The Political Methodologist

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Notes From the Editor

Just before we went to press, we received the sad news that John Williams passed away in his sleep on September 13. John was a great champion of the political methodology section and an outstanding scholar, but more significantly he was a dear friend and mentor to many in our community. He will be greatly missed. We have included a brief obituary at the end of this issue and will offer a fuller memorial in the Spring.

This is the second issue of *The Political Methodologist* under our editorship, and we hope you will agree that we are getting the hang of it. In this issue we lead off with a reflection on methods training in political science over time. A chain of teachers and students from Chris Achen to his student Larry Bartels to his student Simon Jackman to his student Josh Clinton each describe their methods training. Next up is an article by Wendy Tam Cho on open source software for spatial statistics. Spatial dependence has been increasing recognized as a potentially serious problem in political science, and Wendy describes some state of art methods for tackling such dependence and some freely available software that implements those methods. Finally, Jake Bowers offers excellent practical advice on how to use R for analysis and presentation of multi-level data. Of particular note are Jake's figures which are both elegant and powerful and which Jake describes in detail how to create.

For our next issue, we will have an article by Kevin Quinn on the use of OS X in quantitative research and much more. But we are always on the lookout for more material – articles, software reviews, and book reviews. As always, your submissions and ideas for topic to address are most welcome.

The Editors

Articles

A Methodological Education (in Four Parts)

Political methodology has evolved greatly over the last 35 years. In 1969, only a handful of Political Science departments employed specialists in statistics or data analysis. Today, the Political Methodology section is the 5th largest organized section in the APSA.

To get a personal view of the changes in political methodology training over the last three decades, we asked Chris Achen, Larry Bartels, Simon Jackman, and Josh Clinton to share their recollections of their training as grad students and faculty members. These four individuals are links in a chain of methodological training that reaches back to the late 1960s. Achen began his methodological training at Yale in 1968 and then was Bartels' advisor at Berkeley in the late 1970s, Bartels in turn served as Jackman's advisor at Rochester and Princeton in the early 1990s. Finally Jackman advised Clinton at Stanford in the late 1990s and early 2000s. By sharing their experiences, these four scholars give us a window into the changes and continuities in the training of scholars of quantitative political science.

Part I

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In the fall of 1968, I was a very green first-year graduate student in New Haven, imagining that I would study Western European politics. Martin Luther King had recently been murdered. I had worked for Robert Kennedy in the California primary, only to come home from the get-out-the-vote effort the day of the election, turn on the television, and see him murdered, too. The Vietnam War continued on its brutal and morally offensive course over the summer, the ghettoes were exploding, and I was being drafted. Politics was not an abstraction.

Yale then was an exhilarating place. Dahl, Lane, and Lindblom taught there, as did other luminaries and many brilliant associate and assistant professors. Intellectually exciting advances were happening at Michigan and Rochester, and there were several other strong departments. But in the reputational rankings of the time, Yale was a comfortable first-place winner and seemed to deserve it.

I had gone to my undergraduate faculty at Berkeley the previous fall to get recommendations about graduate school. They had asked me what topics and which books interested me. Dahl and Lindblom's *Politics, Economics, and Welfare* had made an enormous impression for its theoretical ambition, as had Donald Matthews' *U.S. Senators and their World* for its methodological creativity. Outside political science, A. J. Ayer's *Language, Truth and Logic* was the book I had thought most worth my engagement. Unanimously the faculty had recommended "Yale, if you can get in." Many months later, to my surprise, the fat envelope had arrived.

So there I was the succeeding fall, on the mean streets. Western European politics didn't work: I dropped the course within two weeks. But having had two undergraduate courses from the Statistics Department and a miserably taught undergraduate research methods course from Political Science, I thought I might as well start my graduate career with something called "Introduction to Econometrics." The subject would undoubtedly be dull and intellectually unimportant, but who knew? Perhaps the purely mechanical skills I learned would save a little research time some day.

On the first day of class, in strode the new associate professor assigned to the econometrics course. Gerald Kramer was a formidable presence. Rigorous and rarely smiling, he worked through the mathematical logic of inference on the blackboard, demonstrating the power of least squares and maximum likelihood approaches. He also drew out the implications for political science, dispatching fat and blubbery targets in the journals with single shots. Yet he was no mathophile. "Don't confuse the level of the mathematics with the level of intellectual achievement," he would say, noting that first-rate qualitative work was always better than second-rate quantitative efforts. We were in the presence of a master.

In retrospect, much of the econometrics he taught sounds uncontroversial now: "Don't compare correlations across different samples." "Point estimates are fine, but don't ignore the standard errors." "Don't talk about .05 significance tests until you understand what it's 5 percent of." "Don't fabricate some ad hoc, ungrounded estimator when you can do MLE instead." "Don't simulate. Prove."

But the ideas and the logical rigor with which he developed them were new to political science at that point, and their implications for empirical work were explosive. Political methodologists in subsequent decades have spent considerable time re-announcing what Kramer taught nearly forty years ago.

Kramer was well ahead of his time, and he paid the customary price. "His standards are too high," many students (and some faculty) said. "He's too rigid." But many of us in his classes could not understand why genuinely coherent ideas were too much to ask for, nor why sloppy or mechanical statistical work was a contribution to knowledge. We came to feel that Kramer represented precisely the high intellectual standards and careful attention to classroom teaching that had attracted us to graduate school in the first place. Far from being politically irrelevant, as the Caucus for a New Political Science claimed at the time and as Perestroika members sometimes allege now, the material Kramer taught seemed to deserve a prominent place among the tools that a serious scholar, concerned about real politics but averse to uninformed grandstanding, would want to know. Logic and evidence, said Aristotle. Kramer honored and advanced that tradition.

I learned a great deal from other stellar Yale faculty, inside Political Science and outside it. Critically, David Mayhew, Al Stepan, and my dissertation supervisor, Doug Rae (all assistant professors) emphasized getting the politics right, making a consistent argument, and writing it up powerfully. *Sine qua non.* In the end, though, it was Kramer's subfield that became my own.

On a Sunday afternoon in the spring of 1972, I met with Jerry to discuss a chapter of my dissertation. He said that my model was unidentified, a ghastly error on my part. I knew what it meant: My estimates were nonsense. I had worked on the chapter for many weeks, apparently with no sense of the most obvious statistical considerations. Quite worried, I asked him to explain what the problem was. He did. But I didn't get it. Feeling dumber than ever, I asked him to go over it again. Now I was really sure I didn't get it. "OK," I said, "but it works like this, and I don't see where the problem is." He explained again. But now I thought I saw something. I set out my view. We went back and forth a time or two more, and then he said, "OK, I guess you're right." I remember the moment as if it were yesterday. That's when I knew that I wasn't a first-year graduate student anymore. And I was so beset by that feeling that I didn't take note of the example he had just set for me about respectful but non-sappy treatment of graduate students, and intellectual honesty about their work.

Kramer thought of himself primarily as a formal theorist. But he was profoundly serious about empirical work, and about the deep structuring that theory and evidence provide to each other. A style of empirical investigation, disciplined by econometric theory and prepared to innovate when the data required it, was what I took away in my twenties. The formal theory of the Sixties, focused on the spatial model of voting, seemed to me empirically irrelevant for the reasons that Don Stokes set out at the time. In the longer run, though, the connection to formal theory that Kramer emphasized has come to seem more important to us all, as formal theory in political science has come to rely more on political understanding and less on economics, and as the limitations of social-psychological explanations have become more apparent. Kramer's agenda – connecting the powerful tools of the formal theorists and econometricians to the sophisticated political understanding of qualitative scholars and to the detailed grasp of reality by the empiricists – became the twenty-first century agenda of quantitative political science, enshrined in the National Science Foundation's EITM Program. The tragedy is that Kramer's failing health did not allow him to enjoy the long career and undoubted honors that would have come his way as the profession at last began to catch up with his ideas.

In 1972 when I went on the job market, all these developments were far in the future. There were no positions in political methodology; the subfield did not exist. Almost none of the journals would publish real methodological papers. On the other hand, any article that used regression analysis was deemed "methodological." The birth of the Political Methodology Section of APSA was nearly two decades away. Happily, Rochester offered me a job anyway while I finished my dissertation.

While at Rochester, I stumbled across a copy of Arnold Zellner's 1971 *Introduction to Bayesian Inference in Econometrics* at the bookstore. (Those were the days in which university bookstores stocked large numbers of intellectually serious mathematical volumes other than course assignments.) The book was a revelation. I read it line by line, pencil in hand, working through the footnotes like a devout acolyte encountering holy writ. The conversion experience was immediate. By 1975 I was teaching statistics and econometrics courses from the Bayesian perspective, and by 1978, I published a Bayesian derivation in AJPS in connection with the study of representation. To this day, I continue to use in my research what I learned then.

Unfortunately, the comprehensive Bayesian teaching lasted only a few years. The relevant software didn't exist, and students were left too isolated from the Seventies mainstream in the discipline. The ideas were right, but perhaps not ready for undergraduates and first-year graduate classes. Happily, though, Larry Bartels took courses from me during this period, did Bayesian publishing himself, and helped train Simon Jackman, who has exploited modern software and contemporary Bayesian ideas in ways that Zellner never dreamed of. And Josh Clinton's career lies ahead.

When I go to political science conferences in Europe, I am struck by the absence of methodological panels and the relative weakness of empirical research there. Important pockets of excellence exist, of course, but taken as a whole, this rich collection of countries, with hundreds of millions of people and centuries-old universities that invented social science, has fallen back. Even Germany, to which a century ago young Americans trekked off to study at the feet of the world's masters of political science, now struggles to maintain international respect. Why has so much of an entire continent lost its way?

Looking back at my own time in the profession, I am struck by the importance of teaching. A handful of highly talented people, who were also dedicated to graduate students and graduate student teaching, made all the difference in my career. Nor is my case unusual. One can look around summer meetings of the Political Methodology Section and see the intellectual lineages: They are relatively few. Just as a lack of genetic diversity can bring tragedy to a species when conditions change, so can the inevitable heavy dependence on a handful of teachers in each country lead to system-wide failures when disastrous wars and bad ideas for managing universities consume most of a continent, as they have in Europe.

So I am delighted to have this opportunity to honor my teachers. The academic generation-to-generation connection is fragile. When it has been successful for so many of us, as in contemporary American political methodology and in political science generally, then there is a legitimate pride to be taken in what we have achieved collectively. But for each of us individually, the collectivity is an abstraction. Just a few people changed our professional lives, and our ties are to them. Thus I am most grateful for the opportunity to thank my graduate school teachers in print. Their influence is with me daily.

Part II

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My first exposure to political methodology came as a 17-year-old freshman at Yale. For some reason I had decided that I might be interested in political science, and I happened upon a course taught by Christopher Achen entitled "Quantitative Methods in Political Science." The fortuitous basis for this life-altering connection was that I was determined not to take any course that met before noon, and Achen was equally determined not to teach any course that met before noon.

Achen's course satisfied some requirement for Yale political science majors, and the rest of the students were seniors who had clearly put off the distasteful necessity as long as possible. However, since I wasn't smart enough or experienced enough to know that it was distasteful I learned a great deal. We got a rudimentary introduction to probability and statistics up through cross-tabulation and regression, as well as brief introductions to content analysis, survey research, and experimentation – and enough handholding to punch SPSS commands on IBM cards. The primary texts were David Blackwell's *Basic Statistics* and Phil Shively's *The Craft of Political Research.*

The preface to Blackwell's book says that "The approach is intuitive, informal, concrete, decision-theoretic, and Bayesian." Achen's lectures and homework assignments were all of those things, too. I remember calculating how many nuclear missiles make a credible deterrent, how likely it is that the better team will win the World Series, and when to break up with one's girlfriend, among other things. The eclectic mixture of elementary formal theory, statistics, and research methodology seemed entirely natural, and I'm sure it shaped my methodological prejudices in fundamental ways.

The term paper I wrote over my first Thanksgiving break was based on survey data from one of the early Michigan election studies. I did a multiple regression analysis of issue voting, with interaction terms to allow for the possibility that more- and less-educated respondents might attach different weights to different issues. Looking back on it 30 years later, I still have no idea how Achen managed to pack enough into a one-semester course to make it possible for someone with no previous experience to write such a paper. (Well, almost enough; I do recall that I messed up the hypothesis tests on the interaction terms, which provoked a gentle comment that "it's a bit more complicated than this.")

