

Weather Forecasters Should Be So Accurate: A Response to "Forewarned Before Forecast"

It has often been said that forecasting elections is a dangerous business. Unlike most of what we do as political scientists, forecasting is the one area where you can be proven wrong and quite publicly at that.¹ As Jay Greene's "Forewarned Before Forecast: Presidential Election Forecasting Models and the 1992 Election" article in the March 1993 issue demonstrates, forecasting elections is even a dangerous business when you are right.

By Labor Day, two months before the election, I produced election forecasts from both my national and state level forecast models.² Both indicated a Clinton victory of modest proportions. The national model forecast based on the second-quarter change in the economy (GNP) and the Gallup presidential trial-heat poll in early September was the most thoroughly tested of the models (Lewis-Beck 1985; Campbell and Wink 1990).³ Based on the sluggish economic growth of the second quarter and Clinton's lead over Bush in the early September poll, the equation predicted that Clinton would receive 52.9% of the popular two-party vote. He actually received 53.5% of the two-party popular vote. The forecast was off by just a bit over half of a percentage point. I should note that the median error of the out-of-sample tests of the model is on the same order, plus or minus one-half of a percentage point. Moreover, to have predicted the incorrect election winner in 1992, the prediction would have had to have been three percentage points off. In the 11 out-of-sample trials, the prediction error only exceeded three percentage points once and otherwise was less than two percentage points off.

Several other forecasts were equally successful. Alan Abramowitz's equation, based on the July presidential approval rating, the rate of economic growth over the first half of the year and whether the incumbent presidential party was running for more than a second term, also was right on target. His equation predicted that Clinton would win 53.7% of the vote, just two-tenths of a percentage point off. Even though Lewis-Beck and Rice incorrectly predicted a Bush victory, based on a revised and elaborated model, their original model came pretty close to the vote. Based on only July approval ratings and economic growth in the first half of the election year, the ingredients of their original model (Lewis-Beck and Rice 1984), Clinton should have been expected to win 52.7% of the two-party popular vote, again an error of less than one percentage point.⁴ All of this is to say there were several forecasting models that were quite accurate in their 1992 forecasts. There were models that did not fare well, most notably Ray Fair's equation that primarily depended on incumbents being reelected and to a lesser degree economic conditions (Fair 1988), but with the exception of the overly elaborated Lewis-Beck and Rice model, the political science models proved to be quite accurate.

One might think that the proof of the pudding is in the eating and 1992 was nicely digested, but Jay Greene warns us that we should be wary of forecasting models and even advises us to abandon the national models. Greene thinks we might just have been lucky that we didn't get indigestion. He, along with Beck (1992), rightly raises the question of how confident we should be in any forecast. This is a good question. He goes on to answer it by drawing 95% confidence intervals around the forecasts. As one might expect, these confidence intervals are large. By this standard the models don't look too good. However, because of the small *N* and the demands of 95% confi-

dence, the errors at these confidence limits exceed the largest actual errors of the model. A more informative method for addressing the confidence question is to report the one-tailed probability (based on out-of-sample errors) of the prediction wrongly predicting the winner. This would tell us what we want to know: based on our experience with the model, what is the probability that it is correctly predicting the winner? By this approach to assessing confidence, we were justified in placing a good deal of confidence in several of the 1992 forecasts and the models appear in a much stronger light.

Greene also compares the systematic 1992 forecasts to the collective forecast of a control group of 19 unsystematically selected people (mostly graduate students) at Harvard. He reports that, as a group, they predicted the election pretty well. Greene does not report the accuracy of their forecasts in prior elections (especially those in which the Democratic presidential candidate did not win). Rosenstone (1983) systematically addressed the general accuracy of pundits in prior elections and found them considerably wanting. Just a few days before the 1992 election, David Broder assembled the forecasts of 15 prominent political pundits (1992). Comparing my national level forecast made two months before the election to the pundits' predictions offered a couple of days before the election, the systematic model proved more accurate than 11 of the pundits, tied in two cases and was only slightly less accurate than two others.⁵ Moreover, when compared to a different sort of control group, the polls conducted just before election day, most of the systematic forecasts, offering predictions several months before the election, are generally more accurate. This general pattern was also evident in 1992. The model's forecast was actually more accurate in its prediction of the November 3 vote than the Gallup/USA Today/CNN poll of registered voters conducted within

the last week of the campaign (Benedetto 1992a). The model missed by six-tenths of a percentage point and the poll missed by two points.⁶

