Political Science for Design of a Sensible Census

I write to applaud the special issue on "The Public Value of Political Science Research" and to offer evidence that were this issue of *PS* heeded it would seriously improve American government. I speak from the little corner of government I inhabit.

Politically (though not operationally) the decennial census got off track some years ago. The story is as follows. For a century and a half, the Census Bureau counted as best it could—knowing that its official numbers were only an approximation of the true count that in all likelihood was always higher than what was reported. There was an undercount. Starting in the 1940s, some clever researchers began to measure the magnitude of this undercount and, no surprise, documented that it was differently distributed across geographic areas and demographic groups. Moreover, the limited data available for this research (vital statistics at first and, later, data generated from the decennial short form) showed that the groups least well counted were racial minorities. This is correct but imperfectly so, and only because race is a surrogate for variables that if available might be more strongly predictive of census coverage errors—social isolation, civic indifference, fear of government, irregular housing, immigrant status, illiteracy, etc.

However it happened, the politics of race relations and the methodology of census taking converged. The 1965 Voting Rights Act raised the stakes, as did the steady expansion of federal formula spending for programs often targeted to those groups the census said were undercounted.

The Census Bureau, supported by a sizeable number of professional statisticians and survey methodologists, came to the conclusion that there was no way to count one's way out of the undercount. It searched for a solution to the persistent differential undercount. The best available alternative was dual system estimation (known in wildlife studies as capture/recapture). I grossly oversimplify, but in summary: Estimate on the basis of a census headcount; independently estimate on the basis of a subsequent sample; match the results; sort out the rates at which different groups are undercounted and overcounted; correct for these coverage errors; report a new and more accurate count.

From the perspective of an apolitical statistical agency, what could be simpler than to improve the way a census is done, and thereby gain accuracy while also advancing racial justice.

It is at this point that a reading of the literature reflected in the *PS* contributions of, especially, Lupia, Laver, and Munger would have helped avert two decades of acrimonious and ill-informed partisan argument, budget games, presidential vetoes, and litigation that twice reached the Supreme Court—with no end in sight.

Just a few examples. The rules of counting were changed on the naive assumption that though there might be a politics of census results, there was no politics of census methods. Well there is a politics of the rules that get you to the results as well as of the results you get. Failure to anticipate this political science truism set the stage for the accusation that the recommended census design had hidden partisan intent. The Census Bureau is now the subject of a political debate in which some believe, quite in error, that the bureau has *a priori* knowledge of how its methods will influence party fortunes. This is a messy and harmful place to be.

Another example. The decennial census has two goals: numerical accuracy and distributional accuracy. Both goals can be reached only if everyone in the country is counted. This not being possible, the Census Bureau designs its procedures on the principle that its first task is to count as many people as possible—i.e., to improve numerical accuracy.

The bureau's critics, however, have largely focused on distributional accuracy. They argue that if improving census methodology might distribute shares differently, the bureau should make no changes. This criticism leaves in place the known numerical inaccuracy (the undercount). Moreover, is distributional accuracy more important across-state (the apportionment count), within-state (the redistricting data), or across demographic groups (federal funding)? Where there are multiple distributional tasks, it is not possible to design a census that can maxmize accuracy for each of these tasks. To improve the count in any given area or for any give group necessarily rearranges the proportionate shares for all areas and all groups. No political leader, as best as I can tell, worked at building a political coalition that agreed on how to make trade-offs when conditions prevent simultaneously maximizing numerical and distributional accuracy. The Census Bureau was left to navigate these choppy waters on its own.

Another example. The statistical correction proposed by the Census Bureau moves shares only at the margins, whether congressional seats or federal dollars. But these marginal shifts have the unhappy symbolic property that they enter political debate as if the future of the republic itself is at stake. Until it happened, no one in government gave much thought to how intensely held partisan preferences could capture something so arcane as dual system estimation and convert it into a symbol of, on the one hand, racial justice, and, on the other, constitutional purity.

