

The Political Methodologist

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Contents

Notes from the Editors	1
Keith T. Poole: NOMINATE: A Short Intellectual History	2
Jonathan Nagler: SCOBIT	6
Michael D. Ward: A Space Odyssey	8
Altman & McDonald: Resources for the Testing and Enhancement of Statistical Software	12
Simon Jackman: Calculating and Plotting Confidence Intervals	14
John B. Londregan: Probit Probability Impacts ..	16
Jonathan Nagler: Review of STATA for Linux ...	17
R. Michael Alvarez: Introducing SCaMP: The Southern California Methodology Program	18
Richard J. Timpone: Political Methodology Goes to Sundance: A Video Series for Professional Development	19
Jeff Gill: Polmeth Annual Report – 1999	22
Gosnell and Poster Awards	22
Random Utility 2000: Workshop and Conference	23
Midwest Political Science Association, 2000 Meeting	23

Program for the 1999 Summer Methodology Meetings	23
--	----

Program for the 1999 APSA Methodology Panels	24
--	----

Gary King: Letter from the President	26
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The Society for Political Methodology	29
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Notes from the Editors

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One of the common misconceptions held by other political scientists about methodologists is that we love complexity for its own sake, building ever more intricate models as a form of entertainment, and perhaps also as a means of erecting entry barriers against competitors. While nothing could be farther from the truth, the notation that punctuates our written work can be intimidating, and it raises the question: if we aren't just in love with complexity, why do we work with what are often very technical models? In this issue we take this question to two practitioners who have developed new statistical estimators, asking Jonathan Nagler and Keith Poole "Why did you go 'high tech'?" Their answers appear in the articles on the "NOMINATE" and "SCOBIT". Both authors developed more sophisticated models to better capture the actual behavior they were observing in their data, not because it resulted in a more esthetically pleasing model. The following article by Michael D. Ward is another instance of a researcher incorporating a more sophisticated technique to better capture the complex and multidirectional interactions among states. "A Space Odyssey" provides a

brief primer on the emerging link between spatial statistics and GIS.

This issue also contains a new section that we hope will become a regular feature of *TPM*, the Programmer's Corner. This month's installment includes some *S* code from Simon Jackman for generating easy to understand graphs of prediction intervals, and a Gauss procedure for measuring probit probability impacts, and estimating the standard errors for the predictions. We hope that these short articles are of practical use, and we encourage readers to submit short pieces on useful bits of computer code.

In a similarly practical vein, we include a summary of an interesting paper by Micah Altman and Michael McDonald on the computational accuracy of statistical software from the 1999 Summer Methodology Meetings, and a review by Jonathan Nagler of STATA for Linux.

This issue also contains a report from Michael Alvarez on SCaMP (Southern California Methodology Program), a set of regular mini-conferences among methodologists in Southern California. Methodologists in other areas with multiple departments within commuting distance may be interested in trying this as well. We also have a letter from the section president, Gary King, on section activities over the past year, a report from Jeff Gill on the state of the Political Methodology web page, and a report from Richard Timponi on the availability of APSA short courses in methodology on video.

The *Political Methodologist* has earned the reputation of being the APSA section newsletter people find useful enough to keep. As new editors, we hope to continue this tradition.

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NOMINATE: A Short Intellectual History

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When John Londregan asked me to write something for *TPM* about NOMINATE and "why we (Howard Rosenthal and I) went high tech" rather than using simpler descriptions of roll call data, I was puzzled for some time on how to explain why we did what we did. The problem is that my whole career has been devoted to testing a "high-tech" spatial theory so of necessity the methods I have employed over the years have been complicated. However, in the spirit

of John's request, I will give a short intellectual history of how and why Howard and I developed NOMINATE in the form we did.

Almost all of my published work has been concerned with the measurement of ideology. In graduate school at the University of Rochester in the early 1970s I read both Converse (1964) and Riker and Ordeshook (1973) and came away convinced that the correct way to measure ideology or Converse belief systems was through empirical estimation of spatial models of choice. The language of politics is full of spatial terms like left, right, and center, and it seemed to me that the spatial model was the ideal model of political choice.

In 1974 I took a course on scaling methods from Dick McKelvey and was struck by the fact that in the psychometrics literature on multidimensional scaling almost all the empirical applications resulted in low dimensional maps. For example, the experimental data on the perception of color and the perception of sound fit simple two-dimensional maps. The same was true of the early published work applying these methods to political data; Duncan MacRae's (1958, 1970) pathbreaking work on Congressional roll calls using factor analysis, and the use of multidimensional scaling on feeling thermometers by Herb Weisberg and Jerrold Rusk (1970; 1972) and George Rabinowitz (1974).

I was puzzled how standard spatial theory could be reconciled with these empirical results. After all, standard spatial theory posited a multidimensional issue space with each issue having its own dimension. In 1976 I had an after dinner conversation with Peter Ordeshook at Dick McKelvey's house in Pittsburgh that resolved this puzzle for me. Peter told me his theory of the "Basic Space"—a small number of underlying fundamental dimensions that generate all the specific issue dimensions (see Ordeshook, 1976). (Mel Hinich was independently working on the same theory—see Hinich and Pollard, 1981, and for a comprehensive defense and discussion of the theory, see Hinich and Munger, 1994).

I was now convinced I had the right theory. Converse's belief system theory with its emphasis on "constraint" fit like a key into a lock with the Ordeshook-Hinich spatial theory of choice.

So in sum, in the late 1970s I had what I felt was the correct theory and my subsequent career has been devoted to figuring out ways to test it. In this regard, I view myself more as an *engineer* than a theorist or methodologist. My aim has been to construct scaling machines that extract basic spaces from data. My early work on interest group ratings epitomizes this (see my 1985 APSR piece).

During AY 1981-82 I was a Post-Doctoral fellow at Carnegie-Mellon University and had the very good fortune of

linking up with Howard Rosenthal. Howard was also interested in ideology because of his in depth studies of French politics and he was also very knowledgeable about spatial theory. In addition, Howard is a skilled methodologist and he convinced me that we ought to try modeling congressional roll call voting. Thus, NOMINATE was born 1982-83 (Howard invented the acronym NOMINAL Three-step Estimation).

When we first started working together on modeling roll call voting, we took as a point of departure a simple scaling program I had developed at the University of Oregon in 1978. The program was called Edith (my wife's middle name) and it, in effect, does a simple Guttman-like scaling of roll calls. The rationale behind Edith is the following. Suppose voting in Congress is entirely driven by one basic dimension – liberalism-conservatism–so that a legislator's degree of liberalism determines all his/her issue positions. Translated into standard spatial theory, members are arrayed from left to right on a single dimension, have symmetric utility functions centered at their ideal points, and when faced with a choice between the two alternatives corresponding to Yea and Nay on a roll call, they vote for the alternative closest to them on the dimension. Therefore, given the legislator ideal points, all roll call votes should look something like the outcomes in Figures 1a and 1b.

Figure 1a: Perfect Spatial Voting

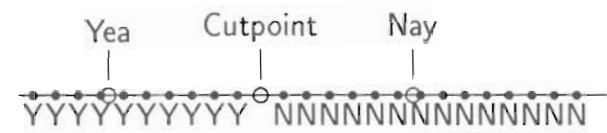
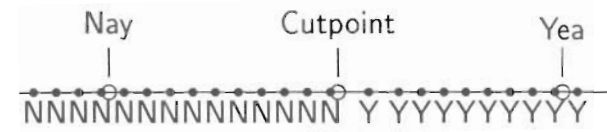


Figure 1b: Perfect Spatial Voting

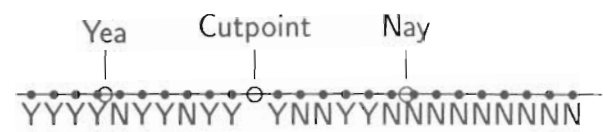


The points marked "Cut Point" divide the Yea and Nay outcomes. A legislator located exactly on the midpoint would be indifferent between voting Yea or voting Nay.

Suppose roll call voting is in accord with this model. Then the scaling problem consists of taking a roll call matrix and "unscrambling" it, that is, finding a rank ordering of legislators and the correct "polarity" (Yea to the left of the cutpoint, as in Figure 1a, or Yea to the right as in Figure 1b) for each roll call such that patterns like those above are

produced for each roll call. This is what Edith is designed to do. Interestingly, if the data is "perfect", as in Figures 1a, and 1b, then the solution is easy and the correct rank ordering is always found.

Figure 1c: Spatial Voting with Error



However, when there is error in the data, with some legislators voting for the alternative farther from their preferred outcomes, as in Figure 1c, things get a bit complicated. When there is error the aim is to find a rank ordering that maximizes the correct classification of the observed Yeas and Nays. This is easier said than done because if there are n legislators, then there are $n!/2$ possible rank orders to check to find the best one. For example, for 50 legislators this number is about 1.52×10^{64} a formidable number even with modern supercomputers. Consequently, Edith embodies a "sensible" search procedure (what the Operations Researchers call a "Heuristic") to find a solution. Namely, a good starting rank order of the legislators is generated and the corresponding cutting points are found. These cutting points are used to get a new rank ordering of the legislators, and so on. At each step the correct classification increases until a rank order is found that produces cutting points that in return reproduce the rank order.

Howard and I thought Edith produced, on balance, reasonable one-dimensional rank orders. But we knew on substantive grounds that during the 60s and 70s (the period we studied first) that there were three loosely aligned voting blocs in Congress–Northern Democrats, Southern Democrats, and Republicans–and this strongly implied that we needed two basic dimensions to adequately account for roll call voting. In addition, Howard argued that even if voting was one dimensional, Edith treated all errors exactly alike and this clearly did not make sense on substantive grounds. For example, Ted Kennedy defecting from his fellow liberals and voting with Jesse Helms seems to be a bigger error than a moderate like John Heinz defecting from his fellow moderates and voting with Jesse Helms.

Our solution was to go "high tech" in that we transferred a standard decision model from economics to a legislative setting. In this model legislators have utility functions and they vote for the alternative on a roll call for which they have the highest utility. This utility function consists of (1) a deterministic component that is a function of the distance between the legislator and a roll call outcome in the basic

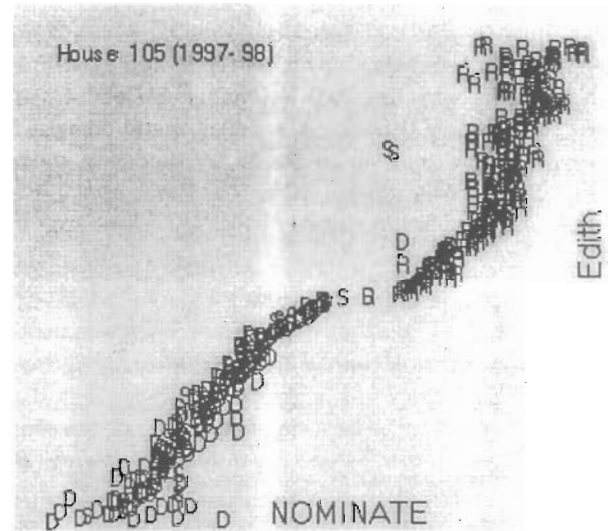
space; and (2) a stochastic component that represents the idiosyncratic component of utility.

We assumed that the stochastic component was a random draw from the logit distribution. Given these random draws, we could then calculate the probabilities of each legislator voting Yea or Nay. Therefore, given a matrix of roll calls the problem is to estimate legislator ideal points and roll call outcomes that maximize the joint probability of the observed votes. That is what NOMINATE was designed to do.

The big advantage of the NOMINATE model over Edith is in its ability to address Howard's point about the errors. If a legislator votes completely at random, that is, deciding to vote Yea or Nay on the basis of a coin flip, then Edith will almost certainly put that legislator at one of the ends of the dimension while NOMINATE will put the legislator near the center of the dimension. To maximize classification, putting the random legislator at the end guarantees about 50 percent correct classification. It is highly unlikely that an interior point will do better and if it does, the interior point is almost certainly near the end of the dimension. In NOMINATE, because the probabilities are functions of the legislator's distances to the outcomes, putting the random legislator at one of the ends of the dimension will produce a mix of small and large probabilities and the joint probability will be lower than placing the legislator near the center of the space where all the probabilities will be nearer to .5.

Consequently, the two methods will differ in their placement of maverick legislators like former Senator William Proxmire (D-WI) who vote with the liberal position on some issues and the conservative position on other issues. For example, the figure below shows a cross plot of the NOMINATE one-dimensional coordinates (horizontal dimension) against the rank ordering from Edith (vertical dimension) for the 105th House (D denotes Northern Democrat, S Southern Democrat, and R Republican).