Having exhausted Yale's one undergraduate course offering in political methodology – and having more enthusiasm than good sense – I thought the logical next step would be to take whatever the graduate students were being offered, which turned out to be Gerald Kramer's second-semester econometrics course. So I marched over to Kramer's office to get his permission to sign up. Alas, Kramer had little patience for overeager undergraduates, and brought our interview to a very abrupt conclusion by asking me some question about covariance matrices of regression coefficients. (I had learned what a covariance was from those bungled hypothesis tests, but certainly did not know that they came in matrices!) Thus, while I did later take the first-semester graduate course and a statistics course or two, I never did connect with Kramer, and most of my subsequent methodological training at Yale focused on simulation models (with Garry Brewer) and game theory (with Steve Brams).

When I began to think about graduate school in political science I got back in touch with Achen, who had moved on to Berkeley after one year on the faculty at Yale. He sent me a patient, fair-sounding rundown of the strengths and weaknesses of some prominent and not-so-prominent graduate programs, then added, "We're pretty good at all that stuff, too." That was good enough for me – at least I knew there would be some afternoon classes. (Current graduate students may be interested to note that there were no elaborate recruiting visits in those days. Indeed, I had never set eyes on Berkeley before showing up there for the first day of classes.)

The political methodologists at Berkeley when I arrived in 1978 were Achen and Merrill Shanks. Achen was on leave my first year, but Shanks taught a course, cross-listed with the sociology department, on causal models, path analysis, and multivariate contingency tables. One of the highlights was reading a draft of his own unpublished manuscript on "The Importance of Importance," an underrated contribution to statistical interpretation that later formed the basis for some of his influential work with Warren Miller on interpreting elections.

The second year of Berkeley's "methods" curriculum at the time was a two-quarter course taught by Achen entitled "Formal Models of Politics." The vague title should have been a sobering hint of how little most of the faculty knew or cared about what got taught in the way of formal theory and quantitative methods. But it had the virtue of giving Achen a good deal of flexibility to adapt the content from year to year in order to expand the range of topics covered and pursue some of his own interests.

When I took the course in my second year, the first quarter was an introduction to Bayesian econometrics based on Ed Leamer's recently-published book, *Specification Searches*. That was an exhilarating experience – Leamer's book remains one of my all-time favorites. I didn't understand much of it the first time through, and I'm sure the same was true of my fellow students. However, I had found the Bayesian worldview quite congenial the first time around, in Achen's undergraduate course; and now it was inspiring to see it laid out much more rigorously – and used so creatively to address some of the most glaring embarrassments of traditional econometrics. (Leamer's slightly later, somewhat broader piece, "Let's Take the Con Out of Econometrics," should still be required reading for anyone audacious enough to run a regression.)

The final exam consisted of two problems. One involved applying the Box-Tiao analysis of transformations to a recent Berkeley dissertation on bureaucratic performance in Thailand. The methodological focus was idiosyncratic, to say the least; but we had already worked on it as a homework problem so had some idea of what we were doing, and I'm sure it was helpful to demonstrate that models could be tailored to specific substantive problems – even in comparative politics! The second problem was a more straightforward analysis of the location of posterior means in a Bayesian multiple regression model. That one had the virtue of being directly related to what we were supposed to have learned from Leamer's book. I still managed to get it wrong; but that was educational, too, and many years later I put the lesson to good use when the same problem arose in a piece I wrote on "Pooling Disparate Observations."

The next quarter, Achen taught a somewhat compressed version of a more conventional intermediate-level econometrics course using the second half of Hanushek and Jackson's *Statistical Methods for Social Scientists* as the main text. The course covered a bit of asymptotic theory, dichotomous dependent variables, simultaneous equations, and errors in variables. As always with such courses, there was a constant tension between spending time on rigorous derivations or careful applications. We certainly did too little of the latter, though that deficiency was partly remedied in research seminars with the likes of Jack Citrin and Ray Wolfinger.

The next year, the first quarter of the "sequence" was a formal theory course based on Harsanyi's *Rational Behavior and Bargaining Equilibrium*, supplemented with bits and pieces of Luce and Raiffa's *Games and Decisions* and readings from Oran Young's anthology on *Bargaining*. That was probably an odd focus in the context of a graduate program with no other formal theory offerings; but Achen managed to convey enough math along with the substance (and vice versa) to make it quite stimulating. (Around the same time, I sat in on Harsanyi's own course in which he presented the work with Selton on equilibrium selection that later won them the Nobel Prize; however, given the limitations of my technical training in game theory, that experience was more bewildering than stimulating.)

The third time I saw it, Achen's two-quarter sequence looked more like a conventional quantitative methods course. The first quarter covered most of the same material as the previous years' second quarters: the general linear model, logit and probit, and simultaneous equations. The second quarter introduced a variety of additional topics, including factor analysis, covariance structures, scaling, ecological inference, nonlinear simultaneous equations, and sample selection bias. By this time I had a job lined up at Rochester and was busy dissertation-writing, so I only sat in very sporadically. Since I knew I would soon be teaching a similar course myself, I did pay some additional attention to the topics and readings. As it turned out, though, I made relatively few (intentional) departures from Achen's model, except to spend more time on a somewhat smaller set of topics, emphasizing model specification, measurement, and interpretation and downplaying issues of estimation.

What I did not do, but should have, was to sit in on Achen's more basic first-year regression course, which I had missed when he was on leave. I suppose I assumed that I had already absorbed most of what Achen had to say about the relationship between theory and applied work, either by taking and eventually TAing his undergraduate course, or by osmosis. But I later realized how much I had failed to absorb when I read the brilliant discussion of the logic of data analysis in his little Sage monograph on *Interpreting and Using Regression*. (As it turned out, I was teaching an applied regression course myself within a few years and no doubt reinvented much of what I would have learned from Achen's course. That served me right for having violated a fundamental rule of graduate education: never pass up a course with a great teacher, regardless of timing or subject matter.)

Along the way, I also took two econometrics courses in the Berkeley econ department. One was a relatively non-technical applied regression course based on Pindyck and Rubinfeld's *Econometric Models and Economic Forecasts*. (Do such courses still exist in econ grad programs? Not at Princeton.) That course was useful mostly for the focus on time-series models, which were practically non-existent in the political science curriculum. The other was a much more rigorous and interesting course taught by Thomas Rothenberg. The main texts were Maddala's *Econometrics* and Johnston's *Econometric Methods*, but I also got a useful dose of Theil's *Principles of Econometrics* (and a less useful dose of Malinvaud's *Statistical Methods of Econometrics*), as well as exposure to some classic papers by the likes of Wald, Koopmans, and Haavelmo.

What I admired most about Rothenberg was that he was just as rigorous about substance as he was about econometric theory. There were frequent discussions of applied econometric work, almost always turning in fairly short order to his favorite question: "What is in this disturbance term?" In addition to the final exam, he had each student write a 10-page term paper critiquing and extending a published piece of applied research. The questions he posed for that assignment provide a nice sense of his intellectual priorities – and an excellent starting point for assessing any paper one reads (or writes):

a. What was the purpose of the research? What questions were asked and what hypotheses tested? Why are these questions and hypotheses of interest?

b. What data were used? Are they reliable? Are they relevant? Are they sufficiently rich to give answers to the important questions? What would constitute ideal data for the problem at hand?

c. What theoretical assumptions (both explicit and implicit) are needed in order to draw inferences about causation from the data?

d. In what direction should further work proceed?

Henry Brady arrived a few months after I did for his first stint on the Berkeley faculty. His appointment was in the public policy school, and perhaps for that reason I never took a course from him, but he served as the "outside" member of my dissertation committee and we interacted frequently at the Survey Research Center (which he now directs). Indeed, one of the most important aspects of my methodological education at Berkeley was the SRC's informal lunch seminar, in which Achen, Brady, and Jim Wiley were leading figures. The usual practice was to pick a book and march through a chapter or two each week. The readings were pretty eclectic, ranging from mathematical psychology to simulation models. I remember learning a good deal from reading Lord and Novick's *Statistical Theories of Mental Test Scores* (which has finally filtered into political methodology due to the recent enthusiasm for item response models), Clyde Coombs' *A Theory of Data* (still vastly underappreciated, except perhaps by Brady), and Cortés, Przeworski, and Sprague's *Systems Analysis for Social Scientists*, among other works. More importantly, I was inspired by the opportunity to be part of a group with such high intellectual standards and such tangible enthusiasm for collaborative methodological exploration.

A few years ago I was scheduled to give a talk at Berkeley and happened to arrive at the Survey Research Center at lunchtime. I went looking for Brady in his august director's office, but found him in the old seminar room leading a large and lively group of students, faculty, and staff members in an informal discussion of a book on smoothing algorithms. As I slipped into a vacant seat to listen I felt very much at home, very pleased that the old traditions were still alive and well, and very grateful for the values I had absorbed during my own years at Berkeley.

Part III

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During my undergraduate studies at the University of Queensland, David Gow told me that I should go to America for graduate study. The Australian system, he argued, was a copy of the British system: after an undergraduate Honors degree one sat at the feet of one's advisor and one learned whatever it was he or she was yet to teach you, and hence was narrow, *ad hoc*, and could be a hit-or-miss affair. The American system, with its emphasis on graduate coursework and exams, generated excellence more reliably, and was the option I ought to pursue. I followed David's advice, and benefited tremendously. All the same, my training in political methodology seems to owe much to luck, to simply being around the right people at the right time.

Undergraduate Training: After three years as an undergraduate I had become deeply skeptical about the use of quantitative methods to study politics. I vividly remember two reactions while leafing through the *APSR*. One was purely ideological and literally sophomoric, a reflection of the content of my undergraduate studies up until then: models, tables, graphs and – of all things – survey data were staples of the hegemonic, neo-liberal program, and, as such, to be regarded with suspicion, if not derision and outright hostility. On the other hand (and this was not a popular view with most of my classmates), I was impressed by the seriousness with which American political science appeared to conduct itself. Staring at the pages of the *APSR* it dawned on me that vast numbers of (predominantly American-based) academics were building careers in pursuit of the notion that politics could be studied with categories and theories that transcended particulars of time and place, and that statistical modeling was a fruitful way to go about this.

Then I got to meet one of these people. In 1986 David Gow took up an appointment at the University of Queensland. Originally from Sydney, David had spent about 14 years in American political science, both as a graduate student and most recently as a professor.[1] Gow was and is a True Believer, one of the small group that helped found the Political Methodology section in the early 1980s and a student of the history of political methodology.[2] My friend Bruce Western (now Professor of Sociology at Princeton) and I were intrigued; here was Gow, an Australian, but conforming almost exactly to our imagined stereotype of one of those *APSR* characters. Closer inspection revealed that in fact, Gow *is* one of those *APSR* characters![3] Gow was invited to give a seminar in the Honors sequence at UQ; later I would recognize his talk a masterful introduction to what is often called scope and method in political science PhD programs. Over the next year or so, in a series of impromptu late night tutorials, Gow led me on a tour spanning Karl Popper, Fisher, Neyman, Pearson, and Przeworski and Teune.

By now I was hooked. At Gow's urging, I took a class in econometrics and immersed myself in the American literature on voting behavior, eventually writing an undergraduate Honors thesis under David's supervision. Late night sessions on statistics and computation were also incredibly valuable: Gow was writing a companion computing volume to the 2nd edition of Judge et al.'s *Introduction to the Theory and Practice of Econometrics*, implementing almost everything in the book using SAS's `PROC MATRIX`, coding up Monte Carlo experiments to demonstrate properties of estimators in different scenarios, implementing some Newton-Raphson algorithms for simple MLE problems and the like. Getting this level of exposure to statistical programming and early on was incredibly valuable; my later transitions to `gauss`/`Splus`/`R`/`C` etc were relatively easy, and coming into graduate school with a reasonable degree of programming skill helped me secure research work in the summers (an especially important consideration for a foreign graduate student).

Finally, Gow and Bruce Western helped me understand that I could and should pursue a PhD in the United States. Gow had shown me Mo Fiorina's *Retrospective Voting in American National Elections* which struck me as an incredibly impressive piece of work, and as the kind of research I would like to do (cutting edge empirical work on political behavior informed by formal models), and Rochester seemed a great place to get that requisite training. Moreover, if Rochester was good enough for Fiorina in 1968 it would be good enough for Jackman in 1988, and besides, it was in New York State and hence close to New York City, right? Dick Niemi called to say I had been admitted, and I accepted over the phone. My professional socialization intensified: more statistics, the Australian version of the ICPSR summer school, an Honors workshop presentation on logit/probit, even a conference paper implementing robust regression co-authored with Western and Gow. Since the Australian academic year runs on the calendar year, I delayed my start at Rochester until January 1989.