There are several lessons that we should take away from the 1992 election. The basic message of 1992 for forecasting models is quite positive. Political scientists have several models that are highly accurate and quite robust, able to survive even the comings and goings of a Ross Perot. We have the ability to forecast elections quite well at least a couple of months before the election. True, a couple of models went down in flames (especially Fair's, much as Lewis-Beck predicted [Morin 1992a]), but several performed quite admirably. In short, Greene's pessimism about forecasting has little grounding in fact. Second, because we have several good models, we should not rely on one alone. The reliability of forecasts can only be enhanced if forecasting models with strong track records based on different indicators point to the same outcome. Conversely, if these forecasting models yield conflicting outlooks, then there is reason to be more wary. Third, along with the forecast itself, as Greene and Beck suggest, forecasters should indicate the confidence level of their forecast. However, this does not detract from the success of the existing models. By even a conservative forecast, the future of forecasting is bright.

Notes

1. For example, Yale economist Ray Fair's prediction of a Bush victory received a good deal of prominent press coverage both before the election (Morin 1992a) and after (Morin 1992b).

2. The forecasts were presented at a roundtable at the APSA meetings and were also available in Campbell and Mann (1992).

3. Greene focused exclusively on my state model (Campbell 1992). This model, building from Rosenstone's earlier model (1983), included several variables to take into account regional realignments that occurred during the period under study, 1948 to 1988.

If these developments were not taken into account we would have run the risk of misestimating other systematic elements of the model. Greene is right in noting that new regional shifts may emerge in future elections. However, at this point I know of no way to anticipate systematically regional partisan shifts (beyond including the results of earlier elections to other offices). However, his comments regarding whether these variables should be included are off base. They are an important part of the model and clearly do not enter into calculating future predictions. For instance, we can fairly safely assume that both the pro-Democratic shift in New England or pro-Republican shift in the South in the early 1960s will not also occur in the next elections. There are, however, two real problems with the state model, whether southern Democratic presidential candidates will continue indefinitely to get an extra boost in that region and the equal weight of each state in estimating the model. For purposes of aggregation, we should place greater weight on forecast accuracy in the larger states.

4. There is one change from the original Lewis-Beck and Rice specification. Rather than using May presidential approval ratings, this reconstruction used the July measure.

5. The pundits predicted the three-way split of the popular vote. To make this comparable to the model forecast of the two-party vote, I calculated a predicted two-party vote for each pundit from the predicted Bush and Clinton votes in the three-way prediction. For example, David Gergen predicted a 42% vote for Bush and a 45% vote for Clinton (with Perot receiving 12%). Gergen's predicted two-party vote for Bush would thus be calculated as 48.3% (42 divided by 87). The model proved to be more accurate than predictions by David Gergen, James Glassman, Morton Kondracke, Ted Lowi, Chris Matthews, John McLaughlin, Robert Novak, Ralph Reed, William Schneider, Ann Stone, and Tim Wirth. It tied predictions by Ed Rollins and Fred Wertheimer and was less accurate than the predictions by Eleanor Clift and Frank Luntz. Collectively, the mean two-party prediction of these pundits was the same as the model's prediction made two months earlier.

6. In calculating the two-party poll prediction, undecideds and others were divided equally between Bush and Clinton. If they were divided proportionately, the polls would have been four points in error rather than two points. Also, this examination of the polls does not address several polling problems: whether to examine likely voters as opposed to registered voters and how to assign undecideds. With some controversy, Gallup in the last days of the campaign assigned most undecided voters to Clinton's column (Benedetto 1992b; Meyer 1992).