Although the correlation between the two measures is .96, note the slight "flaring" at the ends of the plot and a small number of outliers in the center. The flaring is due to a handful of representatives that Edith is placing near the ends of the dimension while NOMINATE is recovering them more to the interior. A good example of this is Dennis Kucinich (D-OH) the controversial former mayor of Cleveland (1977-79). (Kucinich is the right-most "D" at the bottom of the plot.) As a member of Congress he has been strongly pro-labor and takes populist positions on economic issues. However, he is strongly opposed to abortion and votes with the Republican majority for the anti-abortion position at every opportunity. Consequently, his fit is low in both scalings 77.6 percent correctly classified in Edith compared to the overall correct classification of 89.8, and in NOMINATE a geometric mean probability of .615 compared to an overall

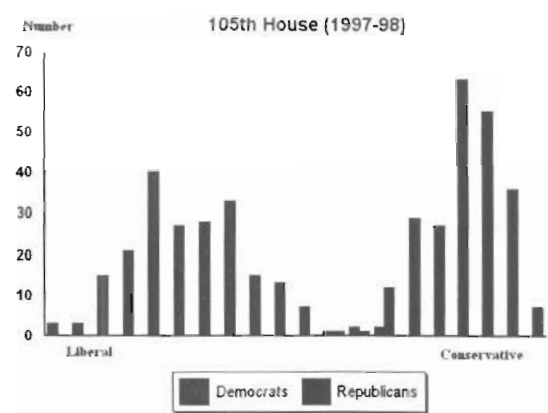


gmp of .756 (the NOMINATE classifications are 75.6 and 88.1 percent, respectively). His NOMINATE score is -.35 (on a 1.0 to +1.0 scale) and he ranks 3 of 438 (that is, he is 3rd most liberal) in the Edith scaling.

Although no placement of Kucinich is entirely satisfactory given the current issue positions of most liberals and conservatives, I believe that placing Kucinich at a center-left position as NOMINATE does is better than placing him at the far-left. Center-Left legislators who fit the basic space model sometimes vote with conservative legislators on economic and social issues but far-left legislators who fit the basic space model always vote the liberal position on all issues. The former strikes me as being closer to Kucinich's actual behavior than the latter.

Both Edith and NOMINATE embody the basic space theory in one dimension. In my judgement, NOMINATE is superior because of the way voting errors are weighted. Unfortunately, this weighting can only be done by "going high tech" and introducing complexity. Complexity has a price. Edith is easier to explain to people and the computer code is simpler to implement. But complexity also has benefits. It produces better substantive interpretations of politics. For example, here is a histogram of the NOMINATE scores for the 105th House. The distribution of the scores is bimodal with hardly any overlap of the two parties. In contrast, a histogram of the NOMINATE scores for any House in the 1970s would show considerable overlap of the two parties. This polarization of the political parties has been steadily increasing since the mid 1960s (Poole and Rosenthal, 1984; McCarty, Poole, and Rosenthal, 1997; King, 1998).

The bulk of our book *Congress: A Political-Economic History of Roll Call Voting* is devoted to showing that important episodes in American political and economic history



can be better understood by supplementing and/or reinterpreting more traditional analyses with the basic space theory of ideology as measured by the NOMINATE scores. This is no accident. This was our goal from the beginning. NOMINATE was never an end in itself. We believed we had the right theory and we set out to build an "instrument" to test it.

I have confined my remarks to one aspect of the error in the one dimensional basic space model. In reality, Howard and I had to push the complexity of NOMINATE to the point that we were working with a dynamic multidimensional model (D-NOMINATE) which required a supercomputer to perform the estimation. We could only study our results by turning them into animations that we could then view on videotape (these can now be viewed on our website <http://k7moa.gsia.cmu.edu> or <http://voteview.gsia.cmu.edu> - in the form of animated gifs). These videotapes were the culmination of our efforts. The animations are relatively easy to understand and interpret despite the tremendous complexity of the theory and the computer programming that produced them. This simplicity of understanding of political events over time would have been impossible without our "going high-tech".

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SCOBIT

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When we do quantitative analysis most of us are trying to draw inferences about population parameters from samples. This necessarily involves assumptions and estimates. Some estimates are straightforward to compute, others are not. Some involve common assumptions that most of us take for granted, others do not. When should we bother to use estimators that are not straightforward to compute, or that involve unusual assumptions?

This choice of whether to use a less restrictive estimator, one that makes fewer assumptions about the data generating process, is faced by researchers all the time. We rarely see generalized least squares run by political scientists. Usually we live with the OLS assumption that the variance of the disturbances is constant. What happens if we are wrong? Our estimated standard errors are wrong, and our statistical tests are incorrect. This leads to a choice faced by all researchers doing quantitative analyses: can we live with an assumption? This is something most of us face from the time we learn the Gauss-Markov theorem and OLS. We all learn that if the Gauss-Markov assumptions are true, then the OLS estimators are the best linear unbiased estimators. And we learn what can go wrong if the assumptions are violated.

The strongest reason to develop or use new methods is that they allow us to test things that could not be tested with an existing method. When we choose between methods there is usually no free lunch. Some methods allow us to make further inferences, if we buy into the assumptions of the methodology. Other methods allow us to test the underlying assumptions behind conclusions we have already drawn. This can lead to further confidence in existing inferences, or it can suggest that those inferences were mistaken.

I'll illustrate this with the development of the Scobit estimator (Nagler 1994).¹ An important question in voting behavior and turnout is whether restrictive registration laws impose the greatest burden on poorly educated voters. If such laws do have a particularly large impact on poorly educated voters, then the existence of such laws could help explain the lower voting rates of poorly educated citizens; and lifting such laws should make the set of voters more representative of the electorate by disproportionately impacting the voting rate of the poorly educated. In their seminal work on the effect of registration requirements and election

laws on voter turnout, Wolfinger and Rosenstone (1980) concluded that registration laws do disproportionately affect the poorly educated. This finding was interpreted to have real substantive importance, and was widely cited. However, the methodology they used to reach this conclusion contained a very troubling assumption. Scobit allowed for the relaxation, and the testing, of that assumption.

Wolfinger and Rosenstone used predicted probabilities from a standard binary probit model to reach their conclusions. They estimated a model of turnout, ran the counter-factual of changing the registration-requirement of interest (number of days before the election that registration closed), then looked at the change in predicted probability of voting for low-education and high-education voters. But, probit (as well as logit) contains a particular technical assumption with substantive implications. If P is the probability that a respondent votes, and X is an independent variable; then generally the quantity of interest is the effect changes in X have on P , which we can write as $\Delta P/\Delta X$. Wolfinger and Rosenstone were interested not just in $\Delta P/\Delta X$ for a particular X (registration laws); rather they wanted to know whether $\Delta P/\Delta X$ varied across education groups. In econometric terms we can think of this as looking for interactive effects between registration requirements and education. But it is easier to think of it in more substantive terms here: will the effect of registration requirements be different for different groups of people?

The probit model assumes that $\Delta P/\Delta X$ is maximized at the point where $P=.5$. This turns out to be relevant to the question at hand. Wolfinger and Rosenstone were interested in estimating the size of $\Delta P/\Delta X$ for two different groups: poorly-educated and highly-educated respondents. But they start with a model that assumes $\Delta P/\Delta X$ is highest for those respondents for whom $P=.5$. Since poorly educated respondents report voting rates closer to .5 than highly educated respondents in the census survey Wolfinger and Rosenstone were working with, this assumption embodied in the probit model was equivalent to assuming that poorly educated respondents were most sensitive to changes in all the independent variables. Now the probit model assumes that the data is generated by a process that guarantees that $\Delta P/\Delta X$ is maximized for $P=.5$. And it imposes this condition on the predicted probabilities. Thus any comparison of predicted probabilities is guaranteed to retain this characterization.

At this point we can see there is a problem with the methodology: one cannot simultaneously assume something is true and test it at the same time. This suggests that the obvious thing to do to obviate the tautology is to relax the assumption that $\Delta P/\Delta X$ is maximized at $P=.5$. This is what the scobit estimator does. Scobit adds a parameter to the standard binomial logit function that allows the shape of

¹Nagler, Jonathan. "Scobit: An Alternative Estimator to Logit and Probit," *American Journal of Political Science*, Vol 38, No. 1, February, 1994.

the response curve to vary.² This means that the point where $\Delta P/\Delta X$ is maximized is not predetermined, but is a function of the data. Thus it becomes possible to test whether poorly educated respondents are most sensitive to changes in registration laws (it turns out that they are), **without** assuming the answer! By specifying interactive terms of interest on the right-hand side of the model, using the more flexible response curve of scobit, we can test whether poorly educated respondents are more sensitive to changes in registration laws than are highly educated respondents.

This seems like a good case for a new methodology: the old methodology was not capable of answering the question at hand. And given the question of interest here, there is no justification I can see for not using scobit over logit or probit. However, as mentioned before – it is not a free lunch. Dropping the assumption of symmetry of the response-curve leads to a loss of statistical power as there is another parameter to estimate. And in practice, this makes it difficult to get estimates that are statistically significant at traditional levels with the sample sizes generally available from survey data. Luckily, in the case of registration laws and turnout, the dataset available was large enough to get significant estimates and it was possible to reject the logit/probit assumption that $\Delta P/\Delta X$ is maximized where $P=.5$. (In this case it turns out that $\Delta P/\Delta X$ is maximized for $P=.40$). However, should scobit always be chosen over logit or probit because it does not force a particular assumption upon the researcher which may be false?

Monte carlo analysis indicates (Nagler 1994) that inferences other than those dependent on variation in $\Delta P/\Delta X$ across some other parameter or group are robust with respect to the assumption. In other words, the assumption can be wrong and it won't affect our inferences if all we are interested in is the average value of $\Delta P/\Delta X$. Thus while Scobit is a great way to both test the assumption, and determine sensitivity to affects among different groups – it need not be used in all cases. The choice between scobit and logit should be based on the hypotheses being tested. Tests of some hypotheses are robust with respect to the symmetry assumption; other tests are completely dependent upon the assumption (i.e., it is a critical assumption; if the assumption is wrong, then the tests are invalid). So cases where scobit should be used are primarily those where the focus is on the interaction between two independent variables.

Conclusion

The most important thing about a methodology is that it lets you test something. If no existing methodology lets you test something, you need a new one. I was

²Whereas for logit $f(x) = (1 + e^{-x})^{-1}$, for scobit $f(x,\alpha) = (1 + e^{-x})^{-\alpha}$.

interested in testing which sorts of respondents are most sensitive to stimuli. Existing methodology explicitly foregoed making such a test by **assuming** who was most sensitive. That was a reasonable assumption if you were interested in things other than who was most sensitive. But, obviously for my interests it was not a reasonable assumption: I wanted to test the assumption!

The use of the Multinomial Probit Model in political science may be looked at in a similar light.³ The MNP model is more general than other discrete choice models in that it does not assume that voters obey the Independence of Irrelevant Alternatives (IIA) property. In other words, it drops the assumption (maintained by common estimators such as Conditional Logit or Multinomial Logit) of IIA. I believe that assumption is wrong when applied to most multi-candidate elections. But does it matter? It *might* not matter for many inferences of the effects of independent variables on probabilities of choosing among an existing set of candidates.⁴ But if we are interested in what happens when we vary the choice set (remove a candidate), or if we want to try to distinguish the correct *theoretical model* of how voters are behaving – then we cannot tolerate making the IIA assumption and the use of the MNP model is very important.

When do we have to drop assumptions? When they are wrong **and** it matters to be wrong. We make lots of wrong assumptions: all models are based on assumptions. The question always is: do the assumptions lead to incorrect inferences? Generally speaking, the better our substantive theory, the easier it would be to make this determination.

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³Alvarez, R. Michael and Jonathan Nagler. 1995. "Economics, Issues, and the Perot Candidacy: Voter Choice in the 1992 Election," *American Journal of Political Science*, Vol 39, No 3, August, 1995; Alvarez, R. Michael and Jonathan Nagler. 1998. "When Politics and Models Collide: Estimating Models of Multi-Party Elections," *American Journal of Political Science*, Vol 42, No 1, January, 1998.

⁴The jury is still out on whether such inferences will be incorrect by a little or incorrect by alot if IIA is violated and MNL or CL is used.

A Space Odyssey

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I first learned about the importance of spatial analysis in an appropriate way, I suppose, as the result of having a geographer move into the office across the hall at the Institute of Behavioral Science. He showed me an article that re-analyzed data that I had often used. The analysis was intriguing, but led to some unusual conclusions. The basic insight was that total conflict in African countries was correlated with conflict in other African countries. I had studied many bilateral situations (e.g., arms races), but this study looked at 54 African countries in their interactions with one another simultaneously. The field of IR had (has) been looking for a way to break through this barrier for many decades. I had tried some different approaches, such as the equations of the statistical equilibrium, and others had looked into the conditional probabilities as a way of stating the problem of simultaneous, multilateral interactions. Neither approach had been very successful (i.e., had mean citation ratios at the median for the discipline, zero).

Since I didn't know much about models of American politics, I didn't have a non-geographic understanding of the term "spatial." So this "spatial" stuff seemed promising methodologically and actually made sense in terms of international relations. So, we tried to replicate this study, largely to learn the ins and out of the approach. In so doing, we found that there were major errors in the standard texts which introduced the models, that the computer programs were arcane and difficult to use (in fact, the original results that propelled us turned out to be incorrect owing to the presence of a stray "1" in the wrong column of the computer input, but this was from the bad old days when computer programs had holes in them on purpose), and we found out that those in the field of geography were quite open, helpful, and by and large interested in similar kinds of questions as political scientists. Most importantly, I found that the statistical approach to understanding spatial phenomenon was extremely powerful.

