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Notes From the Editor

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This issue of *TPM* illustrates the diversity of our subfield. It includes an article by Janet Box-Steffensmeier and her collaborators on their experiences with teleconferencing at Ohio State University. This technology could be coming soon to a classroom near you. This is followed by a colloquium on historical analysis with comments by three paper-presenters at the 1997 Midwest meetings, and one of the discussants asking the question "Does Historical Political Research Pose any Special Methodological Concerns?" Then we turn to methodological techniques, and software. Fang Wang offers a brief summary of the Generalized Method of Bounds developed by Gary King in his *A Solution to the Ecological Inference Problem* (King 1997). This is not a review of King's book, but a short exposition of the methodology. If you have the $16.95 for the book, but not the time to read it, or want a brief primer before diving in, then this article is worth checking out. Neal Beck reviews the latest version of STATA. Ken Benoit and Paul Johnson describe some of the joys (and frustrations) of Linux: an operating system that lets you do everything from run STATA to format and typeset *TPM*. Finally, Harold Clarke tells you about a new outlet to publish your next paper in, and Gary King invites you to first submit it for presentation at the 1998 Political Methodology Summer Meeting in San Diego.

The Multi-site Interactive Video Curriculum in Advanced Data Analysis for Political Science[1]

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In the 1995-96 academic year, an exciting experiment was conducted in distance education. In the fall semester, Illinois, Minnesota, Ohio State, Pennsylvania State, and Wisconsin universities participated in at least one of four interactive multi-site video presentations. In the spring, a class in Time Series Analysis was offered using the same technology with Illinois, Minnesota, Ohio State, and Wisconsin Universities participating. W. Phillips Shively, currently Provost at the University of Minnesota, was the impetus behind the project. He asked interested Committee on Institutional Cooperation (CIC) political science departments to send representatives to a meeting at the 1995 Midwest Political Science Association annual conference to discuss the possibility of launching it. In the summer of 1995, the group met in Chicago with Roger Clark, Director of the CIC, and Michael Staman, President of CICnet. The project went forward with planning, technical, and monetary assistance from the CIC and the participating universities' departments, administrations, and technical staff. Testing for technical compatibility occurred over the summer. The project was launched in the fall of 1995 with Chris Achen, Larry Bartels, Henry Brady, and John Freeman, all former presidents of the American Political Science Association's political methodology group, presenting their current research in a series of interactive video conferences. The fall of 1996 presentations were made by Gary King, Michael Alvarez, Jon Krosnick, and Rene Smith.

[1]**Authors are listed alphabetically. We would like to thank Herb Weisberg for helpful comments.**

The project was intended to build a larger community of scholars in quantitative methods, improve the quality of faculty and student work, and overcome a very practical problem: the expensive nature of graduate instruction in the area of quantitative methods (Freeman and Shively 1995). The project also served as a test for the practicality of multi-site interactive video teaching by people who are novices in using the technology. The long-term vision of the project includes a regular seminar series and regular offering of four advanced modeling classes on a rotating basis. John Freeman is serving as director of the project.

Political methodology is a growing field. Because of the connections within this field that are fostered by the annual political methodology group summer meeting and the comparative advantage the departments of the CIC enjoy in this area, the project became a promising case study. Beyond this, as the empirical puzzles in political science increase in their complexity, learning and developing methodological tools is likely to be useful. Not surprisingly then, the demand for political methodologists and the need for classes to train these individuals drastically have increased. Unfortunately, study in this field demands a significant amount of course work on the part of the student and the availability of courses offered by those who have the experience and skill to teach them. "Because the work builds on a strong base of prerequisites that take time to acquire, and because much of the work involves highly specialized seminars, it is typically taught in very small classes. It is hard for most departments to justify carrying the necessary number of specialized faculty . . . " (Freeman and Shively 1995, 1). As universities mandate higher and higher minimum enrollments for courses, it is also problematic for instructors to regularly offer specialized courses in their area of expertise. For these and other reasons, universities are beginning to recognize the need for distance education offered through interactive video.

This article highlights the advantages and disadvantages of the project, drawing from Sutton's (1997) more systematic evaluation of the course. Our evaluation does not address issues related to the topic of the first course, which was Time Series Analysis, team-taught by John Freeman, University of Minnesota, and Janet Box-Steffensmeier, Ohio State University, or the quality of the syllabus, readings, or lectures. Rather we focus on the use of the technology in the class and the opportunities and problems created by the medium. Suggestions for improvement of future multi-site interactive video courses are interspersed throughout. The primary advantages and disadvantages of the project from the particular perspectives of administrators, instructors, and students are also highlighted in the conclusion.

The Multi-site Interactive Video Class

The mechanics of the seminars worked such that each site saw and heard the person speaking and the speaker saw the last site to speak. The video and audio was "grabbed" simply by speaking (there was about a three second delay for the switch to occur). Twenty-seven students participated as registered students or auditors. A number of faculty also took part. The students had varied backgrounds, areas of interest, and levels of expertise.

Sutton's (1997) evaluation revealed that most of the students enrolled in the class because of the topic and did so with positive expectations about the experience. Sutton concludes that "prior experience with instructional technology was correlated with positive attitudes" and there was not a substantial change in attitudes about the course: "those with positive attitudes retained them in spite of occasional difficulties, and those who were selectively skeptical remained so" (1997, 2). Table 1 summarizes the most frequent likes and dislikes of the students interviewed by Sutton.

Table 1
Students' Most Frequent Likes and Dislikes
(in order of frequency)

LIKES

- Access to instruction not otherwise available
- Opportunity to work with highly regarded faculty
- Graduate programs strengthened by access to expertise from other institutions
- Interplay of team teaching, which broadened the course substantively and methodologically
- Diversity of the participants from each site and an enhanced sense of professional community

DISLIKES

- Mechanics and communication mechanisms, primarily the lower levels of interaction
- Occasionally insufficient support for students at sites without instructors
- Bureaucratic problems of registration and course credit

Source: Sutton (1997).

We discuss the strengths and weaknesses of this medium in comparison to those of a traditional seminar. We start with the premise that the technology is only successful if it affords advantages that are impossible in a traditional format; otherwise, the expenses and extra work born by the university and the instructor are futile.

Strengths of the Medium

When comparing this approach to the traditional format, several advantages of the multi-site interactive video become apparent. First, students interested in learning advanced methods have access to instructors specialized in a particular area, and in the case of the Time Series Analysis course, students benefitted from the expertise and comments on assignments of two instructors. The instructors' substantive interests differed considerably; Professor Janet Box-Steffensmeier focuses on questions in American politics while Professor John Freeman studies those in Political Economy and International Relations. In addition, each instructor also has somewhat different research interests in methodology. Therefore, students are not only learning from people who use advanced methods and research them, but also benefit from having access to instructors with substantive interests similar to their own substantive interests.

Second, participation by faculty sitting in on the course was extremely helpful. These faculty participants tended to stimulate discussion and brought still more diverse substantive interests to the class. Students, as well as the instructors, benefitted from faculty comments and questions. In a traditional format, such access to so many leading scholars would not have been possible. For example, students at Wisconsin benefitted from Charles Franklin's informal discussions with them outside of the classroom, which helped students organize their questions and ideas for class, while students at all four sites were able to benefit from his experience and insight into the subject of the course.

Another advantage that the multi-site interactive video technology affords, which a traditional seminar does not, is the possibility of assignments that provide interaction between the students and the medium. Short, student-led discussions were assigned for many classes and although such presentations are used in traditional seminars, students in this case gained experience by presenting to audiences more diverse than would be found in a typical classroom setting. Moreover, the students had to be well prepared to explain methodological points so that all participants could follow and then field comments and questions from other students and faculty members from all sites. Such assignments provide excellent preparation for conference presentations, job talks, and future use of this type of medium in presentations and teaching.

Instructors and off-site students found that electronic support mechanisms (the web site, E-mail reflector, and E-mail) were particularly useful. From an instructor's perspective, answering a question once on the news group in contrast to sending twenty individual E-mails was more efficient. The distribution of data sets through the web

site was also a big time saver for instructors since students could customize the data set they wanted rather than relying on the instructor to do so. A strength of the electronic support tools is that they are asynchronous, which gives the student access to high-quality support outside the classroom, at their own convenience or when they need it, without having to schedule an appointment with an instructor. The effect, for students who take advantage of these resources, is that students maintain a communicative involvement in the course even when they are out of class, which is particularly useful in a distance education context.

Finally, the seminar gave students the opportunity to network with each other; members of the class that shared similar research interests could contact each other through the E-mail reflector and a list of students and their research interests were available from the web site. An informal meeting for seminar participants was arranged at the Midwest Political Science Conference in Chicago, giving the students an opportunity to meet each other in person. This conference was chosen not for the timing in terms of the course, but for convenience since many participants would be attending the conference.

Challenges

The primary weakness of the seminar revolved around the uneasy communication flow. Certain aspects of the technology made discussion more difficult than it would have been in a traditional seminar setting. For example, reading the nonverbal cues that people use to communicate was not possible. The multi-site video technology used for this course allowed only one audio/ video feed to be transmitted at a time. The automated switching equipment sometimes caused speakers to be cut off or introduced a time lag that produced awkwardly long pauses while the other participants waited for the switching equipment to engage. Perhaps pausing more often during a presentation of material for questions or essentially "calling on" sites in turn after an instructor asked a general discussion question would help. The ability to see all sites at one time also would have promoted in-class discussion. Since stimulating discussion was clearly more problematic in the interactive video setting than in a traditional format, the instructors used student presentations as a way to ensure that all students spoke at least once during the class. While some students thought the class requirement of presenting a published article was a good experience, others pointed out that the presentations again highlighted the lack of easy communication since even these presentations more often took the form of a lecture than a discussion. While some student listeners did pose questions and comments, the burden of the discussion ultimately fell on the presenter, a role which some found uncomfortable because of the communication barriers presented by the medium.

Students at sites with no instructors felt disadvantaged; there is a sense of insecurity of not having equal access to faculty. Everything possible must be done to assure that students at these sites are not, and do not feel, disadvantaged. Using "electronic office hours," a conferencing technology which would enable students to see and talk interactively with instructors one-on-one, would help. But it was not yet available during the course. Electronic office hours technology would also allow the student to run his or her software code to show the instructor assignment problems. The student could pull up his or her computer output, so the instructor could see the student and the output. The development of personal relationships in a video classroom appears to be important, just as it is in a traditional classroom, to facilitate the learning process.

Students seemed particularly troubled by the use of the RATS software, which is one of the most useful programs used in time series analysis. Most of the students were unfamiliar with it, which would be the case in anyone's first class in advanced methodology. The class benefitted from the assistance of an advanced graduate student at the University of Minnesota, Mark Smith, who served as a RATS consultant. However, this resource was underutilized by the students. This was because students found it difficult to discuss programming problems at a distance and because many students fell behind in terms of the due dates of assignments and therefore did not feel comfortable asking about late assignments. The difficulties in using the software necessary for the assignments could be addressed by dedicating more class time to instruction on how to use the software and interpret the results. Needless to say, it is essential that all students have access to and support for the recommended software.

Because of the students' anxiety about not having a local instructor, it appears that dedicating more class time to explicating assignments would have been useful. Offsite students seemed to feel that E-mail (again, a reflector was available as well as personal E-mail) did not provide the needed level of interaction to solve software problems and that sharing results and problems with RATS would have been a valuable exchange in the classroom. Although E-mail was used regularly by students who did not have local access to an instructor, E-mail is not interactive and so ruled out the give-and-take of a conversation. More effective one-on-one communication between the instructor and the students at remote sites, the coordinated delivery of materials through the local library, and better use of the web site, would all help to compensate for the lack of a local instructor.

Although more attention is needed to ease the flow of communication between the instructors and students, the instructors did take great pains to make themselves available to students, an effort which the students noticed and appreciated. Despite the fact that "electronic office hours" were not available, instructors still were available through E-mail, phone or fax. In addition, student evaluations confirmed that instructors dedicated a great deal of time meeting with students to answer questions at their own universities as well as allowing time to communicate with students at the other sites. Both instructors also wrote extensive comments on each piece of written work (five assignments per student), which were mailed to one of the instructors, then mailed to the other instructor, and then mailed back to the student.