References

- Abramowitz, Alan I. 1988. "An Improved Model for Predicting Presidential Election Outcomes." *PS* 21: 843-47.
- Beck, Nathaniel. 1992. "Forecasting the 1992 Presidential Election: The Message Is in the Confidence Interval." *The Public Perspective* 3 (6, September/October).
- Benedetto, Richard. 1992a. "Polling 'Likely' Voters Narrows the Margin." *USA Today* November 2, sec. A.
- Benedetto, Richard. 1992b. "Gap Jumps to Eight Points, Poll Says." *USA Today* November 2, sec. A.
- Broder, David S. 1992. "Pundits' Brew: How It Looks." *The Washington Post* November 1, sec. C.
- Campbell, James E. 1992. "Forecasting the Presidential Vote in the States." *American Journal of Political Science* 36(2): 386-407.
- Campbell, James E., and Thomas E. Mann. 1992. "Forecasting the 1992 Presidential Election: A User's Guide to the Models." *The Brookings Review* (Fall): 22-27.
- Campbell, James E., and Kenneth A. Wink. 1990. "Trial-Heat Forecasts of the Presidential Vote." *American Politics Quarterly* 18: 251-69.
- Fair, Ray C. 1988. "The Effect of Economic Events on Votes for President: 1984 Update." *Political Behavior* 10: 168-79.
- Greene, Jay P. 1993. "Forewarned Before Forecast: Presidential Election Forecasting Models and the 1992 Election." *PS: Political Science & Politics* 26: 17-21.
- Lewis-Beck, Michael S. 1985. "Election Forecasts in 1984: How Accurate Were They?" *PS: Political Science & Politics* 18: 53-62.
- Lewis-Beck, Michael S., and Tom W. Rice. 1992. *Forecasting Elections*. Washington, DC: Congressional Quarterly Press.
- Lewis-Beck, Michael S., and Tom W. Rice. 1984. "Forecasting Presidential Elections: A Comparison of Naive Models." *Political Behavior* 6: 9-21.
- Meyer, Phil. 1992. "Numbers Turn on Undecided Voters." *USA Today* November 2, sec. A.
- Morin, Richard. 1992a. "The Numbers Say It's Bush." *The Washington Post* April 12, sec. C.
- Morin, Richard. 1992b. "Pollsters' 'Natty' Calculations Added Up on Day That Counted: In Academia, However, One Well-Known Political Crystal Ball Lies Shattered." *The Washington Post* November 5, sec. A.
- Rosenstone, Steven J. 1983. *Forecasting Presidential Elections*. New Haven: Yale University Press.

James E. Campbell
Louisiana State University

Election Forecast

After Ross Perot's reentry in the 1992 race, if there was a "common wisdom" among the specialists in American politics with whom I spoke it was that Perot's support would ebb toward the end of the campaign as it became clear that he had no chance of winning and as the competition between Bush and Clinton narrowed. That prediction was wrong—like virtually everything else that political science common wisdom told us about what should have happened during the 1992 election. In my view there is only one model in the literature that can account for Perot's last-period strength, the minimax blame rule (Wuffle, A, "The 'Minimax Blame' Rule for Voter Choice: Help for the Undecided Voter on November 8, 1988," *PS*, summer 1988, 639-40). That model posits that, *in an election in which all the choices are dismal* (albeit some perhaps even more dismal than others) the only sensible strategy is to *vote for the candidate who one is sure will NOT be elected, lest one be blamed for having helped to elect the winner*. If we do not believe that voter support for Perot grew as the campaign wound down to its final days because many voters did not wish to be held responsible for having voted into office whichever bozo it was that actually got elected, then we would have to believe that voters were actually being persuaded that Perot would make a good president. The latter hypothesis is so utterly incredible that it gives us little choice but to accept the minimax blame hypothesis.

A Wuffle
University of California, Irvine

Response to Martin Shefter

Martin Shefter (*PS* December 1992, 676-79) has rightly identified the role of "institutional combat" in accounting for the turmoil surrounding the nomination of Clarence Thomas to the U.S. Supreme Court. For Shefter, the nomination is best

explained by looking to the different policy commitments held by each institution rather than by considering personal animosities between Democratic senators and Judge Thomas or the impact of "divided government" *per se*. Exploring the different policy commitments of the president and Senate, which have taken hold in recent decades, as an explanation for inter-branch political conflict makes great analytical sense.

However, Shefter's analysis misses a much more basic institutional consequence of American constitutional design and leaves unanswered the important normative question of what role the Senate should play in the approval of presidential nominations to the Supreme Court.