The basic insight of spatial analysis works on at least two levels. The most powerful and clever is the statistical and mathematical; the most meaningful is the substantive. Substantively, spatial analysis is used to capture the extent of similarities and differences among the units of analysis that may be attributed to shared geographical context. The external economies of Latin American countries are remarkably similar, as are the external economies of East Asian countries. Yet, these similarities are not the same. Is this true because of accident? Or, is it true because of regionally concentrated causal factors (e.g., export orientation in Asia

and import substitution in Latin America)? Or, is it simply a misperception? Descriptive spatial analysis attempts to address this latter type of question. Inferential spatial analysis provides a method to collect information about the former kinds of questions.

The basic kernel of spatial statistics builds from a correlation coefficient that was developed by an Australian statistician, Patrick Alfred Pierce Moran. Moran had studied Kendall's books during his service during the second World War and took up a position at the Institute of Statistics at Oxford. He later founded the Department of Statistics at the Australian National University. He published in the 1950 volume of *Biometrika* two articles which applied geometrical probability to the question of spatial correlation. In so far as I can tell, he never did another piece of work in the area of spatial statistics, but the Moran's *I* statistics had a profound effect on the field of geography.

The Moran's spatial correlation coefficient *I* is given by:

$$I = \frac{n}{\sum_i \sum_j s_{ij}^s} \sum_i \sum_j s_{ij}^s \frac{(y_i - \bar{y})(y_j - \bar{y})}{\sum_i (y_i - \bar{y})^2}$$

where s_{ij}^s is an element of a row-standardized spatial weights matrix which acquires non-zero entries if *i* and *j* are deemed contiguous. In matrix format, the Moran's *I* statistic for a standardized variable, y^s , is given by:

$$I = \frac{\bar{y}^{s'} S^s \bar{y}^s}{\bar{y}^{s'} \bar{y}^s}$$

This may be easily compared to the normal equation for ordinary least squares regression:

$$\hat{\beta} = \frac{\bar{y}^{s'} \bar{x}^s}{\bar{x}^{s'} \bar{x}^s}$$

I shall return to this point below. The first and second moments of the spatial correlation coefficient, *I*, are known, allowing obtained values to be "tested" against random (or normalized) expectations. Large positive values show strong clustering of similar values on the underlying variable, while large negative values indicate the presence of a checkerboard type of pattern whereby high values are surrounded by low values, and vice versa. The former is called positive serial (spatial) correlation.

To calculate this global correlation coefficient, of one unit's values with those around it, requires additional information. First, a spatial weights matrix is required. Often this is a binary matrix based on a threshold cutoff of distance bands. If the distance between each unit is measured, then the matrix is assigned a value of 1 for distances below a certain threshold, and zero everywhere else. In international relations, this matrix often portrays the existence of

land borders between any two countries (taking a value of 1 in the presence of a physical border). Alternatively, it is easy to use the geographical coordinates of a specific site within each unit. With longitude and latitude coordinates in hand, it is easy to calculate the distance between any two points on an arbitrary sphere the size of the earth using the Haversine formula. Send me e-mail if you would like a copy of S-PLUS functions to calculate any of the formulae discussed in this brief note.

In the field of international relations, some scholars use the distance between capital cities, or minimum distance via commercial air traffic. Harvey Starr and others have developed a variety of different approaches based on the existence of information about railroads, highways, waterways, and the like.¹ Although it seems trivial to select a method of measuring the distance between units, the choice of a distance metric is quite important and consequential. Using land borders ignores, for example, countries that may be close to one another but not share a border. Using capital cities works for small countries, but not for large ones: the distance between Mexico City and Washington, D.C. is about equal to the length of its shared land border. Centroid locations (somewhere close to Russell, Kansas for the U.S.) also work, but some countries (e.g., Croatia) have centroids that are outside of the physical boundaries of the country. In terms of physical distances, Kristian Gleditsch and I have developed a database in which we have measured (using Microsoft Virtual Globe) the shortest physical distance between any two countries (1875-1999); these will be available soon on our Web sites.

However, nonphysical representations of distance are also (even more) interesting, and consequential. In the realm of international relations, trade matrices are especially powerful indicators of economic "distance". Basically, anything that can provide a meaningful measure of distance and is defined in terms of all the units of analysis can provide grist for this particular mill. In essence, the matrix is used to provide a weighted average of our "surrounding" values and the Moran's I determines whether an observation's value is statistically similar to the weighted average of those surrounding it. This weighted average is also called the spatial weight. If you standardize the underlying variable and its spatial weight and then calculate the ordinary least squares regression, the regression coefficient is Moran's I , as shown above. Such scatter plots are often illustrative and show the clustering of positive and negative spatial clusters. Figure 1, taken from Shin/Ward JCR 1999, illustrates the use of these scatter plots. Embedded boxes illustrate regions of statistical "significance" for two and three standard errors.

¹Which are "conveniently" available in ARC/INFO: a (very expensive) commercial GIS program. Those interested in obtaining a copy can contact the distributor at <http://www.esri.com>

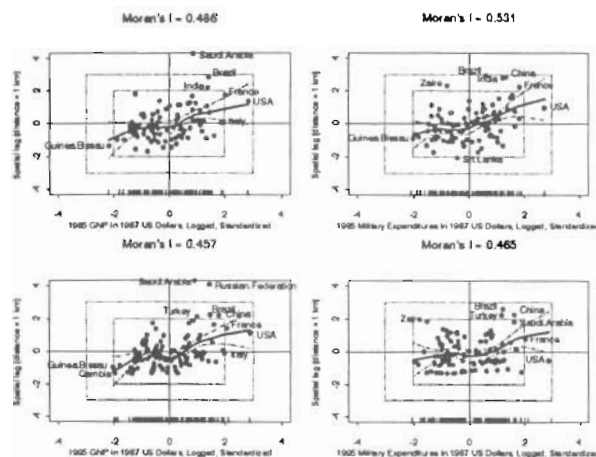


Figure 1: Spatial scatter plots

Using a logic similar to the decomposition of explained variance into each individual observation's contribution (Cook's D), local spatial correlation coefficients provide local measures of the extent to which each observation is clustered or resembles those around it. This provides additional information to global measures of correlation like the Moran's I . The most well known local spatial statistic is the G_i^* , developed by Ord and Getis (1995). The G_i^* statistic for any variable is given by:

$$G_i^* = \frac{\sum_j s_{ij}y_j - \sum_i (s_{ij} + s_{ii})\bar{y}}{\hat{\sigma}_y \sqrt{n \sum_j s_{ij}^2 - \frac{s_{ii}^2}{n-1}}}$$

This statistic can be tested against normal, as well as randomization, assumptions and tells the extent to which each individual observation is spatially clustered. For G_i^* , diagonal entries in the weights matrix are assigned a value of 1 so that each country is contiguous with itself and contributes to the calculation for localized context. By contrast, the G_i indicates the extent of clustering of localized context around location i not including i itself.

A final descriptive approach is perhaps the most fun. Not only can original values be easily (individual mileage may vary) be mapped, but the localized statistics can be mapped in a variety of ways. In my own works these have been color coded², but other approaches such as using different sized symbols are also popular.³

There are really only two wrinkles to all of this so far. First, most datasets are not geocoded, and few researchers

²See Gleditsch and Ward (2000), Shin and Ward (1999) for examples, also visit www.isere.polisci.washington for online versions.

³See Shin's Dissertation on Italian Politics for crystal clear examples.

presently have a matrix of distance representations of the appropriate network of linkages among all their observations. Once you create this, managing it is a bit of a nightmare, if you have any missing data in your original data series that change from variable to variable (i.e., the norm) then you have to manage your spatial matrix carefully. Despite all the wonderful advances in numerical analysis I still haven't found a matrix multiplication routine that is robust to missing values⁴.

If anyone has asked themselves why they are not looking at examples of what I am talking about in this publication, they will have uncovered the second, though transitory, wrinkle. It costs quite a bit to publish color graphics. Two color (i.e., different shades of two different colors, say red and blue) are somewhat cheaper than four-color graphics (e.g., *Wired*), but figure at least a thousand dollars for the set-up costs and several hundred for each color map/figure thereafter. Different publishers have different rates, and there are many minor decisions that also increase the price to the author (quality of paper, et cetera). It would have cost several thousand dollars for a single color map or two in TPM. For some reason I was unable to convince the editors to make this expenditure. As more scholars produce color graphics, this barrier to entry will dissipate, not automatically, but through pressure from authors and associations. Some disciplines already have a pool of money associated with journal costs that cover such high page costs. In so far as I can tell, in political science, it is the author (or author's university or grant) that pays the freight. At present, the cost of a RA for a month in the summer is about the same price. It is probably still better to hire the RA to actually help undertake the calculations, but it is also important to expand our analytical and presentational horizons as well. With pressure the association journals will find a way to support this activity. As web versions of journals become as facile as authors' own Web sites, these kind of graphics will become more common, and I believe we will find them not only cute, but actually informative. My experience is that editors and especially publishers actually like the idea of color graphics, they just don't like to pay for them⁵.

Beyond the descriptive, there are also more elaborate and important uses for spatial analysis. Most forms of regression analysis is based on the assumption of spherical residuals. In the temporal domain this implies that subsequent residuals are not a function of temporally prior ones. In the spatial domain this means that error terms corresponding to a given observation are neither a function of nor correlated with those of its specified neighbors. Statistical models that violate the assumption of spherical error distributions run into serious difficulties when there is serial (temporal or spatial) correlation of error. As is well known,

if the spatial error is correlated with the dependent variable, OLS will yield biased estimates and standard errors. Even if the spatial error is uncorrelated with the dependent variable, OLS no longer provides consistent and efficient estimates though it will be unbiased. Well-known solutions do not apply to the spatial situation because the interdependence observations harbors too many parameters to assess independently.

Moran's *I* statistic provides a test of the existence of spatial correlation of residuals (as well as any random variable). In the context of a simple model, a spatial model (known as the spatial lag model) can be easily developed and estimated by a likelihood function:

$$L = \ln|I - \rho W| - \frac{N}{2} \ln(2\pi) - \frac{N}{2} \ln \sigma^2 - \frac{1}{2\sigma^2} (\vec{y} - \rho W \vec{y} - X \vec{\beta})' (\vec{y} - \rho W \vec{y} - X \vec{\beta})$$

In this specification the spatial weights comprise a predictor variable. This formulation has no analytical solution and must be solved through numerical techniques. Ord (1975) demonstrated that:

$$|I - \rho W| = \prod_i (1 - \rho \omega_i)$$

where the ω 's are the eigenvalues of W , the spatial weights matrix. This trick is useful. By separating the eigenvalues, the interdependencies are reduced, allowing the likelihood function for the spatial lag (autoregressive) model easily to be solved numerically through minimization of the following to yield unbiased, consistent estimates of the unknown parameters, ρ , β , and σ :

$$L_{SAR} = \sum \left\{ \ln(1 - \rho W) - \frac{\ln(2\pi)}{2} - \frac{\ln \sigma^2}{2} - \frac{1}{2\sigma^2} (y - \rho W y - X \beta)^2 \right\}$$

This may be programmed fairly simply in SPLUS (email for code) or estimated in a package - based on a 16-bit interface to GAUSS - called SPACESTAT⁶ that was developed by Luc Anselin. Luc's software is now in version 1.9 and is fairly robust and has lots of fairly nice features (but a 32-bit gui is not one of them). I find that students enjoy Luc's software. However, I have also found that it is pretty easy to do all of this in any object oriented programming language directly, or in statistical packages that allow you access to matrix routines and minimization algorithms. Luc, however, has a plug in that interfaces directly to ARC/INFO which is helpful. That said, my favorite is S-PLUS. Following the same logic the spatial error model assumes that the spatial process is not substantively interesting but simply a nuisance

⁴Bayesians can just impute these values.

⁵Both statements certainly apply at TPM. -Eds.

⁶See <http://www.spacestat.com>.

that screws up the error terms. This can be estimated in the same fashion:

$$L_{SER} = \sum \left\{ \ln(1 - \lambda W) - \frac{\ln(2\pi)}{2} - \frac{\ln \sigma^2}{2} - \frac{1}{2\sigma^2} (y - \lambda W y - X\beta + \lambda W X\beta)^2 \right\}$$

As it turns out, most of this has been developed and widely applied in geography, but widely ignored in political science and economics. That is now changing a bit. Little has been developed in this lineage of scholarship for dealing with noncontinuous variables. Fortunately, the advance of computer technology has meant that even moving the Jacobians outside the evaluation function is not necessary for small problems (say up to about 200 observations), if one is willing to let the estimation proceed while doing something else for a couple of hours. Big problems still require big tricks. More importantly, these problems have been attacked on a different front by the Bayesians, in particular the pioneering work of Julian Besag who introduced the notion of Markov Random Fields to the realm of spatial statistics. I don't have time or space to survey these here, but as a teaser it turns out that the Gibbs sampler approach to MCMC was basically developed to solve this problem. Succinctly stated in Bayesian dialect, the mean and variance are conditional upon the spatial weights matrix. With some exceptions in sociology, most of this work is unknown in the social sciences.

