The Political Methodologist

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Contents

Notes From the Editors

Welcome to the latest issue of *The Political Methodologist*. You might have noticed that *TPM* has moved north, relocating to the Land of Lincoln (a.k.a. the Land of Blagojevich). Thanks, again, to the Texas-Iowa team for their stewardship. We hope to continue the long tradition of providing a high quality newsletter that provides useful information for the members of the political methodology community.

Volume 18 opens with a column from our president that takes an empirical look at our community's interests outside of methodology. This article is followed by a contribution by Neal Beck that discusses how to make regression and related output more user friendly. Following Neal's article are five long abstracts that derive from the graduate student presentations at SLAMM in April. They are coming attractions for interesting articles bound to appear sometime soon in a journal near you. In the announcements section, we review this year's methodology prize winners. Finally, we close with notes from our section president, Jeff Gill.

Thanks to all the contributors to this issue. Please contact the editors with any suggestions or ideas for future issues of *TPM*. Enjoy!

The Editors

Articles

President's Corner: What Do We Care About Besides What We Care About?

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What other subfields interest methodologists? Contrary to the stereotype as narrow-minded bean counters methodologists are deeply and actively involved in substantive questions in the field. This is evidenced by important contributions to empirical political science across many areas. Consider, for instance, applications appearing in *Political Analysis* for Volume 17, 2009: *the third year in a row that it was the top ranked journal in political science by ISI Journal Citation Reports.* The data/questions are drawn from issues in:

- ▷ state-level spending policy
- ▷ bureaucratic policy-making
- ▷ comparing nationalistic attitudes across countries
- ▷ measuring ideology in Congress
- ▷ connections between the economy and public opinion
- ▷ taxation policy and support for public schooling
- ▷ foreign policy attitudes
- ▷ polarization politics
- ▷ political structure and monetary policy
- ▷ genetic influence on political preferences
- ▷ measuring congressional district ideology
- ▷ trade protection as a function of political organization
- ▷ ideal point estimation in Congress
- ▷ ideal point estimation in small chambers
- ▷ estimating latent associations between senators
- ▷ new political parties in developing countries
- ▷ predicting presidential elections
- ▷ apportionment cycles
- ▷ media effects in authoritarian regimes
- ▷ party choice in American elections
- ▷ coattail effects of congressional candidates
- ▷ measuring voter turnout due to polling consolidation.

This hardly sounds like list of topics from a statistics journal. Statisticians actually tend to re-use standard datasets as a way to make a point about some new tool relative to previous treatment of such data. Furthermore the above listing is complete save for only two articles that did not use any real data: King and Zeng ("Empirical versus Theoretical Claims about Extreme Counterfactuals: A Response", Issue 1 (Winter), 107-112), and Robinson, McNulty, and Krasno ("Observing the Counterfactual? The Search for Political (Experiments in Nature" Issue 4 (Autumn), 341-357). So despite making notable technical contributions, *political* methodologists have not lost interest in core *political* questions.

Since I have the same preference for evidence in the form of actual data that nearly all readers of *The Political Methdologist* possess, let's turn to some small analysis. I recently obtained data from Sean Twombly at the American Political Science Association on cross-membership of sections. Specifically, I wondered what other organized sections contain members of the APSA Section on Political Methodology? Since dues are involved this clearly constitutes voting with one's wallet. Figure 1 provides an ordered summary of which of these sections our members also belong to. There is a normative implication in the last sentence that each of these members sees Section 10 as their primary interest. Of course, I see nothing wrong with this assumption.

Some of these totals are unsurprising. Notice that the Elections, Public Opinion, and Voting Behavior Section is easily the largest. This makes complete sense given the historical foundings of the section. Comparative politics is also a large interest for methodologists. Given the broad omnibus definition of comparative politics in political science, this is also a predictable result. It does, however, contradict a popular misconception that political methodology and comparative politics are at different extremes of the epistemological scale in the discipline. Some observers may also be surprised that 15% of our members are also members of the Qualitative Methods section (not shown is the 1017 total for our section). This also suggests that the so-called divide between the two approaches is over-stated. I am personally amused and heartened by the 11 members who choose also to be members of the Section on Politics and Literature. We see from the figure that within political science, methodologists are involved in a wide range of other areas and are certainly valuable coauthors to scholars in these endeavors.

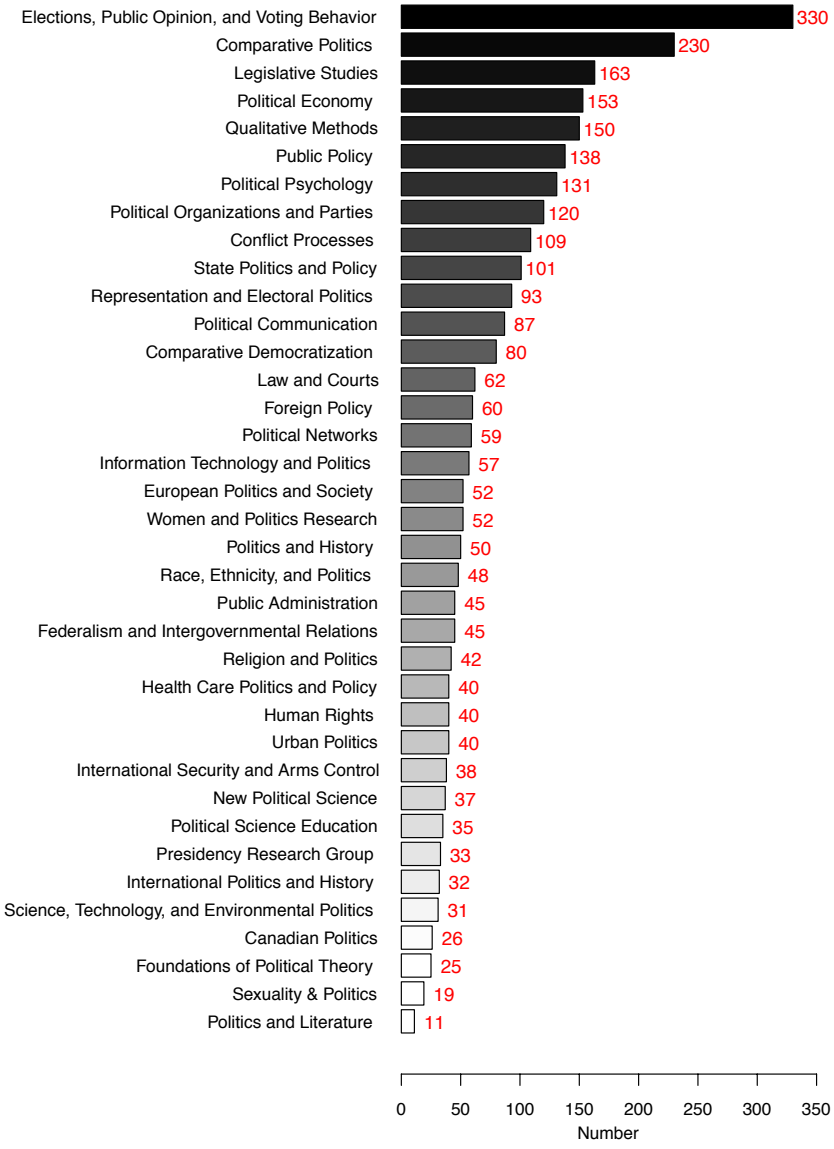

FIGURE 1: WHICH OTHER SECTIONS CONTAIN METHODOLOGY SECTION MEMBERS?