Institutional conflict, combat if you prefer, was built into the U.S. Constitution as one of the republican safeguards against governmental tyranny. Surely this is the meaning and consequence of Madison's famous recommendation in *Federalist* #51: "Ambition must be made to counteract ambition." Conflict between the president and Congress is a "natural" and intentional consequence of giving the branches different constituencies and different terms of office, among other "auxiliary precautions." Thus, regardless of the different policy commitments both branches have developed in recent decades, a more fundamental tension between the president and Congress should always be anticipated—divided government or not.

This lesson in constitutional design may be one of the first experienced—the hard way—by President Clinton.

On the normative issue, no apology need be given by any member of the U.S. Senate who took the "advise and consent" clause of the Constitution seriously. Conservative critics of the Senate would like Americans to believe that the Senate's role in the process of confirming judicial nominees should be limited and *pro forma*. (They may change their minds and behavior quickly if Clinton nominates Laurence Tribe or Mario Cuomo to replace Justice Byron White.) If the Supreme Court nominee is presumptively qualified, conservatives claim, then the Senate should not be in the business of second-guessing the president's judgment.

Yet, this view cannot be reconciled with the development of the "advise and consent" clause of Article II, Section 2 of the Constitution, hammered out at the Constitutional Convention of 1787. Conservatives either know better and have intentionally distorted the development of this clause for political purposes; or they've not read the compelling account of this debate in the *Records of the Constitutional Convention*.

A reading of the *Records* sheds light on the two questions raised by criticism surrounding the Thomas nomination. How did the Senate end up with the power to confirm a presidential nomination to the Court *before* the president can actually appoint the nominee? What was the Founders' understanding of the Senate's role in the confirmation process?

Delegates to the Constitutional Convention spent considerable time debating which branch of *the legislature* should appoint Supreme Court justices. Throughout the convention, various committee reports placed the appointment power in the second branch of the legislature, the Senate. Some delegates, such as James Wilson and Alexander Hamilton, preferred appointment by the executive. They argued that a single individual should be held responsible and accountable for judicial appointments. However, the majority of delegates who spoke out on this issue supported appointment by the legislature. Maryland's Luther Martin and Connecticut's Roger Sherman, for instance, argued independently in favor of legislative appointment on the grounds that it would "be best informed of characters and most capable of making a fit choice." The Senate remained the chamber of choice to appoint Supreme Court justices.

Madison proposed a compromise to bring the executive into the appointment process. But, on at least three occasions from July to August of 1787 the compromise failed to gain support from a majority of state delegations. Finally in early September the "advise and consent" clause was approved, giving the executive a role to play in the selection process by giving the president the power to nominate individuals for evaluation by the Senate.

A typical Madisonian compromise, the "advice and consent" clause combined the virtues of individual responsibility for the selection of nominees by the president with the Senate's superior ability to render a judgment on the nominee.

Contrary to conservative commentary during the Thomas nomination (preceded by the Bork fiasco), Convention delegates presumed the Senate to have the primary burden of selecting Supreme Court justices; the president's role was secondary, a product of political compromise. The president begins the process by nominating individuals to fill vacancies on the Court. But it is up to the Senate to make the difficult and final decisions regarding a president's nominee. The *Records* contain *no evidence* that delegates intended the president's nomination of a Supreme Court justice to be automatically approved by the Senate.

Congressional reforms in the wake of Vietnam, Watergate, and Irangate have been ineffective. Americans know that the balance of power in Washington, D.C. has moved precipitously (if not permanently) from the legislature to the executive—reversing in order of priority the first two articles of the Constitution. The American people are dependent upon the Senate's unapologetic involvement in the selection of Supreme Court justices and I say this knowing full well the Shefter-type institutional combat which may await Bill Clinton. Nevertheless, regardless of partisanship, too much rides on an appointment to the Supreme Court for the Senate to confuse consent with acquiescence.

Mark P. Petracca
University of California, Irvine

Gates Responds to FBIS Concerns

Editor's Note: The following letter was sent to Ian Lustick, University of Pennsylvania, in response to concerns he expressed regarding the effects of proposed budget cuts on the Foreign Broadcast Information Service.