Which takes us to a final observation. Science is generally more

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at home in a Rousseauian than a Madisonian world. But it was Madison who was in Philadelphia. The constitutional provision for the decennial census had firmly in view the political task of redistributing power in accord with the growing and geographically ever-restless population of the new nation, notwithstanding conflicting interests. In thinking about this basic political purpose of the decennial census, it might be good to get Madison back in mind. The decennial census is a scientific enterprise, and it must remain so if it is to approach accuracy. But basic political science theory warns against too-mechanically applying a scientific result to something so inherently conflictual as the distribution of power. Institutional theory tells us that this will push the political fight back to the pre-result stage, that is, in this case, to the scientific method that generates the census count. A politics of scientific *method* is seriously harmful to science, and is not so good for politics either. But it is what we now have.

Political theory could rescue the decennial by working out what is good in the context of what is possible. It would start by noting that when something outside the prevailing distribution of partisan strength might alter that distribution a formulaic application of that external factor does not make political sense. Yet, this is what the decennial threatens to do—not because of its methods but because the population is moving around. Perhaps some analytic thought could be given to a smoothing function that prevents radical shifts in the partisan balance simply because there are geographically uneven patterns of population growth—that is, no given decennial can result in a state losing or gaining more than N congressional seats. A smoothing function could be coupled with a hold harmless decision rule that takes into account what the voters have put in place—that is, a decision rule that recognizes the prevailing patterns of party strength.

Hold harmless provisions are routinely used in federal funding formulae. Some believe they are overused, but they do allow jurisdictionsto plan for orderly transitions as their eligibility for education or medical funds fluctuates with shifts in population numbers. They also direct partisan battles to how funding formulae are written and applied rather than to methods of data collection.

Basic, theoretically informed work in political science can offer the country a way to allocate congressional seats (or other distributional outcomes) that starts with the most accurate decennial count possible *as determined by scientists*, but then gives scope to the play of partisan interests in the final determination of distributional shares.

Kenneth Prewitt, Director
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Approval Voting: A Significant Contribution Overlooked

None of the participants in the March 2000 *PS* symposium titled "The Public Value of Political Research" mentioned approval voting (AV) as a contribution by the political science community to better governance. In my undoubtedly biased opinion, however, AV has been the most significant contribution to come out of political science research, as measured by actual use, in at least a generation.

Since the publication of my and Peter C. Fishburn's *Approval Voting* (1983), six professional societies—American Mathematical Society, American Statistical Association, Institute of Management Science and Operations Research, Institute of Electrical and Electronics Engineers, Mathematical Association of America, and Social Choice and Welfare Society—have adopted AV for the election of their officers. An overview of the experiences of several of these societies, based on analyses of their ballot data, is given in Brams and Fishburn (1992). AV, or a variant, has also been used successfully by the Econometric Society and the National Academy of Sciences for the selection of fellows and by the International Joint Conference on Artificial Intelligence in choosing candidates for awards candidates.

Numerous colleges and universities, including my own, have adopted AV at all levels of decision making—from faculty senates to departmental search committees—as have nonacademic organizations. For example, UN secretary generals are typically selected from a list of five or more candidates by AV. Even a few public elections have been decided through AV, as was a 1990 statewide advisory referendum on school financing in Oregon, wherein voters were presented with five different options and allowed to vote for as many as they wished.

Not bad, I would say, for an idea that five scattered sets of people wrote about independently in the late 1970s (Brams and Fishburn 1983). This is not to claim that AV was unknown before that time. Its origins, if not its name, go back at least to the election of members of the dogi in thirteenth-century Venice (Lines 1986), and probably much earlier.