It has been a long journey, this spatial trip. I have learned a great deal and have gained some considerable insight about the spatial organization of political and economic phenomena in every case to which I have applied these approaches. For some this would indicate the lack of power of this statistical approach, but I think it simply indicates the enormous potential leverage to be gained by applying these insights into areas in which they have been ignored. It turns out that these approaches are still rarely taught in the social sciences, even at places with strong geography departments. The two best syllabi resources I know about in the political science realm can be found in John Sprague's Web site for his GIS courses at Washington University and in John McIver's Web site to support the advanced methods course at the University of Colorado. Luc Anselin also teaches a course on these topics at the Consortium each summer.

The assumption that the units we are interested in analyzing are independent belies our fundamental interest in learning about their interrelationships. Work on strategic choice (e.g., Signorino) has begun to tackle some of these issues at the bilateral level. Yet, we know that the political world is not completely described by bilateral interactions

among agents, but includes important and interesting multi-lateral interactions. My belief is that this saw applies widely beyond IR. The spatial framework - even if you throw away the geographic interpretation of it - helps to address this aspect. Further, despite some recent advances and overtures, there is still a wide gulf between those disciplines that assert that context counts, and those who believe that it may be a nuisance. This is not an especially interesting phrasing of the real question. These spatial tools permit us to evaluate systematically the arguments about how context might affect politics and economics. Spatial approaches hold considerable promise as a systematic way for embracing multi-lateral and contextual information about our complicated world, information that our methodologies tend to ignore at present.

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Resources for the Testing and Enhancement of Statistical Software

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Soon after the development of the mainframe computer, Longley (1967) criticized regression programs using it for being dramatically inaccurate. Approximately every ten years thereafter, each new generation of statistical software has been similarly faulted.

In a startling article, McCullough & Vinod (1999) argue that econometric packages can give "horrendously inaccurate" results and that these inaccuracies have gone largely unnoticed (p. 635-7). Moreover, they argue that in consequence of these inaccuracies, past inferences are in question, and future work must document and archive statistical software alongside statistical models (p. 660-662).

In contrast, political scientists writing about quantitative analysis tend not to discuss issues of accuracy in the implementation of statistical models and algorithms. Few of our textbooks, even those geared toward the most sophisticated and computationally intensive techniques, mention issues of implementation accuracy and numerical stability. When political scientists discuss accuracy in computer-intensive quantitative analysis, they are relatively sanguine about the issues of accurate implementation.

In an ongoing research project, we measure the accuracy of statistical abstractions as implemented in statistical packages popular among political methodologists, such as Gauss, Stata, SST and Excel. We evaluate the use of these abstractions in the context of evaluating complex statistical procedures, such as Gary King's (1997) solution to ecological inference. Our working paper, and an extensive list of resources, is available from our web-site:

http://data.fas.harvard.edu/numerical_stability/

To test the accuracy of statistical packages, we implement a series of benchmarks proposed by the National Institute for Standards and Technology (NIST). Like McCullough and Vinod (1999), we find that statistical packages can produce dramatically different results. For example, consider the NIST data set Hahn1. This data comes from a study of thermal expansion in copper, in which non-linear regression was used to estimate the coefficients of the following equation:

$$y = \frac{\beta_1 + \beta_2 x + \beta_3 x^2 + \beta_4 x^3}{1 + \beta_5 x + \beta_6 x^2 + \beta_7 x^3} + \epsilon$$

Using three common statistical packages and the NIST data, we estimated this equation. We report these estimates with three significant digits, along with the correct estimates (calculated by NIST using multiple precision). Surprisingly, the coefficients and standard errors are all different from one another, sometimes in opposite directions and sometimes off by orders of magnitude.

Table 1a: Will the True Answer Please...

	Program A	Program B	Program C
β_1	-1.70e + 00	1.08e + 00	-8.21e + 03
β_2	5.75e - 02	-1.23e - 01	-2.25e + 05
β_3	1.27e - 03	4.09e - 03	-7.05e + 06
β_4	-2.01e - 07	-1.43e - 06	2.27e + 07
β_5	-2.92e - 03	-5.76e - 03	2.31e + 06
β_6	9.47e - 05	2.41e - 04	8.01e + 07
β_7	-4.46e - 08	-1.23e - 07	1.09e + 06

Table 1b: Stand Up

	Correct Values	
	β	Standard Error
β_1	1.08e + 00	1.71e - 01
β_2	-1.22e - 01	1.20e - 02
β_3	4.09e - 03	2.25e - 04
β_4	-1.43e - 06	2.76e - 07
β_5	-5.76e - 03	2.47e - 04
β_6	2.41e - 04	1.04e - 05
β_7	-1.23e - 07	1.30e - 08

It turns out that Hahn1 is especially troublesome for non-linear least squares solvers. Even so, we find that some statistical packages can give unreliable estimates on many benchmark tests. Others, however, perform well in dealing with the standard types of problems that most users face. It is possible to find, and use, packages that produce trustworthy univariate statistics, linear regressions, and anova estimates. For many users, finding the right package will be sufficient.

In contrast, nonlinear optimization, maximum likelihood, and simulation problems can be intrinsically hard. For these problems, there is no set of tractable, universally applicable, boundedly accurate, robust, solution techniques. Many statistical packages provide random number generators and statistical distributions that are inadequate for serious simulation. Moreover, even the best algorithms, implemented correctly, can go astray on complex problems, and there is often no way of knowing beforehand that problems will occur, and no definitive test that the algorithm

can perform to verify success afterwards. Therefore, political scientists who work with complex simulations cannot leave the solutions up to the developers of statistical applications. They must develop an understanding of the different algorithms themselves, know where each is likely to be appropriate, and apply multiple techniques to test the robustness of their estimations.

We make three recommendations, in short:

1. Choose accurate and robust software, and provide developers with an incentive to make accurate software. (There is good news, publicizing this research helps to provide these incentives - many of the problems we note have already been fixed).
2. For standard analysis, accurate software may be all that we need. For complex simulation, non-linear estimation, or ugly data, we can often get better results by paying attention to how solutions are computed.
3. *Worry.* McCullough and Vinod claim that numerical instability pervades the practice of econometrics, and that this throws into question a wide variety of previous results. We too find that numerical instability is present in methods commonly used for political analysis.

But (perhaps) don't worry too much. In our initial examination of two published studies involving complex statistical models, we do find that changing the implementation can change published standard errors, but not by enough to effect substantive conclusions.

The extent of the effects of numerical instability in political science research is largely unknown. We plan further replication studies to investigate this, and particularly to identify prototypes for maximum likelihood estimation benchmarks. To this end, we invite all researchers concerned about the robustness of their results to provide public replication data and code, and we encourage researchers to contact us in this regard.

In addition to our working paper, and to aid political scientists with these issues, we have compiled an introductory annotated bibliography (below), and constructed a web page:

<http://data.fas.harvard.edu/numerical.stability/>

This webpage has links to resources in the following areas:

Statistical Software Test Data and Test Matrices: Links to the NIST repository of reference data sets with certified computational results, which enables the objective evaluation of statistical software, and to other repositories of data for testing statistical procedures.

Random Number Generators and Test Suites: Links to tutorials, pre-prints, and software for random number generation. Includes links to George Marsaglia's DIEHARD suite for testing random numbers, and the SPRNG test suite from the NCSA, as well as links to sources of true (physically generated) random numbers.

Optimization Resources: Links to optimization software that you can modify, and to optimization test-beds where you can submit your problems for analysis.

General Resources for Numerical Algorithms: Links to general resources for numerical algorithms, including repositories from the *Association for Computing Machinery*, and the *Journal of the American Statistical Association*. This also includes links to libraries of high precision statistical distributions.

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Programmer's Corner

Calculating and Plotting Confidence Intervals

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A good rule of thumb in statistics is that any reported point estimate should be accompanied by some measure of uncertainty, such as a standard error, or a confidence interval. Here I demonstrate how to do this in the context of generating point predictions from a fitted regression equation.

That is, suppose we have fitted a model to some data, say, $y_i = x_i\beta + \epsilon_i$, where x_i is a 1 by k vector of covariates, β is a k by 1 vector of parameters to be estimated, and $i = 1, \dots, n$. Our model fitting procedure gives us $\hat{\beta}$, and now we wish to examine how \hat{y} changes as a function of one of the X variables.

This is reasonably straightforward in theory. Say we are interested in predicted values of y , conditional on some hypothetical value of x_j (the j th independent variable). Then we might set the other $k - 1$ X variables at some plausible values, say their respective means or medians, while we let x_j vary over a range of values. Let $\mathbf{x}^{(t)} = (1x_1^*x_2^*\dots x_j^{(t)}\dots x_k^*)$ be a vector of hypothetical X values, with the starred values indicating fixed values, while $x_j^{(t)}$ denotes the current "plug-in" value for x_j ($t = 1, \dots, T$). Then $\hat{y}|\mathbf{x}^{(t)} = \mathbf{x}^{(t)}\hat{\beta}$. To obtain a confidence interval for \hat{y} we rely on the well-known formulae

$$\text{var}(\hat{y}|\mathbf{x}^{(t)}) = \hat{\sigma}^2 \left(1 + \mathbf{x}^{(t)}(\mathbf{X}'\mathbf{X})^{-1}\mathbf{x}^{(t)'} \right)$$

for "individual prediction" and

$$\text{var}(\hat{y}|\mathbf{x}^{(t)}) = \hat{\sigma}^2 \left(\mathbf{x}^{(t)}(\mathbf{X}'\mathbf{X})^{-1}\mathbf{x}^{(t)'} \right)$$

for "mean prediction", from which we can obtain a standard error and confidence intervals, $\hat{y} \pm t_{\alpha/2}\text{se}(\hat{y})$, where $t_{\alpha/2}$ is a critical value of the t distribution such that α probability mass lies outside the confidence interval around \hat{y} .

At this stage a lot of my students reach for a spreadsheet package, such as Excel. They set up a grid of values for x_j , import (often by hand) the parameter estimates $\hat{\beta}$ and the fixed values for the $k - 1$ static independent variables, create formulae and new columns for \hat{y} , $\text{var}(\hat{y})$, $(\text{se})(\hat{y})$, and the upper and lower limits of the confidence interval. This is quite a lengthy procedure, and difficult to automate, so

much so that unless specifically asked to do it, many students (and grown-ups) don't do it.

However, with just a few lines of code in S-Plus, a lot of tedium can be avoided, as I now demonstrate. Say I have fitted a model of U.S. presidential election outcomes, predicting vote for the candidate of the incumbent president as a function of GDP growth and the approval rating of the incumbent president. In S-Plus, we fit the model with the command

```
reg1 <- lm(vote~gdp+pop,data=preselec)
```

and examine the model fit with the command `summary(reg1)`, which produces the following:

```
Call: lm(formula=vote~gdp+pop, data=preselec)
Residuals:
```

Min	1Q	Median	3Q	Max
-3.82	-2.099	-0.2053	1.931	4.506

Coefficients:

	Value	Std. Error	t value	Pr(> t)
(Intercept)	36.3338	2.8249	12.8622	0.0000
gdp	0.8425	0.5533	1.5228	0.1588
pop	0.3085	0.0638	4.8323	0.0007

Residual standard error: 2.913 on 10 degrees of freedom

Multiple R-Squared: 0.8024

F-statistic: 20.3 on 2 and 10 degrees of freedom, the p-value is 0.0003015

Correlation of Coefficients:

	(Intercept)	gdp
gdp	0.0725	
pop	-0.8852	-0.4517

Suppose we are interested in how predicted vote changes as a function of approval, holding GDP growth constant at 3.5 percentage points. This is easily accomplished with just a few lines of code in S-Plus:

```
xgrid <- seq(20,75,1)
x0 <- list(gdp=3.5,pop=xgrid)
yhat <- predict(reg1,expand.grid(x0),se.fit=T)
yhat.pwise <- pointwise(yhat,coverage=.95)
```

`x0` is a list of two vectors, corresponding to the predictors in the linear model `reg1`; `gdp` is fixed at 3.5, while `pop` is a vector, a sequence from 20 to 75, in steps of 1. We pass the fitted linear model `reg1` and `expand.grid(x0)` to the `predict` function, and also request standard errors be calculated. The `expand.grid` function takes a list as its

argument, and duplicates quantities as needed, such that the output contains all combinations of the values of the arguments; in this case, the scalar `gdp` is simply replicated to be a vector of length equal to that of `pop`. The `predict` function calculates \hat{y} given the parameter estimates in `reg1`, and the hypothetical data on the independent variables contained in `expand.grid(x0)`, along with standard errors for each point estimate. Finally, the `pointwise` function calculates upper and lower 95% confidence bounds; note that the object `yhat` contains the point estimates and standard errors for each $\hat{y}|x^{(t)}$, and also contains the degrees of freedom argument $n - k$, which `pointwise` uses to calculate the critical value of the t distribution given the coverage level selected by the user.