As the late, great Arthur Schiff said, **But wait, there's more!** Note that these categories in Figure 1 are only the APSA-official definitions of other state-approved interests. Many political methodologists retain strong substantive interests in other disciplines.

The dividing lines between traditional academic disciplines have never been more blurred, and analytical methodologies are especially compelling travelers across these lines. In particular the work of political methodologists has resonated in biomedical and public health research. A big part of this is the current interest in causal models by many methodologists, but another part is that observational health studies contain some of the exact challenges that empirical political scientists have had to deal with: reluctant or deceptive subjects, measurement challenges, panel attrition, social interaction effects, instutitional factors, unseen confounding or latent variables, and so on. So some of our colleagues have been publishing in mainstream medical or health journals, such as Jas Sekhon's 2008 paper in *Health Services Research* (43:4, 1204-1222) "Evaluating Health Care Programs by Combining Cost with Quality of Life Measures: A Case Study Comparing Capitation and Fee for Service," Kosuke Imai's 2008 paper in *Statistics in Medicine* (27:24, 4857-4873) "Variance Identification and Efficiency Analysis in Randomized Experiments under the Matched-Pair Design," or Jan Box-Steffensmeier, and Suzie Linn's (De Boef) 2006 paper also in *Statistics in Medicine* (25,20, 3518-3533) "Repeated Events Survival Models: The

Conditional Frailty Model." It is clear that our tools and our contributions are appreciated by many outside of the discipline with methodologists making contributions other fields besides health/medicine, including: sociology, epidemiology, pure statistics, criminology, finance, demographics, economics, data processing, law, linguistics, and so on. This demonstrates that the subfield has reached a new level of scientific maturity wherein other fields are interested in importing our tools. Notice that this is the opposite effect than that described by Neal Beck only ten years ago in Political Methodology–A Welcoming Discipline (*Journal of the American Statistical Association*, Volume 95 (2000), pp. 651-654). We appear to have gone from net importers to net exporters, which is an exciting development.

Making Regression and Related Output More Helpful to Users[1]

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All of us interact every day with computer screens showing tables of regression and related output.[1] We do this so often that our eyes have perhaps gotten used to ignoring the useless output that appears on those screens, though all those extraneous numbers we constantly see surely cannot improve analysis. Too many statistics make it harder for our eyes (and brains) to focus on the important ones. Worse, for students newly encountering regression, this extraneous output can often be misleading. Do we really want to teach our students that every regression coefficient should be tested against the null hypothesis that it is zero? We know enough to do much better, and it is easy to do better. This diatribe is an effort to push us in this direction.

Before beginning let me be clear that I am discussing output seen by the user on a computer screen. Clearly we can write an article or paper picking output as we choose, but, as we shall see, we often have little control over the standard output we see on a screen.[2] It is screen output that concerns me since it is screen output with which we normally interact. The issue is also of concern for students newly coming to regression: if Stata or R[3] produces some output surely it must be important. (Even worse, I as the instructor have forced them to spend effort learning these things, and now I say that much of it is useless!) It should be stressed that fault does not lie with the programmers of Stata or R; they both produce excellent software that does what users want. The problem is with *our* not demanding more useful output, and *our* continuing to perpetuate the mistakes of the past, both in our practice and teaching. And we know that what we do now is not quite right. Bastante![4]

Figure 1 shows what a user sees on the screen after typing a regression command in Stata; the output is for a generic model of votes for House candidates in the US (Jacobson et al., 1994). This output is not customizable by the normal user. While Stata is very commonly used in our discipline, maybe "higher end" packages like R do better? Figure 2, which shows standard regression output using the *summary()* method, disabuses us of that notion. Here, the only possible customization is to allow for "magic stars" indicating significance, hardly a useful customization.[5] Table 1 shows what I think standard output should look like. The alert reader will note that the change is non-trivial. The rest of the article discusses these differences in more detail. There are many good discussions of these issues from a statistical perspective; here I simply reference a few of those discussions. If you have not been convinced by previous articles that not every regression coefficient need come with a test of a null hypothesis that nothing is going on, or that R^2 is comforting but not very useful, this piece is not going to convince you and you can stop reading now. Know,

[1]Thanks to too many friends who do political methodology to name and two friends who realized how awful I am at coding. I also want to acknowledge the kind hospitality of CEACS at the Fundación Juan March in Madrid for allowing me to present an informal seminar which started me down this path.

[1]All said here generalizes to more complicated regression-like output produced by standard maximum likelihood routines.

[2]Obviously there have been numerous previous attempts to make points similar to those made here, but directed towards how results are presented in journals. Gelman, Pasarica, and Dodhia (2002) and Kastellec and Leoni (2007) make an excellent argument for replacing all regression tables with graphs in journal articles. Here I am concerned with the computer screens we look at well before writing the journal article or senior thesis. I would be most happy if journals adopted the perspective of Gelman and Kastellec and Leoni; I would be quite happy if journals simply adopted some of the points below. But this diatribe is only indirectly aimed a journals.

[3]I discuss these two packages because they are most commonly used in political science, and they are also the most sophisticated of the general packages.

[4]Gigerenzer (2004, 604) concludes his article on the foolishness of null hypothesis testing equally strongly. "To stop the ritual [of null hypothesis testing], we also need more guts and nerves. We need some pounds of courage to cease playing along in this embarrassing game. This may cause friction with editors and colleagues, but it will in the end help them to enter the dawn of statistical thinking."

[5]Gelman and Hill's (2007) R package, *arm* provides the *display()* method, which is superior to *summary()* in that it eliminates significance tests and some unnecessary output and limits the number of decimal place (not significant digits!) to two. Moreover, it does not provide confidence (or highest posterior density) intervals. But *display()* is far superior to *summary* though not as good as Jeff Gill's *graph.summary()* mentioned below.

however, that you have decided to play along in Gigerenzer's embarrassing game.[6] If one does not wish to continue playing the game, Stata and R output routines (kindly written by programmers more competent than me) are available. The R program is available on Jeff Gill's website (http://artsci.wustl.edu/~jgill/Models/graph.summary.s; the Stata program can be installed by typing *ssc install leanout*.