Rochester/Princeton: The PhD methods sequence at Rochester in the late 1980s consisted of an introduction to probability and statistics taught by Linda Powell (which I missed, given my late arrival from Australia) and a Gujarati-level econometrics class taught by Dave Weimer (both required classes). Larry Bartels taught an advanced class in the 2nd year of the program, a quick review of regression models, before a set of topics covering discrete choice, systems of equations and measurement models (factor analysis and analysis of covariance structures). There was no required text but Larry's syllabus suggested that we own

[1] Bruce Stinebrickner, a Yale PhD (and co-incidentally, a classmate of Chris Achen and Neal Beck), was Head of Department at UQ at the time, and was instrumental in bringing Gow to Queensland.

[2] e.g., David John Gow, "Quantification and Statistics in the Early Years of American Political Science, 1880-1922", *Political Methodology*, V11 (1985): 1-18.

[3] e.g., David John Gow, "Scale Fitting in the Psychometric Model of Judicial Decision Making," *American Political Science Review* V73 (1979): 430-441.

Hanushek and Jackson's *Statistical Methods for Social Scientists* or Johnston's *Econometrics Methods*. Glancing over my file of homeworks and lecture notes from Fall 1989 I see that for anything beyond regression, our homeworks were surprisingly light on exercises with real data. A typical Bartels homework from the late 1980s for an advanced class would be to show that a systems of equations or covariance structure setup was or wasn't identified. Larry also ran a fourth, workshop style class where students worked up projects into papers; the thrust of this course was to get us doing more than running econometric bells and whistles through political science data, but to work hard on identifying the substantive issues and precisely how a particular model or estimator would shed light on these issues.

In none of my Rochester methods classes were there lab sessions or classes devoted to implementation with available software; I think Larry's position was that we would figure that out for ourselves, and that teaching us how to figure out what is *estimable* is more important than the mechanics of *estimation* itself. Today it difficult to imagine a graduate methods class that would be so "hands-off" with respect to issues of implementation (my own teaching style is to switch over from theory to examples and real data and analysis in `R`); although frankly, with today's programs capable of optimizing user-defined likelihood functions, or the relative ease with which one can toss fanciful models into `WinBUGS`, it might not be such a bad thing if graduate methods classes spent more time on something as fundamental as identification.

While at Rochester I took a class on discrete choice econometrics in the Economics Department: a good deal of the class was spent on properties of maximum likelihood estimators (i.e., ch 4 of Amemiya's *Advanced Econometrics*), and definitions of various types of models (type 5 tobit, anyone?). This class was both eye-opening and incredibly frustrating. On the one hand, my econometric and statistical literacy sky-rocketed; on the other, in 14 weeks of lectures not once we did look at an actual application of any of the models with data. I've seen this pattern replicated repeatedly; good students go to Economics departments looking for advanced methodological training, and return having been exposed to a lot of asymptotics, but with their data analytic skills unimproved or degrading. As a result I try to steer students towards genuinely applied econometrics classes (which usually aren't in the econometrics PhD sequence at a place like Stanford, and are more likely to be in labor, public finance or trade), or to the Statistics Department.

The dearth of actual data analysis in the advanced classes I took had several causes, not least of which was the computing power available to political scientists at the time. Harold Stanley's 286 was the fastest machine in the Rochester Political Science Department (in the summer of 1990 Harold let me use his office and machine to run the Monte Carlo experiments that appeared in Larry's quasi-IV paper[4]), and anything beyond linear regression was still costly. For instance, as a graduate student at Rochester in 1989, ordered probit usually meant a trip to the basement computing lab, cosying up to a monochrome green VT102 terminal and running LIMDEP on a remote IBM mainframe. Few graduate students owned their own desktops, and the PC lab at Rochester comprised only 3 or 4 painfully slow, loud, and hot XTs (largely faculty discards); this meant that the few PC-based programs for advanced modeling were beyond my reach.[5] The bottom line was that up until about 1991, at least at Rochester, anything beyond regression was a mainframe-only chore. And data analysis as we know it today (e.g., interacting with the data via spreadsheets and graphics) was just unimaginable on a mainframe, circa 1990.

Sometime around 1991-92 a lot changed, and quickly; as it turned out, my first years of graduate school (1989 and 1990) were the tail-end of the mainframe era. PCs got faster (and kept getting faster), and color monitors started showing up in computer labs. It became increasingly apparent that I needed to get my hands on `gauss`; Gary King was using the `maxlik` routine in `gauss` to implement MLEs, and Neal Beck had used it to implement a Kalman filter in a *Political Analysis* article. On a trip out to visit Bruce Western at UCLA I first saw `Splus` running on a PC in the impressive Social Science Statistics Lab there; the first time you click on a scatterplot to identify an outlier is a wondrous moment, and my approach to statistical computing and data analysis was forever changed.[6] I also discovered some new Sun workstations hidden away in a quite corner of the Rochester computing center; `Splus` running under the `X11` windowing system on the seemingly huge greyscale Sun monitors was tremendous fun, and if it meant going to another building to do my work, well so be it.[7] The Internet started to be more than an e-mail network, and things like `ftp` meant data and code was suddenly easy to share.

But overshadowing these technical advances was that in 1991 Princeton stole Larry away from Rochester. This was momentous; I teased Larry that I would kidnap his kids if I didn't get to go along with him (he assured me that that wouldn't be necessary), and in the end I wound up spend-

[4]Larry M. Bartels, "Instrumental Variables and 'Quasi-Instrumental' Variables", *American Journal of Political Science* V35 (1991): 777-800.

[5]For instance, Jeff Dubin and Doug Rivers' SST is/was a marvelous piece of software, managing to squeeze respectable performance out of the computing power available at the time.

[6]See my enthusiastic embrace of the brave new world in "`GAUSS` and `S-PLUS`: a comparison." *The Political Methodologist* V6, No.1 (1994): 8-13.

[7]From that point on my statistical computing slowly shifted from PC based to being based around various NIXs (UNIX, HP-UX, Linux and now Mac OS/X).

ing three years as a visiting student in the Woodrow Wilson School. Don Stokes, the then Dean of the Wilson School, had made sure Larry was well catered for on the research side, with fellowships and office space for RAs. In turn, Larry entrusted me with ordering hardware and statistical software for his fledging group, and I obliged with getting the fastest PCs then available, and a nice suite of statistical software. Best of all, Larry was only two doors away, and he was incredibly generous with his time; in addition to serving as my dissertation advisor and mentoring me in the ways of the profession, Larry helped me explore the world of Bayesian statistics that I was increasingly interested in.

Bayes. In the winter of 1990 (my 2nd year at Rochester), I was the TA for the PhD regression class. Larry Bartels was invited to give a guest lecture on Bayesian approaches to econometrics. I knew nothing of the Bayesian approach at that stage, other than that it involved the use of prior information, which sounded uncontroversial enough. Bartels' lecture was the first and still the best Bayesian critique of frequentist statistics I've ever encountered. I'd always been troubled by aspects of the frequentist approach, at least as conventionally practiced in social-science settings: point null hypotheses, research findings that live-or-die at point-oh-five, inferential procedures that rest on properties of statistics over imaginary, repeated applications of sampling processes. Bartels' critique brought all of that together, showing that Bayes provided a coherent framework with which to make the probability statements about parameters that it seemed everyone wanted to make (and often do make), but are not what the frequentist approach supports.[8] My memory of this lecture was that it quite revolutionary in its implications; Larry reminds me that as a minimum, he wanted to show people that one could still run regressions as usual, but interpret the results in a more sensible, Bayesian way (i.e., with the right ignorance priors, least squares regression estimates are what a Bayesian would report as the posterior mean for β, etc). In light of the demolition job I thought Bartels had just delivered, I wondered how I could go back upstairs and run and report regressions with any degree of self-respect. Nonetheless, upstairs we went, back to whatever it was we were doing, some of us giving lip-service to the Bayesian approach in our interpretations of parameter estimates and their standard errors. But that really was that. Bartels' one hour lecture was as much Bayes as one would encounter in the Rochester quantitative methods program, although probably an hour more than one might see almost everywhere else in a political science PhD program in 1990. There was no Bayesian community at Rochester to go for additional guidance: again, this was 1990, before the mid-1990s, MCMC-led Bayesian revival.[9]

On the other side of the country, in UCLA's Sociology program, Bruce Western was encountering a similar critique of conventional, frequentist practice, largely led by Dick Berk. On a summer trip out to LA, Bruce and I sat in on a biostats lecture by Rod Little: this was the first time I'd seen a practicing Bayesian in full-flight with applications, unapologetic about the use of priors, pointing out that one could "sod the data" if one's priors were sufficiently stringent. One of Little's texts was Peter Lee's 1989 book, the source of the quote in the footnote, above.

Back at Rochester, Dick Niemi passed along a paper by Andrew Gelman and Gary King using Bayesian methods to estimate seats-votes curves that appeared in *JASA*.[10] Niemi had shown me the seats-votes setup in my first year of graduate school (leading to my first two refereed publications, both co-authored with Niemi), but Gelman and King had clearly taken the literature to another level. My work with Niemi relied on the "multi-year" method a la Tufte to estimate seats-votes curves (and hence, estimates of electoral bias and responsiveness); each election contributes a x and a y (votes and seats shares, respectively) to a data set that one would then use to estimate seats-votes curves via a log-odds on log-odds regression. This method was useful in generating sweeping, historical characterizations of electoral systems, but not especially useful in assessing the bias and responsiveness of a just-implemented or yet-to-be-implemented districting plan in a specific jurisdiction. Gelman and King solved this problem with an elaborately parameterized simulation model (including a mixture model and multi-year analysis to bound the magnitudes of year-to-year, district-level shocks) all wrapped up in a fully Bayesian framework: estimation employed the EM algorithm and the Tanner and Wong data augmentation algorithm, the latter a special case and forebearer of the Gibbs sampler (the workhorse MCMC algorithm). I was enormously impressed by the statistical sophistication applied to a problem I had worked on myself, and, in particular, how helpful Bayesian simulation methods could be: coming up with a flexible parameterization to attack a problem is one thing (and impressive in itself), but it was clear to me that the computational

[8] I shared Peter Lee's confusion: "When I first learned a little statistics, I felt confused... Not because the mathematics was difficult...but because I found it difficult to follow the logic by which inferences were arrived from data.... the statement that a 95% confidence interval for an unknown parameter ran from -2 to +2 sounded as if the parameter lay in that interval with 95% probability and yet I was warned that all I could say was that if I carried out similar procedures time after time then the unknown parameters would lie in the confidence intervals I constructed 95% of the time." *Bayesian Statistics*, Oxford University Press, 1989, p.vii.

[9] Only after leaving Rochester did I realize that Martin Tanner was over in the biostatistics department. Tanner's *Tools for Statistical Inference* went through three editions with Springer, and was a very useful resource for me as I found my away around the fast-moving Bayesian literature in the mid-1990s.

[10] Andrew Gelman and Gary King "Estimating the Consequences of Electoral Redistricting", *JASA*, V85 (1990): 274-82.

tools Gelman and King used were incredibly powerful and of widespread applicability.

Shortly after the move to Princeton with Bartels, armed with a fast machine of my own and a copy of `gauss`, I set about replicating the Gelman and King setup. This work was far removed from my dissertation, and a less tolerant advisor would have warned me off it. Instead, Larry generously helped me decipher the Gelman and King article, helping me work through the Bayesian analysis of the mixture model ("...the Dirichlet is conjugate to the multinomial...") and exactly what was being computed at each step of the way. Under the guise of "I'm working on something for Larry," I took over a conference room in the Woodrow Wilson School for a week, whiteboarding up my assault on the Gelman and King piece; in addition to being my first foray into serious Bayesian computation, I was also learning `gauss` by throwing myself in the deep end. Gary King patiently answered many e-mails seeking help or clarification. I then disappeared into Princeton's fabulous Firestone Library, collecting Australian election returns, which I then fed to my implementation of the Gelman and King setup (with a slight generalization to deal with the rampant malapportionment in Australian jurisdictions). The result was a *BJPS* piece, far and away the longest and most technical thing I had done up until that time, convincing me that perhaps there was a future in this Bayesian business.

Around the same time Bruce Western and I worked up a draft of what would become an *APSR* piece.[11] We wrote this piece after realizing that via largely independent routes (Bartels for me; Dick Berk for Bruce), we had arrived at almost identical positions regarding Bayesian versus frequentist approaches in statistical work in the social sciences. We argued that repeated sampling was an inappropriate foundation for statistical inference in many settings (in particular, cross-sectional statistical analysis in comparative politics); Bayes, we argued, provided a solution to this problem, as well as a way to formally incorporate the often-abundant but non-quantitative prior information available to students of comparative politics (e.g., historical accounts and narratives).