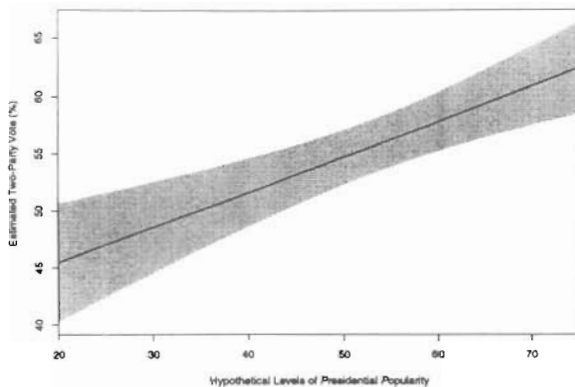


Figure 1: *Predicted Two-Party Vote Share, conditional on $\Delta GDP = 3.5\%$ and varying levels of the incumbent president's popularity. The shaded area represents a 95% "mean prediction" confidence bound.*

This ends the calculation, and we are now ready to plot the results. I produce the plot shown in Figure 1 with the following commands:

```
plot(xgrid,yhat$fit,
     type="n",
     xlab="Hypothetical Levels of Presidential
         Popularity",
     ylab="Estimated Two-Party Vote (%)",
     xlim=c(20,75),xaxs="i",
     ylim=c(min(yhat.pwise$lower),
            max(yhat.pwise$upper))
)

polygon(x=c(xgrid,
            rev(xgrid)),
        y=c(yhat.pwise$upper,
            rev(yhat.pwise$lower)),
        border=F,
```

```
col=6)
```

```
lines(xgrid,yhat.pwise$fit)
box()
```

The `plot` command is actually largely cosmetic, setting things up for the `polygon` and `lines` commands later. Passing `type="n"` to `plot` means that no actual plotting is done, but we get the axes, labels, and a plotting area defined by `xlim` and `ylim`. Note that `ylim` is a vector of length two, the minimum and maximum of the confidence intervals (we need these to fit on the graph).

`polygon` graphs the confidence interval, by defining a set of (x,y) points that enclose the area to be shaded; I select the color with the option `col=6`, which produces a medium shade of grey on most printers. The confidence intervals are given by the contents of what `pointwise` produced for us: here `yhat.pwise$lower` contains the lower 95% bound, and `yhat.pwise$upper` contains the upper 95% bound. The fitted value \hat{y} is in `yhat.pwise$fit`, which I plot as a solid line, using the `lines` command, superimposed on the confidence region.

Finally, note that the `predict` function produces "mean prediction" standard errors, while we are often interested in individual prediction. To remedy this, I use the following function

```
mypointpredict <- function(fit, newdata,
                           se.fit=T, type="ind")
{
  yhat <- predict(fit,newdata,se.fit=se.fit)
  sigma <- yhat$residual.scale
  if (type=="ind")
    yhat$se.fit <- sqrt(((yhat$se.fit)^2)
                      + (sigma^2))
  yhat
}
```

This function is simply a "wrapper" function for the usual `predict` function, but by default, corrects the standard errors to be those for "individual prediction", rather than "mean prediction". Simply substituting the call to `predict` with a call to `mypointpredict` will produce the correct standard errors and confidence intervals for "individual prediction", if so desired.

Finally, note that the actual calculation is extraordinarily simply to accomplish in S-Plus; just three of four lines. Producing a good-looking graph takes a good deal more keystrokes, but even this could be automated to some extent by wrapping it up in a function.

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Probit Probability Impacts

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The well known probit model is one of the work horses of applied political analysis. Applications range from individual level analysis of voting intentions to macro-level studies of military coups and international trading treaties. Most applied political scientists also recall, with some annoyance, that probit coefficients are not as easy to interpretation as coefficients from linear regressions. Instead, the impact of an explanatory variable on the event probability being modeled depends on the estimated coefficient, and on the normal probability density function evaluated at the data point of interest. Those in search of a more general sense of "how large is the effect" can move from this to some sort of average impact. While this is all straightforward enough, the problem of calculating standard errors for use in constructing confidence intervals about these "typical" or "average" impacts regularly embarrasses applied researchers. The overall effect is to discourage political scientists from reporting these impacts, making probit results harder to understand.

There is a divide in the treatment of changes in the explanatory variables between explanators that can vary continuously, such as income, or age, and "dummy" variables, such as indicators for a survey respondent's sex, or whether a leader came to power within the rules of the preexisting constitution. Thus we may ask our estimates, what is the "typical" effect of a small increase in income on a 1996 survey respondent's probability of voting for Bob Dole? What would be the impact on a "typical" leader's probability of leaving power during the current year from a small increase in age? In this issue I will focus on the impact of continuous explanatory variables, leaving the dummy variable case to the next issue.

To measure impacts of this first type for an individual case in our data, with explanatory variables represented by the $k \times 1$ vector \vec{x}_i and estimated probit coefficients by the conformable vector $\vec{\beta}$, we simply differentiate the predicted event probability {something like the probability the survey respondent votes for Dole, or the probability of the national leader losing power during the current year}, which is represented by the standard normal cumulative density function $\Phi(\cdot)$ evaluated at $\vec{x}_i'\vec{\beta}$:

$$\Phi(\vec{x}_i'\vec{\beta})$$

with respect to it's j^{th} element, where j indexes the variable whose impact we want to measure {e.g. income, age }:

$$\phi(\vec{x}_i'\vec{\beta})\hat{\beta}_j$$

If we want to know the "typical" impact, call it Δ_j , one option is to simply calculate the average value of this impact over the N observations in our sample:

$$\Delta_j = \frac{1}{N} \sum_{i=1}^N \phi(\vec{x}_i'\vec{\beta})\hat{\beta}_j$$

This is the point at which a haze of anxious confusion settles over many analysts. How should we go about calculating the standard errors for Δ_j ? The so-called δ -method, which relies on a von Mises expansion {for all practical purposes, a first order Taylor's Series approximation about the expected value for $\vec{\beta}$ }, leads us to the following easy to program expression for the standard deviation of Δ_j in terms of the estimated standard deviation of $\hat{\beta}_j$:

$$Sd(\Delta_j) \approx \frac{\sum_{i=1}^N \phi(\vec{x}_i'\vec{\beta})(1 - \vec{x}_i'\vec{\beta}\beta_j x_j)}{N} \hat{Sd}(\hat{\beta}_j)$$

The following Gauss procedure "impact1" calculates this expression:

```
proc (2) = impact1(x,bprob,vcvbprob);
    local db1, ctr, vcv1, sd1, M;
    db1 = sumc(pdfn(x*bprob))*bprob/rows(x);
    M = pdfn(x[1,]*bprob)*
        (eye(rows(bprob))
        -(x[1,]*bprob)*bprob*x[1,]);
    ctr = 2;
    do while ctr le rows(x);
        M = M + pdfn(x[ctr,]*bprob)*
            (eye(rows(bprob))
            -(x[ctr,]*bprob)*bprob*x[ctr,]);
        ctr = ctr + 1;
    endo;
    vcv1 = M*vcvbprob*(M')/(rows(x)^2);
    sd1 = sqrt(diag(vcv1));
    retp(db1,sd1);
endp;
```

The procedure "impact1" takes as its inputs an $N \times k$ matrix of explanatory variables; x , the estimated β from a probit model; $bprob$, along with its estimated variance-covariance matrix; $vcvbprob$. It calculates $db1$, a $k \times 1$ vector whose j^{th} element is Δ_j , and $sd1$, a $k \times 1$ vector of explanatory variables whose j^{th} element is the estimated standard error for Δ_j . These results can then be reported along with the usual probit coefficients, standard errors, and t-ratios.

I have compared results from this procedure with bootstrapped standard errors with 10,000 replications, and the answers are very close, with the most significant difference being that between the 3 seconds or so needed to

estimate the probit model complete with impacts and their standard errors on a medium-sized (2,798 observations with 12 explanatory variables) dataset and the 2 and a half hours needed for 10,000 bootstrap replications on the same dataset.

When some of the explanatory variables are "dummies" these formulas, based on the δ -method need to be modified. I will turn to this modification in the next issue.

* * * * *

Software Review

Review of STATA for Linux

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This review is meant to cover the Linux version of STATA, not to be a comprehensive review of STATA. It will be a short review because the conclusion is simple: STATA for Linux just works. And it is seamless to go between STATA on different platforms. This is not to say that using STATA under Linux is identical to using it under Windows. Running STATA under Linux is not a point-and-click operation. But you have the basic two options of running STATA interactively, or executing sets of commands from .do files.

There are three issues that generate the most cross-platform confusion: 1) installation; 2) compatibility of file-formats (datasets) across platforms; and 3) graphics. STATA is perfect on (2); and excellent on (1) and (3). And of course everyone gets curious about two performance issues that can vary cross-platform: capability to handle large datasets; and speed.

Installation of STATA was very easy. There was one glitch: the supplied command to unpack the files on the diskettes did not work, but one email to STATA got the appropriate fix for the Linux distribution I was using (RedHat 5.2). After that installation was straightforward. One of the nice things about STATA is that it instructs the user on how to verify installation at the end of the process.

One of the best things about STATA is that the file-format is identical across platforms. This means that datasets can be exchanged between Linux users, HP-UX users, Windows users, Mac users, etc. with no file-conversion to worry about. This is an excellent feature: it is too bad not all statistics packages offer this. STATA can input data a variety of ways. Many people create STATA datasets by

using either the STAT/TRANSFER program, or the DBMS-COPY program. Both programs convert datasets between a range of popular formats. Since STATA datasets are the same across platforms; someone can use STAT/TRANSFER on a Windows machine to convert an Excel file or SPSS-WINDOWS dataset into a STATA dataset; and that STATA dataset can then be given to someone running STATA under Linux. STATA also reads ascii datasets straightforwardly with either the `infix` command (for reading fixed format ascii), the `infile` command (for reading free-form ascii), or the `insheet` command (for reading comma-delimited datasets output from any spreadsheet).

All code for STATA other than commands for printing graphs is also perfectly compatible across platforms. So one can move between different platforms, and nothing needs to change. Graphics worked well in the Linux version. Printing graphs is different under Unix than under Windows, and this is an area where code is not going to be 100% compatible across Unix and Windows machines. The *STATA Graphics Manual* has separate sections for printing graphs under Unix, Windows, and Macs. Both viewing graphs and producing output worked fine. The section in the manual is very clear and demonstrates how to use the `gph` command to save graphs as encapsulated postscript or postscript output. This way the files can be printed or viewed online using ghostview. If one is running .do files it is generally easier to save the output as separate graphs and then look at the output with ghostview; but this is probably a matter of taste. The typical sequence would be to include the command:

```
graph e y, saving (evsy, replace);
```

This assumes that `e` is a variable containing residuals, and `y` is the dependent variable. This command would graph the residuals against the dependent variable. The graph is also saved as a .gph file (evsy.gph). `gph` is the format STATA uses to store graphics and it is described in the manual, but the details are not essential. A postscript file can then be generated with the `gph` command:

```
gphpen evsy -n -dps
```

A warning: it would be very difficult to figure out how to print graphs without the *Graphics Manual*. The typical user is not going to find the `gphpen` command by luck.

The *STATA Getting Started: Unix* manual seems to suggest that there can be quirks in memory usage running STATA under some variants of Unix. There is an issue that memory released by STATA to the system might not really become available. First, this is probably a non-issue since most users would never write a command to release memory. Second, most users on Linux machines would be running on

single-user machines, so overall system issues become a little less important. In any event, I did not notice STATA doing anything unusual with memory. I was running STATA on a dual-processor machine with 256MB of RAM. I normally call STATA with a 100MB memory size (`stata -b -k100000`). I routinely run two STATA jobs simultaneously (thus using 200MB of memory for STATA), and do not notice any system performance degradation. And when STATA is done all system memory usage appears clean. I did not perform any speed tests to compare performance under Linux to performance under Windows. But STATA seemed fast.

STATA has programming capabilities in it: users can define programs, and even store them as separate files to be called from within other programs. This has led to one of STATA's more desirable features: the availability of many routines written by other users. One does not see add-on modules being sold for STATA (as is the case with GAUSS), but one does see code freely available written by other users. For instance, the Clarify set of programs by Michael Tomz, Jason Wittenberg, and Gary King greatly increases the utility of STATA to users, without any need to purchase additional software.¹ And users who learn STATA's programming language can extend those programs to cover other statistical routines. To be clear: STATA's programming language is not as powerful as a language such as GAUSS. But it does allow for reasonably complex manipulation of data. And the volume *Maximum Likelihood Estimation with STATA* gives users examples of programs to perform estimation.²

In January 1999 STATA version 6 became available. This review is not meant to cover the new features (a list is available at: <http://www.stata.com/stata6/>). However, commenting briefly on the set of limited dependent variable models: there are a fair number of new estimation commands, including random-effects probit models, scobit, and probit models with Heckman-style sample selection. The scobit routine works well, and offers several useful options such as the ability to compute the Huber/White sandwich estimator of variance. But it does automate, nor even mention, one of the peculiarities of scobit: that one may have to estimate the model twice: once with the dependent variable as given, and once with the zeros and ones reversed, to be sure one has found the correct value of α .³

Summing up: STATA occupies a valuable niche among statistical software programs. It is easy enough to use that many people find it ideal for training graduate-students with.