Source	SS	df	MS		
Model	12444.3156	4	3111.0789	Number of obs =	285
Residual	9808.7793	280	35.0313547	F(4, 280) =	88.81
Total	22253.0949	284	78.355968	Prob > F =	0.0000
				R-squared =	0.5592
				Adj R-squared =	0.5529
				Root MSE =	5.9187

Chal_Vote	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
Prior_Office	1.090079	.9347121	1.17	0.245	-.7498763	2.930034
Chal_Spend	2.678987	.2779197	9.64	0.000	2.13191	3.226064
Inc_Spend	.8178706	.6055194	1.35	0.178	-.3740776	2.009819
Pres_Vote	.3731286	.0377052	9.90	0.000	.298907	.4473501
_cons	3.191908	3.661175	0.87	0.384	-4.015014	10.39883

FIGURE 1: STANDARD STATA REGRESSION OUTPUT: REGRESSION OF VOTE FOR HOUSE CHALLENGER, 1992

```
Residuals:
      Min        1Q    Median        3Q       Max
-19.22607  -4.03022   0.04261   4.03308  17.22183

Coefficients:
             Estimate Std. Error t value Pr(>|t|)
(Intercept)   3.19191    3.66117   0.872    0.384
Prior_Office  1.09008    0.93471   1.166    0.245
Chal_Spend    2.67899    0.27792   9.639   <2e-16
Inc_Spend     0.81787    0.60552   1.351    0.178
Pres_Vote     0.37313    0.03771   9.896   <2e-16

Residual standard error: 5.919 on 280 degrees of freedom
Multiple R-squared: 0.5592, Adjusted R-squared: 0.5529
F-statistic: 88.81 on 4 and 280 DF,  p-value: < 2.2e-16
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FIGURE 2: STANDARD R OUTPUT: REGRESSION OF VOTE FOR HOUSE CHALLENGER

Variable	$\hat{\beta}$	SE	95% CI
Prior Office	1.1	.9	(-.8 & 2.9)
Chal Spend	2.7	.3	(2.1 & 3.2)
Inc Spend	.8	.6	(-.4 & 2.0)
Pres Vote	.4	.04	(.3 & 0.4)
Constant	3.2	3.7	(-4.0 & 10.)
$\hat{\sigma} = 5.9$			
Number of observations: 285			

TABLE 1: WHAT OUTPUT SHOULD LOOK LIKE: REGRESSION OF VOTE FOR HOUSE CHALLENGER

[6] One might decide to play that game for final publication given the power of editors and referees, but surely this game does not need to be played on every screen of regression output. Or one might decide that the only way to change things is to change editors and referees.

Significant and decimal digits

Let me start with something simple. I hope that none of us[7] believe that our regression estimates are accurate to seven significant digits. But this is how many digits Stata outputs, and there is no option to change this.[8] Ah the high priests say, Stata is just for the untutored, let them use R and all problems will be solved. But while R has a *digits()* option, which is a great idea, the output in R, as produced by the standard *summary.lm()* command, is, as written, not capable of producing fewer than 3 decimal places (and will produce as many integer digits as the regression indicates). So we all look at perhaps seven digits for every number we see, we all know this is ridiculous, and we all continue to allow this to happen. And this is for only the simplest issue, where there can be no cogent argument for what is, by default, current practice.

Computer programmers strive for enormous numerical accuracy but, alas, social science data are not quite so accurate. When I feel optimistic I might believe our results are accurate to two digits. We see fewer and fewer articles and presentations which simply take the digits reported by whatever computer package. But even if some journals can enforce a more reasonable number of digits, the number of digits seen on a screen simply confuses the eye, and does so for no good purpose. And surely there is more danger of the student being misled about the accuracy of regression results.

Now, alas, the number of significant digits and the number of digits reported is not the same thing; does 0.00034 have two, five or six significant digits? The analyst should know, but a computer program cannot. Journals (including the one I edited) often limit regression coefficients to two decimal places, but allow coefficients like 9462273.24. There is no easy way around this, and it is not obvious that users would understand that 9462273.24 should probably be reported as 9500000 (or perhaps 9000000).[9]

The issue is problematic since we (in general) are not really good at comprehending really small or really big numbers. Physicists and astronomers, after all, have decided they need both Ångstrom units and parsecs. Thus analysts should strive to have coefficients that are easy to understand, that is, a relatively small integer followed by perhaps a single decimal place. This is particularly helpful in regression, where the coefficient tells us the "impact" (whether causal or not) of a unit change in a variable. If that unit is too small (say measuring household income in dollars), the coefficient will be minuscule and hard to interpret; a simple rescaling to income in thousands of dollars solves many problems at once. Similarly, we would not want household income measured in millions of dollars; the counterfactual of a one million dollar increase in my income is truly a counterfactual, and the regression coefficient is going to be misleadingly large. Researchers should also try for meaningful scalings, and attempting to get reasonably sized (small) regression coefficients is one good way to try to enforce this. There is no perfect solution; we do not want income measured in hundreds of dollars, or area measured in hectohectares.[10]

Thus, for all these reasons, regression program should (at least by default) output numbers as n.d or nn. If coefficients do not fit into this scheme, it should normally be easy enough (and good) to rescale that variable (or, if relevant) the dependent variable. This should be the easy part of the argument, though practice shows this might not be as easy as I would hope. I looked at the 10 most recent quantitative articles in what should be our most sophisticated methodological journals, *The American Journal of Political Science* and *Political Analysis*. In both journals, the typical number of decimal places reported was three, with an additional units place, leading readers to believe that authors felt their results were accurate to four significant digits. While this piece deals only with output on computer screens, clearly we also need to worry about how that output appears in published articles. But that is a different task.

No irrelevant ancillary statistics

Stata and R (and all other statistical packages that I know of) show a number of ancillary statistics and tests that are of little or no interest, and certainly not of such interest that they should appear on every screen of output. (Where they are are of interest it is easy produce them after estimation.) These statistics and tests mislead many students into being overly excited when they should not be (and vice versa) and they may lead even experienced analysts into not focussing on what is really important.

The most egregious output is the F-test of the null

[7] In this diatribe "us" is some combination of readers of TPM and the larger number of users of quantitative methods in our discipline. Apologies to (the small number of) the innocent.

[8] On June 4, 2010, while this piece was in production, Stata 11.1 was released. This version allows the user to format how statistics appear. It is now trivial to limit output to two decimal places (with no attention to significant digits), or to force, as I would prefer, all output to appear as n.d or nn.d. I would urge all Stata users to upgrade to 11.1 and use the command "set cformat %3.1f, perm" in their profile. Clarity would also be improved by setting pformat and sformat to %1.0.

[9] Scientific notation can solve this problem. In some sense it solves the problem too well, by providing sensible mantissas, though at the cost of somewhat hard to comprehend characteristics. By solving the problem of significant digits too well, scientific notation provides no incentive for meaningfully rescaling the data.

[10] Thus, sometimes it might make most sense to report a coefficient of 0.032. No hard and fast rule is going to work here. But any system similar to the metric system allows us to get good scaling within a range of at most 3 digits (decimal plus integer).

hypothesis that all coefficients are zero, and the associated ANOVA table. This must be the least interesting null hypothesis in the world; why this is standard is beyond me. Even more puzzling, why do I always want to see the ANOVA table which is the basis for this test? I do not think it controversial to advocate dropping these items from standard screen output.

It is more controversial, but correct, to drop R^2 from the output (King, 1986, 675-8).This is a meaningless, unitless number that is supposed to give us comfort if it is close to one (how close?). There is no reason for a student to believe that a high R^2 is good or that a low R^2 is bad; we surely do not want students just adding variables to build up that R^2. Do we think that a regression which includes an independent variable that is almost identical to the dependent variable is superior to a regression without such a variable? And if R^2 is useless, so is the adjusted R^2; for any decent size data set the adjustment is trivial. (If one wanted some arbitrary number, the BIC would be better, but I am not advocating any numbers that are not directly of interest here.) So R^2-related statistics (and their maximum likelihood wannabes) should also disappear from standard screen output.