I presented this work to the Political Methodology summer meetings at Florida State in 1993, and was shocked by the hostility of the reaction from the audience. I later realized I had walked into a mine field, that many in the Methodology group had encountered the Bayesian/frequentist debate (in the aftermath of Ed Leamer's wonderful 1978 book, *Specification Searches*), and were letting me hear their well-rehearsed positions (and all at once, or so it seemed). Punch-drunk, I flailed around trying to salvage my talk from the constant interjections flying across the room, even failing to recognize helpful comments from people like Chris Achen, friendly to the Bayesian position. Afterwards, John Jackson took me aside to cheer me up, consoling me with the view that the liveliness of the session was an indicator of the impact of the ideas in my talk. Throughout it all, Larry said nothing, not that I expected him to, and fair enough too; nothing that happened to me that morning in Tallahassee was unfair or unprofessional. In retrospect I was woefully under-prepared to give *that* kind of talk to *that* kind of audience: the parts of the Bayesian program I was stressing in the talk — subjective probability and informative priors in a small n setting – were uncontroversial to Larry, Bruce, and myself, but clearly the rest of the profession wasn't so convinced.

The paper attracted a mixed set of reviews at the *APSR*: one reviewer was enthusiastic, one was doubtful, and another took the curious position that while Bayesian ideas were of course the right way to approach the problems we laid out, graduate students shouldn't be given journal space in the *Review* to make those arguments. Larry encouraged to us hold our ground in revise-and-resubmit, and in the end our piece was published. But the experience led me to be pessimistic about the prospects for the widespread adoption of Bayesian ideas in the profession. Besides, I'd invested considerably in the then-emerging Bayesian computational tools (data augmentation, the Gibbs sampler), and I could see tremendous payoffs down that less controversial avenue, where, for the most part, the priors are vague and the analysis via MCMC is formally Bayesian, but, for all practical purposes, is "maximum likelihood by any other means". We'll see what can be done about that in the years ahead.

Post-Graduate. When I joined the faculty of the University of Chicago in 1994, I lucked out again. Chicago's Business School was home to Arnold Zellner, the leading Bayesian econometrician of his generation, meaning that the Chicago GSB was a Bayesian beachhead of sorts. A group of younger scholars there were beginning to ride the Bayesian/MCMC wave, in particular, Peter Rossi, Rod McCulloch and Nick Polson. In addition, in my first year at Chicago the Statistics Department there ran a speaker series devoted entirely to MCMC. McCulloch and Polson met gave me helpful advice on how professional statisticians really *did* MCMC; in particular, McCulloch pointed out that implementing MCMC for non-toy problems usually meant writing your own code in a real programming language like `C`, and Polson gave me a gentle introduction to the literature on convergence results for MCMC algorithms.

Stanford has been a wonderful place for my continuing development as a methodologist, and principally because of my senior colleague Doug Rivers. Doug's methodological training is perhaps the one we should be reading about.

[11] Bruce Western and Simon Jackman, "Bayesian Inference for Comparative Research," *American Political Science Review*, V88 (1994): 412-23.

Rivers excels at so many facets of all that we call methodology and has been nothing short of an inspiration for me. Doug was initially quite skeptical about MCMC and the Bayesian approach more generally (I'd say now that position has moderated to "a little skeptical"); Doug's blend of skepticism, enthusiasm, but above all, a demand for rigor, has been immensely valuable for Josh Clinton and myself in our joint work, using Bayesian models to analyze rollcall data. Via my colleague David Laitin, I was introduced to Persi Diaconis (quite simply, one of the world's most important living Bayesians), who has also been a gracious and valuable resource for me and my students.

In short, I've been extremely lucky: Gow and Western turning me on to quantitative social science, and helping get me from Australia to Rochester; riding Larry's coattails from Rochester to Princeton and Larry presenting me with the Bayesian fork in the road; a job at Stanford, great students, and Rivers' interest in using Bayes to move the football on a range of problems. I don't regard my methodological education as complete: a good methodological training gives us what we need to keep learning over one's career (indeed, anything less has to be considered insufficient in our field). I hope my luck continues.

Part IV

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My instruction in quantitative methods took place between 1996-2002 while a student in the Political Science Department at Stanford. Although I would not presume to claim that my path was either typical or ideal, I hope that a recounting of my experiences is at least somewhat informative (if only to suggest to my students that having to self-learn more than one statistical package is not an exceptional burden). Before describing my experience, some context might be useful. I entered Stanford straight from the University of Rochester with aspirations to work at the intersection of formal and normative theory. Consequently, I entered graduate school having taken only three classes in quantitative methods (one class each in: Political Science, Economics and Statistics). Following graduation, I left Rochester for Stanford almost immediately – spending the summer in Stittville, NY seemed a less exciting prospect than spending it in the Bay Area. While working for John Ferejohn and Doug Rivers on a project that went nowhere that summer (but which was nonetheless educational), I took Real Analysis in the Math department and a refresher "Math for Economists" class. Although perhaps a bit clichéd, the summer was notable in that it truly opened my eyes to the excitement offered by statistical investigations. Many times John, Doug and I would huddle in front of a computer – monitors were much smaller then – to analyze data I had collected. The collaborative give-and-take and the excitement of seeing whether our hypotheses were supported by the data was unexpectedly rewarding; I began to reassess my planned course of study.

When I formally began the program in the fall, I took the department's three class quantitative methods sequence consisting of: probability and inference (Hamilton *Regression with Graphics*), linear models, and a somewhat mysteriously vague class entitled "Topics" class (which used Greene's *Econometric Analysis* and turned out to cover limited dependent variables and advanced topics in linear models using matrix algebra (e.g., panel data, GLS)). The "bad news" was that the department had only one faculty teaching the three methods classes. The "good news" was that the one faculty member was Doug Rivers – who was both exceptionally clear and insightful. All three classes were more theoretical than applied – perhaps in part due to the fact that the department's computer "lab" at the time consisted of 8 woefully out of date and underpowered computers (only 4 of which seemed to work reliably on any given day and the 4 that worked varied daily). What computing assignments there were used Excel and SPSS (first class), SPSS (second class) and SST (third class).

Having exhausted the department's offerings, Doug's advice was to take more classes in the Economics Department. There were a plethora of Ph.D. methods sequences at Stanford – Political Science, Economics, the GSB, Statistics, Sociology, Psychology, and Education (to name a few) all had sequences. Although it was possible to have quantitative methods as first or second field, to do so required going outside of the department and taking the (first year) econometrics sequence and exam in Economics. Given the size of the department, the philosophy was to try not to replicate what was available elsewhere – why have the department teach a class in econometrics when it was possible to cross the street and take it from Amemiya? Consequently, in my second year, several of us crossed the street and took the three course econometrics sequence consisting of: introduction to probability and linear models (Amemiya's *Introduction to Statistics and Econometrics*), linear models (Goldberger's *A Course in Econometrics*), and GMM (Davidson and MacKinnon's *Estimation and Inference in Econometrics*). The sequence was almost exclusively theoretical – with homework problems on deriving estimators'

[12] Here is a sample question from an (in-class) midterm from the first class : "The probability distribution of X is given by $P(X = 1) = \theta^2$ and $P(X = 0) = 1 - \theta^2$, $0 \leq \theta \leq 1$. Suppose the sample of size n from this distribution produces r ones and $n - r$ zeros. Obtain the maximum likelihood estimator of θ (NOT of θ^2) and prove its consistency and asymptotic normality directly without using the general theory of consistency and asymptotic normality of the maximum likelihood estimator. Carefully indicate which convergence theorems you are using at each step of your

asymptotic properties rather than on analyzing and interpreting data.[12] What data analysis there was in the second and third classes required the use of MATLAB. While taking the econometrics sequence, Alan Wiseman somehow talked Doug Rivers into teaching a second "topics" class for a small group of interested students which worked through Kalbfleisch and Prentice's *The Statistical Analysis of Failure Time Data*. The class was notable in that Doug never got any actual credit for teaching it (something that only now, on "the other side," do I fully appreciate); his compensation consisted of being treated to dinner by those who took the class (which is quite another story).

My decision to study quantitative methodology intensively was finalized by Simon Jackman's arrival in my second year. It was TAing for Simon in my third year (who was teaching the second course and co-teaching the third course with Doug) that the importance of visualizing and interpreting data was stressed – a point never emphasized (or even really mentioned) in my econometrics classes. Simon also (thankfully) introduced me to the benefits of S/R and LaTeX and he further urged me to take classes in the Statistics department (which I took while TAing in my third year).

The Statistics sequence (which used Rice's *Mathematical Statistics and Data Analysis*, Weisberg's *Applied Linear Regression* and Chambers and Hastie's *Statistical Models in S*) was very focused on the application and interpretation of statistical models – although the data being interpreted seemed invariably to pertain either to the Stanford Heart Project or the classification of crabs. Although the classes were in no means "cookbook," the emphasis was on understanding the data and choosing appropriate analytical methods. Thankfully, the statistics department consistently used S/R in its classes. This was in contrast to my prior experiences in which every class "supported" (i.e., required) a different program. While taking classes in the statistics department I also took a time series class in Econometrics department (which used Hamilton's *Time Series Analysis* and TSP) and Simon and Doug combined to team-team teach a class on Bayesian statistics to a small group of us (using Tanner's *Tools for Statistical Inference* and Gelman et. al. 's *Baeysian Data Analysis* with S and WinBUGS). At the time, no department offered a class in Bayesian methods and although the word "Bayesian" was occasionally uttered in the econometrics sequence, the sequence was taught almost exclusively from a frequentist (or "classical" depending on your position regarding Bayesianism) perspective. The Statistics classes I took never even mentioned the word except for when Bayes' Rule was discussed.

Although I persisted in taking additional statistics classes, the more (if not most) valuable experience I received was from working directly with Simon and Doug. During my third year of graduate school Silicon Valley was in full bloom with new companies starting everywhere. Doug Rivers started a company (then InterSurvey, now Knowledge Networks) at this time and, following my year TAing for Simon and Doug, I took a summer job at the company to help pay the ridiculous Bay Area rents. Besides getting my hands dirty – sometimes much too dirty – working on issues dealing with survey research, sampling, and sample quality, the work provided an opportunity to apply the tools I had learned in my classes (and explore potential dissertation topics given my access to survey data). My time at Knowledge Networks became especially valuable once Simon was hired as a consultant to oversee election polling. Since Simon and Doug were down the hall from me at Knowledge Networks, I was able to easily track them down and talk about research issues. Our proximity also led to us collaborating on several projects. I fondly remember Election Night (and early morning) 2000; Simon, Doug and I had spent the summer developing a electoral college forecasting model using state-level polls and Knowledge Network samples. Simon and I spent the evening (and early morning) in the office watching as election results trickled in – comparing our predictions (and those of other survey-houses) to the reported results and trying to diagnose what went wrong in the model when our predictions were mistaken. Although it was certainly a "geeking out" experience, doing "real-time" political science was a nice change from the usual up-late-at-night-and-I-need-another-cup-of-coffee-so-I-can-finish-coding-up-this-estimator/data experience that normal research seems to entail.

Working with Simon and Doug was critically important to my education because it was through such interactions that I fully understood the context of my class knowledge. It is one thing to be able to complete a problem set, it is quite another to define a reasonable (and hopefully interesting) research question, define and collect the data needed to answer the question and choose the proper analytical tools. This knowledge is best learned by doing, and I was fortunate to have two professors who were exceptionally accessible and willing to assist as I began my stumble towards discovering (hopefully at least some of) the answers.

While consulting for KN, Simon was successful in securing a grant from Stanford to establish a computing center with office space and workstations for graduate students interested in statistical methods and visualization. After spending the summer working closely with Simon, this arrangement provided me with both excellent facilities and, more importantly, the continued ability to bug Simon about issues/concerns that arose in my research. Although I benefited from this arrangement, it probably was not the best arrangement for Simon, whose leave year was interrupted

derivation. Compare the asymptotic variance you have obtained with that indicated by the general theory of the maximum likelihood estimator."

more frequently than he would have wished (another thing that I understand having reached the "other side").