Another disadvantage of the medium is that the class size is larger than the typical advanced modeling class or traditional graduate seminar. Although the students benefit from the diversity of perspectives among members of the class, it is more difficult to facilitate class discussion with larger classes. In addition, the medium made students feel apprehensive about commenting in class. Many students were concerned that their comments, good or bad, would be heard by people at four major universities as well as be taped. As a result, the class had more of a lecture format than a traditional seminar. Both faculty and students recognize that methods classes often are lecture- rather than discussion-oriented, but even so anxiety about speaking in class was a big issue for many students.

The difficulties for instructors to use a blackboard spontaneously and for students to be able to keep pace taking notes during the lecture were a hurdle. Since words and equations cannot be easily read on a blackboard through this medium and the equations were so complicated that presentation programs, such as PowerPoint, could not be employed easily, the distribution of lecture notes became necessary. Although preparing specialized (video compatible) lecture handouts ahead of time increased the workloads of the instructors tremendously, they were extremely helpful to the students. The handouts included notes as well as the equations that the instructor normally would have written on the board. During lectures, the instructors tended to move at a quick pace, so the availability of prepared lecture notes made it possible for the students to focus more on listening to the instructor and thinking about the lesson than feverishly taking notes. The quicker pace also resulted in less connect time and therefore cost savings. Once more, the format and medium meant instructors needed more time to prepare for class – writing large, legible handouts of all lecture notes rather than using a blackboard. There will soon be secure web sites, which is important because of copyright issues, that will allow the instructors to make the lecture notes available more quickly and with less hassle.

Although the interactive video medium was intended to facilitate cross-site communication among students, the degree of interaction among sites was less than the instructors expected. There appear to be at least two reasons for this. First, most students initially did not develop the same collegial bond with students at other sites that they did with their local classmates, which would discourage cross-site communication. Second, the students did not seem to feel any pressing need for or benefit from this sort of communication. Designing the course so that the web site and E-mail reflector had to be used more often would have helped to minimize this problem to some extent.

Offering the course simultaneously at four universities also raised practical problems such as incompatibilities between quarter and semester schedules, complications in assigning credit, and difficulties in coordinating reserve materials at the libraries. Ideally, local distribution of class materials would be provided by the library systems. But this task is complex and has not yet been fully organized, which is frustrating for the instructors and students alike. These difficulties will be resolved as coordination among the universities continues. A training session or "how-to book" with the minimally needed information about organizing and teaching an interactive video course would be a great resource. The goal of this book should be to allow the instructors to walk into a video classroom and concentrate more on pedagogy than technology. A more general training session for participants would probably help ease the transition to a video classroom. As it was, we learned the technology on our own in some summer pilot sessions and the fall seminar series. Universities are aware of these issues. Martie Parsley, a graduate student at Ohio State University is working to provide distance education resources on-line. These training materials are important to accommodate interested novices and to encourage others.

Surprisingly, the technical difficulties, such as dropped video connections or distorted audio were not major issues. The technical difficulties decreased over time and the participants got more comfortable, and as a result, more patient when problems did occur. The room arrangements varied a great deal from site to site. Close-ups of people, in contrast to long room shots, were better (although this required more attention from the participants at some sites who served as camera operators). It was difficult to establish a rapport with a person whose face was so fuzzy that one could not establish eye contact. The costs of multi-point video sessions are dropping and most universities have the needed facilities. However, costs should not be overlooked. The 1995-96 project was a success due to the funding support of the CIC, universities, and departments involved. The approximate cost was $200 per hour for the use of the facilities and connect charges. These charges

continue to drop as the technology becomes more common and improves.

In sum, more use of supportive technology, including electronic office hours, a secure web site, and E-mail reflector for a discussion group, will go a long way toward eliminating most of the disadvantages discovered from the multi-site interactive video project.

As innovators in research as well as teaching, we must utilize resources that through their technological sophistication, may solve logistics problems that face political science departments today. Given the large scale of cooperation and funding necessary for such a project, it will not replace the traditional seminar. But, multi-site interactive video is a resource which may allow advanced methods classes to be offered which otherwise would not be offered. The speaker series was offered again in the fall of 1996 and is scheduled for the fall of 1997. The next course, maximum likelihood estimation, is planned for spring 1997 and will be taught by Charles Franklin, University of Wisconsin.

Primary Advantages and Disadvantages from Various Perspectives

Every student Sutton (1997) interviewed was asked to summarize what he or she thought were the primary advantages and disadvantages of interactive, distance education. We are happy to report that the advantages outnumbered the disadvantages. "Most students' attitudes were pragmatic: Although they acknowledged the various problems with the mechanics of the course, their interest in the subject provided more than enough motivation to make the necessary adjustments in order to gain access to its content and they felt the course would improve with age" (Sutton 1997, 4). From an instructor's perspective and that of a participant in the speaker's seminars, high motivation seems to be a key element in the success of multi-site interactive video seminars. Teaching an audience with little motivation through this medium, such as an undergraduate required course, would probably be unbearable for all involved. In short, unmotivated participants would not put up with the technological glitches.

Administrator Perspective - W. Phillips Shively, University of Minnesota

I unfortunately did not have as much contact with the course as I had hoped to have. As a political scientist myself, I had even entertained the vain hope that I might be able to participate myself, but the pressure of time made that impossible. Therefore, my comments are more in the abstract – why I chose to support the experiment, and to continue it into the current year.

Collaboration of this sort offers the opportunity to build very strong programs that would otherwise be impossible for any one school to provide. In political data analysis, for instance, if we could harness the resources of the entire Big 10 – or even just add Michigan to the four current participants – we would have a research and instructional group unmatched anywhere in the world. There would not even be any close rivals. Further, the instructional program for graduate students could be offered much more efficiently, freeing up the faculty for other courses. Thus, if the transaction costs of a course like this can be made sufficiently low, we can simultaneously achieve terrific quality and save some resources.

It is essential, if the strategy presented above is to work, that the people involved form a real research community, not just a group exchanging courses. I think the "brown-bag" component, plus occasional face-to-face meetings, are essential.

Administrator Perspective - Brett Sutton, CIC evaluator

This course was of great interest to the CIC as an experiment in the delivery of multi-institutional instruction. The collaborative approach to higher education exemplified by this seminar is one of the goals of the CIC Learning Technology Initiative, a program endorsed by the CIC provosts to support the cooperative development and use of advanced instructional technologies within the consortium. A major purpose of this program is to identify the most effective mechanisms for extending the instructional resources and faculty expertise of the CIC universities. The time series course offered to administrators an opportunity to experiment with compressed video as a classroom technology, to learn more about how instructors and students adapt to this approach, and to begin to assemble evaluation data that will be useful to faculty innovators who plan similar projects in the future. Offered in an experimental setting that facilitated the development of realistic solutions to the technical and administrative aspects of distance education, this course is useful and interesting quite apart from its instructional success.

Perhaps one of the more positive outcomes of this experiment is the distinction the students made between the value of the course and their recognition of the ways the organization and delivery of the course could be improved. The ability of the seminar to satisfy an educational need making available to students a graduate seminar that they would ordinarily have little or no access to–is significant. This suggests that it is worth making the effort to provide better distribution of class materials, to deploy the right kind of supporting technologies, and to develop better strategies for encouraging communication, particularly

when most of these problems can be addressed with currently available technologies. An unanswered question is whether this instructional approach can be made more cost-effective, so that it might be offered on a larger scale. But with the prospect of higher-bandwidth digital links between the CIC universities and the development of improved forms of multimedia conferencing, the prospects are good. The continuing willingness of faculty to embark on these experiments, and the similar willingness of administrators to pay for them, also is a contributor to the success of the Learning Technology Initiative.

Faculty Perspective - Janet Box-Steffensmeier, Ohio State University

The speaker series was a major payoff of the 1995-96 project for faculty. It contributed to professional development and community building by providing the opportunity to hear top presentations in political methodology and to interact with a broader set of colleagues on a more regular basis. Cost could be seen as a negative. But if distributed on a regular basis to all four universities, any one university would participate in four times as many seminars, and, when other universities are hosting, some costs are saved.

The best aspects of the time series analysis course for me was to teach with an exceptional scholar and teacher, John Freeman, and to work with some of the best students at four CIC universities. The biggest negative was the greater time commitment, which included learning to use the technology, more grading, and various logistical matters.

Faculty and Project Coordinator Perspective - John Freeman, University of Minnesota

The opportunity to build and solidify a network of scholars was most welcome. The ability essentially to continue the Methodology Section's Summer Meeting in the fall was something many of us had talked about, but until now never realized. Teaching with Janet Box-Steffensmeier was very satisfying. Her perspective on various time series issues was most enlightening. The students were stimulating as well. They asked many fine questions and produced interesting time series analyses of political problems. For these reasons, I found the experience quite rewarding.

Among the areas for improvement are the technology. Discussion and fruitful exchange of ideas will occur faster and more fully once we have the ability to observe all sites simultaneously, and to ask questions in a more orderly fashion. In turn, the quality of the seminar will improve.

One final comment, while this course allows one to "reach" a large number of students and to develop professional relationships with them, the pedagogical challenge is the same as in any course. The need for one-on-one exchanges with students – dialogue and thorough, critical evaluations of their projects – is still present. Only the medium through which this dialogue and evaluation occurs, in part, changes.

Student Perspective - Kathy Powers, Ohio State University

The key advantages that this medium brought to the classroom, in my opinion, were access to methods classes that may not have been otherwise available and access to leading scholars in the field of methodology. The opportunity to gain presentation experience as well as experience in fielding comments through the student-led discussions was also very valuable.

The most significant disadvantages were the lack of efficiency in coordinating information at remote sites, large class size and the difficulty in facilitating discussion in this format. These classes demand a great deal of work on the part of the instructor as well as the student. Instructors must dedicate more time to class preparation than in a traditional seminar while students must take more initiative to seek help. Overall, the disadvantages can probably be alleviated by utilizing technology that can facilitate interactive communication between students and instructors at off-sites.

References

Freeman, John, and W. Phillips Shively. 1995. "Proposal for a Multi-site ITV Curriculum in Advanced Data Analysis and Modeling for Political Science." University of Minnesota. Typescript.

Sutton, Brett. (1997). "An Experimental Video Seminar on Time-Series Analysis in Political Science: Evaluation Report." [Online]. Committee on Institutional Cooperation. Available: http://www.cic.net/cic/lti/tseval.html [January 23, 1997].

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Kathy Powers is a graduate student at Ohio State University. Her research interests focus on regional political and economic integration, international trade, and institutions. She participated in the Summer Statistics Institute at the Inter-Consortium for Political Science Research at the University of Michigan and has attended the Summer Political Methodology Meeting in Ann Arbor.

W. Phillips Shively is a Morse-Alumni Distinguished Teaching Professor of political science at the University of Minnesota, and is Provost for Arts, Sciences and Engineering there. His research is on electoral behavior in Europe and North America, generally based on areally aggregated data. He is co-author of Cross-Level Inference (University of Chicago Press, 1995).

Brett Sutton's research and teaching experience spans a number of areas, including sociolinguistics, literacy, qualitative research methods, information technology, the sociology of science, and organizational planning. Recent publications on these subjects have appeared in Library Quarterly, and The Encyclopedia of Library History. He currently holds the position of coordinator of the CIC Learning Technology Initiative.

Buried Treasure: Theory and Historical Data[1]

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Footnotes

Footnotes tell the story. Or at least that is what historians seem to believe. History journals are crowded with footnotes that scurry along with the text, notes as long as paragraphs that cluster three or four to the page. At their best, footnotes offer insight into the adventure of discovery. They not only document the source of a quotation but they illustrate the process of transmuting obscure, forgotten texts into evidence and data and argument. And footnotes alone do not carry the whole work of illustration. Images of lithographs, prints, paintings, murals, handbills, posters, caricatures, and photographs fill the major history journals. On the cover of a recent issue of the American Historical Review, peasants thresh golden rows of grain, against a background of green trees and a blue sky filled with birds. Another twenty-seven images, all in black and white, illustrate the lead article. This is a lot of pictures, even for a history journal. Another article in the same journal, this on the emergence of clock-oriented timekeeping in various societies, makes do with just two photographs. The Journal of American History, the flagship journal for historians of the United States, generally includes four or five illustrations in each article as well as a black-and-white cover illustration.