This leaves the number of observations and the estimate of σ, the standard deviation of the distribution which theoretically has generated the errors. N is useful because analysts so often compare regressions with different numbers of observations (whether due to missing data or something else). Such a comparison is difficult, at best, and researchers should always know how large their "sample" is. For time-series and time-series–cross-sectional data, programs should report the "sample period" (in meaningful dates) and, in the latter case, the number of units as well as the overall N.

Why $\hat{\sigma}$? This is a very nice interpretable number, a number which has the same units as the dependent variable. It tells the analyst how far a typical observation is from the regression line. This, unlike R^2, is an intuitive and meaningful number; if a dependent variable is GDP per capita in thousands of 2005 US dollars, and if the standard error or estimate is 10 (thousand US dollars), we know that a typical country is within about \$10,000 of the regression line (which may be good or bad).[11]

The regression table

Turning to the regression coefficients, clearly we need the coefficient (with fewer than 7 digits) and its standard error and its 95% confidence interval.[12] But why does every coefficient estimate we ever see come with an associated t-test of one specific null hypothesis (H_0:$\beta_k = 0$) and the associated p-value. As Gigerenzer (2004) and Gill (1999) and many others, have persuasively argued, hypothesis testing is a deeply flawed activity. But we need not even go this far to note that there is no reason *always* to look at the t-statistic and p-value for one specific null hypothesis test that the true value of a parameter might be zero. If we care about that hypothesis then we can simply check whether zero is contained in the reported confidence interval.

Alas, students (and others) misunderstand the meaning of significance tests. They often think that a failure to reject the null hypothesis means that they have shown that a parameter value is zero (or even small); they think that lower p-values indicate that a parameter is more important. And how many scholars, thumbing through a huge list of coefficient estimates, ignore those that have $p > .05$ and focus on the others, sorting estimates into significant and insignificant. We all know this is not the right practice, and it should not be aided and abetted by our computer programs. We should not be interested in simply whether a coefficient is "significant." We go to great trouble to estimate coefficients in units that give a huge amount of information; simply looking at the unit-less t or p just discards that information.

Focussing on the one simple test of the null that $\beta = 0$ also misleads students into not thinking about the hypothesis of interest. Sometimes we are interested in a series of coefficients, sometimes we are interested in the equality of coefficients, sometimes we care if they are near one, and so on. Current regression output makes it appear that the thing we naturally care about is one specific null. So whatever one thinks about hypothesis testing logic, current regression output is highly misleading.[13]

[11] The same examination of recent regression tables in the two journals indicates that everyone seems to believe that R^2 (or its pseudo-friends) is important, as is the likelihood. No one seems to believe that $\hat{\sigma}$ is worth reporting.

[12] 95% is about as good as any other choice. In some recent articles using Bayesian methods, authors have reported 80% "highest posterior density" (i.e. confidence) intervals. Why those who use Bayesian computations are happy with 4:1 odds while others are used to 19:1 odds is, at best, unclear, though of course the 80% intervals are comfortingly smaller.

[13] Alas, as previous, the output in our top journals indicates that no one finds confidence intervals of enough value to report and lots of results get labeled with magic stars, with the magic star always related to a test of whether some true parameter value might be zero. As Gerber and Malhotra (2008) clearly show, our journals seem to lack results which correspond to p-values just above .05. Gerber and Malhotra focus on publication bias; my interpretation of their results is that anyone clever enough to do a regression, and employed in a profession that values publication, upon seeing a key result with a p slightly above .05, will have no trouble finding a new specification with a p happily just below .05. So all this focus on stars and p-values simply leads to p-values which in fact are not p-values at all. Looking at the Gerber and Malhotra results, we can be quite sure that, even if we believe in the null hypothesis testing paradigm, our own work must be violating that paradigm.

Interpretative Bayesianism/Subjectivism

So now we see only useful output. If the coefficients themselves are of interest (as in regression), it is hoped that analysts will focus on those, looking at the numbers in terms of units, not simply asking if the estimate is "significant." For more complicated models, clearly other quantities of interest (and the uncertainty associated with those) must be estimated (King, Tomz and Wittenberg, 2000). But what is critical is that analysts and students not undo all their good work by basically running a hypothesis test in their head, that is, simply seeing which confidence intervals contain zero (in which case magic stars will do the same thing more efficiently). So how can better use be made of the uncertainty estimates?

Confidence intervals are difficult for classical (frequentist) statisticians to interpret. Few if any students remember the correct frequentist interpretation of a confidence interval five minutes after the final exam in their first course (if they ever knew it). Most people I know interpret a confidence interval as "it is likely that the parameter value lies in the interval." Such a statement makes sense only to someone who believes in subjective probability (Savage, 1954), where the probability of a statement being true is given by the odds you would be willing to give on a bet that the statement is true. So there are no frequentists in fox-holes. But can we use classical frequentist methods and then interpret results like a Bayesian?

Subjective probability developed independently of Bayesian inference. As Fienberg (2006, 16) notes, Savage's book mentions Bayes only once. But clearly subjective probability and Bayesian inference are now joined more closely than that.

Fortunately (or not!) most (not all, but most) Bayesian analyses done in political science are not really Bayesian, in that they use a highly uninformative prior (and I have yet to see a second study use the first to update said prior). Thus most Bayesians in political science are what I would call computational Bayesians, that is, they take advantage of the great power of Bayesian computational methods to produce results for very complicated models where standard classical methods fail. But, for simple things like regression (and simple maximum likelihood like logit and probit), for a reasonable sized sample (say at least 50) and a highly uninformative prior, the numerical results from a Bayesian and classical analysis are essentially the same (remembering how many significant digits we really have).[14] Thus one can take the 95% confidence interval computed classically and say that one would offer a bet at 19:1 odds that the parameter value lies in this range. This is simply a formalization of how almost all of us interpret confidence intervals in practice. Thus we can use the nice output to say that we are pretty sure that the true parameter is at least so big and and no bigger than something else. This seems like the most useful way to summarize what the data is saying about the parameters and their associated uncertainty.

Ten Commandments

1. Produce screen (and journal) output that is as meaningful to the analyst (and reader) as possible.

2. Make your output as easy to read as possible. In particular, variables should have meaningful names that relate to the underlying concepts.

3. Produce no more digits than are significant. If unsure, two is a generous guess.

4. Produce numbers that the human brain can easily process (typically between .1 and 9.9).

5. Choose units for your variables that make interpretation simpler.

6. Report all interesting numbers in meaningful units.

7. Do not provide uninteresting summary statistics; if they are really needed, they can be produced later. Provide interesting summary measures (such as $\hat{\sigma}$) that have units.

8. Provide only parameter estimates and indications of uncertainty of those estimates. This will usually be done via standard errors and confidence intervals.

9. Do not routinely produce tests of standard null hypotheses that a parameter is zero. Do not use stars or other markers to denote levels of significance.

10. Break any rules that conflict with the first.