My methods training was "messy" in that it drew from many different classes in many different departments – each of which took a different perspective on what it meant to analyze data and evaluate models. Although not all that I learned has (yet) been useful to me, what was informative was the exposure to the different perspectives taken by economics, statistics and political science. However, when I look back to my graduate education, what cemented and defined my training was the ability to work closely with Simon Jackman and Doug Rivers. Their perspectives, suggestions and criticisms enabled me build on the foundational knowledge provided by my classes. The classes I took at Stanford provided me with a set of tools (and the ability to learn new tools), while interactions with Simon and Doug provided a context for using the tools and moving from task of completing problem sets to the ongoing task of conducting original research. I can only hope that I am able to provide similarly valuable advice to the students that I now teach.

Software notes

Open Source Spatial Data Analysis

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Spatial econometric techniques have so far not enjoyed widespread popularity in political science research, but this is a trend that appears to be changing course. The reasons are, of course, complex, but likely are rooted in three factors. First, there is a growing availability of geo-coded data. Second, the spatial toolbox has been burgeoning in recent years. Finally, and certainly a very significant factor, the software is now moving to the freely downloadable and/or open source realm, and so is readily available to any and all who are interested in conducting spatial analyses.

The primary advantage that accrues from analyzing the spatial dimension of a certain set of data is that we can move away from theories that incorporate only individual decision-making, whether across time or in a singular incident, in an isolated realm. That is, the individual need no longer be seen as an atomistic actor. Instead, we can consider theoretical frameworks that place the individual's actions in the context of his "neighborhood," where behavior can be compared to and observed in relation to the behavior of others in close proximity. Certainly, most of us would contend that our actions on practically any realm are often due not only to socio-economic factors, but also to our social context. Over and above our personal demographic profile, we are profoundly influenced by our neighbors, family, and colleagues. Accordingly, being able to account for context as well as individual level factors is appealing from a theoretical point of view.

Perhaps not surprisingly, spatial analyses are important not only for substantive reasons, but for statistical reasons as well, and these two dimensions are inextricably linked in this context. On the substantive front, spatial models allow us to examine critically theories about the political behavior of individuals in the proper context. Aspatial models omit this spatial component and thus allow one to examine the individual primarily as an atomistic actor only. Statistically, if spatial processes underlie the behavior of interest but are not accounted for in the model, inferences will be inaccurate and coefficient estimates may be biased. Erroneously ignoring spatial dependence (in the form of a spatial lag) may create bias and inconsistency in the same way that we understand the omitted variable problem to affect OLS estimates (Anselin 1988, 1990). Alternatively, when the spatial error structure is ignored, simple inefficiency is apparent in the estimates but the standard errors are biased (Anselin and Griffith 1988). Hence, even if one were not interested specifically in the spatial effect but only in the aspatial effects, omitting the possibility of a spatial aspect from the model may affect the interpretation of the results, spatial and otherwise. The case for spatial analyses, then, is compelling indeed.

The purpose of this article, however, is not to discuss spatial analysis per se, but simply to review the newly available software. In this endeavor, I limit my discussion to free software, both that which can be downloaded for free (GeoDA) and that which is part of an open source environment (the spdep module for R). In both these cases, the software is currently "under construction," but under active development with new functionality being added rapidly. In addition, limiting the discussion to this set of software is not much of a limitation, since the open-source arena is currently the most active area of development and is likely to be the primary source shortly.

GeoDa

GeoDa is an acronym for Geographical Data Analysis, and is a free software program designed by Luc Anselin and his assembled team. As the name suggests, its function is to provide a user-friendly graphical software tool for spatial data analysis. The package is a stand-alone, easy to use, visual and interactive software package, aimed at non-GIS users. GeoDa is still in the developmental stages (version 1.0 has yet to be released), but the current version (0.9.5-i) is highly functional in many regards.

Indeed, the current version of GeoDa has functionality ranging from simple mapping to exploratory data analysis, the visualization of global and local spatial autocorrelation and spatial regression. Notably for users of Stata who have grown accustomed to Stata always having the latest and greatest, this is an area that Stata has yet to conquer. In fact, functionality in Stata is sufficiently limited that what Stata has implemented is too primitive for most users. In contrast, GeoDA helps the user perform some simple mapping and geovisualization of data, provides tools for exploratory spatial analyses, allows one to examine spatial autocorrelation, and provides tools to run and analyze spatial regressions. Users who are familiar with SpaceStat will notice some similarities in functionality, though GeoDA excels in that it provides a vast array of graphical tools that were not available in SpaceStat.

The functionality of GeoDA can be classified into 6 categories

1. Spatial Data Manipulation and Utilities
   - data input from shape file or text
   - data output to text
   - grid polygon shape file creation from text
   - centroid computation
   - Thiessen polygons
2. Data Transformation
   - variable transformation and variable creation
   - spatial lag variable construction
   - rate calculation and rate smoothing
   - data table join
3. Mapping
   - Choropleth Maps
   - Standard deviational maps
   - percentile map
   - outlier map
   - circular catogram
   - map movie
   - conditional maps
   - smoothed rate map
   - excess rate map
4. Exploratory Data Analysis (EDA)
   - histogram, box plot, scatter plot, etc.
5. Spatial Autocorrelation
   - spatial weights creation (rook, queen, distance, k-nearest)
   - higher order spatial weights
   - spatial weights characteristics
   - Moran and Local Moran statistics with inference and visualization
6. Spatial Regression
   - OLS with diagnostics
   - ML spatial lag and spatial error model
   - predicted value and residual maps

In summary, GeoDA is highly functional, is still a work in progress, but under active development. At present, GeoDA runs only in a M$ Windows environment, but plans are under way to bring the code to a cross-platform and open source environment. Once, the open source environment is integrated, the sky's the limit and one can easily imagine the rate of progress increasing to barreling speeds. In addition, efforts are being directed at shoring up the spatial regression component to include estimators other than ML, to include spatial panel data models, and to include models other than the discrete locations in the "lattice" case.

The GeoDA program is free and can be downloaded from the Center for Spatially Integrated Social Science (CSISS) software tools web site at

`http://sal.agecon.uiuc.edu/geoda_main.php`.

In addition, some support and training can be obtained from tutorials on the web site as well as informally through the Openspace mailing list. Signing up for the Openspace mailing list is through the web site

R: spdep

Users of the R statistical package will be pleased to find that spatial data analysis packages are also under active development in R. To boot, GeoDa and the spdep module for R are highly complementary. The main advantage of spdep over the current version of GeoDA is that spdep, like all modules in R, is customizable and fully extensible by users (though GeoDa seems to be moving in this direction with its trend toward open source). The main disadvantage may be that although some mapping capability is present, the spdep package, does not provide the dynamic linked visualization capabilities available in GeoDA.

The spdep package is created with other packages in mind, and so interaction between GeoDA and GIS packages like ArcView, while not completely seamless, are also not overly burdensom. For instance, spdep will read in shapefiles with the `read.shape()` command. Some simple plotting will then be possible through the maptools package and the `plot()` function. Spdep will also read in GAL and GWT weights matrices created by GeoDA as well as output these weights formats for use in other programs.

All the standard functions for basic spatial analysis are available in spdep. One can easily compute Moran's I and Geary's C, the local moran, Gi and Gi* statistics as well as assess their significance. A variety of weights can be computed: k-nearest neighbor; distance weights; contiguity; and higher-order contiguity. On the modeling front, spdep provides a battery of LaGrange Multiplier tests to help determine functional form and an implementation of both the spatial autoregressive error model and the spatial autoregressive lag model.

Another boon is that like all R contributed packages, the syntax should be familiar to R users. Running a regression and saving a regression object is done through the familiar `lm()` function call. Given this regression object and a spatial weights object, one can conduct a Moran's *I* test for residual spatial autocorrelation using the `lm.morantest()` function. Testing to determine model specification again uses the `lm` object along with the spatial weights object and can be achieved through the `lm.LMtests()` function that provides the Langrange multiple test statistics to help distinguish between spatial error and spatial lag models. The robust forms of the Lagrange Multipler test statistics are provided through `lm.LMtests()` as well. Finally, one can carry out maximum likelihood estimation of the spatial model though `lagsarlm()` (for spatial lag) and `errorsarlm()` (for spatial error). The format for the spatial regressions mirrors the `lm()` function, so that the command

```
> lagmodel <- lagsarlm(vote ~ age + income + edu,
                              data=nes, spwts)
```

produces a spatial regression object, `lagmodel`, that one can then examine via the familiar command, `summary(lagmodel)`, as one might have done with an `lm()` object. The difference is that one must specify the spatial weights matrix, a spatial regression is run, and a few different test statistics are reported. Veteran R users should feel right at home. The `errorsarlm()` function is similar in all respects, but runs a spatial error model rather than a spatial lag model.

A Simple Example

Following is a simple example just to walk through a set of commands that one might use in a spatial analysis. To begin, read in your data and load the spdep package. On a side note, as part of the spdep package, a number of data sets (along with associated weights matrices) are provided, so one can experiment with those data sets. Many of the supplied data sets are "famous" in that they have been analyzed by many researchers or been the subject of examples in textbooks, and so they are good pedagogical items.

```
# read in data
> puts4 <- read.csv("puts4.csv", na.strings=c("."))
           library(spdep)
> library(spdep)
spdep, version 0.2-8, 2003-12-19:
 a package for analysing spatial dependence,
 use help(get.spChkOption) for help on integrity
 checking
```

In my data set, I obtained the latitude and longitude coordinates by geocoding the data in ArcView and then saving them as part of my main data set. Next, I wanted to define distance based neighbors, so I used the `dnearneigh()` function. Also note that I wrote a little routine to convert miles to kilometers, since the default in spdep is kilometers.

```
# get latitude/longitude coordinates
> ptcoords <- as.matrix(cbind(puts4$X.coord,
                              puts4$Y.coord))

# distance based neighbors in kilometers

#(1 kilometer = .621 miles)

> milestokm <- function(miles) {
  return(miles/0.621)
```

```
}

> put4d1 <- dnearneigh(ptcoords,0,milestokm(2),
                       lonlat=TRUE)
```

There are two types of neighbor objects in spdep. I will skip the details here, but the spatial regression routine requires a `listw` object and the weights functions create an `nb` object. However, it is easy to convert one to the other. The main difference between the two types of weights object is format. The `nb` object has a list of vectors that lists the neighbors for each observation. The `listw` object has the neighbor list along with values for the spatial weights. The conversion command is simply `nb2listw()`.

```
# convert to listw object
> put4lw1 <- nb2listw(put4d1, zero.policy=TRUE)
```

You can run a regression as you normally would. In these particular data, I was interested in modeling political participation, so I included a raft of the usual suspects (age, income, etc.) along with some others. Be sure to save this regression object, since you will need to use it in other commands.

```
> pp4m <- lm(Polpar4 ~ Edu + Inc + Age + Female +
                       Black + Hisp + Asian +
                       Natoth + Polknow + Polint +
                       Ideoext + Macher + Trust +
                       Effcomm, data = puts4)
```

The first step in a spatial analysis is often to compute the Moran's I test statistic for spatial autocorrelation. There are two "tricks" to this. First, you need the `lm` object from above. Second, you need to pass it the spatial weights as a `listw` object rather than an `nb` object. We have both already. The `listw` object can then be passed along with the `lm` object to the `lm.morantest()` function. A significant p-value indicates that the data depart from a pattern of spatial randomness. The `print()` function provides some nice output.

```
> put4mmoran <- lm.morantest(pp4m,put4lw1,
                             zero.policy=TRUE,
                             alternative="two.sided")
> print(put4mmoran)
```

The next step given evidence of spatial structure in the data then is how to model the spatial structure. The search for model specification usually begins with a series of LaGrange Multiplier tests. These are available through the `lm.LMtests()` command. Several tests are embedded within this command, including the robust and standard test for the spatial lag and spatial error specification.

```
> put4mlg <- lm.LMtests(pp4m,put4lw1, test="all",
                        zero.policy=TRUE)
> print(put4mlg)
```

From the output, one might see that both `LMerr` and `LMlag` are significant, so then one would examine the robust forms and make a determination based on whether `RLMlag` or `RLMerr` had a lower p-value.

If the diagnostics led to a spatial lag model, then this would be simple to implement and achieved through the `lagsarlm()` (Lag Spatial Autoregressive Linear Model) function.

```
> put4mlag <- lagsarlm(Polpar4 ~ Edu + Inc + Age +
                                 Female + Black +
                                 Hisp + Asian +
                                 Natoth + Polknow +
                                 Polint + Ideoext +
                                 Macher + Trust +
                                 Effcomm,
                                 zero.policy=TRUE,
                                 data = puts4, put4lw1)

> summary(put4mlag)
```

Decisions on which model to run obviously depend on the output from the various commands issued. However, the example above, hopefully, gives a good flavor of how such an analysis would proceed.