In articulating basic principles of scientific research design, King, Keohane, and Verba (1994, 26) emphasize that all good research is replicable. But replicability, they argue, tends to assume different forms in quantitative and qualitative research. "In quantitative research, scholars focus on replicating the analysis after starting with the same data," whereas qualitative research depends on the footnote for ensuring replicability. Through footnotes and bibliographic essays, according to King, Keohane, and Verba, "succeeding scholars should be able to locate the sources used in published work and make their own evaluations of the inferences claimed from this information." Footnotes, however, are more than instruments of qualitative research. Properly, they are instruments of all original empirical work. Hypothesis-testing, which is essential to research in the social sciences, requires rigorous, clearly specified hypotheses and it requires systematic data that bear on those propositions. Principles of good research design apply not only to the problems of establishing rules for collecting and analyzing data but to the prior problem of reckoning with the obscurity of data.

Obscurity

Historical data, of course, are obscure because they survive only in fragmented and shadowy ways. Contemporary data are less obviously obscure. Political scientists have access to impressive databases containing political, economic, and social statistics and to the results of a half-century of voter surveys. The obscurity of contemporary data is rooted not in the inaccessibility of the data but in the distance that often separates the data-gatherer from the data-analyzer and hypothesis-tester. Shortening that

[1]**This paper was originally presented at the 55th annual meeting of the Midwest Political Science Association, 10-12 April 1997, Palmer House Hilton, Chicago, Illinois. In thinking about and writing this paper, I relied on several helpful discussions. I am grateful to Amy Bridges, Rui de Figueiredo, Dick Fenno, Temma Kaplan, Eric Lawrence, Bob Putnam, Rene Smith, Steve Smith, Tom Sugrue, and Margaret Weir, and, of course, to Ken Shepsle, Stephan Thernstrom, and Sid Verba. I am also grateful to the Woodrow Wilson International Center for Scholars for support.**

distance is not easy. To the extent that social scientists come into close and continuing contact with their original sources, the immersion can sometimes overwhelm the central project of hypothesis-testing and theory-building. The complexity and peculiarities of the story can take precedence over general propositions.

As an example of this danger, I offer the first draft of my dissertation. The dissertation's central argument is that local institutions-Catholic churches, Jewish synagogues, schools, parish halls, community centers-shape neighborhood attachments. Comparing the migration patterns of Catholics and Jews, I found that Catholics were both more likely than Jews to resist the entry of new ethnic groups into their neighborhoods and less likely to leave after the entry had occurred. Reconstructing the development of Boston neighborhoods since the nineteenth century, I argued that these behavioral patterns were the products of two different institutional environments. When I wrote the first draft, I had this general theory in mind. More than that, I had developed a set of hypotheses and collected a large body of data to test those hypotheses. But the draft itself was nearly unreadable. Rather than present my hypotheses clearly and explicitly, I buried them deep inside a mountain of stories. One after another, for almost three hundred pages, I documented the development of these Boston neighborhoods and the histories of their churches and synagogues and community halls.

What resulted was neither good history nor good political science. Despite ample footnotes and photographs, the history was directionless. There was little narrative and even less point, just a hodgepodge of excruciatingly well-documented monographs on individual neighborhoods and institutions. Stark patterns that I had identified when I had begun gathering evidence and that I saw confirmed as I had returned for more and more evidence-the constant mobility of Jewish institutions, the geographical boundaries of Catholic parishes, the hierarchical nature of Catholic authority and the autonomy of synagogues-were not identified in the text in any clear fashion. My approach in writing the draft had been to tell the stories of each institution and to allow readers to discover these stark patterns on their own. With hypotheses, evidence, and argument all carefully concealed, the draft was designed more as a treasure hunt than as a piece of social science.

In subsequent revisions, I have taken the argument and hypotheses out of hiding and thoroughly reconceived the text. The final draft of the dissertation-and, even more, the book that has grown out of this project-specify the dependent and independent variables, lay out the different rules that define Catholic and Jewish institutions, and draw on various sorts of evidence to demonstrate that residential patterns are rooted in the rules defining neighborhood institutions. I identify and examine alternative explanations. The text itself, as much as the footnotes, reconstructs the logic of the research design, the sources of my data, even the ways in which I developed controls for religion, ethnicity, and culture to demonstrate the independent effect of institutions.

Scaffolding

But lost in this final text is the long process of research itself. In the beginning, there were no hypotheses, only questions. When the first question came, I was a senior in college, working on my honors thesis. I was studying voting behavior in Boston in the 1920s and 1930s to understand the dynamics of the New Deal realignment. As part of the project, I was locating old precinct maps, census data, and other sources on the ethnic and class composition of the city's neighborhoods. One day, in the spring of 1986, I met with officials of the Boston Redevelopment Authority, showing them a large map of the city that I had shaded to identify densely concentrated ethnic populations. As I was about to leave, one of those officials asked if she could take one more look at the map. After I unrolled the map and placed it on a large desk, she pointed to the large orange-brown area that I had shaded on the map and expressed her surprise that African Americans had settled in these parts of Roxbury and Dorchester by 1930. I followed her finger and realized her mistake. "Those aren't blacks," I remember saying. "Those are Jews." As we both looked back at the map, she explained to me that the areas that I had shaded as Jewish neighborhoods in 1930 had become the city's largest black neighborhoods by the early 1970s.

A few years later, assigned a research paper in a graduate seminar in American social history, I thought back to that brief exchange and set out to learn more about racial succession in Dorchester and Roxbury. Focusing on the out-migration of Jews and the in-migration of African Americans, I analyzed the relationship between neighborhood change and the black-Jewish antagonisms of the late 1960s and early 1970s. I read several newspapers, analyzed census data, and searched for monographs on these neighborhoods, but found little on this relationship. Still, as I learned about these neighborhoods, I became increasingly puzzled by the explanations offered in various books and articles for the rapid exodus of Boston's Jews. The most widely held explanation attributed the exodus to a mortgage program established by Boston banks in 1968, but my own examination of census data, newspapers, and contemporary accounts of the neighborhoods suggested that the vast majority of the city's Jews had already moved by 1968. I had no alternative hypothesis for the swift exodus, but I was increasingly intrigued by the question.

When I finally began fashioning a dissertation prospectus, I returned to this question and formulated a crude hypothesis. I had recently read Wilson's account of the concentration of poverty and social problems in urban neighborhoods, in which he argued that the suburbanization of middle-class blacks had contributed to the decay of urban institutions and to neighborhood instability (Wilson 1987). Recalling that many of Dorchester's Jews in the late 1960s and early 1970s had expressed their concern about the synagogues and community centers that were closing in their neighborhoods, I speculated that local institutions are necessary for strong, stable residential communities. To test this hypothesis, I proposed to look at the Jewish and black communities of Dorchester and Roxbury. In discussions with my dissertation committee, I realized that I needed to fashion a set of hypotheses that were more tightly focused and more amenable to testing than this. So many things differed between the Jewish community of the 1940s and the black community of the 1990s that it was difficult to envision a research design that would permit me to investigate the impact of institutions in the two contexts.

As I sat with my committee, we began talking out loud about finding an urban Jewish community that had acted differently from Dorchester and Roxbury's Jewish community. We were grappling, together, to identify a control group. Drawing on personal experiences and on our knowledge of the literature, we realized that the Boston Jewish pattern seemed to be common to most American cities. The rapid Jewish exodus from Boston had its counterparts in the Bronx, in Philadelphia, in Chicago, and in Washington. That is when I started thinking about Catholics, Catholic institutions, and the different urban experiences of Catholics and Jews. I did not know much about Dorchester's Catholics, but I was vaguely aware that there was a large Catholic community in Dorchester and Roxbury and that those white Catholics had continued to live in their old neighborhoods long after Jews had left Dorchester and Roxbury. I realized that I had never come across an account of a Catholic church in Dorchester closing its doors, though I had found routine accounts of synagogue closings, and I began formulating a theory that linked the different neighborhood attachments of Jews and Catholics to the persistence of their institutions.

This was a first step. I spent the next year reading parish correspondence and pulpit announcements kept in files in the archdiocesan archives, sacramental records stored in the basements of Dorchester churches, and records of Jewish agencies. I began looking anew at census data and newspapers. And I started making regular trips out to Dorchester and Roxbury, meeting with priests and ministers and community leaders, walking down streets, collecting hundreds of photographs of buildings, sitting on church pews, walking down the corridors of parochial school buildings, and breaking into abandoned temples. Over and over, I kept asking the same question. I asked it of people, I asked it of old documents, I asked it of buildings. Why, I asked, did Jewish synagogues and community buildings close when Catholic buildings did not? A basic set of answers came into view in those first few months, and those answers-differences in membership rules, in the rootedness of institutions, in sources of authority-eventually gave structure to my final argument. I found those answers in my evidence, generating hypotheses as I began learning about Catholic and Jewish institutional rules, then testing the validity of those initial hypotheses as I expanded my investigation to include many more institutions and much larger periods of time.

Those hypotheses were themselves linked to another set of hypotheses about the impact of institutions on neighborhood attachments. As I learned that Catholic parishes were territorial units, with strictly defined boundaries, I speculated that parish boundaries maintained racial boundaries. Once blacks had successfully moved across those boundaries and settled in a new parish, according to an initial formulation of this hypothesis, white Catholics throughout the new parish would quickly move out. Hypothesis in hand, I began to learn more about the patterns of residential change. I began by studying a few parishes in a few years and discovered that the hypothesis needed refinement. While parish boundaries did appear to coincide with racial boundaries and while white Catholics did appear to defend those boundaries, racial change in the outer sections of a parish did not appear to cause a parishwide panic. On the contrary, Catholics continued to live in racially integrated parishes long after the initial change had occurred. The church building itself, like the parish boundaries, identified discrete areas of white residential persistence. Eventually, I created a set of block-level maps of these neighborhoods for each federal census between 1940 and 1990, coding these maps according to block-level census data. The first maps helped me in refining these hypotheses, while the later maps permitted me to test these hypotheses with fresh sets of evidence. This process of uncovering the connections between institutions and neighborhood change coincided with continued study of the nature of the rules that bound Catholics, but not Jews, to churches and boundaries.

Developing theory, collecting data, and testing hypotheses were thoroughly intertwined throughout the first year of this project, though in later stages I continued to uncover new bodies of evidence to test those hypotheses. The first draft of the dissertation reflects, far too well, the difficulties in sorting out evidence from theory. But even an ideal draft, one that specifies the theory and the findings with clarity, could not recapture the uncertainties, the confusions, and the dead ends that all good research entails.

"Social scientists often begin research with a considered design, collect some data, and draw conclusions. But this process is rarely a smooth one and is not always best done in this order: conclusions rarely follow easily from a research design and data collected in accordance with it," King, Keohane, and Verba (1994, 12-13) write. "Researchers often become discouraged. They mistakenly believe that other social scientists find close, immediate fits between data and research. This perception is due to the fact that investigators often take down the scaffolding after putting up their intellectual buildings, leaving little trace of the agony and uncertainty of construction."

Rewards

The work of recovering data from history is painstaking. New data are buried treasure, lost and unmapped. Reading a late-nineteenth-century newspaper to learn about the construction of a synagogue, the deliberations of the Republican caucus in the United States Senate, the activities of a local political club, or the legislative business of a state legislature means reading newspapers that are unindexed, that lack headlines, and that are printed eight columns to a page in extremely small type. Reading letters in an archive means reading one end of a two-sided correspondence, reading only the letters that were not deposited in another archive or destroyed altogether, and reading handwriting that is often barely legible. Reading lists of voluntary associations in city directories means first locating the directories, which are scattered in local historical societies all over the country. None of this is easy, but it is work that is part of the enterprise of discovering fresh sources of data to test and develop social science theories, and it is an adventure.