For those who bristle at commandments, all of the items above can be rephrased as promises, with first-person pronouns. My goal is to get the data to speak as clearly as possible, particularly to students. So I conclude with some vows. I will no longer teach what I know to be nonsense, and no longer participate in nonsensical statistical rituals to please reviewers and editors. I will implement best practices, and endeavor to have my tools enhance those practices.

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[14] This is not to deny that Bayesian computations are superior for hard problems, even with uninformative priors. Nor would I deny that Bayesian methods provide superior results for more complicated models. But even in those cases, one can still interpret the classical confidence intervals as a subjectivist would, conditional on the somewhat inferior classical model.

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SLAMM Abstracts

The 2010 St. Louis Area Methods Meeting (SLAMM) was held April 16 and 17 at Washington University, co-hosted by the Center for Applied Statistics and the Department of Political Science. The second day of the conference was reserved for graduate student presentations. The editors invited students who made presentations at the meeting to submit long abstracts of their work.

Are We Testing What We Think We're Testing? A Theoretical Evaluation of Methods for Testing Hypotheses about Temporal Changepoints

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Applied time series analysis is frequently used to study questions of great importance in political science. For example, scholars may be interested in patterns of democratization, changes in Supreme Court voting behavior, or the determinants of civil conflict. In this research we often make the implicit assumption that the relationship of interest is static across all subsets of the time series. While it is possible that this assumption holds in some instances, without sound theory it is important that this assumption be explicitly addressed and tested. Yet, determining how to properly conduct these tests is a more complex question.

In this paper, I advocate the use of Bayesian multiple changepoint models to test for potential structural breaks in time series data. In doing so, I focus primarily on the theoretical congruence (or the lack thereof) between the nature of the question tested and the methodological approach used. Certain approaches commonly used for testing for structural breaks (e.g. Chow tests) require the researcher to specify the potential changepoints *a priori*. Further, many of these tests are limited to the detection of a single structural break. Bayesian multiple changepoint models provide a theoretically more appropriate alternative to the commonly used techniques for dealing with changepoint problems by allowing the changepoints to be estimated as a parameter simultaneously with the other parameters. Moreover, Bayesian changepoint models allow for the estimation of the number and location of this structural breaks without having to specify their values *a priori*.

To illustrate the application of this approach, I present a substantive example from an analysis of the determinants of judicial decision making when reviewing administrative agency decision making. *Chevron USA, Inc. v. Natural Resources Defense Council* is one of the most widely cited decisions in the history of the U.S. Supreme Court, and many scholars claim that it completely reshaped administrative law. In essence, the *Chevron* decision held that courts were to defer to agency interpretation of statutes

when reviewing agency rule making decisions. Yet, scholars debate the actual impact of the decision.

In the existing literature, three conflicting theoretical explanations of *Chevron's* impact dominate. First, there is the argument that the federal courts began to take an increasingly deferential stance in reviewing agency decisions in the wake of the decision. A second view holds that judicial preferences are the primary determinant of judge's votes. Finally, a third view posits a more complex relationship between law and attitudes. The empirical evidence adjudicating between these competing theories has been mixed and inconclusive.

To provide a more rigorous examination of the impact of *Chevron* on judicial review of agency decision making, I replicate the Richards, Smith, and Kritzer (2006) analysis using a Bayesian change point model. This provides a more robust assessment of their hypotheses, as the model treats potential change points as parameters to estimated simultaneously with the coefficients of interest. As a result, rather than simply testing the binary possibility that a structural break occurred (or did not occur) with the *Chevron* decision based on the results of a post-estimation diagnostic test, I am able to evaluate the probability that a break occurred with the *Chevron* decision or at some other point(s) given the data.

While the historical tradition in the judicial politics subfield involved frame questions as a dichotomous choice between 'the law' and 'attitudes' as the determinant of judicial decision making, this paper adds to a recent stream of literature that argues that this relationship is more complex and interdependent. My analysis provides confirmatory evidence for Richards, Smith, and Kritzer's (2006) finding that the *Chevron* decision altered the way ideology impacts the votes of Supreme Court justices.

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A Survey Experiment on the Multidimensional Nature of Ideology

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The concept of political ideology has always been difficult for researchers to grasp. What is known is that ideology clearly matters to at least some individuals in determining their political behavior. Recently it has been argued that some of the problem in measuring ideology stems from the fact that in the mass public there are at least two distinct dimensions of ideological thinking: social ideology and economic ideology (e.g., Feldman and Johnston 2009; Treier and Hillygus 2009). These studies have used factor analysis or IRT models of large batteries of policy preference questions to define the dimensions of ideology, effectively deriving ideology from preferences. A simpler way to get at multiple dimensions of ideology would be to directly measure them by altering the typical unidimensional liberal-conservative continuum question. The main research question involved in changing the measure of ideology is whether individuals are politically sophisticated enough to conceptualize and report different ideological views on the different dimensions of ideology. This paper reports the results of an original survey experiment designed to investigate a how a two-dimensional measure of ideology would work. Will individuals report differences in their ideological self-placements for economic vs. social issues? Is reporting different placements on social ideology and economic ideology contingent on education or political sophistication? What are the impacts of different specifications of ideology on predicting policy preferences?

Using a randomized, three-track survey experiment, I investigate how respondents self-placements along the liberal-conservative scale change when they are asked to place themselves separately on economic ideology and social ideology scales. The data was obtained by a random-digit dial, state-wide representative survey of Oklahoma residents conducted by the University of Oklahoma Public Opinion Learning Laboratory in late 2009. Respondents assigned to the first track received the traditional single-item ideology measure. Respondents in the second and third tracks received two ideology questions each, one asking for their ideological views on economic issues and the other asking for their ideological views on social issues. The second track simply named the dimensions, and the third track primed the respondents by providing examples of economic and social issues. The distributions of the ideological self-placements for each measure differed slightly, and there were some significant mean differences between the measures. More importantly, when given two different ideology questions, only 56.8% of respondents in the second track placed themselves in the same place for both economic and social ideology. That number drops to 45% of respondents in the third track who placed themselves in the same spot on the ideology scale for social and economic ideology.

Differences in placements do not appear to be based on education or level of political sophistication, but that could be a product of the sample—Oklahoma has a narrower ideological and education-level distribution than the nation as a whole. Closer analysis shows that the differences in placements are a product of individuals simultaneously holding more liberal social views and more conserva-

tive economic views, lending support to the idea that many individuals are cross-pressured between different views on multiple dimensions of ideology. Further support for the multidimensional design comes from using the measures to predict policy preferences: when ideology is used to predict responses to policy questions, the experimental measures show considerable improvement in predictive power over the single-item traditional ideology measure.