Basic Overview

This is a very basic overview of the functionality in the latest software available for spatial analysis. Indeed, both GeoDA and spdep are in rapid development and their functionality will undoubtedly increase (likely markedly so) even between the time this article is written and when it appears in print. For instance, at present, because of computational complexity, it is difficult to run a spatial regression with a large number of observations, however, this is one area of active development that promises to be fully functional in the near future. There is much to look forward to in this rapidly developing field.

References

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Using R to Keep it Simple: Exploring Structure in Multilevel Datasets

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Point 1: Before starting R have a question you want to answer.

In the United States, the relationship between education and political participation in the cross-section is well known: better educated people tend to be the types that get involved in politics. Extant theories suggest that this relationship exists because (1) people who are better educated have more of the skills they need to do the work of political participation (Verba, Schlozman and Brady, 1995) and (2) people who are better educated are more likely to know other political actors, i.e. they have higher status in the kinds of social networks that matter for political involvement (Nie, Junn and Stehlik-Barry, 1996; Rosenstone and Hansen, 1993; Huckfeldt, 1979).

If we looked at the relationship between education and participation in other countries, would these theories travel? Would education as "civic skills," such as knowing how to write letters or chair meetings, have the same impact on civic activity in a country where nearly everyone is highly educated as in a country where very few people are literate? Probably not. Would education matter as much in a place where social status is conferred at birth as it does in a place where social status is tied to a college degree? Probably not. These suppositions raise the question about what might be determining who participates across a variety of places, where education plays different roles in society. Answers to this question might tell us something new about the societal bases of political activity, as well as providing new perspectives on 50 years worth of literature based largely on studies of ordinary citizens in the USA.

The question I posed about whether the theories of political participation travel is in essence a question about whether a relationship between variables measured at one level (among ordinary people) depends in some way on variables measured at another level (among countries). This article is written now because we are finally getting data to address such questions directly on a large scale via the Comparative Study of Electoral Systems (CSES Secretariat), the World Values Survey (Inglehart et al.), the African/Euro/Latino Barometers (The Afrobarometer Network; European Commission; Lagos and The Latinobarometro Corporation), and others. Also, within the study of domestic politics, more and more researchers are gathering data on attributes of both citizens and the electoral districts or other geographies in which they are embedded. In fact, I wager a bottle of Kwak (a Belgian beer) that the number of articles in political science journals concerned with quantitative analysis of datasets with more than one type of unit of analysis has more than doubled in the last 10 years.

A dataset used to answer questions about people nested within geographies tends to have fewer countries/districts/states (i.e., macro-level units) than individuals/towns/firms (i.e., micro-level units). And analyses based on such datasets tend to focus on how differences across macro-units somehow condition or influence the differences within macro-units.[1] To address the question I posed earlier, I use the World Values Surveys and European Values Surveys 1999-2001 combined file (ICPSR # 3975) for the micro-level variables of educational attainment and political participation. For macro-level data I use information from the OECD in 2003 on the percent of the population aged 25 to 64 who have a college degree. Since I am using these data to illustrate a series of techniques rather than to answer exhaustively an analytic question, I selected 25 countries, mostly from Europe, to show high variance in the country-level educational context. The political participation variable counts the number out of five possible activities done by individuals in the surveys.[2] The final dataset contains 36,174 people across 25 countries, and the amount of information available within countries ranges from 968 peo-

[1] I am leaving time out of this article to keep it simple.

[2] The activities are signing a petition, joining in boycotts, attending lawful demonstrations, joining unofficial strikes, or occupying buildings.

[3] The dataset used to produce this article is available for download at `http://www.umich.edu/~jwbowers/papers.html`. The article itself was produced using Sweave (`http://www.ci.tuwien.ac.at/~leisch/Sweave/FAQ.html`). Sweave is a system for embedding R code and output within a LaTeX document. That is, this article was produced from one single file that contained both text in LaTeX format, and chunks of R code. Sweave ensures that R code is printed out with ">" to mark the beginning of a command line and a "+" to indicate that a command line has continued. In the actual R code I typed the commands without ">" or "+" characters. See my file for an example of how this was done. An excellent introduction

ple in Iceland to 4607 people in Turkey (50% of the countries have between 1015 and 1522 cases). Here is what the data set looks like for a few survey respondents:[3]

```
> fulldat[c(1:5, 36169:36174), c("country",
+     "id", "partic", "educ2", "pctcollege")]

            country     id partic educ2
1           Austria  26875      0     1
2           Austria  26876      0     1
3           Austria  26877      1     3
4           Austria  26878      0     1
5           Austria  26879      0     1
36169 United States 114302     NA     3
36170 United States 114303      0     3
36171 United States 114304      1     2
36172 United States 114305      0     4
36173 United States 114306      1     5
36174 United States 114307      0     3
      pctcollege
1              7
2              7
3              7
4              7
5              7
36169         28
36170         28
36171         28
36172         28
36173         28
36174         28
```

This excerpt of the full dataset shows six columns: the row numbers that are automatically generated by R; the label of the country in which the survey took place (`country`); respondent identification number (`id`); number of participatory acts reported by that person (`partic`); the educational attainment of that person (`educ2`); and the percent of adults aged 25-64 who have a college degree in that country (`pctcollege`). Notice that `pctcollege` and `country` are the same for every respondent within a given country. This is a very common structure for multilevel datasets.

Point 2: Plot First, Model Later

Just to be concrete, say I wonder (1) whether the relationship between participation and education differs across countries, and (2) if it does differ, whether the difference could have something to do with inequality in educational attainment within countries. The motivation behind the second question is to shed some light on the theory that, in addition to teaching people civic skills, education matters for participation because it allocates social status in a society. In a place where everyone has a college degree, we would not expect education to distinguish participators from non-participators.

The techniques I propose here are meant as a prelude to any statistical procedures that might be suggested to estimate coefficients and related stars (err, standard errors). These techniques will help you decide which kind of analysis you will eventually want to conduct and which modeling assumptions are more or less tenable. The idea is simple: run a regression for each country and then plot the coefficients.[4] The idea of running 25 different regressions, one per country, and making at least 25 different plots is enough to make most peoples' eyes glaze over. This is where R (R Development Core Team, 2004) makes life much easier. The following 2 lines of R code run 25 regressions, collect the 25 regression objects in a list named `theregs`, and add names to the list object for the appropriate countries (naming things turns out to make life a bit easier down the road).[5]

```
> theregs <- lapply(unique(fulldat$country),
+     function(x) {
+         lm(partic~educ2,
+             data=fulldat[fulldat$country==x,])
+     })
> names(theregs) <- unique(fulldat$country)
```

To make things easier for plotting, I collect the coefficients from the regressions into a matrix called `coefmat` and the standard errors into a matrix called `semat`. Then, I combine coefficients, standard errors, and country-level information into a data frame that I can use for plotting. I also make a new macro-level variable called `educgroupsQ` which breaks the country-level educational context variable into three groups — low, middle, and high. Finally, I print the first three rows of the `coefmat` matrix.

```
> coefmat <- matrix(unlist(lapply(theregs,
+     coef)), ncol = 2, byrow = TRUE,
+     dimnames = list(names(theregs),
+         c("Intercept", "Educ")))
> semat <- matrix(unlist(lapply(theregs,
```

to R syntax can be downloaded from `http://cran.r-project.org/manuals.html`.

[4]This idea is not new to me or really that new in general. For great conversations about this, though, I should thank Chris Achen, Steve Heeringa, Dave Howell, Karen Long Justo, and Phil Shively and the Comparative Study of Electoral Systems for hosting us all for a day. Michael Herron and Cara Wong provided important comments and criticisms on this article. And Jusko (2004) presents some other ways to plot within country coefficients in the context of presenting a meta-analysis style approach to estimating the effects of country-level characteristics on individual-level outcomes.

[5]This is another place where I'm playing a bit fast and loose. The educational attainment scale is coded 0=incomplete primary education, 1=completed primary education, 2=incomplete secondary education, 3=completed secondary, 4=incomplete university level, and 5=completed university level. This is not an interval level measure, but I am treating it as such for the purpose of illustration here.

```
+     function(x) summary(x)$coef[,
+         "Std. Error"])), ncol = 2,
+     byrow = TRUE, dimnames = list(names(theregs),
+         c("SEIntercept", "SEEduc")))
> coefsedf <- data.frame(coefmat,
+     semat)
> coefsedf$country <- factor(row.names(coefsedf))
> coefdf <- merge(coefsedf, themacrodat,
+     by = "country")
> row.names(coefdf) <- as.character(coefdf$country)
> coefdf$educgroupsQ <- cut(coefdf$pctcollege,
+     quantile(coefdf$pctcollege,
+         p = c(0, 0.25, 0.75, 1)),
+     include.lowest = TRUE)
> print(coefdf[1:3, ], 4)

        country Intercept      Educ
Austria Austria 0.4031417 0.2028249
Belgium Belgium 0.7135492 0.2241669
Brazil   Brazil 0.3608015 0.2346767
        SEIntercept     SEEduc
Austria  0.04234842 0.01665069
Belgium  0.06801391 0.02143756
Brazil   0.05695525 0.02247269
        pctcollege educgroupsQ
Austria          7      [7,11]
Belgium         13     (11,19]
Brazil           8      [7,11]
```

Figure 1 shows the scatter plots of participation by education within nine of the 25 countries, jittered to show the density of the points at each coordinate. The panels are plotted in order of the percent of their population aged 25-64 who have a college degree (with the United States having the highest proportion and Austria the lowest). The plots for the countries in the lowest education group are colored black, the middle group is colored dark gray, and the highest group is colored light gray. Each panel contains a regression line (the straight one) and a line connecting the mean participation levels at each level of educational attainment (the not straight one). I included the line of means as a check for non-linear relationships. The percent attending college in the country is also printed in each panel.

```
> themeans <- tapply(fulldat$partic,
+     list(country = fulldat$country,
+         educ2 = fulldat$educ2),
+     function(x) mean(x, na.rm = TRUE))
> somecountries <- c("Austria", "Brazil",
+     "Chile", "Spain", "Sweden",
+     "Japan", "Denmark", "Norway",
+     "United States")
> smallcoefdf <- coefdf[coefdf$country %in%
+     somecountries, ]
>countriesInOrder<-as.character(smallcoefdf$country[
+                     order(smallcoefdf$pctcollege)])
> thecols <- gray(c(0.1, 0.5, 0.8))

> par(mfrow = c(3, 3), pty = "s",
+     mar = c(1, 1, 2, 1), mgp = c(1.5,
+         0.5, 0), oma = c(3, 3,
+         0, 0))
> ps.options(pointsize = 12)
> for (i in countriesInOrder) {
+     plot(jitter(fulldat[fulldat$country ==
+         i, "educ2"]),
+     jitter(fulldat[fulldat$country ==
+         i, "partic"]), type = "p",
+         col = thecols[unclass(coefdf[i,
+             "educgroupsQ"])], xlab = "",
+         ylab = "", xlim = range(fulldat$educ2,
+             na.rm = TRUE),
+         ylim = range(fulldat$partic,
+             na.rm = TRUE), cex = 1)
+     title(main = i, cex = 1)
+     text(0, 4.5, paste("% College=",
+         coefdf[i, "pctcollege"],
+         sep = ""), pos = 4, font = 2,
+         cex = 1)
+     abline(coef(theregs[[i]]))
+     lines(0:5, themeans[i, ])
+ }
> mtext(side = 1, "Educational Attainment",
+     outer = TRUE, line = 1)
> mtext(side = 2, "Number of Participatory Acts",
+     outer = TRUE, line = 1)
```

The Lattice graphics (Sarkar, 2004) sub-system within R provides a quicker and more elegant way to produce a similar plot.[6] Figure 2 shows this plot. It is my experience that lattice graphics (functions like `xyplot()`) are usually less flexible than the lower level plotting commands (e.g., functions like `plot()`). However, they can produce reasonable plots more quickly than the lower level commands — for example, in this next example, the regressions are run and plotted with the `panel.lmline()` function. This means that if our plotting were to stop here, we wouldn't have needed to create our dataset of coefficients and standard errors.

```
> library(lattice)
> fulldat$countryEducOrder <- factor(fulldat$country,
+     levels = levels(fulldat$country)[
+                 order(coefdf$pctcollege)],
+     ordered = TRUE)
> latticeplot <- xyplot(partic ~
+     educ2 | countryEducOrder, data = fulldat,
+     cex = 0.7, subset = fulldat$country %in%
```