In their photographs and in their footnotes, history journals document this work and, implicitly, render the judgment that discovering evidence and bringing the reader close to that discovery is an essential part of the craft-the methodology-of history. These journals celebrate the work of data-gathering and acknowledge the obscurity and elusiveness of evidence. Though many historians do not regard themselves as social scientists, this work is a fundamental task for any social scientist. Sifting evidence-gaining familiarity with the original sources of any data that are utilized in a study, recognizing when specific evidence called for by a theory simply does not exist in the real world, creating new bodies of data-this is work that must be done even as hypotheses are being generated and shaped. Evidence does not exist to test many hypotheses, and most new theories and hypotheses arise only from direct encounters with evidence.

Footnotes, at least as a routine instrument for identifying sources, have been evacuated from political science journals. Political science articles are not cluttered on every page with the constantly distracting news that all data, quantitative as much as qualitative data, secondhand data even more than primary data, are abstractions and judgment calls. Most sources are tucked away, neatly, in parentheses. For many articles, which emphasize the analysis of existing data or the development of new theory, there can be no objection to this method of citation. But, as a universal method, this style is unfortunate. Not only does it offer little recognition-and, hence, incentive and reward-for the necessary work of discovering and assembling new bodies of data but it reinforces the mistaken impression that hypothesis-testing and data-gathering belong to different spheres.

References

King, Gary, Robert O. Keohane, and Sidney Verba. 1994. *Designing Social Inquiry: Scientific Inference in Qualitative Research.* Princeton: Princeton University Press.

Wilson, William Julius. 1987. *The Truly Disadvantaged: The Inner City, the Underclass, and Public Policy.* Chicago: University of Chicago Press.

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Reflections on History, Method, and Political Science[1]

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Of course, much that constitutes good research design is not distinctive to historically-oriented political science. We all need sharply-defined and significant puzzles, criteria for selecting cases (from one to a great many), appropriate strategies for inquiry, and command of relevant tools. Notwithstanding, attempts not just to work on historical materials but to make history a constitutive part of our research programs demands special attention to periodization, evidence, portability, and comparison.

[1] **This paper was prepared for the panel on "Methods for Historically-Oriented Political Scientists," Midwest Political Science Association Annual Meeting, Chicago, April 11, 1997.**

It is easier to do social science without taking history seriously. After all, the analytical space between the kind of social science that builds models or creates causal stories irrespective of the dimension of time and, in turn, the kind of history that refuses to separate processes and mechanisms from their embeddedness in place- and time-specific constellations is treacherous. History, the historian E. P. Thompson once wrote, knows no regular verbs; but just as grammarians learn to parse, diagram, and conjugate irregular verbs systematically by following prescribed rules of conduct, so must we.

1. **Periodization**. No historical enterprise of any kind is possible without at least an implicit scheme for periodization. "At the very least," Burnham has noted, periodization "is an organized prioritization of a vast and otherwise unmanageable flow of raw data." As an ordering of the significance of time, "a periodization scheme asserts that certain facts are at the core of what is going on, while others are more or less subsidiary to them."[2] Such schedules of priorities braid philosophies of history, analytical narratives, and hierarchies of variables. They also constitute ironic acts of willful simplification deployed to pursue the meaning of complexity. It is useful, I think, to distinguish four overlapping yet particular types of periodization schemes: those on the grand scale (like Marx's epochal delineation of feudal, capitalist, and post-capitalist modes of production or Karl Polanyi's claim that economic history has moved through the stages of reciprocity, redistribution, and market exchange); those organized by processes greater in scope than any particular country or place (as in periodization by war or economic cycles); those with a focus on processes bound up with place (as in theories about critical elections and realignment, ideological cycles, and presidential time in the United States); and those which are descriptive (when periods are given recognizable tags like the Age of Jackson or labels like the Gilded Age). My own work has oscillated primarily between the second and the third types. Among other subjects, I have written about political responses to black migration from colonial settings (the American South and the Caribbean) to 'mother country' liberal regimes (the North and Britain) treating immigration and the formation of structures of political representation as key processes and about the origins of the division between the labor and residence-focused identities and practices of urban residents in the United States. I usually proceed both retrospectively and prospectively. Retrospectively, in that I find or identify a puzzling or significant situation in the present (contemporary inequalities in racial representation or the perplexing fact that adversaries in American big cities usually share understandings that urban conflict concerns neighborhoods and services rather than workplace issues) and I seek to discover its 'origins' at a branching moment of relatively high uncertainty. These are moments when options open and then close off. I am particularly interested in identifying such moments, specifying the feasible set of options available with respect to relevant processes at the given time, understanding why this set of alternatives contracted, and considering what the durable 'path dependent' implications of such resolutions have proved to be. I search neither for a single grand organizing narrative nor do I merely want to offer a chronicle. Rather, I work in the middle-range, where I am agnostic about the number of relevant processes and potential schemes of periodization that might be appropriate, but where I also am convinced that key historical moments are those characterized by structural situations which magnify uncertainty, volition, and choice.

2. **Evidence**. History does not leave us systematic and nicely ordered data-bases. As John Goldthorpe assertively has put the point, history bequeaths relics but social science creates evidence, hence it is at an advantage.[3] This inescapable contrast presents difficult choices for those of us who work with historical materials. We can choose to focus only on subjects or materials where it is possible to construct evidence by manufacturing time-series of different kinds: of elections, congressional votes, budgets, the size of armed forces, and the like, in which each unit counts as any other. Or, we can go further to interrogate less systematic evidence methodically. I have done some of the former (as in a paper on the ante-bellum military for a collaborative project on the impact of international influences on American political development). This kind of work is relatively straightforward. It is when we move into the domain of 'relics' that the work gets harder. Here, I think we face two limitations but can try to turn them to advantage. First, we need to restrict the level of claim to what might be called simplified plausibility, aiming to develop lean narratives motored by crisp questions and modes of inquiry (more lean and crisp than most historians usually find comfortable). Second, we need to compensate for the irregularity of evidence by oscillating our perspectives; that is, by deliberately not putting all our eggs into single methodological or substantive baskets but by rotating the axes of our analyses. We also can compensate, at least in some measure, in a

[2] Walter Dean Burnham, "Pattern Recognition and 'Doing' Political History: Art, Science, or Bootless Enterprise?," Lawrence C. Dodd and Calvin Jillson, *The Dynamics of American Politics: Approaches and Interpretations*. Boulder: Westview Press, 1993, pp.66-67. For an earlier discussion, see, Walter Dean Burnham, "Periodization Schemes and 'Party Systems': The System of 1896 as a Case in Point," *Social Science History*, 10 (Fall 1976).

[3] John Goldthorpe, "The Uses of History in Sociology: Reflections on Some Recent Tendencies," *British Journal of Sociology*, 42 (June 1991).

tandem way, by engaging the full range of historiographical constructions of the same case materials, as Ian Lustick suggested in his recent *APSR* piece.[4]

3. **Portability**. Most members of our discipline, at least much of the time, seek to discover causal generalizations or fashion models that can be deployed independently of specific cases or instances. The more one works historically, however, the more difficult and problematical the pursuit of such impulses proves to be. Much behavioral political science asks questions and crafts responses written in the present tense. We ask whether campaigns, rather than a specific campaign, make a difference; we study war not the history of individual wars, and we seek to discover systematic relationships as if they exist outside of time. But if relations among variables are not invariant across time because the nature of the data points themselves change so much that the very categories and objects of analysis do not remain constants (a presidential campaign in 1924 is vastly different from one in 1984), the standard political science quest for generalizations which are valid irrespective of the given situation may exact too high a price (at a minimum, we need to be clear about the parameters within which we believe our generalizations hold, and why). So, too, do constant, rather than situated, approaches to rationality and human agency (for this reason, there is more of an affinity between seriously historical political science and bounded rationality–especially its realist sense of constraints on individuals not only because of their cognitive limits as decision-makers but as a consequence of restrictions imposed by structural situations–than with microeconomics-motivated rational choice work, especially when it takes its most aseptic, least embedded, and least institutionalist forms). When deductive theory is developed and applied in tandem with specific historical instances (as, say, in Avner Greif's brilliant work on late medieval Genoa[5]), the issue arises as to whether the goal of the exercise is that of employing relevant dynamic models to grasp mutually dependent interactions at the heart of key strategic processes in the specific instance or whether the purpose is that to build models that can be ported to very different cases.

My orientation to present-tense and portable social science is constituted by a mix of skeptical reserve, open-mindedness, and appreciation for the findings, tools, and grab-handles they offer. In any event, I usually work the other way around. I deploy theory (both analytic and normative) and the kitbag of propositions about political behavior we possess to function as connecting ligaments in historically-grounded inquiry. In putting together, as I presently am, an investigation concerned with the making and character of post-Second World War liberalism in the United States, I do not feel bound to organize the work to test the fit or power of a particular model or set of propositions, Rather, in keeping my eye riveted on my analytically-parsed object of analysis which stylizes historical events and outcomes as researchable puzzles, I feel utterly free to be opportunistic at key moments in the text's analysis of ideas, institutions, and partisanship, provided I explain precisely what I am doing, and why, at that moment in the book's exposition and argument. The closer I come to the narrative manner of the historian, the more compelled I feel to make my dianoetic reasoning for each part of the argument clear; nonetheless, it is the historical 'story' and its tough puzzles rather than the analytic as such that primarily drives the text.

4. **Comparison**. All social science is explicitly or implicitly comparative and counterfactual. But the comparisons we make are bedeviled, especially when we work historically, by difficulties with experimental and quasi-experimental designs given the small number of often deeply incommensurable circumstances with which we work. Comparativists in our discipline have presented us with four principal approaches to comparative research: the global or large-process, the case-comparative, the variable-comparative and the configurative. The first, as, say, in Immanuel Wallerstein's work on the world-system or Charles Tilly's and Hendrik Spruyt's competing accounts of European statebuilding endeavors to transcend the problem of cases by focusing on the larger relations and processes of which they are a part; this is the first-cousin of the grandest of periodization schemes, sharing its problems of abstraction and scope. The second seeks by a variety of more or less self-conscious efforts to justify the selection of cases and maneuver to overcome the problem of too many variables and too small an N by mustering soft versions of experimental approaches by making particular cases place markers for variables (the United States stands in for democracy or Nazi Germany for totalitarianism), thus raising vexing issues of biased case and dependent variable selection. The third, by contrast, decomposes cases into variables which are analyzed and compared via multivariate techniques, running the risk of slicing and dicing them so as to destroy the complex integrity of historical instances. The fourth incorporates the gains of the first two inside what might be called a relational and configurative form of analysis that interrogates different cases from the standpoint of a common agenda of concepts, models, and methods while taking great care not to unduly reduce the specificity of each (Barrington Moore's Social Origins of Dictatorship and Democracy provides an

[4] Ian Lustick, "History, Historiography, and Political Science: Multiple Historical Records and the Problem of Selection Bias," *American Political Science Review*, 90 (September 1996).

[5] Avner Greif, "The Institutional Foundations of Genoa's Economic Growth: Self-Enforcing Political Relations, Organizational Innovations, and Economic Growth During the Commercial Revolution," in Robert H. Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry R. Weingast, Analytic Narratives, forthcoming.

emblematic example). The best that can be hoped for beyond the elaboration of each case is the development of a series of guarded generalizations stopping well short of strict verification. I am a partisan of the fourth approach, thinking its balance of costs and benefits superior to the others but still far from optimal. Since each of these strategies exacts a high cost, we would do well to heed the sociologist Stanley Lieberson's recent call for modesty and caution when we are confronted with the attempt to draw conclusions from the comparison of a small number of instances.[6]

When I work these days, and when I consider how the elements of periodization, evidence, portability, and comparison might inform what I do, I often think about one of my favorite essays by a social scientist, Albert Hirschman's "Rival Interpretations of Market Society: Civilizing, Destructive, or Feeble?" This paper is concerned with the relationship among diverse and conflicting hypotheses about fundamental historical processes, and, from another angle of vision, with the compatibility of apparently incompatible theories and methods and with modesty in social research. Hirschman examined the emergence in the 18th and 19th centuries of arguments in favor of market society (commerce was seen as a civilizing agent, and an instrument of peace) and arguments that took the form of a critique of capitalism (markets undermine the moral, civic, and economic foundations of society). He further distinguished critiques which see current problems as caused by survivals from a feudal past and those which see unbridled capitalism as the problem. What, he asked, are we to make of this diverse and contradictory theoretical and analytical tableau? He urged us to refuse to choose. "However incompatible the various theories may be," he wrote, "each might still have its 'hour of truth' as it applies in a given country or group of countries during some stretch of time. This is actually how these theses arose, for all of them were fashioned with a specific country or group of countries in mind." Moreover, he cautioned, contradictory theses can hold at the same time. Thus capitalism can be self-reinforcing and self-undermining simultaneously. The moral basis of capitalist societies can be replenished and depleted at the same instant. "It is now becoming clear," he concluded, "why, in spite of our lip service to the dialectic, we find it so hard to acknowledge that contradictory processes might actually be at work in society," and he ended by asking, "is it not in the interest of social science to embrace complexity, be it at some sacrifice of the claim to predictive power?"[7] The more we work with historical materials as more than illustrations, tests, or grists for specific theoretical mills, the more Hirschman's formulation resonates.