Dear Professor Lustick:

During our brief discussion at the Council on Foreign Relations luncheon, you remarked about how valuable to you the open source products disseminated by the Foreign Broadcast Information Service (FBIS) are to you. I would like to thank you for your encouraging comments on these publications.

You also expressed concern that budget cuts might bring a curtailment of FBIS products, and I would like to address these concerns more fully. Like the Federal Government as a whole, the Intelligence Community does indeed face some relatively austere times ahead. Nonetheless, we plan to continue providing information that has traditionally been made available to the private sector; specifically, FBIS plans no curtailment of its products that are available to the public through the National Technical Information Service (NTIS).

I would add, though, that in addition to budgetary considerations, there are two other issues—copyright and electronic dissemination—that have fed speculation that open source products would be cut back. The issue of copyright is significant, and we have devoted considerable effort, in conjunction with other government departments and agencies, to determine how it affects public dissemination of unclassified material. Following the United States' adoption of the Berne Convention, which extends copyright protection to foreign works, some instances of public dissemination of FBIS products may carry with them the risk of legal liability unless steps are taken to recognize and protect the interests of the copyright owner. We are also currently working closely with the Department of Commerce (of which NTIS is a part) to create an arrangement whereby FBIS products can continue to be made available to the public in a manner consistent with copyright law. Under this arrangement, and consonant with the "fair use" exception, we intend to continue supplying FBIS materials to the Library of Congress and to the Federal Depository Library Program, which includes university libraries and some public libraries.

The second issue, electronic dis-

semination of FBIS materials to government users, has led to private sector concerns that FBIS products will no longer be made available to private subscribers through NTIS. Rest assured that we are working with NTIS to make soft copy delivery available to public subscribers. We expect this service to develop simultaneously with electronic dissemination to government customers.

I appreciate your interest in FBIS products and thank you for sharing your concerns. FBIS and I take great pride in the utility of these publications to the private sector, and we are always pleased to hear that our products are valued. If you have further questions, I would be happy to address them. Also, you may feel free to share this letter with or reproduce it for other interested scholars.

Sincerely,

Robert M. Gates
Director of Central Intelligence

The Librarian of Congress

We wish to inform our scholarly constituencies of the background behind certain recent actions taken by the Library.

The Library of Congress is, as you know, both the national library serving scholars and the nation's 115,000 other libraries in various ways, and an agency of the legislative branch. All too often, its budget is casually included by the news media or regarded by the public as part of "the cost of Congress."

Seeking to reduce that cost, Congress cut all of its agencies' budgets except those of the Library and the Botanic Garden by an overall 6.5% this year. The Library received a 1.9% (\$6.1 million) increase, for fiscal year 1993.

The problem is that the Library required \$12.5 million more than it received just to stay even—to pay mandatory federal pay and cost-of-living increases to its staff and inescapable price increases on the goods and services it buys. Just to keep up with its "mandatories," the Library, like many other federal agencies, needs a 6% budget increase per annum.

In addition to paying for these

“mandatories,” the Library of Congress faces major core demands that cannot be further deferred: we need to ensure the security of our unique collections, preserve them for future generations, and reduce the backlog of unprocessed materials so that they can be made available to our users.

Unlike private enterprises, the Library cannot trim pay and benefits or lay off selected personnel. The Library did begin a partial hiring freeze last spring, anticipating leaner times ahead. It also reduced travel and put a special team to work uncovering inefficiencies. It shifted money from buying new electronic technology—the Library’s future—to “absorbing” the mandatory salary increases. But it also had to pay for new security measures and for catalogers to continue reducing the Library’s backlog of 31 million uncataloged items: these are the Library’s top priorities.

We have had to make other painful choices about where our diminishing resources should go. We do not have funds to fill a number of key reference and other public service vacancies for the foreseeable future, and reading rooms and book-stacks are inadequately staffed to give readers effective access to the Library’s collections. Therefore, we plan to shorten reading room hours in the Business, Law, Local History and Genealogy, Main, Microform, Newspaper and Current Periodicals, and Science reading rooms. By reducing weekly hours in these reading rooms from 77.5 to 68.5, we hope to maintain the quality of service our users have become accustomed to expect, as we assist readers in identifying and retrieving the materials they need for their research.