Results from this pilot study show support for the hypotheses that individuals will provide different self-placements on the different dimensions when given the opportunity, and that measures created by the multiple dimension questions will better predict policy views than the traditional single-question ideology measure. A national survey will be run using this experiment in the next few months, complete with sufficient policy preference questions and political conceptualization questions to allow for more in-depth analysis of the questions asked by this study. Thinking about and measuring ideology in a multidimensional form could lead to a better understanding of how the concept relates to policy preferences in the mass public, as well as shed light on why ideology is important to models of political behavior despite evidence that many individuals do not think ideologically (Jacoby 1991) or are not sophisticated enough to connect their policy preferences to their ideological views (Converse 1964). Perhaps the problem is that individuals simply do not think of ideology in the way that surveys have been measuring it for over 50 years, and researchers need to explore the validity of the ideology measure for the mass public. That is precisely the goal of this project.

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Markov Regime-Switching Panel Analysis of Militarized Interstate Disputes

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The conflict of interests—a necessary condition for the outbreak of military disputes—varies over time for each dyad. Estimating the existence or non-existence of conflicting interests is not an easy task since it is essentially unobservable. This research proposes a way to jointly estimate the effects of covariates on the likelihood of military dispute and the dyad-specific transitions between conflict and non-conflict latent regimes using a Bayesian Markov regime-switching panel probit model. The dyad-specific intercept is allowed to switch back- and-forth between two values—the higher value indicates the existence of conflicting interests between the two states while the lower value indicates the non-existence, where the regime- switching is assumed to follow a dyad-specific Markov transition probability. Monte Carlo experiments explore that the effects of regime-independent covariates are biased toward zero in the conventional approach with unobserved time-constant cross-sectional heterogeneity, and that the bias gets larger as the dyad-specific differences between the two latent regimes increases. The data analysis of military dispute onsets empirically confirms the evidence of such an attenuation bias.

Foreign Aid or National Preference? The Analysis of UN Vote Buying

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How does U.S. foreign aid influence voting decisions in the UN General Assembly? Existing statistical analyses are disconnected from the underlying spatial voting model, and provide mixed results. I illustrate that without controlling for the effects of country ideal points, existing approaches can generate very misleading inferences on vote buying in the United Nations. I build a statistical model that matches the underlying theoretical voting model with the feature of vote buying. In the statistical model, the direction of the aid's influence on a resolution is determined by an indicator function that captures how the United States votes on that resolution. The statistical model *simultaneously* estimates country ideal points and a source of vote buying through U.S. aid. The results suggest that although in certain cases aid exerts a substantial effect, in the majority of cases countries' voting decisions are invariant to U.S. aid. The ability of the United States to buy votes is conditional, depending on the characteristics of the country

and the resolution. This finding differs strikingly from the uniform effect of aid found in the existing literature—aid either increases voting similarity or has a null effect.

Furthermore, I introduce a *hierarchical* structure into my baseline model to allow for heterogeneity in issue areas and recipients when vote buying is examined. In the two extended models, I estimate an individual aid parameter for each issue area in the former and for each recipient-year in the latter, instead of estimating a single aid parameter in the baseline model. The findings show that vote buying is most effective on Middle East resolutions, but it fails to work on issues related to nuclear security. The results also suggest that being an enemy of the United States dramatically reduces the effectiveness of UN vote buying while its effectiveness is immune to the existence of other cooperative relationships with the United States (e.g., trade).

In this study, I provide a mathematical proof of model identification. It demonstrates that the proposed statistical model needs the same identification restrictions as a statistical voting model without the feature of vote buying. In addition, I show analytically that the commonly discussed endogeneity problem, resulting from the reciprocal influence of voting patterns on aid allocation, is minimized in my statistical model. This is because the bias of endogeneity can be either positive or negative, depending on whether the United States votes "Yes" or "No" on the same issue. The overall bias, after averaging out the positive and negative biases, is expected to be minimal.

Estimating Ideological Positions of Candidates and Contributors from Campaign Finance Records

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Many applications in political science require reliable ideological measures of individuals and groups. Methods designed to estimate ideal points from roll call votes have been highly successful in recovering precise ideological estimates but are limited to legislators with voting records. In contrast, methods for scaling political texts show great promise in extending estimation outside the confines of legislative bodies but are not yet able to locate individuals with much precision. Scaling campaign finance data offers an attractive middle ground between the aforementioned methods. Contribution data are abundant and rich in ideological content. This makes it possible to recover ideal points that rival roll-call estimates in terms of their reliability and accuracy. At the same time, scaling contribution data shares lexical analysis's promise of extending the reach of ideological estimation to include a more comprehensive set of political actors.

I develop a statistical scaling method that models contribution decisions as a multinomial discrete choice problem using an item response model. The method builds upon item response models used for educational testing and a similar group of statistical methods developed to recover ideological positions of parties and legislators from text sources. In addition to the spatial parameters found in these models, I include a comprehensive set of non-ideological candidate covariates widely believed to influence contribution behavior, such as incumbency status, electoral competitiveness and committee assignments. The paper presents results from a joint scaling of PAC and federal candidates during the 1980–2008 election cycles. I establish face validity by showing that the method recovers ideological positions for incumbents that strongly correlate with ideal point estimates recovered from voting records. Additional model testing demonstrates that the model closely fits the data.

A useful quality of the model is that it places candidates and contributors on the same scale. Figure 1 shows the distributions of PACs and Candidates from a joint scaling of the 1980–2008 Election cycles. Reminiscent of DW-NOMINATE scores, candidates divide along party lines forming a bimodal distribution. In contrast, the distribution of PACs has a single mode located between the parties.

The method also provides platform for testing hypotheses about contribution behavior, just as NOMINATE and other roll call scaling methods have done for the study of legislative behavior. The paper illustrates this by evaluating competing hypotheses about which motivations best explain the contribution behavior of PACs. By integrating ideological and non-ideological motives into the model, I forgo the starting assumption that PACs are either selecting like-minded candidates or buying access and other legislative services, and instead allow the data to speak directly to the question at hand. The results reveal that strategies vary across categories of PACs. Some PACs condition their giving primarily on ideological proximity; other PACs condition their giving on primarily on non-ideological considerations; but most PACs mix strategies.

Although the analysis in the paper is limited to federal contribution data, the most promising feature of the method is the ability to tie together state and national politics simply by virtue of the dataset. The private individuals, special interest groups, firms, and other organizations that contribute to candidates at different levels of politics are natural bridge actors that provide the glue needed to hold the scaling together. This makes it possible to construct ideal point estimates in a common space for not only elected legislators but also unsuccessful challengers, presidential and gubernatorial candidates, state judicial candidates, ballot measures and other campaigns, all together with the numerous individuals and organizations funding the campaigns.