[6] The manual for Lattice graphics explains: "Trellis Graphics is a framework for data visualization developed at the Bell Labs by Rick Becker, Bill Cleveland et al, extending ideas presented in Bill Cleveland's 1993 book *Visualizing Data* (Cleveland, 1993). Lattice is best thought of as an implementation of Trellis Graphics for R." (Sarkar, 2004)

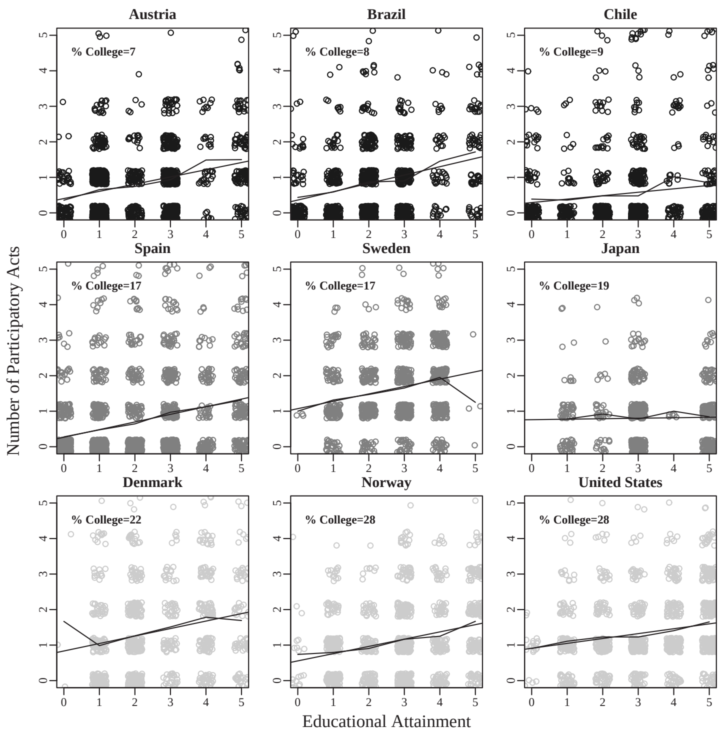

Figure 1: Within Country Regressions and Means Sorted by Proportion of the Population with a College Degree

```
+         somecountries, xlab = "Education Level",
+     ylab = "Number of Participatory Acts",
+     panel = function(x, y, ...) {
+         panel.xyplot(jitter(x),
+             jitter(y), col = "gray",
+             ...)
+         panel.lmline(x, y, ..)
+         themeans <- tapply(y, x,
+             function(x) mean(x,
+                 na.rm = TRUE))
+         llines(as.numeric(dimnames(themeans)[[1]]),
+             themeans, col = "black")
+     })
> print(latticeplot)
```

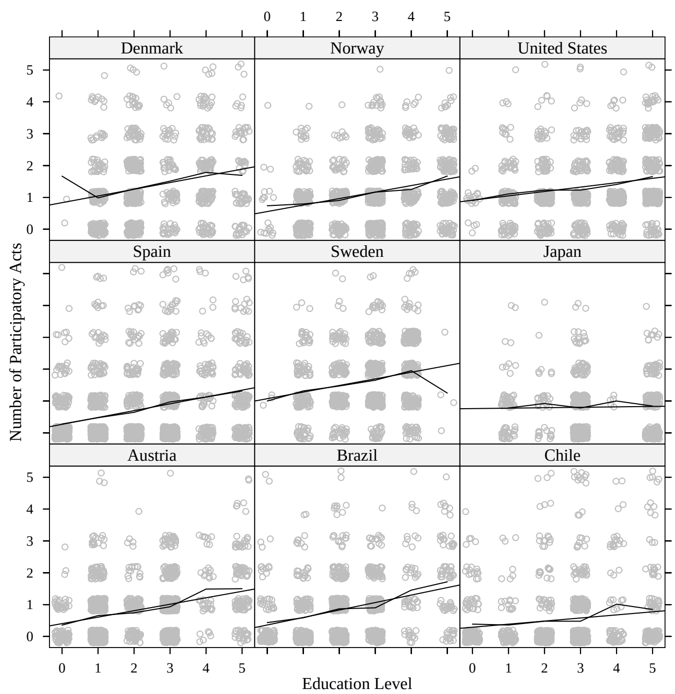

Figure 2: Within Country Regressions and Means Sorted by Proportion of the Population with a College Degree: Lattice Graphics

[7]The thick mean lines do not take into account the amount of information that went into the estimation of the different lines. I leave the creation of a weighted average line as an exercise.

If we do not have a continuous macro-level variable, we might want to plot these regression lines groups together — with the mean line overlaid. Figure 3 shows the within country regression lines (bounded at the limits of the education variable) plotted together within groups defined by the percent of the population receiving a college degree.[7]

```
> thefits <- lapply(theregs, function(x) {
+     thenewdata <- range(x$model$educ2)
+     data.frame(cbind(x = thenewdata,
+         y = predict(x, newdata =
+              data.frame(educ2 = thenewdata))))
+ })
```

```
> ps.options(width = 7, height = 3,
+     family = "Times", pointsize = 10)
> par(mfrow = c(1, 3), pty = "s",
+     mar = c(1, 1, 2, 1), mgp = c(1.5,
+         0.5, 0), oma = c(2, 2,
+         0, 0))
> for (i in levels(coefdf$educgroupsQ)) {
+     plot(range(fulldat$educ2, na.rm = TRUE),
+         range(unlist(lapply(thefits,
+             function(x) range(x$y)))),
+         type = "n", xlab = "",
+         ylab = "", main = paste("Range % College=",
+             i))
+   lapply(thefits[coefdf$educgroupsQ==
+         i], function(x) {
+         lines(x$x, x$y, col = gray(0.5))
+     })
+   tempdf<-data.frame(thefits[coefdf$educgroupsQ==
+         i])
+     meanpartic <- rowMeans(tempdf[,
+         grep("y$", names(tempdf))])
+     lines(c(0, 5), meanpartic,
+         lwd = 3)
+ }
> mtext(side = 1, "Educational Attainment",
+     outer = TRUE)
> mtext(side = 2, "Number of Acts",
+     outer = TRUE, line = 1)
```

Now let us look at the change in the individual level effects of education on participation by the country-level educational attainment level more systematically. Since we have collected the coefficients and standard errors in a dataset, we can look at how the relationship between education and participation varies across countries based on the educational inequality at the country-level, by treating these coefficients as data and the standard errors as weights.

Figure 4 shows the coefficients from within country regressions plotted against the percent of college educated within a country. The line segments through each point show the ± 2 standard error range around each point — to alert us to the amount of information used in the calculation of that estimate. The straight lines in each panel show the regression of the coefficients on country level education. The wiggly lines are a non-parametric regression. And, points that fall far from the regression line are labeled. Both the linear regression and the non-parametric smoother are weighted by the standard errors of the within country regressions.

```
> EducOnPctCollW <- lm(Educ ~ pctcollege,
+     data = coefdf, weights = 1/SEEduc)
> interceptOnPctCollW <- lm(Intercept ~
+     pctcollege, data = coefdf,
+     weights = 1/SEIntercept)
> ps.options(width = 7, height = 4,
+     family = "Times", pointsize = 10)
> attach(coefdf)
> par(mfrow = c(1, 2), pty = "s",
+     mar = c(2, 3, 2, 1), mgp = c(1.5,
+         0.5, 0), oma = c(2, 0,
+         0, 0))
> plot(pctcollege, Educ, type = "p",
+     ylim = range(Educ - 2 * SEEduc,
+         Educ + 2 * SEEduc), main = "Slopes",
+     xlab = "", ylab = "Effect on Participation")
> segments(pctcollege, Educ - 2 *
+     SEEduc, pctcollege, Educ +
+     2 * SEEduc)
> abline(EducOnPctCollW)
> plot(locfit(Educ ~ pctcollege,
+     data = coefdf, weights = 1/SEEduc,
+     alpha = 1/2), add = TRUE)
> weirdpoints <- abs(resid(EducOnPctCollW)) >
+     quantile(abs(resid(EducOnPctCollW)),
+         p = c(0.85))
> text(pctcollege[weirdpoints], Educ[weirdpoints],
+     as.character(country)[weirdpoints])
> plot(pctcollege, Intercept, type = "p",
+     ylim = range(Intercept - 2 *
+         SEIntercept, Intercept +
+         2 * SEIntercept), main = "Intercepts",
+     xlab = "", ylab = "Mean Participation")
> segments(pctcollege, Intercept -
+     2 * SEIntercept, pctcollege,
+     Intercept + 2 * SEIntercept)
> abline(interceptOnPctCollW)
> plot(locfit(Intercept ~ pctcollege,
+     data = coefdf, weights = 1/SEIntercept,
+     alpha = 1/2), add = TRUE)
> weirdpoints2 <- abs(resid(interceptOnPctCollW)) >
+     quantile(abs(resid(interceptOnPctCollW)),
+         p = c(0.85))
> text(pctcollege[weirdpoints2],
+     Intercept[weirdpoints2],
+     as.character(country)[weirdpoints2])
> mtext(side = 1,
+      "Percent of Population 25-64 years old
+       with a College Degree",
```

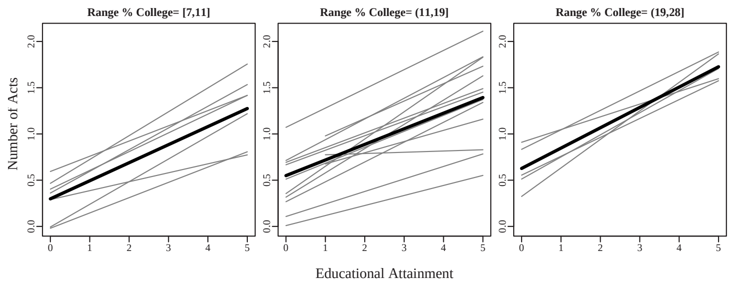

Figure 3: The Relationship between Educational Attainment and Political Participation by Educational Context of Countries

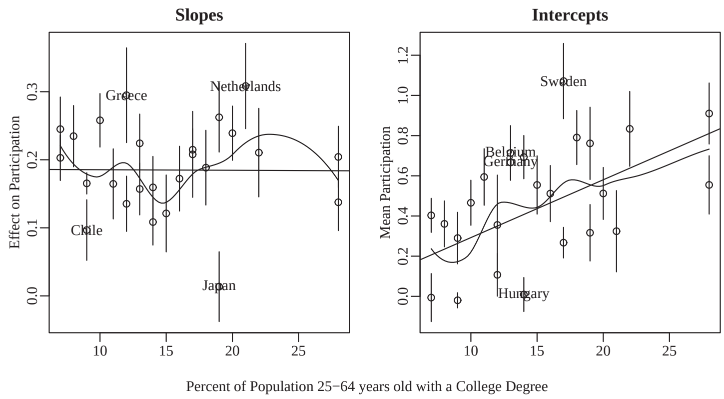

Figure 4: The Relationship Between Education and Participation as a function of the Educational Inequality Of A Country

```
+       outer = TRUE, line = 0)
> detach(coefdf)
```

In the slopes panel, we see that the relationship between an individual's education and her participation does not depend in any simple linear way on the percent of the population who has a college degree, although there might be some interesting non-linear pattern. Furthermore, we see that Japan is not well characterized by the process that captures the other countries. In the intercepts panel, we see that countries where more people have college degrees tend to have higher participation rates among people with low educational attainment than countries where fewer people have college degrees. In other words, it appears that there is a participation benefit that accrues from living in a place where many people have more education — even for people who do not have high educational levels. Both panels also alert us to the fact that certain countries do not fit the general patterns very well: Japan stands out for having almost no relationship between education and participation, and Sweden and Hungary are places where those with low education appear to participate more and less, respectively, than would be expected given the educational context in those countries.

So far, it is also clear that people who have more education are more likely to get involved in petitions, boycotts, demonstrations, illegal strikes, and sit-ins than people who have less education, *in nearly every country in the dataset.* This result is surprising given my expectations about how political institutions (and social status) ought to be differently related to education in different countries. We have also seen that Japan does not have a pattern like the others. In Japan, in 1999-2001, education did not appear related to participation at all. Notice that if we had skipped this plotting step, we would have estimated some kind of model that produced a coefficient for something like the "average relationship between education and participation" across countries. This coefficient might have had many stars, or not. And, these plots suggest that an average slope might do a good job summarizing these patterns (i.e., the slopes do not look that different across countries). However, the lack of relationship in Japan would have been smoothed over. Since this is an article about how to use R, I will not speculate about what this result means for Japan or for the overall question I posed at the beginning of the article. However, it is worth noting that the plots have taught us something about the phenomenon we care to understand, which would have been hard to pick up in a model specified before looking at the data.

