influential articles in journals outside the quantitative canon), which never figure in such analyses. Ideally, we should all reject such dubious and invidious ranking exercises and devote our energies to excellence in research, teaching, and contributions to our community. However, as long as rankings haunt the dreams of deans and funders, let us at least strive to make the numbers more representative of substance and diversity.

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