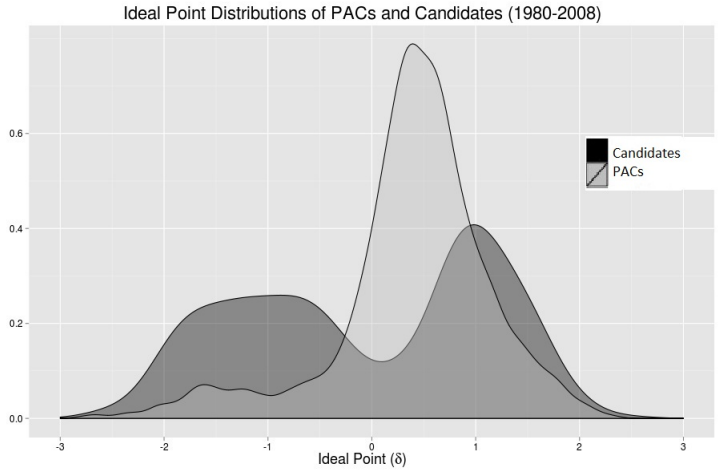

FIGURE 1: THE DISTRIBUTIONS OF PACS AND CANDIDATES FROM A JOINT SCALING OF THE 1980-2008 ELECTION CYCLES

Announcements

Gosnell Prize 2010

Committee:
Matthew Lebo (chair), Kenneith Kollman, and Betsy Sinclair

We are very pleased to announce that Jong Hee Park, of the University of Chicago, is the 2010 winner of the Gosnell Prize. His paper, "Joint Modeling of Dynamic and Cross-Sectional Heterogeneity: Introducing Hidden Markov Panel Models," is available at the following site http://polmeth.wustl.edu/conferences/methods2009/resources/papers/Park_JongHee_PolmethPaper_Finalv3.pdf. This paper was presented during The Society for Political Methodology 26th Annual Summer Meeting held at Yale University. We also thank the committee, Matthew Lebo (chair), Kenneth Kollman, and Betsy Sinclair for their hard work in sorting through many excellent papers presented in 2009.

Williams Award for the Best Dissertation

Committee:
Guy Witten (chair), Michael Colaresi, and Jonathan Nagler

In recognition of John T. Williams' contribution to graduate training, the John T. Williams Award has been established for the best dissertation proposal in the area of political methodology. The 2010 committee consists of Guy Whitten (chair), Michael Colaresi, and Jonathan Nagler. The recipient this year is Teppei Yamamoto from Princeton University. Teppei's work is centered on methods for causal inference, including causal attribution, causal mediation, causal moderation, and causal inference with measurement error. Congratulations to Teppei for this award.

Career Achievement Award

Committee:
Nancy Burns (chair), Jake Bowers, Janet Box-Steffensmeier, Tse-min Lin, and Jim Stimson

We are pleased to announce the 2010 recipient of the Society for Political Methodology's Career Achievement Award. This award recognizes scholars who have made intellectual contributions that have given the field new ideas and new tools, while, at the same time, they have given the field sustaining institutions. This year's recipient is Gary King, the Albert J. Weatherhead III University Professor at Harvard University.

With *Unifying Political Methodology*, Gary began a career's worth of pointing our way to new intellectual agendas. *Designing Social Inquiry* (1994) alone has been cited

more than 3,000 times. It has had a profound influence on the conduct of social science, instilling a practice of scientific rigor in a generation of qualitative and quantitative political scientists.

Gary's career has been filled with introducing, teaching, and then thoroughly mainstreaming new frame-shifting methodological approaches. His contributions have been so successful that methods that once seemed out of reach to many are part of the fabric of our work.

Gary has approached his research with a sharp sense for how to improve the discipline's methods and for how to communicate those improvements to a wide audience. He has made field-changing contributions on a wide variety of methodological topics, including missing data, research design, causal inference, survey research, and ecological inference. He is the author of more than 115 journal articles, 15 public domain software packages, and 8 books, many of which are used both within and outside academia. He appears in the ISI's list of the most highly cited researchers in the social sciences. Gary has won more than 25 "best of" prizes and awards for his methodological work. His impact has spanned decades; he won the Pi Sigma Alpha Award for the best paper at the Midwest Political Science Association's annual conference in 1993, 1998, and 2005. Gary is a Member of the National Academy of Sciences, a Fellow of the American Academy, and a Fellow of the American Statistical Association.

He is a very effective teacher and mentor. Scores of his students have gone onto careers at leading universities around the world. His mentoring has extended across the field, well beyond his students.

Gary's institutional impact on the field comes not just from the way his ideas have helped form the intellectual toolkit for the field, but also from his informal and formal institution building. Perhaps the most important of these institutional contributions comes in the form of the norms Gary created and sustained for the profession. Gary changed the norms of the field via the provision of free, easy-to-use software. He taught a generation of methodologists by example; as a consequence of his work, it is now standard practice to make free (and now open-source) software available, making it possible for good ideas to become part of practice much more rapidly. Almost single-handedly, Gary created and made wide-spread the norm of replication in political science. In addition, he has worked tirelessly to foster data sharing across the social sciences. In his formal institutional work, Gary participated in the first Ann Arbor meeting of the Society for Political Methodology. He was influential in the 2006 Political Methodology report that guided the section in new directions. He was the founding editor of The Political Methodologist. He served as President of the Society for Political Methodology.

Thank you to the many nominators, from whom we have borrowed text.

Miller Prize

Committee:
Dan Wood (chair), Kosuke Imai, Greg Wawro, and Burt Monroe

Each year the Miller Prize for is awarded for the best work appearing in *Political Analysis* the previous year. The 2010 Miller Award committee, consisting of Dan Wood (chair), Kosuke Imai, Greg Wawro, and Burt Monroe, chose the article by Daniel Corstange "Sensitive Questions, Truthful Answers? Modeling the List Experiment with LISTIT", which appeared in Volume 17, Number 1, Winter 2009, and can be accessed at http://pan.oxfordjournals.org/cgi/reprint/17/1/45 Congratulations to Daniel on this important honor.

Best Statistical Software Award

Committee:
Jas Sekhon (chair), Micah Altman, Kosuke Imai, Andrew D. Martin, and Simon Jackman

On behalf of the Political Methodology section, we congratulate Jeffrey A. Dubin and R. Douglas Rivers on winning our second annual Statistical Software Award for their work on Statistical Software Tools (SST). The award committee—Micah Altman, Kosuke Imai, Simon Jackman, Andrew Martin, and Jasjeet Sekhon (chair)—thank all who submitted nominations. The award recognizes individuals for developing statistical software that makes a significant research contribution. The committee also judged the software's quality and level of innovation.

The volume of software produced by political scientists has increased markedly in recent years. This is largely due to the relative ease with which R packages can be developed and circulated, and the steadily rising level of computer literacy in the profession. But for the section's second Statistical Software Award, the committee decided to recognize a less recent contribution, acknowledging the enormous challenges of developing general-purpose statistical software in an era when computers were much smaller and slower than today.

SST was a critical research tool for political methodologists and econometricians. At a time when desktop statistical computing was still something of a novelty, SST enabled researchers to estimate complex models reliably and efficiently. SST's great flexibility and low cost led it to be the tool of choice for researchers applying cutting-edge statistical methods to the study of politics.

SST was a landmark in statistical software development. It allowed researchers to use a wide variety of two-step, limited dependent variable, and maximum likelihood (ML) models including, probit, multinomial logit, Tobit, and random utility models. SST was ahead of its time in its application of statistical computing methods. For example, SST used analytical first and second derivatives and adaptive step sizes in optimization — some widely used statistical packages do not do this today. SST also supported arbitrary user-defined maximum likelihood models. Multiple datasets could be loaded at one time and manipulated. Vectors of different sizes were allowed for a given dataset, an uncommon feature even today. SST also came with support for matrix algebra and an elegant command-line parser.

SST was a substantial engineering effort. It is very fast. Key parts of it were written in machine code. The creators wrote their own virtual memory routines in order to work around the limitations of the then ubiquitous 8088 Intel hardware. When 32-bit Intel chips became available in the form of the 80386, SST was one of the first 32-bit applications. The level of software engineering in SST is impressive, and an example to us all. No political scientist has yet produced software as general or as elegant as SST. Jeff and Doug should be proud indeed.

A Note from our Section President

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The Society for Political Methodology moved forward on a number of fronts over the last year. This *Note From the President* highlights our developments and achievements over this period and comprises mostly information that was announced at the business meeting in Washington, DC.