We have surveyed reading room use and found that Tuesday and Friday evenings, on average, are the least busy. We will close at 5:00 p.m. on those evenings, beginning Friday, January 29, 1993.

By reducing hours of service in seven of our 22 labor-intensive reading rooms on Tuesday and Friday evenings and closing on Sundays during the summer, we will save \$60,000 in overtime costs and re-allocate 27,000 hours of staff time to handle the 34% increase in calls for books

since the stacks were closed for security reasons last spring. An additional saving of \$327,000 will result from rescheduling police shifts and cutting the hours when entrance doors are open.

Under current laws, union agreements, and federal regulations, the Library’s future cost-cutting options remain very limited. They will continue to be pursued.

As I am sure your members realize, the Library must also modernize its aging technology, and continue to attract the best “knowledge navigators” as catalogers and reference specialists. In serving the nation as well as the Congress, the Library is developing plans to bring its key collections in all formats through modern technology to libraries, schools, colleges, and research institutions in all 50 states. Thus, at the same time as we seek to make our use of existing resources as effective as possible, we must seek additional funds—public and private—to bring the Library into the electronic age.

James H. Billington
The Librarian of Congress

On Max Lerner

The obituary for Max Lerner by James MacGregor Burns in *PS*, Vol. XXV, No. 4 (December 1992) was touching and conveyed a sense of the brilliant scholarship and deep moral principles which informed his work. Max Lerner was indeed the “complete political scientist.” But Professor Burns omitted one of Max Lerner’s principal professional affiliations—The New School for Social Research. For nearly thirty years, from 1960 to 1989, Max Lerner taught politics at The New School, having already known the university’s president, Alvin Johnson, through their collaboration on the *Encyclopedia of the Social Sciences* in 1927. We at The New School would merely like the opportunity to express our shared grief at having lost one of our own.

Jeffrey Wengrofsky
The New School for Social Research

Editor’s Note: After Cecilia G. Manrique’s article, “Network Resources for Political Scientists,” appeared in the December PS, the address to USA Today Headline News was changed. The address is now 129.22.8.38.

Response to Singer

J. David Singer’s response to my article, “Practitioners and Political Scientists,” is very much appreciated. In spite of the fact that he missed the major point of my essay, his own comments do an excellent job illustrating what I consider to be one of the major problems separating political scientists such as Professor Singer from practitioners.

To begin with, there is a tendency—evident in the title of his remarks, “A Political Scientist Responds to a Practitioner”—to assume that practitioners cannot also be political scientists; an assumption I object to strongly. Political science theory has much to offer—in spite of the hostility on the part of many practitioners to mention of the word “theory.” But the opening of a dialogue will hardly be facilitated, if practitioners are excluded “ex cathedra” from the field of political science by theorists.

Second, Singer criticizes my essay because of its “epistemological innocence.” For some reason, Singer does not believe that political scientists, who are also practitioners, understand such terms. As evidence to support his position, he cites my failure to define what I mean by IR theory. (The relevant passage stated that “there has been an explosion in IR theory since the publication of Morgenthau’s *Politics Among Nations*.” He asks if by IR theory I mean “infra-red (presumably theory), information retrieval (again presumably theory), or international relations (theory)?” Are you serious, Professor Singer? Is the logical construct of such a sentence so difficult to understand? As far as I am aware, Morgenthau had little to say on topics such as infra-red theory or information retrieval theory. If this is not the case, perhaps Singer would be good enough to enlighten us. Obviously, what was meant—insofar as my essay was concerned—was

international relations theory, something other readers—who have written me letters in response to the article—have not had a problem understanding. (For the sake of Singer, I promise that in future essays I will spell it out. I made the faulty assumption that its meaning was obvious.)

Singer goes on to criticize my observation that the findings of projects “such as COW” (The Correlates of War Project—his abbreviation) sound silly to the practitioner and that the latter do not pay enough attention to the long-term implications of such studies. Rather than making such blanket observations in defense of his own sacred COW, I would suggest that a dialogue between practitioners and theoretical political scientists might be better served if he provided some practical examples to substantiate his position. Hundreds of thousands of dollars and innumerable hours have gone into the COW project. By now, one would have expected that something of even tangential utility to the policy maker would have emerged. If this is the case, it would be helpful to know what it is. Unfortunately, as both Singer and his disciple, John Vasquez admit, “A . . . discouraging fact is our failure to achieve any significant theoretical breakthrough.”¹ If this is the case with theory, what is the situation with regard to the practical implications of his work? How much longer must we wait until there is something of practical relevance from the COW project for either practitioners or students interested in a career in the Foreign Service or related fields? Another decade or two? Another fifty years?