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[6] Stanley Lieberson, " Small N's and Big Conclusions: An Examination of the Reasoning in Comparative Studies Based on a Small Number of Cases," Social Forces, 70 (December 1991).

[7] Albert Hirschman, "Rival Interpretations of Market Society: Civilizing, Destructive, or Feeble?," Journal of Economic Literature, 20 (December 1982), pp.1480-1481.

The New Institutionalism and the Study of Old Institutions[1]

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"History is a pack of lies about events that never happened told by people who weren't there."
- George Santayana

Those who cannot remember the past are condemned to repeat it.
- George Santayana (The Life of Reason, 1906)

History, in general, only informs us of what bad government is.
- Thomas Jefferson, ca. 1800

Introduction

This essay is about a philosophy of science, with a small "ph". My thesis is one that has become ubiquitous to positive political theory in recent years: institutions matter. The notion that "institutions matter" is not exactly a novel idea, nor is my focus on the application of the principles of positive political theory to the study of the institutions of the past. But my appeal here for greater attention to the principles of PPT and the New Institutionalism in historical work remains valid and important to the current generation of students of government if for no other reason than that most of the cases available to us for "testing theories" are historical ones. If PPT is to fulfill its promise to enrich the "scientific" content of social science research, practitioners must continue to expand its application to the study of historical phenomena and continue the dialogue begun between economics, political science, sociology and history in large part by the "new economic history" school, by such scholars as Alfred H. Conrad and John R. Meyer, Robert W. Fogel, Douglas C. North, and J. Morgan Kousser, among others.

[1] **An earlier version of these remarks was presented at the Annual Meetings of the Midwest Political Science Association, Chicago IL, April 10-12, 1997. Thanks to John Aldrich, Henry Brady and Renee Smith for comments on the earlier draft.**

In this essay, I will first test the waters by sketching a brief description of PPT and the New Institutionalism. I will then move to wading depth to argue that PPT is an appropriate tool for the study of Old Stuff. Finally, I will move to the deep end, treading water as best I can in order to suggest some methodological implications raised by the application of PPT to research in political history.

What is PPT/New Institutionalism?

For many of us, the New Institutionalism and positive political theories are, like hard-core pornography, subject to Potter Stewart's Law: hard to define precisely, but "I know it when I see it." As a consequence, it is darned hard to find a good definition of the paradigm written by a practitioner. Hence, I will turn first to a description provided from the outside, by self-described practitioners of the (non-rational choice) "historical institutionalism" variant of New Institutionalism.

> For the rational choice scholar, institutions are important as features of a strategic context, imposing constraints on self-interested behavior.... Thus political and economic institutions are important for rational choice scholars interested in real-world politics because the institutions define (or at least constrain) the strategies that political actors adopt in the pursuit of their interests (Thelen and Steinmo, 1992: 7).

Institutions are "the rules of the game that constrain individual choices and provide incentives for individual action" (McCubbins and Sullivan, 1987: 3). Practitioners of PPT seek to employ the tools of game theory and social choice theory to model real social situations, ranging from households' investment decisions, to legislators' design of statutes, to judges' interpretation of the law, to nations' making and breaking of alliances. Institutions structure strategic behavior in politics. Whereas in the abstract, relatively "unstructured" social interactions often lack equilibrium properties, real social interactions almost always are imbedded in institutional contexts, which very often impart equilibrium properties to those interactions (Shepsle 1986). New Institutionalists often take institutions as givens in order to model the consequences of institutional forms for social outcomes.

The goals of this enterprise are scientific - "the discovery of first principles and the identification of the empirical generalities to which they pertain" (Ordeshook, 1995: 176) - and practical - the bridge-building enterprises that Ordeshook labels "engineering." For the most part, the advancement of science is a social phenomenon; no single paper or research project is likely to provide the critical push that would allow us to announce with confidence that science has been advanced. Yet through the general weight of research, we hope the discipline as a whole manages gradually to learn both narrow facts and generalizable principles about the world around us.

Ordeshook pushes this line of reasoning even further - that we should seek knowledge not merely for the sake of knowledge (the "life of the mind"), but for reasons of practical importance as well. He argues that "the failure of political science to connect its practical objectives to its theoretical and methodological ones results in confusion about the phenomena that warrant theoretical treatment, the distinction between theory and model, and, ultimately, the methods whereby we make scientific advances" (1995: 182). Positive political theory is not immune from these criticisms. At the same time, however, the attention necessarily paid in formal modeling exercises to the basic assumptions about context and choice help make PPT a desirable vehicle for social-scientific research.

In general, PPT New Institutionalists prejudicially view institutions as solutions to collective dilemmas, i.e., as social welfare-enhancing equilibrium solutions to social conflicts (Bates, 1988; March and Olsen, 1984). Hence, a common enterprise for economic historians has been to demonstrate the efficiency of various institutional innovations. Not surprisingly, this superficially rather glib approach to some of the pathologies of history (slavery; unemployment; famine; war; prostitution, etc.) raises the hackles of many a solid citizen.

But the variants of PPT resident in political science and the public choice school of economics are much less appropriately subject to these critiques for two reasons. First, American political science is grounded in the critical study of institutions. The moral philosophies and pamphleteering of Locke and various Whig and radical critics of the British crown and parliament of the 1600s and early 1700s played an important role in the development of American thought about politics and political institutions. Bernard Bailyn's work on the origins of American politics reminds us very clearly that Americans have been deeply concerned with issues (and perversities) of institutional design from the very beginning. Hence, PPT-imbued political scientists (particularly Americanists) often differ from their more "conventional" Americanist brothers and sisters more in the detail than in the grander scheme of things. Witness Nelson Polsby's well-traveled query, "So what's new about the New Institutionalism?" (quoted in Thelen and Steinmo, 1992: 3; see also Koelble, 1995). Polsby has a valid point - one that young PPT sharks generally learn only after they have left the easy feeding that comes from tagging along with Mom, and have to learn to kill for themselves (a choice that young sharks best make early in life anyway, since the

adults in the discipline are well-known for eating their - or their colleague's - young).

Second, the mother's milk for PPT scholars today are Condorcet's Voters' Paradox and Arrow's Impossibility Theorem. We learn with our very first breaths that agenda control matters, and that determining what a community "wants" is non-trivial and cannot glibly be inferred from what a "community" implements through its local institutions. Hence, we fully expect the empirical world to be rife with patently unfair and, perhaps, even suboptimal (by some social-welfare criterion) institutional arrangements. As Riker taught us, it is political science, not economics, that truly deserves the label, the "dismal science." Politics is all about disequilibrium; institutions are in large part about how to keep disequilibrium at bay.

Why PPT for the study of Old Stuff?

Positive political theory provides a relatively consistent theoretical framework for constructing and evaluating models of political phenomena. It is healthy for an academic discipline as short on "Laws" and convincingly general "theorems" as is political science to reexamine periodically the validity of well-known "facts, " often enough perpetuated through chains of citations, which, when unraveled to their sources, become ever-more elusive or fantastic. More so than perhaps any other research perspective in the social sciences, rational choice/PPT provides a paradigm for scientific discovery. That is not to say that rational choice models unambiguously dominate other approaches in every instance, nor that non-rational choice approaches lack value (scientific or otherwise). Indeed, as Ordeshook (1992: 5)has argued,

> only the naive student believes that we can understand most of politics merely with a sustained effort at formal modeling and game-theoretic analysis.... [T]he insights that contemporary [formal] political theory offers must be combined in an artful way with a substantive understanding of the problem[s] at hand gained through experience.

The theory demands of its practitioners that they make a concerted effort to ground explanations in the incentives and (reasonably imputed) preferences of individuals. Concocting credible preference orderings for individuals is difficult enough in research on contemporary events. It is even more challenging for historical researchers because we are so far removed from the actors and events and because we are so often forced to interpret historical events through the interpretive lenses of third parties whose motivations may not be well known.

As Shepsle noted recently, it is high time that PPT scholars got on with the issues of empiricaqlly testing models of real political events. Greene and Shapiro's Pathologies of Rational Choice, he writes, "hammers home the fact that, for too long, positive political theory has been primarily a series of modeling exercises for which the excuse that its results are not yet ready for prime-time empirical testing is becoming increasingly lame," (1995: 216).

What are the Implications of PPT for the Methodologically-inclined?

For most practitioners of PPT, "rational choice theory" is paradigmatic. In other words, when we move towards testing our models, "We are unprepared to reject rationality and, thus, we are not 'testing' rational choice theory as much as we are estimating its parameters, refining our formulations of it, and adding to our intuition. ...our research can be productive only if we have a practical objective that organizes research around something other than the false belief that we are testing fundamental theoretical principles" (Ordeshook, 1995: 184). The testing enterprise is about comparing competing explanations. Often PPT scholars have found their models to be without apparent competition, at least in the sense that other explanations have been difficult to nest within the statistical model employed by the PPTer. When a modeler finds himself in such a situation, two things should happen. First, bright red lights should start flashing in one's head, to the effect of "What is the point of my paper? Why haven't other smart people thought of this before?" If, after proper introspection, the modeler remains convinced that he can pass the "So what?" test, the second issue hits - namely, what is the proper null model against which to assess my apparently original and brilliant idea?

Alternatively, for the less brilliant of us, we should redouble our efforts to generate adroit stylizations of existing explanations of similar phenomena in the literature. When Jonathan Katz and I were writing about committee tenure in the post-bellum House, a good deal of our efforts were devoted to coming up with what we thought were fair stylizations of the positions taken by Polsby and his co-authors on the one hand and Price on the other, vis-a-vis the development of the seniority system. While not everyone might agree with our characterizations of those works, at least we provided our readers with relatively firm targets.

Secondarily, the testing enterprise is about statistical modeling issues. Positive political theory places a strong emphasis on thinking hard about, first, the implications of one's model and second, the in-principle testable implications of one's model, and third, the feasible, testable implications of one's model. Contrary to popular belief, we like data. In fact, we love data. We just happen to be

somewhat particular about the kinds of data we want to collect and employ. Namely, a well-conceived model of an historical phenomenon will, with luck, inform the modeler as to what data he or she need collect in order to test the model. While the research enterprise is invariably a hybrid, inductive/deductive process, the goal in paradigmatic research should be to build deductively valid models that are testable, at least in principle. We then, as resources and tenure-clocks permit, proceed to gathering and analyzing the relevant data, rather than simply mining available data for new questions.

This perspective has broad implications for methodology. In my view, the best political science is driven by the questions one asks, not by the statistical aptitudes one has failed to acquire or the statistical fads one has dabbled in of late. Formal theories (both hard and soft variants), quoth Shepsle, "don't always lend themselves to...high-powered forms of [statistical] analysis. Indeed, precisely because such theories employ carefully specified concepts and often draw subtle distinctions, the normal sorts of data problems that plague all the social sciences are magnified" (1995: 219).

That is not to say that we are off the hook methodologically. Rather, it says that PPT scholars (and historically-oriented PPT scholars in particular) should endeavor to become widely read in the methodological arts. We need to design statistical models that are appropriate for our theoretical models and our data, rather than the other way around.

References

Bates, Robert H. 1988. "Contra Contractarianism: Some Reflections on the New Institutionalism." *Politics and Society* 16: 387-401.

Koelble, Thomas. 1995. "The New Institutionalism in Political Science and Sociology." *Comparative Politics* 27: 231-243.