For those who haven't yet heard, AV is a voting system in which voters can vote for, or approve of, as many candidates (or other alternatives) as they wish in multicandidate elections—that is, elections with more than two candidates. Each candidate approved of receives one vote and the candidate with the most votes wins. In scores of articles and books published over the last twenty years, several colleagues and I have argued that, in single-winner elections, AV does a better job of identifying a consensus choice (i.e., a Condorcet candidate, who can defeat all other candidates in pairwise contests) than plurality voting (vote for only one), or plurality voting followed by a runoff between the top vote-getters. Additionally, AV compares favorably with more complex ranking systems, like the Borda count or the

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Hare system of a single transferable vote, based on normative criteria like monotonicity and nonmanipulability.

This is not to say that there is consensus concerning the superiority of AV over other voting systems. For recent assessments, see "Symposium: Economics of Voting" (1995), Brams (2000), and Brams and Fishburn (forthcoming).

As adoptions of AV continue (see Brams and Fishburn 2000), and its extrapolation to other elections, including presidential, are studied (e.g., Brams and Merrill 1994), it is perhaps excusable that—more than twenty years after the first analytical articles on AV appeared–AV is no longer considered an innovation. Nevertheless, I think it is worth remembering that its provenance, on the research side, lies in political science. (Some of us have lobbied in the public arena as well.) Indeed, NSF, which began supporting AV research soon after the first articles on it appeared, has repeatedly cited AV as an exemplar of how funds spent on theoretical research in political science can quickly yield useful products.

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Searching for Dollars or Sense?

Allow me to compliment you on your editorial creativity for beginning the special issue on "The Public Value of Political Science Research" (*PS* March 2000) with a brilliant Marxist satire on the commodification of our discipline: "Evaluating Political Science Research: Information for Buyers and Sellers" by Arthur Lupia. What an imaginative dystopia he paints for us: political science abandoning the search for truth and the promotion of social justice and going where the money is. I have but one minor quibble and a question.

In his satire, Professor Lupia pretends that he would have political science direct even more of its research product to what society wants and is willing to pay for. But, as I'm sure he knows society doesn't want anything. Certain groups and individuals do, and some can pay for what they want and others can't. Economists refer to this as "effective demand." The Swiftian edges of Professor Lupia's satire would have cut even deeper had he openly declared that a political science of this type could only serve the rich and the powerful. And, he might have followed up, if welfare mothers want a political science study of their own, let them find a way to pay for it.

My only question is—is this sorrowful landscape meant to be a picture of where political science is heading or of where it (we) have already arrived?

Bertell Ollman
New York University

U.S. Global Hegemony a Reality Unlikely to Fade

Kenneth Waltz' provided a powerful and admirably concise argument against the widespread overemphasis on interdependence and globalization as signaling the demise of the state and the lessened salience of international politics in his James Madison Lecture, "Globalization and Governance" (*PS*, December 1999). Moreover, he made a telling point in observing that "the most important events in international politics are explained by differences in capabilities of states, not by economic forces operating across states or transcending them" (698). However, his conclusion offers several observations about the United States and its world role that seem less compelling than the rest of his argument.

First, Waltz contends that, in its military budget, "the United States continues to spend at a Cold War pace." As evidence, he notes that in real terms, "America's 1995 military budget approximately equaled the 1980 budget, and in 1980 the Cold War reached its peak" (699). But this comparison belies the current reality. The share of GDP devoted to defense has dropped from a mid-1980s peak of 6.5%, at the height of the Reagan buildup, to just 3.2% in 2000. While it is certainly possible to debate baselines, the practical realities of major reductions in active duty personnel, aircraft, and combat ships are very real, and it is thus more accurate to conclude that while the U.S. maintains a strong lead in its military capabilities, it has nonetheless cut its relative level of defense spending quite substantially. At the same time, this smaller share of a very large GDP allows the U.S. to maintain its status as the only country with a truly global power projection capability, and at a spending level that is readily sustainable.