Finally, rather than engaging in a self-righteous defense of his sacred COW, Singer should concentrate on the main point of the article; that as a result of the work done by scholars such as Singer, the estrangement of policy makers from what they consider to be practically irrelevant political science is growing. It just may be that the practitioners are right—something that does not appear to have dawned on Singer.

In response to my *PS* article, a distinguished British political scientist suggested to me that responsibility for the current situation is evenly

divided; both practitioners and IR (international relations) theorists are part of the problem. He was right; something I can attest to, having spent many hours arguing the value of IR (international relations) theory to practitioners. My British colleague suggested, correctly I believe, that both sides have much to do to overcome this gap that is becoming a chasm. I agree. Unlike Singer, my British colleague understood the purpose of the article—a call for the opening of a dialogue; not a self-righteous defense of sacred COWs that appear to perpetuate themselves with little or no sign that they will lead to anything of practical utility.

Dale R. Herspring
The National War College

1. J. David Singer, “Introduction,” in Singer and Associates, *Explaining War: Selected Papers from the Correlates of War Project* (Beverly Hills: Sage, 1979), p. 14, as cited in Robert Lieber, *No Common Power* (New York: Harper, 1991), p. 251. See also John A. Vasquez, “The Steps to War: Toward a Scientific Explanation of Correlates of War Findings,” *World Politics* XL (October 1987): 108-45.

Response to “Uncle Wuffle’s Advice to Assistant Professors”

Having supportive relatives in this business can be a very good thing. As an advanced graduate student in the throes of dissertating, Uncle Wuffle was there with good advice I took to heart.¹ Then, as a real (post-Ph.D.) political scientist, I appreciated Uncle’s one-Wuffle crusade, and pondered the possibilities.² Today, I’m a brand new assistant professor, and Uncle Wuffle, as if on cue, is there again.

I have two reactions to “Uncle Wuffle’s Advice to Assistant Professors.”³ At first glance, this new advice has a certain common sense attractiveness: “never volunteer” for service (rule 2), expect to “publish or perish” (rule 3), “don’t overteach” (rule 4), “make some friends” and “find a mentor” on the faculty (rules 5, 6), don’t give up (rule 7). But even accepting the wisdom of this advice, I fail to see how rule 1—“At faculty meetings and elsewhere, assistant professors should be seen and not

heard” (emphasis added)—fits in with the others. It should be revised, relativized, or resisted.

When the chair conscripts us onto a committee—let’s say a recruitment committee—surely, Uncle Wuffle would not advise us to refuse. But when we are subsequently asked to rank the candidates and to defend these rankings publicly, Uncle Wuffle would have us shut up: “You don’t have to be a rocket scientist (or a rational choice modeler) to recognize that opposing the pet projects and favorite job candidates of one’s colleagues is not likely to improve one’s tenure prospects.”⁴ In fact, we would have to be very smart indeed to divine the “favorite job candidates” of our senior colleagues in the first place. In my one experience on such a committee, I was surprised by how radically my senior colleagues’ preferences changed *during* the process. The majority, understandably, took little interest in the committee’s work, and most only expressed their preferences—if at all—*after* the committee was asked to report (collectively and individually). All of this may only *support* rule 1: if you don’t have detailed preference schedules of *all* of your senior colleagues at hand, shut up.

Uncle’s rule 1—absolute and comprehensive as proclaimed—may also be inherently repugnant to both tenured and untenured faculty. It is to me. Assistants should be encouraged, for obvious reasons, to *learn* courtesy and discretion in dealings with senior colleagues.⁵ But why go further? Uncle Wuffle needs to be asked: can following rule 1 ever hurt an assistant? That is: could a strategy of scrupulous deference and silence, pursued over a period of six years or more, in faculty fora specifically designed for the airing of opposing views, not actually weaken support for an assistant up for tenure? Do senior colleagues *not* want to know who they are voting for in tenure decisions? I offer these amendments to rule 1: Assistant professors who willfully or willy-nilly ignore rule 1, should: 1(a) take all the more seriously rules 2-7; and 1(b) keep their subscriptions to the *APSA Personnel Newsletter*, and their vitas, current.