March, James G. and Johan P. Olsen. 1984. "The New Institutionalism: Organizational Factors in Political Life." *American Political Science Review* 78: 734-749

McCubbins, Mathew D. and Terry Sullivan, eds. 1987. *Congress: Structure and Policy*. New York: Cambridge.

Ordeshook, Peter. 1995. "Engineering or Science: What is the Study of Politics?" *Critical Review* 9: 175-188.

Shepsle, Kenneth. 1995. "Statistical Political Philosophy and Positive Political Theory," *Critical Review* 9: 213-22.

———. 1986. "Institutional Equilibrium and Equilibrium Institutions." In Herbert Weisberg, ed. *Political Science: The Science of Politics*. New York: Agathon.

Thelen, Kathleen and Sven Steinmo. 1992. "Historical Institutionalism in Comparative Politics." In Sven Steinmo, Kathleen Thelen and Frank Longstreth, eds. *Structuring Politics: Historical Institutionalism in Comparative Analysis*. New York: Cambridge.

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Does Historical Political Research Pose Any Special Methodological Concerns?

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It is quite a privilege to be able to comment on three such rich and insightful papers. The most remarkable attribute to each one is the highly personal, sometimes even moving, encounters with historical research each presents. I cannot do this aspect justice. Let me instead make a few summary comments and add a couple of observations, but first let me pose a question.

The question is simply what makes a piece of research historical? I won't answer that question now. I will add two observations to it and ask that you keep the question in mind until the end. The first observation is that those who, like me, use the NES data tend to see it as more or less a single piece – entirely contemporary. The 1996 study marks 44 years since the first full survey, and next year's survey will take the field a full half-century after Michigan's first election study in 1948. The NES user community thinks there is nothing unusual (and probably a lot good) about a study of party identification or of turnout that uses the 1952 data, or the 1972 survey, or the 1992 study, or all three and more together. All are contemporary. Yet the 44 year interval spans a full 20the life of this nation, and the longer interval spans from Jefferson's presidency to the demise of the Whig party – at least two full historical eras by anyone's scheme. One is hard pressed to find much of anything about the Federalist-Jeffersonian Republican era that virtually anyone would say is of the same epoch as nearly anything in the rising Democratic-Republican era. The second observation is that my Duke colleague William

Chafe is, by any definition, a historian, and I am a political scientist, yet his writings are virtually all of post-World War II American politics, and I have begun to accumulate a body of studies of events that predate most of the events in his work by a good century or two.

Three Views of the Utility of History in Political Science

A few years ago, I claimed that we can think of three views of historical evidence. I called them (in an off-hand, verging on flippant, way); "history as data," "history as comparative politics," and "history as history." Much of the material presented in these three papers can been seen in these terms. Sala addresses "history as data" compellingly – which, in light of these essays, should clearly be retitled as "history for hypothesis testing." Gamm presents a vivid account of history as comparative politics. Katznelson examines all three and adds the strongest claims about what I called "history as history."

History for Hypothesis Testing: All three are keenly aware of the most obvious methodological problem; one that cannot be avoided in "doing history" (especially in its narrowest sense of studying "old institutions," to quote Sala). I'd call it "missing data," but that understates it too much. I'll extend Katznelson's "relics" and Gamm's "footnotes" to make the analogy later to paleontology, tapping both that discipline's extremely scanty and its extremely selective (that is to say, biased) record. Why, then, bother with these and other problems and thus why bother with history? The answer is that history offers two things to the contemporary record that are virtually irreplaceable.

The first thing is simply that history offers far more observations over 200 years (to continue to use the U.S. as example) than even 50 years, the longest definition of "contemporary." Those are often the minimum number of years needed to accumulate enough observations even to consider examining many ideas. Kramer's famous article (1971) on economics and congressional elections barely had enough observations to test the simplest of models and yield anything like reasonable standard errors (and that went back to the end of the last century – quite far for these kinds of model estimations). Tufte's midterm voting model (1975) nearly saturated the set of usable observations.

The second reason is more important even than scraping together enough data points to estimate even the simplest time series model. This reason is that the historical record offers us with circumstances that permit testing of theories – perhaps to test some theories at all, and more commonly to test different combinations of circumstances, to test different hypotheses, and/or to test them in different ways than the contemporary conditions permit. Sala provides an apt illustration with his work on abstention in Senate voting (Forgette and Sala, 1996). The much higher abstention rates over the first 150 years, plus, in the Senate afforded them the ability to test aspects of the theory of conditional party government in ways simply not possible using today's Senate. Given that the results were positive, they corroborated the theory in a particularly strong way, by virtue of testing new hypotheses on new kinds of data – much better than testing it the same way on the same kind of data, differing only by virtue of being a few years older. In my work on political parties (1995), I was able to test aspects of what I called the "strategic parties" hypothesis (because it is a variant of the Jacobson-Kernell [1983] strategic politicians hypothesis) precisely because of a condition that could only be found in the historical record – only one effectively organized major party.

History as Comparative Politics: I was struck by the similarity between Gamm's description of his work on Boston politics from the New Deal era on and a description Robert Bates wrote (1997b) of the process of doing his research on the politics of the International Coffee Organization (1997a). Most simply, both did field research. Both had that kind of direct immersion in the people, places, and institutions that makes the subject seep into your very pores. Both, as a first result, were at times lost in the fascination and complexity of their subject. But both, in the end, needed that immersion to make their theorizing possible and effective. I won't dwell on this point. Gamm and Bates' descriptions are too vivid and moving for me to do anything but recommend them to you.

Besides, Katznelson made the point I would have made, but he did so in his usual clear, simple and elegant way. "All social science is explicitly or implicitly comparative and counterfactual." He goes on to note, "But the comparisons we make are bedeviled, especially when we work historically, by difficulties...." He goes on to list four. There is, however, one that he missed, and one that I would like to raise as my comments about the third category. Before turning to this, let me add a note. "Comparative politics," as Katznelson made clear, is not the study of more than one nation. It is the study of politics by anything like a scientific method, whether it is of the U.S., the international system, former Soviet Republics, or the political psychology of a voter.

History as History: There is a problem with "doing history," if one takes seriously the argument that there is significance to the sequencing of the events studied – if one takes seriously "path dependency" as a major aspect of that about which one is theorizing. The simplest statement of the problem is: If the sequence being studied is path dependent, you have only one fully independent observation for each path-dependent process. No matter how many years, events, and people intrude, the nature of path

dependency is that every observation is, in part, dependent upon every other one. In my study of political parties, if the process is path dependent, then the whole of American party history is, in effect, a single case study. Moreover, a truly path dependent process differs from its independent cousin by virtue of the former never losing the importance and impact of the starting point, leaving therefore something critical outside of, and inexplicable within, the theory being examined. I'll return to this point below.

Some Observations:

Let me close with three comments.

1. The Methodological Problem, or What We Can Learn from Paleontology

The greatest methodological problem facing the historically oriented is that of missing data (often massively so) and systematic bias in what happens to survive as the available record. Here we should draw the analogy with paleontology. They have recorded for them a very, very small fragment of the past. Stephen Jay Gould likes to use the analogy of having an average of one letter from every fourth page of a book from which one can try to infer the full contents of the book. In addition, however, it is a very biased set of observations that make it into the fossil record – namely that which can be fossilized. Only rarely do any traces of soft tissues make it. Mostly, it is teeth and bones that do. So (as he also likes to say), it is not just a random sample of a letter every fourth page, it is a sample almost exclusively of vowels. Achen (1983) noted this problem as one of the greatest methodological issues facing the discipline, even though he was thinking of contemporary data. It is a pair of related problems raised to some considerable power in studying political history. The tasks are, of course, much like he called for then. I would only add that, perhaps also like paleontology, a revival of interest in indirect and unobtrusive interests, so brilliantly studied by Webb, et al. (1966), is well worth doing. Historians know this. Indeed, the turn to social history is a serious attempt to balance the biases of missing data – a far scantier (and harder to locate) set of "relics" can be found for the general public than for political and military elites. So, too, is Bates, et al.'s, notion of "analytical narratives" (forthcoming) to be understood at least in part as a method for using theory to close the missing data gaps. This leads me to my second observation.

2. Path Dependency, or Is the World One Big but Still only One Case?

Game theory has continued its recent development to become all but dominated by the derivation of multiple equilibria, and this is most nearly ubiquitous in the case of repeated and dynamic games – the closest analogues to historical processes. Path dependency is, thus, an ordinary feature of the most advanced products of rational choice theory. This observation is, of course, offering a prime example of what Katznelson refers to as "present-tense and portable" social science increasingly confronting the problems of multiple equilibrium and path dependency. As I noted above, a path dependent process, like the great majority of time series methods, never loses the importance of the first observation, and that first observation stands outside of the explanation (whether theoretically unexplained by game theory or statistically unexplained by time series estimation). Thus, at least as far as our imaginations have yet stretched, path dependent and multiple equilibrium processes have contingency – historical contingency – as a necessary part of the explanation. Katznelson seems to infer from the inherently contingent nature of historical explanation and from his wonderful appreciation of the richness and texture of history that he stands "skeptical, if open-minded, about present-tense and portable social science." He uses Greif's essay (forthcoming) as illustration.

What are we to make of Greif's essay, indeed of all the essays in Analytical Narratives? My current understanding (aided in no small measure by the experiences both Gamm and Bates report of eventually coming to see the [general] hypotheses while doing "comparative politics") is that historical contingency (and thus path dependency) is compatible with the principles of scientific explanation. Even the hard core logical positivists saw a scientific explanation as the conjoining of theoretically derived (and context general) propositions with empirical specifics of the case at hand. Greif and the others in Analytical Narratives are doing game theory; general, present-tense, portable game theory, but they are also doing case specific, historically contingent empirical research. Their propositions are general, but they are also specific. All or nearly all theoretical propositions in political science are conditional in the sense of "if...then." The empirical work is telling them the "ifs," the theoretical work is telling them the "thens." The conditions normally advanced as "theorems" in political science are, or hope to be, based on "ifs" that are sufficiently common as to be of general interest (e.g, if the voting system is plurality, etc., then Duverger's Law follows). The analytic narrative strategy does not care about the general applicability of the conditions, it does care to get the appropriate conditions for this particular case. There are other problems with analytical narratives, but the point is that the strategy is a serious attempt at combining theory with historical contingency. At its best, historical political science is scientific explanation (but it is not at least by itself, tense-less, portable theory), and Katznelson reveals his scientific instincts by being "skeptical, if open- minded."

This point, one might note, closes the circle by tieing "history as history," at least under the best (and presumably rarest) of circumstances back into being one with Sala's

"history as hypothesis testing." The three aspects that organized these comments, that is, are in reality unified. We are all historians and comparativists when doing political science. And, it probably is better to compare us to paleontology, a historically contingent, narrative science, than to physics, at least the simply present-tense everywhere portable parts of physics. Katznelson may reply that he differs from Sala, Gamm, and myself. But I think the difference is slight. I see it as the difference between Riker and Fenno, the one who leads with theory versus the one who leads with data, the theoretician versus the soaker and poker. What made Rochester's program so successful, however, was that the program was not simply Riker with most of his students taking Fenno's course. Rather the program was Riker and Fenno, joined for many of us and joined inseparably.

A side note: One "escape" from path-dependency, i.e., contingency, is to consider history not as a monotonic function of time, but as repetitious by virtue of being periodic or cyclical. Thus, one attraction of the partisan realignment division of American political history is that there is no longer a single historical case, but there are several cases of realignments, alignment eras, etc. Of course, this and other "solutions" to the contingency problem are useful only to the extent that periodization or cyclicality are tenable. Thus, Skowronek (1993) found historical cycles relevant for understanding the politics of the presidency, but he also found that the increasing density of institutions and the like, that was monotonically increasing with time, mattered. As a result, he could make some very interesting comparisons, but he found the comparisons he could make to be quite limited as well, because presidents at the same point in political (periodic) time were still located at a different point in institutional (or monontically increasing) time.