It has an excellent set of canned routines for common and advanced statistical routines that is updated often. It has user-supplied routines to supplement those within the base code itself. And it is a reasonable tool to use for data manipulation. The Linux version makes STATA available for people running a robust operating system. And it does so in a way that lets users share code and data independently of which operating system they use. The main point of this review: STATA for Linux just works.

* * * * *

Introducing SCaMP: The Southern California Methodology Program

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Over four years ago, a number of political methodologists in the Southern California area got to thinking. This group — including myself, Neal Beck, Jonathan Nagler, Jonathan Katz, and Mohan Penubarti — realized that between ourselves, our colleagues, and graduate students in the area, that we probably had enough people interested in political methodology to have periodic meetings in our area. This was the birth of SCaMP, which stands for "The Southern California Methodology Program".

The first meeting of SCaMP was held on April 28, 1997 on the UCLA campus. There we started what has evolved into our standard one-day mini-conference format: two 60-75 minute paper presentations and one 60-75 minute "workshop" presentation. In the last two years, SCaMP has been held for one-day mini-conferences at Caltech, UCLA, UCSD, and UCR. One innovation at many of the later conferences was to provide time for a graduate student, soon to be on the job market, to present their research to the group.

This past spring, however, in celebration of three full years of SCaMP, we met for a two-day conference in San Luis Obispo, on the campus of the California Polytechnic State University. There, not only did we have a number of faculty paper presentations, we also had an excellent graduate student poster session. We also had a lot of fun along California's beautiful Central Coast!

In the next academic year, SCaMP promises to continue to be a vital part of the cross-campus intellectual life in Southern California for political methodologists. In the fall, SCaMP is scheduled for the beautiful UCLA Lake Arrowhead Conference Center, high in the mountains east of Los

¹Michael Tomz, Jason Wittenberg, and Gary King (1999). CLARIFY: Software for Interpreting and Presenting Statistical Results. Version 1.2.1. Cambridge, MA: Harvard University, June 1. <http://gking.harvard.edu/>

²*Maximum Likelihood Estimation with STATA*, 1999, Gould, William and William Sribney, STATA Press, College Station.

³See Nagler, Jonathan, *American Journal of Political Science*, Vol 38, 1994, Appendix A.

Angeles. We will also meet at two of the area universities, in all probability UCSB, Caltech, or UCR. If you would like information about attending one of these SCaMP meetings, email one of the organizers: Michael Alvarez, Neal Beck, Jeff Gill, Mohan Penubarti, or Jonathan Nagler (email addresses are available on the inside back cover of this issue).

During the last three years, we have learned a number of very important lessons through SCaMP. First, we all have been amazed by the amount and level of exciting research that is going on in Southern California. SCaMP has dramatically increased our communication in our region about political methodology and has clearly made our research better. Second, we have used SCaMP as an excellent means to help professionalize our graduate students. At every SCaMP meeting, the number of graduate students in attendance is at last equal to if not greater than the number of faculty participants. The graduate students have clearly enjoyed the opportunity to meet faculty and students from other campuses in the region and have learned a great deal about political methodology which they probably would not have learned otherwise. Last, through our workshop program we all have learned about new techniques and political methodologies.

SCaMP is even now expanding to include methodologists and their students from Northern California and Arizona. We welcome anyone who wants to participate in SCaMP.

But more importantly, we encourage others to follow our lead and to organize methodological discussion groups in their areas. It just takes a few people, a classroom, and the promise of the possibility of a free lunch to get methodologists and students together for a mutually beneficial experience. By increasing our intellectual interactions, we will produce better political methodology in the future.

* * * * *

Political Methodology Goes to Sundance: A Video Series for Professional Development

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The Society for Political Methodology and the Political Methodology Section of the APSA continue to play an important role in the discipline. While some of the organizational achievements are independent of technology (at

least indirectly) – such as the increasing prominence of the annual methods conference and the return of a quarterly journal – other aspects are fundamentally tied to technological advances – in particular the polmeth listserv and website making software and working drafts of research papers available (url's for all of the websites mentioned in this article are provided at the end). In 1998, an additional contribution was begun with the creation of the Political Methodology Video Series.

The inspiration for the video series was the successful continuing education series of videos produced from short courses of the American Statistical Association (available for rent from the ASA: \$50/ASA members; \$700/non-members). The goal of the Political Methodology Video Series is to make methods information as widely available as possible and provide an additional resource for the further development of scholars and students. In this way individuals who are not able to attend the APSA short courses in person (or those who would like a permanent resource) can still view and gain from the presentations.

In 1998, with funds from the section and a grant from the APSA for organized section innovations, two of the three-hour long methodology short courses at the annual meeting were filmed and made available for purchase. These were the courses on time-series cross-sectional modeling and event history analysis. Thanks to the funding, the tapes from the first two short courses in the series have been made available through APSA Publications for the cost of duplication (\$30/tape +S/H).

Over 50 videos were distributed from the original two courses in the first 8 months. Jerry Goldman and I have recently received a Learning Technology Grant from the Committee on Institutional Cooperation that will allow us to extend the reach of the video series even further. The grant will allow us to produce videos from three of the short courses at this year's meeting and to make them available over the internet for free. The use of streaming video technology has increased greatly in recent years and is becoming very common at websites devoted to news, sports and entertainment. Again, inspiration for this application comes from another academic innovator, the Chance website, which provides video lectures focusing on issues dealing with probability.

By late summer or early fall of 1999, we plan to have the two videos from the 1998 conference digitized and available for viewing through internet connections. Later in the year, the three new entries will be produced and digitized and also made available through the internet. These videos will be produced at a high enough level of quality to make distribution through new Internet2 institutional connections worthwhile as well. Thus, by the end of the year, individuals on Internet2 wired campuses will be able to receive the

new videos through this very high broadcast quality format as well as through the traditional internet with conventional streaming technology.

In addition to general information about the series and the videos, scholars have been able to obtain ancillary materials for the videos through the series website to assist in following the presentations. While the first entries included dozens of pages of useful handout materials, new additions will prove even more useful for scholars. In some cases, links and pointers will be provided for additional papers and software from the presenters to further aid the understanding and implementation of specific estimations discussed in the courses. Thus, the videos and the series provide a combination of fundamental methodological knowledge along with practical solutions for substantive research projects.

The strength of the video series and any ancillaries is, of course, the content of the information. We have been fortunate to have the involvement of some of the top methodologists in the initial entries in the series. The courses provide valuable methodological instruction that will be useful to scholars working in a variety of substantive domains. Information about the videos presently in the series and those that will be produced in Atlanta at the conference this year is provided below. Further information about the series and links to the streamed videos and ancillaries are available at the Political Methodology Video Series website. While not the same as being there, missing the short courses no longer means that one has lost the opportunity provided by these valuable section activities. Through videos and the internet, the Methods Section now provides an additional service to students and scholars interested in extending their knowledge in areas of importance to political scientists.



Videos Filmed at APSA Short Courses in 1998

Taking Time and Space Seriously (Particularly in Comparative Politics and International Relations): A Short Course on Time-Series-Cross-Section Data

Presenter: Nathaniel Beck, University of California, San Diego

This video presentation provides an introduction to time-series-cross-sectional (tscs) data with particular stress on applications to data in comparative politics and international relations. Topics covered include: the advantages of tscs data, the setup of tscs data, estimation with tscs data (OLS, GLS, PCSE's), the dynamics of tscs data, and tscs data with a binary dependent variable. The material is accessible to anyone who has had a course in regression or is familiar with OLS.

Event History Models in American Politics, Comparative Analysis, and International Relations

Presenters: Janet Box-Steffensmeier, Ohio State University and Bradford Jones, University of Arizona

Events-oriented data are prevalent in political analysis. Whether one is examining the onset of military conflict, the termination of a political coalition, or the ending of a legislative career, the issue of timing of an event—that is, when some event occurs—is implicitly important. And while events-oriented data are common in American politics, comparative analysis, and international relations, methods for analyzing events history are less well understood. In this video presentation, an intermediate introduction to event history methods is provided. Topics considered include: 1. Why standard regression models are inadequate in the face of event history data. 2. Nonparametric and parametric estimation of event history models (including a look at the Cox proportional hazard model and the Weibull distribution). 3. Inclusion of time-varying covariates (TVCs) and interpretation of TVCs in the context of event history data. 4. Special problems that arise with event history data (including a consideration of duration dependency, simultaneity of TVCs, and heterogeneity). For this course, no prior knowledge of event history methods is necessary; however, a basic understanding of the classic linear regression model as well as a "conceptual" understanding of maximum likelihood estimation is assumed.

Videos to be Produced from APSA Short Courses in 1999

Interpreting and Presenting Statistical Results

Presenters: Gary King and Michael Tomz, Harvard University, and Jason Wittenberg, MIT

Social scientists rarely take full advantage of the information available in their statistical analyses. The complicated tumble of numbers that pour out of our statistical software are not always in a form close to our substantive concerns, and so we miss many empirical findings as

well as opportunities to present quantities that are of greatest substantive interest for our research. The presenters demonstrate an approach, built on the technique of statistical simulation (which they also describe), to extract currently overlooked information from any statistical method and to interpret and present it in a reader-friendly manner. This technique makes it possible to understand the results of any statistical method, and also to express them for others in a more informative and transparent manner. These techniques are illustrated with replications of several published works, showing in each case how the authors' own conclusions can be expressed more sharply and informatively, and, without changing any data or statistical assumptions, reveal important new information about the research questions at hand. An additional paper and easy-to-use software to implement their suggestions will also be available.

Time Series Models for Event Counts

Presenters: John T. Williams and Patrick Brandt, Indiana University

Political scientists do not have adequate tools to perform time series analysis on event count data. The presenters discuss two models that are useful for modeling time series event counts. The PEWMA model is an exponentially weighted moving average model for persistent time series. The PAR(p), an autoregressive model of order p, is an alternative dynamic specification. The presenters concentrate on models where one wants to include covariates in the analysis. That is, their focus is helpful for those individuals who are doing regressions on time series event counts. They cover how to identify the correct model, estimate and interpret it. Additional papers and the software for these models will be available as well.

What To Do About Missing Data in Political Science

Presenters: Gary King, James Honaker, Anne Joseph and Kenneth Scheve, Harvard University

The presenters introduce a statistically valid, widely applicable, and easy to use, method of analyzing data with missing values. After applying the procedure described, researchers can conveniently implement whatever statistical procedures they would have used if they had no missing values. Using this procedure can substantially reduce bias and inefficiency, and reduce standard errors and confidence intervals. In contrast, all commonly used procedures to cope with missing data in political science are known to be biased and wasteful of information. The presenters begin by introducing the concept of "multiple imputation," a widely applicable approach in theory that has seen almost no use in the social sciences practice since its invention two decades ago. The reason for this discrepancy between theory and practice is that the computational algorithms necessary have

been slow, difficult to implement, impossible to run with existing commercial statistical packages, and demanding of considerable expertise (even experts disagree on how to use them). To avoid these problems, an algorithm is introduced that they developed that is between dozens and hundreds of times faster than the leading method and is very easy to use. They also quantify the risks of current political science missing data practices, show how to use the new procedure, and demonstrate the advantages of the methods through artificial data as well as via replications of published research. The presentation is targeted for individuals with experience with regression analysis or more advanced statistical procedures. An additional paper and software are available to accompany this presentation.

Internet Sites Mentioned in this Article

The Political Methodology Video Series:
<http://psweb.sbs.ohio-state.edu/methods.videos/>

Other Sites

American Political Science Association Publications:
<http://apsanet.org/Publications/>

American Statistical Association Videos:
<http://www.amstat.org/education/coursevideo.html>

Chance Website:
<http://www.dartmouth.edu/~chance/>

Society for Political Methodology:
<http://polmeth.calpoly.edu/>

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Polmeth Annual Report - 1999

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This past year was eventful for the Society's webpage and paper archive. It moved from UC Riverside to its new home at Cal Poly, it was featured in the Chronicle of Higher Education (<http://polmeth.calpoly.edu/45a02901.htm>), and it got a new look. New features include a separate page for computing resources (very important to all of us), a search engine, a new format for viewing abstracts prior to downloading, a brief format for browsing papers, a move to pdf files for easier transmission and storage, more details on the organization and activities of the Section, greatly expanded links to teaching and research resources, and access to methods oriented syllabi. Of course by looking at the traffic on the site (over 1,000 hits per month), all of you probably know this already.