Second, Waltz (700) argues that friends and foes will seek to balance against the current international predominance of the United States. For America's traditional allies, at least, this assessment remains quite doubtful. A decade after the end of the Cold War, countries in western Europe continue to retain their ties with the United States as a hedge against future security uncertainties in the areas to their east and south. Notwithstanding recent calls for developing a European defense identity, the EU states' own continuing inability to achieve an effective common foreign and

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security policy leaves them little choice. In addition, though Waltz dismisses the characterization of the U.S. as a benign hegemon, the reality of this role does help to explain the absence of balancing. Indeed, Waltz's distinguished former student, Stephen Walt (1987), has previously argued that countries balance not against power *per se* but against threat.[1] And, whatever the friction in the relationship, the United States is not seen as a threat. Ironically, it is only if the U.S. disengages from its security commitments in Europe and Asia that Europe and Japan are more likely to rebuild their own military power and to return to great power balancing against regional rivals (Joffe 1999).

Third, Waltz implies that America's predominance is likely to be ephemeral and that the U.S. should be less assertive in its international leadership. However, in the absence of an alternative great power challenger, it is not evident that America's current military status will be threatened for at least the next couple of decades. In the economic realm, though predictions are notoriously difficult (bringing to mind the economists' own tongue-in-cheek caution about never combining a specific prediction with a date), there is, again, no obvious claimant. This represents quite a contrast with forecasts made as recently as the mid-1980s (Vogel 1985). Moreover, America's lead in culture, technology, and in the all-important information revolution shows no signs of diminishing and may actually be widening.[2] As for the issue of leadership, in the absence of more effective international institutions (cf. Rwanda, Srebrenica), common problems ranging from humanitarian intervention, to international financial stability and economic openness, to the environment are more likely to be effectively addressed if the United States remains actively engaged, especially as a catalyst and leader of coalitions, than if it pulls back.

The real longer term peril is less likely to be one of America's overextension or of its galvanizing an international coalition against itself than of the consequences should the United States opt for withdrawal and abdication. The latter course is more likely to prove harmful to efforts to develop a more benign international political and economic environment and also to U.S. national interests.

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Notes

1. It should be noted, however, that Walt (1998/99) has argued for America to begin disengaging from Europe. For a rejoinder, see Lieber (1999).
2. Wohlforth (1999) provides a compelling argument for the robustness of American primacy.

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American Political Science Association

MINORITY IDENTIFICATION PROJECT

THE CONCEPT

The Minority Identification Project is a collaboration of undergraduate and graduate political science programs to attract talented minority undergraduate students to graduate study and, ultimately, to increase diversity in the political science profession.

Faculty in university and college undergraduate programs talk with minority students about professional careers in political science and send the names of promising minority candidates for graduate study to the APSA. Participating graduate institutions actively recruit students identified by the Project, and make special efforts to provide financial aid to those admitted to their programs.

HOW TO PARTICIPATE

The Minority Identification Project is open to all schools and students. If you are interested in any aspect of it, please contact:

Sue Davis at sdavis@apsanet.org
Titilayo Ellis at tellis@apsanet.org
1527 New Hampshire Avenue NW
Washington, DC 20036
phone: 202/483-2512
fax: 202/483-2657

Here are some of the basic steps for participating:

Students: If you would like more information about a career in political science and about the Minority Identification Project, contact your undergraduate advisor. Ask him/her to submit your name to APSA. You can also contact APSA directly.

Undergraduate Faculty: Please meet with your minority students as soon as possible in the academic year, and send APSA the names of those a) who would be promising graduate students, b) with whom you have met and discussed professional careers in political science, and c) who have expressed an interest in being included in this program. Send the name, current and permanent address, phone number, E-mail address, race/ethnicity, GPA, graduation year, and a brief comment (optional) that would offer insight into the student's academic and personal strengths to a graduate school recruiter. Please submit names of seniors and second semester Juniors by mid-April for the Spring Round and by mid-October for the Fall Round of the Minority ID Project.

Graduate Schools: Core graduate schools receive names of students and mailing labels by the end of April and October and may begin contacting students immediately. Other graduate schools interested in receiving the names of students identified in this program should contact Sue Davis or Titilayo Ellis at APSA.

GRADUATE SCHOOL CORE PARTICIPANTS

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