3. The Answer to My Original Question

The answer is that a problem looks contemporary, if in addition to the time frame including years from the post-World War II, the data are thickly collected; there are fewer missing observations and those that are observed are broadly, if not quite truly randomly drawn from the universe. The closer we are to the NES-like sets of observations, the more contemporary our analysis; the greater the missing and the greater the biased nature of our observations, the more we look like we are doing a bit of paleontology; the more historical our work – no matter what the dates of the observations. Thus, when Bates studies Africa, he is deeply into history, even when the observations come from the 1990s. When he moves to Latin America, he moves to something more nearly approximating the Gilded Age or even the Progressive Era (and, perhaps, in Mexico or Brazil, it may be like studying the New Deal). Similarly, with publication of works like Martis (1989), the Jeffersonian era has metaphorically moved forward in time dramatically, at least in certain aspects like Congress, elections, and parties. Thus, at least as a first cut, "doing history" means addressing a set of methodological problems.

References

Achen, Christopher H. 1983. "Toward Theories of Data: The State of Political Methodology." In *Political Science: The State of the Discipline*, ed. Ada W. Finifter. Washington, D.C.: American Political Science Association.

Aldrich, John H. 1995. *Why Parties? The Origin and Transformation of Political Parties in America.* Chicago: University of Chicago Press.

Bates, Robert H. 1997a. *Open-Economy Politics: The Political Economy of the World Coffee Trade.* Princeton, N.J.: Princeton University Press.

Bates, Robert H. 1997b. "With One Ear Cocked." *Harvard Magazine.* March-April. 99,4: 32-35.

Bates, Robert H., Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry R. Weingast. Forthcoming. *Analytical Narratives.*

Forgette, Richard, and Brian R. Sala. 1996. "Conditional Party Government and Member Turnout on Senate Recorded Votes, 1873-1935." revised version of paper delivered at the 1996 Annual Meeting of the Southern Political Science Association, Atlanta, GA November 6-9.

Gamm, Gerald. 1997. "Evidence and Argument: The Lessons of History." Prepared for delivery at the Annual Meeting of the Midwest Political Science Association. Chicago, IL, April 10-12, 1997.

Greif, Avner. forthcoming. "The Institutional Foundations of Genoa's Economic Growth:Self-Enforcing Political Relations, Organizational Innovations, and Economic Growth During the Commercial Revolution." In Robert H. Bates, et al., *Analytical Narratives.*

Jacobson, Gary C. and Samuel Kernell. 1983. *Strategy and Choice in Congressional Elections*, 2nd ed. New Haven, CT: Yale University Press.

Katznelson, Ira. 1997 "Reflections on History, Method, and Political Science." Prepared for delivery at the Annual Meeting of the Midwest Political Science Association.Chicago, IL, April 10-12, 1997.

Kramer, Gerald H. 1971. "Short-term Fluctuations in U.S. Voting Behavior, 1896-1964". *American Political Science Review* 65: 131-43.

Martis, Kenneth C. 1989. *The Historical Atlas of Political Parties in the United States Congress: 1789-1989.* New York: Macmillan.

Sala, Brian R. 1997. "The New Institutionalism and the Study of Old Institutions." Prepared for delivery at the Annual Meeting of the Midwest Political Science Association. Chicago, IL, April 10-12, 1997.

Skowronek, Stephen. 1993. *The Politics Presidents Make.* Cambridge, MA: Harvard University Press.

Tufte, Edward R. 1975. "Determinants of the Outcomes of Midterm Congressional Elections." *American Political Science Review.* 69:540-554.

Webb, Eugene, Donald T. Campbell, R.D. Schwartz, and L. Sechrest. 1966. *Unobtrusive Measures.* Chicago: Rand, McNally.

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An Alternative Conjecture - Guide to King's *A Solution to the Ecological Inference Problem*

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When data is aggregated, some information may be lost. For example, suppose you have two sections of 10 students each rating your TA's performance. The first section has 5 good students and the second has 4. The TA is rated either positive or negative. In section one, you notice that all the positive scores are given by the 5 good students, and you know that the TA only got 4 positive marks in the second section. Would you bet $100 that it was the 4 good students who recommended your TA? Maybe not. There are chances that less than 4 good students did. Why? Well, maybe one or two of them did not think the TA was recommendable given that more than half of the section was not doing well. And maybe one of the not-so-good students got extra help from the TA when the TA tried to raise the average level of the section, thus gave him a positive rating. As you can imagine, only reporting that 4 recommended the TA does not convey enough information; the more interesting question is who gave the TA the positive recomendations.

Now imagine a method that can estimate who did when you have a number of sections. That is what Gary King claims in his book, *A Solution to the Ecological Inference Problem – Reconstructing Individual Behavior from Aggregate Data* (Princeton University Press, 1997). In this article I offer a simplified version of the 342-page book. The first part is a brief introduction to the problem of ecological inference, followed by a more detailed explanation of the method. You will encounter some mathematical formulas, but you will not get lost even if you skip the mathematical part. And after you see this approach, I will talk more about how easily you can do it on your computer should you find it applicable to your research.

The Problem

Vote outcome is usually reported in aggregate form due to secret ballot. To study either vote choice or the turnout rate, the picture we get from the data is often incomplete. Take the case where the data is reported at the precinct level, the following illustrates how the aggregate data is usually structured.

Race of Voting-Age Person	Voting Decision: Democrat	Voting Decision: Republican	Subtotal Turnout
black	λ_i^b	$1-\lambda_i^b$	β_i^b
white	λ_i^w	$1-\lambda_i^w$	β_i^w
	D_i		T_i

Race of Voting-Age Person	Voting Decision: Not Turnout	
black	$1-\beta_i^b$	X_i
white	$1-\beta_i^w$	$1-X_i$
	$1-T_i$	n_i

where:

1. X_i, T_i each indicates a proportion out of n_i.
2. β_i is the proportion who vote out of X_i. [β_i^w is the proportion of whites who vote in the i^{th} precinct; β_i^b is the proportion of blacks who vote in the i^{th} precinct.]

3. The superscripts indicate the race of voting-age person.[1]
4. λ is the proportion of voters voting for a Democrat.

Empirically, the quantities denoted by the English letters in the table are what's get reported, while those denoted by the Greek letters are what we want to find out. As the running example to explain the model, let's look at the right half of the table and solve β with T and X first. An "accounting identity" summarizes the relationship between the knowns and the unknowns:

$$T_i = \beta_i^b X_i + \beta_i^w (1 - X_i)$$

The problem now is that for every observation i, we have two parameters to estimate: β_i^b and β_i^w.

If you have ever encountered problems where there are more parameters than observations, you know there are a couple of options. One is to give up on the problem, at least quantitatively. The other, which is often preferred since it brings possible publications, is to reduce the number of parameters in certain ways till you can handle the problem. For example, one way to estimate the accounting identity is to assume that all β_i's are the same. This is substantively equivalent to assuming that the proportion of whites voting is constant across precincts, and that the proportion of blacks voting is constant across precincts. Then we have two parameters (β^w and β^b), and as many observations as the number of precincts. Such an estimation method was proposed by Leo Goodman in 1953 (Goodman 1953), and still shows up in journal papers nowadays from time to time.

Besides the problem that β_1 may not be the same as β_{101}, a practical constraint also prevents Goodman's regression from giving reliable answers. That is, sometimes the estimated β is out of the range of $[0, 1]$. And it is rather hard to face a negative turnout rate or a turnout rate over 100%. More precisely, we can narrow down the range of β_i's as follows:

$$max\left(0, \frac{T_i - (1 - X_i)}{X_i}\right) \leq \beta_i^b \leq min\left(\frac{T_i}{X_i}, 1\right)$$

$$max\left(0, \frac{T_i - X_i}{1 - X_i}\right) \leq \beta_i^w \leq min\left(\frac{T_i}{1 - X_i}, 1\right).$$

On the other hand, the problem here is not that different from classic regression analysis. For simplicity, suppose we observe a bunch of data points in a two dimensional space. Assuming the Gauss-Markov conditions, we fit a regression line to extract information about the origin of the data by minimizing the discrepancies between the line and the data. In other words, we obtain $\hat{\beta}$ and its distribution to summarize the information the data carry. Now the case goes up one dimension. A slight rearrangement of the accounting identity shows that instead of data points, now we observe data lines, all of which have negative slopes and are in a unit square:

$$\beta_i^w = \left(\frac{T_i}{1 - X_i}\right) - \left(\frac{X_i}{1 - X_i}\right)\beta_i^b. \qquad (1)$$

So instead of fitting a line in two dimensions, we can fit a two dimension contour to encircle the intersections of the many observed lines. Figure 6.3 from King's book provides a visual feeling of how it can look (see next page for figure). Notice that every line in the graph is determined by equation (1). See that they all show negative slopes.

Like the regression lines which are the projection of a three-dimensional distribution onto a two-dimensional surface, the contour lines in the figure are also the projection of a bivariate distribution, with the parameters of the distribution printed at the top of the figure. In fact, the figure corresponds to the graph following it (figure 6.4 from King's book.)

To put this in words, what this approach does is to assume the parameters we want to estimate, each β_i^b and β_i^w, are drawn from some distribution distribution. The distribution will have a set of additional parameters that we can estimate. With the estimated distribution, we can then obtain a univariate distribution as each negative-sloped lines in Figure 6.3 slices the three-dimensional distribution in Figure 6.4. The two-dimensional cross section will then be the posterior distribution of each β_i conditional on all the information borrowed from all other precincts.

II. *"A Solution"* Summarized

Three assumptions are required by the model:

Assumption 1. β_i^b, β_i^w are generated by a truncated bivariate normal distribution conditional on X_i, i.e.,

$$P(\beta_i^b, \beta_i^w) = TN(\beta_i^b, \beta_i^w \mid \mathcal{B}, \Sigma)$$

where

$$\mathcal{B} = \begin{pmatrix} \mathcal{B}^b \\ \mathcal{B}^w \end{pmatrix} = E\begin{pmatrix} \beta_i^b \\ \beta_i^w \end{pmatrix}$$

$$\Sigma = \begin{pmatrix} \sigma_b^2 & \sigma_{bw} \\ \sigma_{bw} & \sigma_w^2 \end{pmatrix} = Var\begin{pmatrix} \beta_i^b \\ \beta_i^w \end{pmatrix}$$

[1] You may understand "white" as "non-black", and "Republican" as "non-Democrat".

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Assumption 2. β_i^b, β_i^w are mean independent of X_i.

Assumption 3. Values of T_i in different precincts are independent after conditioning on X_i.

You may get the feeling that you have seen these assumptions somewhere. Does the Gauss-Markov theorem ring a bell?

For simplicity of notation, let's call the vector of parameters of the truncated bivariate normal distribution in assumption 1 Ψ, and the untruncated version of it $\breve{\Psi}$. They each have 5 elements in the vector: 2 mean parameters, 2 variance parameters, and a covariance parameter. Obviously they are what we want in order to derive the point estimation of all $\beta's$. For the convenience of estimation, $\breve{\Psi}$ is transformed to ϕ with Fisher's "Z transformation" (1915). It is important to keep in mind that Ψ, $\breve{\Psi}$, and ϕ can be derived from each other easily, and we first approach ϕ which is most easy to estimate.

Bayesian updating yields a way to derive ϕ from the known data of $T_i's$. Specifically, $P(\phi \mid T) = P(\phi)P(T \mid \phi)$, where $P(\phi)$ is the a prior, and $P(T \mid \phi)$ is the likelihood function. For the a prior, the mean parameters as elements in the vector ϕ are assumed to be flat, the variance parameters are log half-normal with variance of 0.5, and the covariance parameter is normal with mean 0 and standard deviation 0.5. The likelihood function is multiplication of normal distributions of T_i conditional on $\breve{\mathbf{\Psi}}$. Given both, we can figure out the posterior distribution $P(\phi \mid T)$.