There is currently an ongoing effort to further integrate polmeth with *Political Analysis* and The Political Methodologist. Right now there are: instructions for submission, back issues of TPM, and editorial policies. As *Political Analysis* transforms into an expanded quarterly, more information will be added. We expect to add several new features to the PA and TPM pages in the near future.

There are several areas where we would like to improve. In particular, more software (S-PLUS/R, Gauss, etc.) written by political scientists is needed. Currently the site offers code for bootstrapping, jackknifing, neural networks, data handling, and links to sites like Gary King's and the KEDS project. We could use lots more. If you have developed a software solution to some problem or application that others are likely to see, this is your chance for immortality. Also we currently have 24 syllabi from methods courses of different flavors. I would like to see us dramatically increase this number. Even if you haven't written your syllabi in html, send it to us and we will run it through a translator. However, the best method is for us to link to your site where the syllabus gets updated. Finally, I'm sure there are some high quality data sites out there we've missed. Please send us an email listing your favorites either for research or teaching.

Our continuing focus is on quality of the offerings at the site (papers, conference information, useful links) as well as reliability. Polmeth lives on a server which is manned 24 hours a day, seven days a week, and gets backed up weekly. The dedicated server also provides very good throughput as I hope you've noticed.

And keep those papers coming... we've streamlined the submission process a little bit and hope that as the conference season approaches you use the archive as a way to

get early comments. Naturally all working papers are welcome, not just methodological contributions.

While there are only 683 (900+ on the listserv) members of the Society, many other political and social scientists use the site to get a picture of the latest cutting-edge quantitative research in the field. I hope that as members you feel comfortable offering suggestions, criticisms, and improvements so that we can continue to make this an important and well-traveled site.

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Announcements and Activities

Gosnell and Poster Awards

1999 Political Methodology Poster Award Committee, composed of Adam J. Berinsky of Princeton, Suzanna De Boef of Penn State (Chair), and David Kimball of SIU, gives the poster award to

**Kevin Clarke, University of Michigan,
for "Testing Nonnested Models of the
Democratic Peace,"**

The 1999 Gosnell Prize Committee, composed of Jonathan Katz of U of Chicago, Michael MacKuen of UNC (Chair), and Philip Paolino of Texas, award the Gosnell Prize to

**Nathaniel Beck, University of California,
San Diego, Gary King, Harvard University,
and Langche Zeng, Harvard University (on leave from GWU),
for "Improving Quantitative Studies of International
Conflict: A Conjecture,"**

The Gosnell Prize is given annually for the best work in political methodology presented at any political science conference in the preceding year. The Political Methodology Poster Award is given annually to the best poster given at any political science conference in the preceding year. Both awards are sponsored by the APSA Section on Political Methodology and the Society for Political Methodology.

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Random Utility 2000: Workshop and Conference

Fuqua School of Business, Duke University, August 3-8, 2000.

This is an interdisciplinary and international meeting of interest to political scientists, in particular to young scientists interested in utility theory and/or modeling. NSF sponsored funding is available for junior scholars.

Limited number of participants. Application deadline: 12/1/99. Contact: Michel Regenwetter and Aleksandar Pekec, The Fuqua School of Business, Duke University, e-mail: ru2000@fuqua.duke.edu, www.fuqua.duke.edu/ru2000.

* * * * *

Midwest Political Science Association, 2000 Meeting

Call for Papers: Methodology Section

The central focus for the Methodology section at the 2000 Midwest Political Science Association Meetings is to establish political methodology as a well-developed and productive subfield of the discipline. Therefore, we are particularly interested in papers that highlight areas where political science has made distinctive methodological contributions, rather than merely importing techniques from other fields.

If possible, we intend to organize panels around specific areas of methodological inquiry. These include (but are certainly not limited to): ecological inference; time series; graphical techniques; Bayesian applications; measurement theory; qualitative choice models; scaling methods; simulation; and foundational questions of research design, method, or inference. We also strongly encourage papers that feature contributions in statistical and mathematical computing. And, we particularly welcome proposals for complete panels organized around these or any other relevant themes that fit under the broad heading of political methodology.

Submissions for papers, panels, and discussant assignments should be emailed to one of the co-section heads.

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Program for the 1999 Summer Methodology Meetings

Venue: Texas A&M University, July 15-17, 1999

Thursday, July 15th Morning

Opening Remarks and Conference Coordination:

Gary King, Harvard University and President of the Society for Political Methodology; Charles Johnson, Head of the Department of Political Science, Texas A&M; Woodrow Jones, Dean of the College of Liberal Arts, Texas A&M; Marianne Stewart, National Science Foundation; Dan Wood, Department of Political Science, Texas A&M.

Paper Presentations:

Elisabeth Gerber, Kristin Kanthak, and Rebecca Morton, UCSD and Iowa, "Selection Bias in a Model of Candidate Choice." Discussant: R. Michael Alvarez, Caltech

William Greene, New York University, "Some Practical Issues in the Computation of Econometric Models." Discussant: Mohan Penubarti, UCLA.

Thursday, July 15th Afternoon

Gary King & Langche Zeng, Harvard and GWU, "Logistic Regression in Rare Events Data." Discussant: Philip Schrodtt, University of Kansas.

Curtis Signorino, Rochester, "Between the Unique and the General: The Statistical Implications of Choice and Structure." Discussant: Neal Beck, UCSD.

Poster Session

Friday, July 16th Morning Panel A

Jeff Lewis, Princeton, "Estimating Voter Preference Distributions from Individual-Level Voting Data (with Applications to Split-Ticket Voting)." Discussant: Dean Lacy, Ohio State.

Jasjeet Sekhon, Harvard, "The Economic Sophistication of American Politics: American Public Opinion and Monetary Policy, 1973-1997." Discussant: Chris Achen, Michigan.

Panel B

Janet Box-Steffensmeier & Chris Zorn, Ohio State & Emory, "Modeling Heterogeneity in Event History Analysis." Discussant: John Freeman, U. Minnesota.

Robert Sherman, Caltech, "Tests of the Validity of Complete-Unit Analysis in Surveys Subject to Item Non-response or Attrition." Discussant: Ken Scheve, Harvard.

Joint Session

David Alsobrook, Chief Archivist, George Bush Presidential Library, "Doing Research in a Presidential Library."

Friday, July 16th Afternoon Panel A

Patrick Brandt, Burt Monroe & John Williams, Indiana U. "Time Series Methods for Compositional Data." Discussant: Jonathan Katz, U. Chicago.

Jonathan Wand & Walter Mebane, Cornell. "Testing a Policy Moderating Model of Campaign Contributions Using a Longitudinal Model for Count Data." Discussant: Jim Stimson, U. North Carolina.

Panel B

Brad Palmquist, Vanderbilt. "Models of Proportions Data." Discussant: Charles Franklin, U. Wisconsin.

Melvin Hinich & Hazem Ghobarah, Texas-Austin. "The MAP-B Program with Macro and Micro Applications." Discussant: Michael Herron, Northwestern U.

Saturday, July 17th Morning

Henry Brady & Laurel Elms, UC Berkeley. "Estimating Age-Period-Cohort Effects with Noisy Data and Imprecise Age Categories". Discussant: Jonathan Nagler, Harvard.

Simon Jackman, Stanford. "Averaging Over Relevant Considerations: a Statistical Model of the Survey Response." Discussant: Jeff Gill, California Polytechnic.

Saturday, July 17th Afternoon

Micah Altman & Michael McDonald, Harvard & Vanderbilt. "The Robustness of Statistical Abstractions: A Look 'Under the Hood' of Statistical Models and Statistical Software". Discussant: Vince Wiggins, Stata Corporation.

John Londregan, UCLA. "Deliberation and Voting at the Federal Convention of 1787." Discussant: Nolan McCarty, Columbia.

Section Business Meeting.

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Program for the 1999 APSA Methodology Panels

Venue: Atlanta, September 2-5, 1999

Friday 8:45am - 10:30am

8-1 HETEROGENEITY IN TIME SERIES DATA

Chair: John E. Squier, University of Maryland

Papers: Heterogeneity and Individual Party Identification, Janet M. Box-Steffensmeier, Ohio State University & Renee M. Smith, University of Rochester

The Dynamics of Campaign Contributions in U.S. Congressional Elections, Jonathan Wand, Cornell University & Walter R. Mebane, Jr., Cornell University

Time Series Methods for Compositional Data, John T. Williams, Indiana University & Patrick T. Brandt, Indiana University

Disc: John R. Freeman, University of Minnesota, Minneapolis

Saturday 3:30pm - 5:15pm

8-2 UNDERSTANDING AND TESTING CAUSAL CLAIMS IN POLITICAL SCIENCE: IMPLICATIONS OF RECENT DEVELOPMENTS FROM THE PHILOSOPHY OF SCIENCE

Papers: Causal Inference in Case Studies: From Mill's Methods to Causal Mechanisms, Andrew Bennett, Georgetown University

Simple, Conditional, and Redundant Causation in Social Science Research, Bear F. Braumoeller, University of Illinois, Urbana-Champaign

Probabilistic Processes: Causation in the Social Sciences, Kevin Clarke, University of Michigan

Beyond Holy Wars: Practical Implications of Bayesian Statistics for the Analysis of Political Science Data, Simon Jackman, Stanford University

Disc: Renee M. Smith, University of Rochester

Friday 3:30 - 5:15pm

8-3 LOGIT, PROBIT, HETEROGENEITY, AND HIERARCHY

Chair: Glenn Beamer, University of Virginia

Papers: A Random Coefficients Logit Model for Issue Salience Heterogeneity, Garrett Glasgow, California Institute of Technology

Economic Perceptions in a Heterogeneous Electorate, Jennifer R. Willette, University of California, Riverside

A Panel Probit Analysis of Campaign Contributions and Roll Call Votes, Gregory J. Wawro, Harvard University and Columbia University

Friends and Foes: The Structure of Communication, Cooperation, and Conflict in Policy, Daniel Carpenter, University of Michigan & Kevin M. Esterling, University of Chicago & David Lazer, Harvard University

Disc: Jonathan N. Katz, University of Chicago

Thursday 1:30pm - 3:15pm

8-4 METHODOLOGICAL INNOVATIONS IN INTERNATIONAL RELATIONS

Chair: Richard E. Chard, Georgia State University

Papers: Globalization and the Decline of Power Politics, Mohan Penubarti, University of California, Los Angeles

Modeling Selection Effects in Tests of Game-Theoretic Models, Anne E. Sartori, University of Wisconsin, Madison

Disc: D. Scott Bennett, Pennsylvania State University

Friday 1:30pm - 3:15pm

8-5T MODELING STRATEGIC ACTION

Chair: Susan Moffitt, University of Michigan

Papers: The Signalling Value of Party Campaign Contributions, Wendy K. Tam Cho, University of Illinois at Urbana-Champaign & Brian J. Gaines, University of Illinois, Urbana-Champaign

Learning What We Can't Learn from Roll Call Votes: Strategy, Agenda and Dimension, William Hixon, Michigan State University & Aaron E. Wicks, University of Rochester

Timing in the Legislative Process, J. Tobin Grant, Ohio State University

Disc: R. Michael Alvarez, California Institute of Technology

Friday, 10:45 am - 12:30 pm

8-6 METHODOLOGICAL INNOVATIONS IN COMPARATIVE POLITICS

Chair: Richard Jankowski, SUNY, Fredonia

Papers: The Effect of Autonomy on Development, Irfan Nooruddin, University of Michigan

The Architecture of Analysis: Understanding the Units Problem in Comparative Politics, Evan S. Lieberman, University of California, Berkeley

Extracting Time-Series Information from a Cross-Section of Respondents: An Applied Example with Chinese, Pierre Landry, University of Michigan

Disc: Randolph T. Stevenson, Rice University

Saturday 10:45am - 12:30pm

8-7 MODELS OF MEASUREMENT

Chair: Cristina M. Ling, Georgia State University

Papers: The Implications of Different Operationalizations and Measurements of Constituency Diversity, Girish J. Gulati, University of Virginia

What's Your Temperature? Thermometer Ratings and Political Analysis, Nicholas Winter, University of Michigan & Adam Berinsky, University of Michigan

A Stochastic Model of Attitude Change: Theory and Applications, Mark Berger, Duke University & Michel Regenwetter, Duke University

Disc: Bradley Palmquist, Vanderbilt University

Saturday 8:45am - 10:30am

8-8T CAMPAIGNS

Chair: Andrew J. Padon, Michigan State University

Papers: Campaign Dynamics and Vote Determinants, David A. Peterson, University of Minnesota

Candidate Performance and Voter Learning in Presidential Nomination Campaigns, Philip Paolino, University of Texas, Austin

Coordinating Voting and Moderation in American Midterm House Elections, Walter R. Mebane, Jr., Cornell University & Jasjeet Sekhon, Harvard University