Point 3: Plot to Check Assumptions for Future Modeling

Many popular modeling strategies for multilevel data rely on assumptions about relationships between the slopes and intercepts in within country regressions. For example, most "random effects" or "random coefficients" or "multilevel" models (whether estimated using maximum likelihood or Markov-Chain Monte-Carlo simulation) assume that the slopes and the intercepts can be thought of as drawn from a multivariate normal distribution. How plausible is such an assumption in this case? A priori, it is hard to know. It is plausible that we could have some countries with very strong relationships and others with no relationships — leading to a bimodal distribution of slopes. If this were the case, then the assumptions about normally distributed random coefficients would not be tenable.

The top row of figure 5 shows qqplots where the slopes and intercepts are plotted against what would be expected, were these variables drawn from a normal distribution. The bottom row shows the non-parametric density estimates for the slopes and intercepts. Overall, the slopes and intercepts do look like they could have been drawn from a normal distribution, for a few outlying points.

```
> par(mfrow = c(2, 2), pty = "s",
+     mar = c(3, 2, 2, 1), oma = c(0,
+         0, 0, 0))
> qqnorm(coefdf$Intercept,
+        main = "Are Intercepts Like a
+                Normal Distribution?")
> qqline(coefdf$Intercept)
> qqnorm(coefdf$Educ,
+        main = "Are Slopes Like a
+                Normal Distribution?")
> qqline(coefdf$Educ)
> plot(density(coefdf$Intercept),
+     main = "Density of Intercepts")
> rug(coefdf$Intercept)
> plot(density(coefdf$Educ),
+     main = "Density of Slopes")
> rug(coefdf$Educ)
```

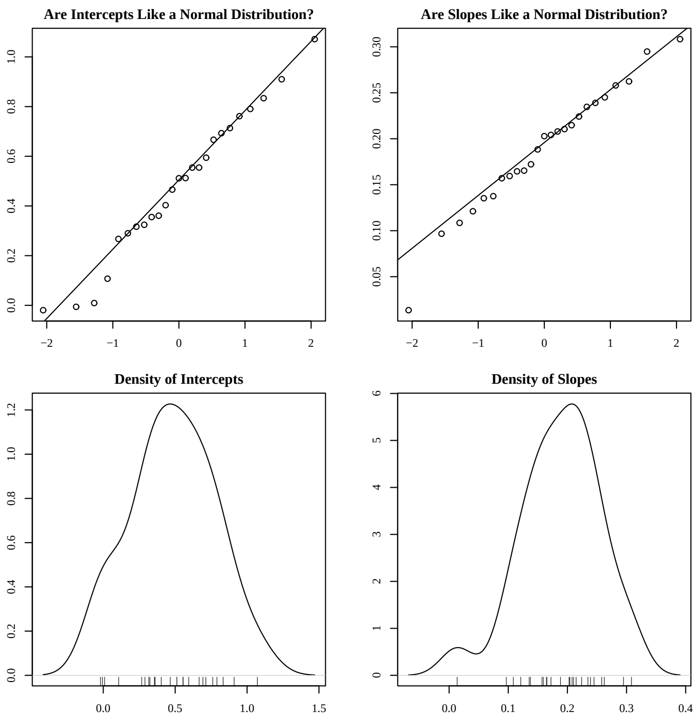

Figure 5: Assessing the Marginal Distributions of the Slopes and Intercepts

What kind of multivariate normal distribution generated these marginally (mostly) normal-looking slopes and intercepts? Are the slopes and intercepts correlated with one another? Figure 6 shows the joint distribution of the slopes and intercepts. The smoothed contour lines representing bivariate density are overlaid on the scatterplot of the slopes and intercepts. We do not see an extremely strong relationship here, and the correlation is -.13. This suggests that countries where people with lower education participate more (i.e., higher intercepts) tend (weakly) to be countries where the relationship between education and participation is weaker than in countries where the least educated participate less.

```
> ps.options(width = 3, height = 3,
+     family = "Times", pointsize = 10)
> par(mfrow = c(1, 1), pty = "s",
+     oma = c(0, 0, 0, 0), mar = c(3,
+         2, 1, 1), mgp = c(1.5,
+         0.5, 0))
> plot(coefdf$Intercept, coefdf$Educ,
+     pch = 19, col = "black", xlab = "Intercepts",
+     ylab = "Slopes", cex.lab = 1)
> plot(locfit(~Intercept * Educ,
+     data = coefdf, alpha = 3/4,
+     scale = T, kern = "rect", deg = 2,
```

```
+     family = "dens"), add = TRUE,
+     col = gray(0.5), drawlabels = TRUE)
> text(coefdf$Intercept[coefdf$country %in%
+     c("Japan", "Sweden")],
+     coefdf$Educ[coefdf$country %in%
+     c("Japan", "Sweden")], c("Japan",
+     "Sweden"), pos = 2)
```

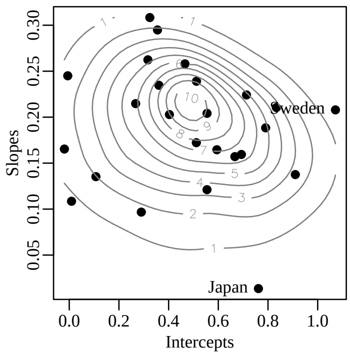

Figure 6: Assessing the Joint Distribution of Slopes and Intercepts

Pretend that these plots were done on a dataset with all of the countries in the World Values Survey, and that within country models included appropriate controls for variables that might be confounding the relationship between education and participation. And, pretend that I had multiple measures of the educational inequality within these countries. If I saw results like those shown here, I would have a story to tell about the expectations that were generated from the previous theories: The theories appear to travel well although educational inequalities in participation appear to be slightly ameliorated within countries where more of the population is better educated. I would also have new questions to spur further research — what is going on with Japan and Sweden? Finally, I would be set up to make reasoned choices about the next steps I might take if I wanted to estimate, say, a single coefficient representing how the relationship between education and participation changes on average across countries. Although running and displaying 25 regressions (or 100) might be a daunting task in other statistical analysis environments, I hope that I've shown (1) how R can make the tasks of getting to know this kind of data easy. and (2) how an interesting substantive story can be told simply and persuasively without an asterisk in sight.

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Section Activities

In Memorial: John T. Williams

Virginia Gray and I (his advisors) and especially Mike McGinnis, his friend and colleague since graduate school, deeply regret to inform you that John Williams passed away in his sleep September 13.

While an obituary and further remembrance will appear in the next issue of The Political Methodologist, I want to inform you that, through the inspiration of Mike and with the support of Simon Jackman, Janet Box-Steffensmeier, and Jonathan Katz of the Society for Political Methodology, the following serves as a more fitting tribute than flowers:

In recognition of his contribution to graduate training in the field of political science, the Political Methodology Section of the American Political Science Review has established the Jotwilli (John T. Williams) Travel Fellowship to support graduate students presenting papers at professional conferences or participating in specialized training programs. Each year, recipients of this award will be selected by a committee of his colleagues from that section. Contributions towards the establishment of this fellowship can be sent to Professor John Aldrich, Department of Political Science, Box 90204, Duke University, Durham, NC 27708-0204. Please make your checks or money orders out to the Society for Political Methodology.

Sadly,
John Aldrich

PolMeth04: the 21st annual summer meetings of the Society for Political Methodology

Over 120 faculty and students attended this year's summer meetings at Stanford University, on July 29-31. The program included 16 papers on a diverse set of methodological topics, including the analysis of panel data, ecological inference, text-based analysis of *Congressional Record*, causal inference, and Markov chain Monte Carlo algorithms. Sessions devoted to software written by and for political scientists were held on the Friday and Saturday mornings of the conference. In addition, editors of four of the profession's most important journals attended the meeting, conducting a valuable "meet the editors" roundtable: on behalf of the section I thank Bob Erikson (*Political Analysis*), Kim Hill (*American Journal of Political Science*), Bill Jacoby (*Journal of Politics*) and Lee Sigelman (*American Political Science Review*) for their participation in PolMeth04. The complete conference program, a list of all attendees, and photographs of conference sessions are available from the conference website, `http://polmeth04.stanford.edu`.

This year's conference broke with tradition in a couple of respects. First, the meeting was probably the largest the section has held in recent years; again, on behalf of the section, I thank all the attendees who opted to stay in hotels, and those faculty that were able to pick up the expenses of their students. Second, instead of the traditional invited lecture from a local statistician or econometrician, this year we opted to use "distinguished locals" in discussant roles, and were encouraged to be expansive in their remarks. Persi Diaconis (of Stanford's Statistics Department) discussed a paper coauthored by Jeff Gill (UC Davis) and George Casella (of Florida's Statistics Department) on simulated annealing for exploring multimodal posterior densities, and Guido Imbens (of Berkeley's Economics Department) discussed a paper coauthored by Henry Brady and John McNulty (UC Berkeley) assessing the causal impact of consolidating polling places in Los Angeles County. Third, we ran more papers in plenary sessions than in recent years, by holding some paper-givers and discussants (and the audience!) to a compressed time schedule. I welcome feedback from conference attendees on the strengths or weaknesses of these innovations.

At the now traditional Friday night student poster session, over fifty five graduate students (and some faculty) presented posters. A committee of Sunshine Hillygus (Harvard), James Honaker (UCLA), Dean Lacy (OSU), Walter Mebane (Cornell), Kevin Quinn (Harvard), and Anne Sartori (Princeton) awarded the prize for best poster to two students: Marisa Abrajano of New York University, and Gabriel Lenz of Princeton University. On behalf of the section, I extend our congratulations to Marisa and Gabriel.

The pages of *TPM* are also an appropriate place to acknowledge the support we received for this year's meeting. Graduate student travel is funded by a grant from the National Science Foundation. Support at Stanford came primarily from the Methods of Analysis Program in the Social Sciences (MAPSS), an initiative strongly supported by Karen Cook, Cognizant Dean for the Social Sciences in Stanford's School of Humanities and Sciences. The Department of Political Science at Stanford also supported the meetings under the auspices of its Munro Lecture series; on behalf of the section, I thank Paul Sniderman, chair of the Department, for supporting the meetings and for the support he gave to political methodology more generally over his term as Department chair. Stanford's Institute for the Quantitative Study of Society, and its Director, Norman Nie, also generously supported the meeting. For valuable administrative support, I thank Jackie Sargent and Eliana Vasquez from the front office of the Department of Political Science, who worked above and beyond the call of duty in helping to make the meetings a success.

Finally, I am happy to announce that the 2005 summer meetings will be held at Florida State, and that the 2006 summer meetings will be held at UC Davis. I now know first hand what it takes to host summer meetings, and so, in advance, and on behalf of the section, I thank our colleagues at FSU and Davis for their service to political methodology. More details on both meetings will appear on in the next issue of *TPM* and on the PolMeth web server.

Simon Jackman
Stanford University

Section Awards

Two awards were announced at the section business meeting at APSA.

The Harold Gosnell Prize for the best methodology paper, presented at a conference between August 1, 2003 and July 31, 2004 was awarded to Henry Brady and John McNulty for their paper "A 'Natural Experiment' on the Costs of Voting: Methodologies for Analyzing Data when the Treatment is Nearly Randomized", presented at the 2004 summer meeting.

The Warren E. Miller Prize for the best paper in the current volume of Political Analysis (Volume 12) was awarded to David Park, Andrew Gelman and Joseph Bafumi for "Bayesian Multilevel Estimation with Poststratification: State-Level Estimates from National Polls".

On behalf of the section, I thank the awards selection committee of Jim Stimson (chair), Doug Rivers, Wendy Tam Cho and Phil Schrodt.

Simon Jackman
Stanford University

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Subscriptions to *TPM* are free to members of the APSA's Methodology Section. Please contact APSA (202 483-2512, `http://www.apsanet.org/about/membership-form-1.cfm`) to join 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 should be sent to the editor by e-mail (jblewis@ucla.edu) if possible. Alternatively, submissions can be made on diskette as plain ascii files sent to Jeffrey B. Lewis, Department of Political Science, 4289 Bunche Hall, University of California at Los Angeles, Los Angeles, CA 90095-1475. LaTeX format files are especially encouraged. See the *TPM* website, `http://polmeth.wustl.edu/tpm.html`, for the latest information and for downloadable versions of previous issues of *The Political Methodologist*.

TPM was produced using LaTeX on a PC running MikTex and WinEdt and a Mac running teTex and TextEdit.