Administrative Information

The SPM is now fully incorporated as a legal entity in Nevada. This combined with the insurance policy in place ensures us legal protection when negotiating contracts and running events such as the Summer Meeting. By the end of the calendar year we will achieve non-profit status with the IRS. This is also an important step since it saves us an obvious expense. Also in terms of governance we added two new Fellows of the Society for Political Methodology: R. Michael Alvarez and John Brehm. This is a governance issue because the Bylaws state that Fellows are the Directors of the Corporation. So we also welcome two new members to the Board of Directors.

Our webpage at http://polmeth.wustl.edu underwent a dramatic improvement, even though the interface looks familiar. This was necessary to streamline our paper and syllabus submission process and also to make application and registration for the Summer Meeting more convenient. I thank Andrew Martin, Troy DeArmitt, and Jonathan Rapkin for working hard on the technical issues associated with this improvement.

We also have a new award that will be given for the first time in 2011: The Political Methodology Emerging Scholar Award, which is designed to honor a young researcher, within ten years of their degree who is making notable contributions to the field of political methodology. This will enable us to recognize our stellar young colleagues and the work that they are doing. I am pleased to announce that Simon Jackman has agreed to a two year term as inaugural chair of the committee to select recipients. Please send him any nominations that you might have over the course of the 2010-2011 academic year.

Since this is an even-numbered year, we elected two new officers: Fred Boehmke as *Member at Large* and Luke Keele as *Treasurer*. Fred is already hard at work on a survey of departments that will inform us about how methodology is taught across the discipline. Anyone who has served at Treasurer knows that it is "real work." Therefore I appreciate Luke's willingness to set aside some of his other duties and serve in this capacity. At next year's APSA meeting we will elect a new *President* and *Vice-President*, meaning that I will step into the presidential helicopter on the South Lawn, wave to the press corps, then fly off into retirement and autobiography book deals.

Meeting

The 2010 Summer Meeting at the University of Iowa was enormously successful and I thank our Host, Fred Boehmke. This meeting attracted 155 attendees, staying in the hotels and dorms in Iowa City. There were 21 paper presentations at panels over 12 sessions, 16 faculty posters, and 45 graduate student posters. I was particularly impressed with the high quality of the graduate student work at this meeting. Be sure to check out Fred's

photos at the conference webpage: http://www.polisci.uiowa.edu/polmeth/index.html. Organizing this meeting is an immense amount of work and there are always logistical challenges that appear unexpectedly. So it is important to appreciate the major efforts of our hosts.

Our next Summer Meeting will be at Princeton in 2011 with Kosuke Imai as our host. The next three meetings are already scheduled for: UNC Chapel Hill/Duke in 2012, the University of Virginia in 2013, and the University of Georgia in 2014. This is a reassuring list of high quality hosts and it highlights interest that general faculty and administrators have in our activities.

The 2010 APSA meeting methods panels were successfully organized by Jonathan Wand. He was able to put together 19 panels in total, including a theme panel: "A Sea Change in Political Science Methodology?" I thank Jonathan for his terrific work. Not to be outdone, Dave Peterson organized 20 methods panels for the MPSA this year. This included blunt and funny panel title: "So You Have Some Results: Methods for Getting the Most Out of Your Results." I am pretty sure this panel was about results. I also thank for Dave for his efforts.

We are expanding our conference activities in Europe in dramatic fashion. First, the Society is responsible for organizing the panels for the European Consortium for Political Research (ECPR) conference 25–27 August, 2011 in Reykjavik, Iceland. A call for papers will go out this Fall once the panels are established from the organizers (Marco Steenbergen and myself), although you may hear from individual panel chairs in the interim. Second, we have established a significant relationship with the European Political Science Association (EPSA) along the lines of our important relationship with the American Political Science Association (APSA). This includes organizing all future methods section panels for the EPSA annual meetings, including having exclusive editorial control in the way that we have at APSA annual meetings. I am very pleased to announce that Jude Hays will be the our panel organizer for the 2011 EPSA methods section. Note that this meeting will take place in the Guinness Brewery in Dublin! I hope, like me, that you find these European developments exciting. This is an important step in increasing our global network.

Committees

This last Spring all of the standing committees were updated. We continue to enjoy enthusiastic support for our activities and the willingness of members to do committee work is a cornerstone of our success. At the APSA meeting we announced the most recent of these updates with Gary King (chair), Corinne McConnaughy, and John Freeman rotating-off the Long Range Planning Committee, and Jeff Lewis (chair), Lonna Atkinson, and Thomas Plumper rotating-on in their place.

Political Analysis

By far the biggest changes this past year have taken place with regard to *Political Analysis*. We are now almost through the first year of the new editorial team of R. Michael Alvarez and Jonathan N. Katz. Notably, we retained our #1 ranking by ISI Journal Citation Reports for the third year in a row. The Oxford University webpage notes that "The journal has been ranked 1 out of 112 journals with an impact factor of 3.756." This is a stunning achievement for such a young journal in our discipline and is clearly a reflection of how important our non-methodologist colleagues value our work.

This year we also put the journal out to bid for future publishers. The original contract with Oxford University Press ends in 2012, and since this process can be time-consuming the officers thought it prudent to start early. After analyzing several very high quality bids, the Publications Committee (Robert Erikson [chair], Phil Schrodt, Gary King, Orit Kedar, and Jonathan Katz [ex officio]) recommended the Oxford University Press contract, which was approved by the Directors. In addition to continuing our very productive relationship with OUP, we added several favorable terms, including:

- better royalty terms that escalate with increased sales,
- a six year contract,
- bigger size (10.78 x 8.25 inches), which means better graphical layouts and approximately four more articles per volume,
- electronic access through `highwire` and `jstor` (three year moving window),
- more editorial support,
- increased marketing efforts worldwide,
- the ability for members to opt out of print access for electronic only access, saving $5 (and some trees!),
- rotating free online access to other sections that we designate for one year periods as a way to market to other political scientists.

In addition, when the new OUP webpage rolls-out there will be an opportunity for non-APSA members to subscribe to the journal. This means that scholars in other fields who have overlapping interests will not be excluded by imposed APSA membership costs.

Future Issues

The most pressing current concern is the renewal of our NSF grant for supporting graduate student attendance at the Summer Meeting, as well as some specialized events

and regional conferences. This will be submitted in the January NSF cycle, and I welcome your comments and suggestions this Fall as the ad hoc committee writes. With two European conferences this Summer, I hope that all TPM readers will consider sending in a paper proposal when the calls come out, or just attending to support our European initiatives. The APSA panel organizer for 2011 is Chris Zorn, so you can also expect to see a call for proposals from him as well. Hopefully many of you responded to the call from Jason Seawright for the 2011 MPSA methods panels. Clearly there are lots of opportunities to present methods work in the forthcoming year.

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Subscriptions to *TPM* are free for members of the APSA's Methodology Section. Please contact APSA (202-483-2512) if you are interested in joining the section. Dues are $25.00 per year and include a free subscription to *Political Analysis*, the quarterly journal of the section.

Submissions to *TPM* are always welcome. Articles may be sent to any of the editors, by e-mail if possible. Alternatively, submissions can be made on diskette as plain ascii files sent to Wendy K. Tam Cho, 240 Computing Applications Building, 605 E. Springfield Ave., Champaign, IL 61820 . LaTeX format files are especially encouraged.

TPM was produced using LaTeX.