Disc: Larry M. Bartels, Princeton University

Saturday 1:30pm - 3:15pm

8-9T FEMINIST METHODOLOGY IN POLITICAL SCIENCE

Co-sponsored by Women's Caucus, Panel 1

Chair: Mary E. Hawkesworth, Rutgers University

Papers: Gender Regimes and the Study of Globalization, Jane Bayes, California State University, Northridge

Walking in the Fields of Gloves: Comparing Women's Groups across National Boundaries, Naomi Black, York University

Devising Methods to Study Intersectionality, Jane Y. Junn, Rutgers University

Methodology, Gender Power, and the Study of Politics, Rita Mae Kelly, University of Texas, Dallas

Disc: Eloise Buker, Denison University

Thursday 10:45am - 12:30pm

4-1 COMPUTATIONAL MODELS IN POLITICS

Co-sponsored by 8-10

Chair: Kenneth W. Kollman, University of Michigan

Papers: Recovering Behavioralism: Adaptively Rational Strategic Behavior with Endogenous Aspirations, J. Bendor, Stanford University & Michael Ting, Harvard University

The Emergence of Novel Trading Institutions: Interactions between Communalist and Individualist Societies, Ravi Bhavnani, University of Michigan & Susan Lee, University of Wisconsin

Protests, Elections, and Other Forms of Political Contagion, Paul E. Johnson, University of Kansas

Electoral Competition with Endogenous Voter and Party Positions, John E. Jackson, University of Michigan

Disc: Ruth Lane, American University & Anthony J. McGann, Duke University

Thursday 3:30pm - 5:15pm

8-11 CONCEPTS AND THEORY

Co-sponsored by the Committee on Conceptual and Terminological Analysis, Panel 1

Chair: Jennifer Widner, University of Michigan

Papers: Concepts and Comparison in Game Theory, Gerardo Luis Munck, University of Illinois, Urbana-Champaign

Conflicting Imperatives, Concept Formation, and Theory Building, Andrew C. Gould, University of Notre Dame

Theory-Driven Concept Formation: The Challenge of Perverse Cases, Russell Faeges, University of Notre Dame

Disc: Barbara Geddes, University of California, Los Angeles & David Collier, University of California, Berkeley

Friday 3:30 - 5:15pm

21-2T EXPLAINING CONFLICT: COMPARING NEW QUANTITATIVE APPROACHES

Cosponsored by 8-12

Chair: Bruce Bueno de Mesquita, Stanford University

Papers: Explaining Conflict: The Generalized Additive Model, Nathaniel Beck, University of California, San Diego

Logistic Regression In Rare Events Data, Gary King, Harvard University & Langche Zeng, Harvard University

Explaining Conflict: The Quantal Response Equilibrium, Curtis S. Signorino, University of Rochester

Explaining Conflict: Censoring, Selection Bias and Interdependence, Alastair Smith, Yale University

Disc: Michael D. Ward, University of Washington & Paul K. Huth, University of Michigan

Fri 5:30 pm

Business Meeting

Fri 6:30 pm

Reception in Honor of Current and Former Editors of "Political Analysis" on the Journal's 25th anniversary.

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Letter from the President

Gary King
Harvard University
gking@harvard.edu

The Political Methodology Section of the American Political Science Association, and the nearly coincident Society for Political Methodology, have been very active this year, with many new developments you may find of interest. I save for last the most important development, transforming *Political Analysis* from an annual to a quarterly, and including a subscription as part of every member's annual section fee. Many of us believe this development marks an important landmark for our journal and our field.

Summer Meetings

By all accounts, the 16th annual summer meeting of the Society for Political Methodology was a terrific success (thanks in large part to Dan Wood and his crew at Texas A & M who hosted the event). To accommodate our largest attendance thus far, we went to split sessions for one of the three meeting days and stayed in plenary for the other days. Given the expected rate of growth, it looks like we will be able to keep this schedule for at least the next few years.

If you have not attended one of these meetings, I encourage you to apply. Most of those who have participated view these meetings as among their most lively and intellectually engaging academic experiences. Graduate students and faculty interact closely over the continuous formal and informal sessions. We hope you will propose to attend, give a paper, or appear as a discussant. All graduate students are expected to participate in our poster session. Participation and attendance at the conference is by invitation only, but we have expanded invitations substantially in the last few years.

We hold the meetings every summer at a different university. If your university is interested in hosting our

meeting, please contact me soon, as we are presently considering proposals for the next few years.

TPM: The Political Methodologist

Our thanks to Jonathan Nagler, our outgoing editor of the section newsletter. Our new editors are John Londregan and Mohan Penubarti. They both encourage everyone to consider submitting articles to TPM. This is one of the few section newsletters that readers routinely read and keep for reference, so its worth helping support this enterprise. We have arranged so that future issues will be shrink-wrapped with *Political Analysis* and sent out on a regular schedule.

Web site and paper server

The URL for the section's website is:

<http://polmeth.calpoly.edu>

Thanks to Jonathan Nagler, who created and ran the web site from its inception. Our new web-master is Jeff Gill, and he has already produced a brand new site with numerous new features. Don't miss our extremely successful paper server, where you can upload your papers and have them automatically archived with a notice sent to our membership, or download those of others. This is the longest continuously operating server in the discipline, and is widely known even outside of political science (see the 7/17/98 article from the *Chronicle of Higher Education* at <http://polmeth.calpoly.edu/45a02901.htm>). At the web site, you will find information on the paper server, our summer conferences, methodology panels at other political science conferences, our mailing list, TPM, *Political Analysis*, various resources for computing, teaching, and research, organizational information, and other features.

Political Methodology Video Series

Richard Timponi is Director of our new Political Methodology video series. Rich has arranged a generous grant from APSA to cover the costs of videotaping two of our short courses – Time-Series Cross-Sectional Data by Neal Beck and Event History Analysis by Jan Box-Steffensmeier and Brad Jones – and selling the videos to APSA members at cost. For further information, see our Political Methodology Video Web Site at:

<http://psweb.sbs.ohio-state.edu/faculty/rtimponi>

Electronic Mailing List

Our long-standing political methodology mailing list has been institutionalized by moving it to H-Net. Members are now at around 900. Our moderators are Robert Franzese and Brad Jones. To join, see:

<http://polmeth.calpoly.edu/>

Political Analysis

The field of political methodology has become quite a success story. Our summer meetings started out with a dozen participants 15 years ago and now are over 100. The quality and quantity of our scholarship is far superior to what it once was. Our members now have representation on the editorial board of every journal in the discipline that would be relevant to our work. Methodologists now hold jobs at virtually every research university and college in the country, and our courses are taught to every graduate student and undergraduate. The problem is a lack of publication outlets for those creating methods, adapting methods from other disciplines, doing highly sophisticated empirical analyses, and advancing related developments in formal theory. Our hard-working publications committee made the following recommendations, and after a good discussion an unusually well-attended business meeting unanimously approved our plan.

The plan is to expand *Political Analysis* to a more prestigious, visible, and aggressive quarterly journal, and to increase section dues to cover sending a print subscription to all our members.

After extensive negotiations with several presses competing for our business, I recently signed a contract for the section with Westview Press, now a subsidiary of Perseus books (they own Basic Books, among others). The main features of this contract ensure the following:

1. *Political Analysis* will appear on a very regular schedule every quarter, with the first issue due Fall 1999. Submissions have already begun, and our new editor, Neal Beck, encourages you to contact him if you have any questions.
2. From final submission of the papers in an issue from our editor to publication and mailing will be only 4 months at the start and 3 months after an issue or two. This means that the turnaround time for authors from acceptance to publication will be very fast.
3. The Section will pay \$15 per member per year to have 4 quarterly issues of *Political Analysis* mailed to them.

Section dues will therefore increase to \$25 when the journal starts publishing. In addition, any graduate student who feels they cannot afford the increase is encouraged write a letter to our Secretary-Treasurer and will receive a \$15 refund, hence making our dues the same as most other sections. This is an outstanding deal for individuals receiving a journal (far less than other proposals we've received, and less than even the student rate at most existing journals). It is also a terrific deal for the field, since we will have a major journal instantly, with the prestige of the section membership behind it.

4. There will also be economies of scale in that Westview will include TPM in the *Political Analysis* mailing and will charge us only the marginal increase. TPM will thus appear more regularly and the Section will benefit a bit financially.

5. Westview will mount a large advertising campaign to announce the renewal of the journal — now in its 25th year (if you include the current *Political Analysis* and its predecessor, *Political Methodology*). The advertising will also help us keep the number of section members high given the dues increase, and will help increase submissions as those publishing in the journal will receive a lot of additional visibility.

6. Westview will take *Political Analysis* and market it aggressively to libraries, get it in catalogues and index services, etc. It will generally raise the visibility of the journal and those publishing in it.

7. The Section retains the copyright to all contents, and authors retain the rights to include their articles in subsequent book projects, etc. Straight reprinting would follow the usual rules.

8. The Section chooses the editor and editorial board and in this way controls all content in the journal.

9. Westview will consult with us on all issues of design and produce a high-quality print journal. The editor will have a veto over any objectionable advertising.

10. Westview will do all copyediting, typesetting, preparation of camera-ready copy, etc., which we now do. This will free up our editor to spend more time on content, which is of course our expertise. The press will provide the editor a stipend and editorial assistant. They will give the section a small royalty as well.

11. Westview will produce a web copy of the journal and provide members free access. They will also take additional material so that we can create a value-added web addition of *Political Analysis*, including such items as replication data sets, special programs, or even interactive products (such as allowing users to choose which variables to include in a specification, or to create predictions dynamically given

specified inputs). This type of development will obviously play an important role as the world moves from print to electronic formats.

12. Upon termination of the agreement by either side, *Political Analysis* becomes the property of the Section. So there is little risk for us if we decide not to proceed.

In helping to make this happen, the field owes a debt to the able staff of the APSA who helped advise us on a variety of legal and administrative matters, and to Westview Press for their flexibility, expertise, and professionalism.

Political Methodology Officers, Editors, Committee Memberships

One of my primary goals for the Political Methodology Section has been to open our decision making processes, our meetings, our journal, and all of our other activities, to everyone interested in the intellectual developments in this vibrant field. One way of accomplishing this has been to formalize some of our informal decision practices by appointing committees, publicizing their membership, and encouraging everyone to consult widely. If you have any suggestions for the section or related activities, please contact me or the relevant officers or committee members, all of whom are listed on the inside back cover of this issue.

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The Society for Political Methodology

Officers and Committee Memberships

President: Gary King, *Harvard University*

Vice President: Nathaniel Beck, *University of California, San Diego*

Secretary-Treasurer: Philip Schrod, *University of Kansas*

Member at large: Janet Box-Steffensmeier, *Ohio State University*

Editor, *Political Analysis*: Nathaniel Beck, *University of California, San Diego*

Editors, *The Political Methodologist*: John Londregan, *UCLA*
Mohan Penubarti, *UCLA*

Political Methodology Webmaster: Jeff Gill, *California Polytechnic State University*

Political Methodology Listserv Moderators: Robert Franzese, *University of Michigan*
Bradford S. Jones, *University of Arizona*

Director, Political Methodology Video Series: Richard Timpone, *Ohio State University*

Principal Investigator, NSF Grant Renewal: Charles H. Franklin, *University of Wisconsin*

APSA Political Methodology Section Organizer: 1999: Nancy Burns, *University of Michigan*
2000: John Williams, *Indiana University*

Publications Committee: Chris Achen, *University of Michigan*
Michael Alvarez, *California Institute of Technology*
Nathaniel Beck, *University of California, San Diego*
Janet Box-Steffensmeier, *Ohio State University*
Gary King, *Harvard University*
John Londregan, *UCLA*
Walter Mebane, *Cornell University*
Ken Meier, *Texas A&M University*
Jonathan Nagler, *University of California, Riverside*
Mohan Penubarti, *UCLA*

1999 Gosnell Prize Committee: Jonathan Katz, *University of Chicago*
Michael MacKuen, *University of Missouri – St. Louis*
Philip Paolino, *University of Texas*

1999 Political Methodology Poster Award Committee: Adam J. Berinsky, *University of Michigan*
Suzanna De Boef, *Pennsylvania State University*
David Kimball, *Southern Illinois University*

1999 Annual Meeting Selection Committee: Mohan Penubarti, *UCLA*
Curtis Signorino, *University of Rochester*
Langche Zeng, *George Washington University*

Committee on Site Selection and Meeting Format: Charles H. Franklin, *University of Wisconsin*
Gary King, *Harvard University*
John Williams, *Indiana University*

The Political Methodologist

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Subscriptions to *TPM* are free to members of the APSA's Methodology Section. Please contact APSA (212 483-2512) to join the section.

Submissions to *TPM* are welcome. Articles should be sent to the editors by e-mail (jbl@ucla.edu or mohan@ucla.edu) if possible. Alternatively, submissions can be made on diskette as plain ascii files sent to John Londregan or Mohan Penubarti, Department of Political Science, UCLA, Los Angeles CA 90095-1472. \LaTeX format files are especially encouraged. See the *TPM* web-site [<http://polmeth.calpoly.edu>] for the latest information.

TPM was produced using \LaTeX on a SPARCstation running Solaris.