Uncle Wuffle’s advice is always well intentioned and benevolent. But,

and this is my second response, it also seems to be tailored to a somewhat cynical, self-interested game with one prize—promotion.⁶ Underlying the advice, I think, are two mutual reinforcing assumptions: (1) assistants are maximally vulnerable (powerless); (2) tenure is the only good that counts. Core values such as collegiality and free exchange of opinion and ideas, commitments to instruction and to students, and other goods, are to be sacrificed for tenure. In this game, even one's friendships and intellectual relationships are essential instruments, means to an end. If this is the game, some of us won't want to play.

Finally, the assumption that assistants are powerless is only valid in a formal way.⁷ Many institutions (senior colleagues) are playing a parallel, or overlapping, game—to convince skeptical assistants not to go elsewhere, both *before and beyond the tenure decision*. In fact, the market is not closed to anyone; for some assistants, the market is easily reentered; for some hiring institutions, the market is not always favorable. What are the rules of that game?

A Stone
University of California, Irvine

1. A Wuffle, "Uncle Wuffle's Advice to the Graduate Student," *PS* (Dec. 1989): 838-39.

2. See works cited in note 1 to A Wuffle, "Uncle Wuffle's Advice to the Assistant Professor," *PS* (March 1993): 89-90. Wuffauldians will also want to consult A Wuffle, "A Corollary to the Third Axiom of General Semantics," *Journal of Theoretical Physics* 4

(1992): 238-40, which provides among other things a proto-explanation for the paradox that political scientists, endowed with (on paper) perfect information, perform less efficiently than laypeople (spouses and their equivalents) in negotiating human mazes.

3. "Uncle Wuffle's Advice to the Assistant Professor," *op. cit.*

4. The balancing test may make jurists shudder: principles of collegiality, free speech, and the free exchange of ideas are trumped, *a priori*, by the "pet projects" of senior colleagues.

5. A process driven perhaps by making what are for senior faculty obvious mistakes.

6. Uncle Wuffle offers generic advice to assistant professors, not (more narrowly) helpful hints on enhancing tenure prospects.

7. Tenure is power.

Uncle Wuffle Redux

Much of Uncle Wuffle's advice to untenured assistant professors (*PS*, March, 1993) can be summed up in a few words: to get tenure you should adopt the obsequious, suspicious, self-serving mentality and manner of a lackey. Presumably, once tenure is granted you will metamorphose into an independent, forthright, public-spirited colleague whom everyone respects. Perhaps in a sequel Uncle Wuffle will explain how this miraculous transformation comes about.

Edwin Fogelman
University of Minnesota

On Women at the Democratic and Republican Conventions

Is Jo Freeman's article in the March *PS* an example of political science or an opinion piece? I

thought the former while I read through the section on the Democratic convention—although I was highly uncomfortable with her sweeping generalizations and lack of supporting data. But then came the section on the Republican convention where I encountered phrases like "feminism is a dirty word," "party loyalty is a virtue among Republicans, much more than among Democrats," "Goldwater devotee," "fifth column," "control of the national party has shifted so far right. . . ," "fear of homosexuality . . . is pervasive," "concern with maintaining parental authority over children is prominent," "the Republican Party is a hollow shell," and on and on—all without supporting data.

But the most off-putting aspect of the article was the picture of Phyllis Schlafly in front of an assembly characterized as a "front group." In my day, a "front group" was an organization that concealed its actual sponsors and intentions. Is Freeman suggesting this is the case with the Republican National Coalition for Life?

To be honest—and speaking as a liberal Democrat—I think Freeman is right about most of her points. But as a political scientist I demand more than off-the-cuff generalizations and value-laden, sometimes offensive, language.

Is this article an example of what the editors and readers of *PS* consider "political science"?

James P. McGregor
U.S. Information Agency