Having a posterior distribution of ϕ brings us one step closer to obtaining $\breve{\Phi}$, which will determine the distribution of $\beta's$. If you like, you can use the above three assumptions to derive the conditional distribution of β_i^b on T and $\breve{\Psi}$. Or you can trust me that it looks like this rather complicated thing below:

$$P(\beta_i^b \mid T_i, \breve{\Phi}) = N\left(\beta_i^b \mid \mathcal{B}^b + \frac{\omega_i}{\sigma_i^2}\varepsilon_i, \sigma_b^2 - \frac{\omega_i^2}{\sigma_i^2}\right) \frac{\mathbf{1}(\beta_i^b)}{\mathbf{S}(\mathcal{B}, \mathbf{\Sigma})}$$

where

$$\begin{aligned} \omega_i &= \sigma_b^2 X_i + \sigma_{bw}(1 - X_i), \\ \varepsilon_i &= T_i - \mathcal{B}^b X_i - \mathcal{B}^w(1 - X_i), \end{aligned}$$

$$\mathbf{S}(\mathcal{B}, \mathbf{\Sigma}) = \int_{max\left(0, \frac{T_i - (1 - X_i)}{X_i}\right)}^{min\left(1, \frac{T_i}{X_i}\right)} N\left(\beta_i^b \mid \mathcal{B}^b + \frac{\omega_i}{\sigma_i^2}\varepsilon_i, \sigma_b^2 - \frac{\omega_i^2}{\sigma_i^2}\right) d\beta^b,$$

$$\beta_i^b \in \left[max\left(0, \frac{T_i - (1 - X_i)}{X_i}\right), min\left(1, \frac{T_i}{X_i}\right)\right].$$

But keep in mind what we ultimately need is the unconditional posterior distribution of β_i^b. There are a couple of way to get there. First, we can try to integrate it up from ϕ. Namely, we solve for

$$P(\beta_i^b \mid T) \propto \int P(\beta_i^b, \phi \mid T) d\phi$$

which proves to be too much to handle. Alternatively, we go back to $\breve{\Psi}$ to deal with

$$P(\beta_i^b \mid T) = \int_{-\infty}^{+\infty} P(\phi \mid T) P(\beta_i^b \mid T, \breve{\Psi}) d\breve{\Psi}$$

with all the help we can get from the computer. Instead of carrying out the integration, we can use Monte Carlo method to simulate the posterior distribution. The logic is that with enough sample points of $\breve{\Psi}$, $P(\beta_i^b \mid T, \breve{\Psi})$ will be equivalent to $P(\beta_i^b \mid T_i)$. In other words, the computer simulates the integration process by filling the area to integrate with all possible values, thus gives us a kernal density of the unconditional distribution.

The exact steps to accomplish this are not difficult. Remember that we already got the distribution of ϕ. Now, draw a ϕ value from $P(\phi \mid T)$, and convert it back to $\breve{\Psi}$. Then, insert this $\breve{\Psi}$ value to equation (2), and draw a value of β_i^b randomly from it. Repeat this procedure and plot all the values of the β_i^b in a histogram. Given enough repetitions, the histogram will become smooth and resemble a kernal density. This kernal density is the approximation of the distribution of β_i^b. And if you take the mean of this distribution it will be your point estimate of the turnout rate by blacks in that precinct!

So this is the "generalized method of bounds." Now that you have obtained point estimates of parameters for each β_i, don't hesitate to go back to the table at the beginning of the section and see what else you want to get next. And from the precinct-level results, it is easy to calculate the parameters of interest for more aggregated level areas, such as district or county. The method also applies to estimation of joint probabilities in larger than 2 by 2 contingency tables. For more interested readers, the book also provides discussions on how the model works with slight adjustment when some of the assumptions are not met.

III. Regarding Implementation

Of course now the problem is how difficult it must be to turn the above calculation into computer language. When the author of this method patiently leads you through

his deduction steps on paper, he does not want to lose you in the real data crunching either. To achieve that, he prepared two computer programs. One is written in GAUSS language, called EI, that will do most of the work for you. The other is a menu-based system that does not require any software, called E_zI. Both are available from the web at `http://Gking.Harvard.Edu`. Since EI is capable of more functions, I will briefly describe the procedure here.

First of all, you need the statistical software GAUSS on your computer. It is a widely used program for statisticians and applied statisticians in many disciplines. A module is available for GAUSS that does maximum likelihood estimation. You will need to install it as well. Then you are ready to download the EI program. Follow the detailed instructions in the file `README`, you will know how to put different files into their directories. At this point, you can use the enclosed `sample.asc` file to test run the program. As you will see, after you successfully initialized EI, all you need to do to get the results is to give the program the observed T_i, X_i, and n_i either in ASCII format or as a GAUSS data set. EI is also able to draw a variety of graphs, as all the figures in the book.

Last Remark

Compared with the other methods attempting to solve the ecological inference problem, the generalized method of bounds is just "a solution". While I have no intention at all to discuss the relative merit of each method (in another 50 pages), it is not a bad idea at all to recapitulate why the generalized method of bounds can distinguish itself.

It is first a parametric approach to estimate quantities that lack apparent parametric structure. Take the TA ranking example again. We can first assume that there exists a truncated bivariate normal distribution on the percentages of good students across sections who think their TA is recommendable. It is probably consistent with your intuition that you can deduct the evaluation of the TA by the good students in section two with what you see in section one. Since the distribution chosen is truncated, all the estimates will fall within the [0, 1] interval and avoid unexplainable results. More importantly, you need to have many sections. The estimation of each quantity of interest depends upon the borrowed explanatory power from each and every other observation, systematically yet not deterministically. It's a case of "power in number." Without enough data, extreme values often have disproportionate influence, and the confidence interval falls rather wide. Last but not least, this approach sets a good example for applying the Bayesian thinking and numerical simulation in political data analysis. Maybe now fewer political science students will look into *Famous Political Philosophers* to find out about Bayes.

Fang Wang is a doctoral student at Caltech. Her research interests include methodology and its application to the study of public opinion and racial politics. Her dissertation is a systematic and comparative study of the voting on Proposition 187 and 209 in California, shedding new lights on the theories of interest politics and symbolic politics.

Stata 5 — A First Look

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Version 5 of STATA was released in October, 1996. Since *TPM* recently reviewed Version 4 (Charles Franklin and Jim Wenzel, Vol.7, Number 1), I will focus here on changes between the two versions. The look and feel of STATA has not changed between the two versions; all the features that Franklin and Wenzel praised remain unchanged. A few annoying features have been fixed up, and several new exciting statistical routines have been added. (For those considering whether to upgrade, a full list of new features is available on the STATA Web site: `http://www.stata.com`.)

A New Manual — Hooray!

Most users will be glad to hear that STATA now has a new manual which does not quite have the look and feel of the previous version. Rather than attempting to categorize commands, forcing users to guess whether a command was "5d" or "5s," the 3 (yes 3!) volume reference manual is now organized alphabetically. No more guessing which volume contains which command. The package also comes with a very nice and comprehensive User's Manual, which should enable those new to STATA to get started fairly painlessly.

The manual is also very nicely written. Each command is fully documented, with a full description of the syntax (no hidden "features"). Each command also comes with a chatty discussion of how it might be used and a lot of examples. There is also a decent discussion of the underlying statistical model and a listing of all formulae in a consistent notation. The user does not have to guess what STATA is actually doing. The cost of this is that you have to wade through 20 pages to see all the documentation of the regress command, but here more really is more. The User's Manual also makes it easy for the impatient to avoid much of the reference manual. One particularly nice feature of the manual is that it says things like "econometricians call this technique conditional logit but biometricians call it case-control." There are few manuals that speak as clearly to these two disparate audiences.

Under the Hood

The user of Version 5 might think that they had mistakenly reinstalled Version 4 — the screen presentation and interface is identical. Since the interface wasn't bad, this is a good thing. And users of Version 4 will be happy to know that there is full backwards compatibility. While those of us who like new toys might be disappointed, there are two new features under the hood that might dramatically enhance the look and feel of STATA . The first is the ability to program menus, such as a menu to handle graphic or regression. Those familiar with the most recent versions of STATAQUEST (the undergraduate version of STATA) have already seen these menus. While I am not sure why a UC undergraduate cannot type `regress y x`, my undergraduates seem to like these menus. (I stuck with SYSTAT for many years to satisfy this demand for icons.) These Stataquest menus may be incorporated into STATA via a free update which is available from the Stata web site. (The menus can then be turned on and off, which is nice.)

The other new feature under the hood is a series of low level graphics routines. Those appear to give the user as much command over low level graphics (such as font size) as we now get in SPLUS . Those with anal co-authors can now spend as much time in STATA getting the perfect legend box as they now do in SPLUS . But, as of now, these low level commands cannot be used interactively. Presumably they will become more useful as people write routines which take advantage of these commands, but if you want to do interactive design, painstakingly choosing every feature of a graph, you are still going to have to use SPLUS . In any event, the high level graphics that most people care about are still quite nice. The new version makes it easier to produce postscript for embedding in other documents.

There are also a few changes in memory management that should improve STATA 's performance. Version 5 also eliminates the annoying need for the user to partition memory before reading in large datasets. No one will miss the error message "not enough room to add more variables."

New Statistical Routines

All routines that were available in Version 4 are still available. Even where commands have been changed, people can still use the old formats. The workhorse regression routines appear reasonably similar in the two versions, with the new version having more of a commitment to robust standard errors (and a nice consistent treatment of them). The new version provides a rewritten set of survival and panel routines and a new set of survey commands.

Panel Data

Perhaps the most important new feature is STATA 's implementation of the Liang and Zeger (1986) "general estimating equation" which extends the GLM framework to longitudinal studies. There is no longer any excuse for researchers with binary dependent variable panel data to ignore temporal dependencies. The new routines also allow for easy estimation of random effects models with binary or count dependent variables. And of course the new routines compute panel correct standard errors as well as the usual GLS estimates. The data setup for all these routines is quite nice, and it is easy to generate both unit and time dummy variables. This set of routines puts STATA at the top of the pack for panel/time-series–cross-section data.

Survival

The rewritten survival routines allow for the flexible estimation of both Cox proportional hazard and Weibull (including exponential) models. In particular, both allow for time varying covariates and repeated events, as well as right censoring and left truncation. The data setup for time varying covariates or repeated events is often hellish; STATA makes it almost painless. The routines all have nice graphics, and allow the user important diagnostic tools, including martingale residuals.

SPLUS sets the standard for survival routines for the applied researcher. STATA now appears to do more than SPLUS , and with less pain to the researcher. LIMDEP still has some parametric features that STATA lacks (generalized gamma, split populations and heterogeneous Weibull) but my guess is that most survival analysts will find STATA to provide all that they need. This set of routines is a major advance.

Surveys

A new set of routines allow for the generation of summary statistics and regression/logit/probit which allow for complicated sampling designs (clusters and stata). These are painless to setup, and should allow for more accurate inferences.

For Whom Does it Work?

STATA is clearly a general purpose package, competing with the likes of SAS and SPSS . It is much smaller than those two (it fits easily on 3 disks), but seems to have much of the power of its larger cousins. It handles messy data, makes it easy to do whatever we like to do to our data and clearly allows users to do most of the statistical things that most people do. Franklin's review of Version 4 concluded "If I didn't own Stata, I'd rush out and buy it. ... And if you own a half dozen packages ... you will find that you use Stata more than any of the others for data

analysis." There is little doubt that Version 5, with its major improvements in survival, survey and panel analysis, will meet even a greater proportion of the analysts computing demands.

This is not to say that we can abandon that half dozen other packages. Serious time series users will want to use something like EVIEWS (STATA doesn't handle integrated time series and doesn't have some other nice features that are only available in pure time series packages). The serious choice analyst will still need LIMDEP (STATA is fine for logit and probit, both conditional and unconditional, but has no support for nested multinomial logit and mprobit). The modern regression user is still going to have to use SPLUS , and the serious simulator will probably continue to use GAUSS (though STATA does have a decent maximum likelihood routine and a flexible programming language which allows for simulations and bootstraps). But users of all those packages still need a general purpose workhorse, such as STATA . STATA continues to do more and more of the very technical statistical routines. Version 5 has made impressive strides, obviating some of the need for specialized programs. Perhaps Version 6 will knock out another specialized competitor. It seems to me as though STATA may be the general program of choice for many, if not most, readers of *TPM* and if not now, may soon, obviate the need for at least some of those other half dozen packages.

Ordering

Stata Corporation may be contacted at (800)STAT-APC or via their web site. Version 5 runs on most reasonable platforms. For Windows (3.1 or 95), a single user academic license is $395 ($195 to upgrade from Version 4). Generous laboratory site licenses are available.

References:

Liang, Kung-Yee and Scott L. Zeger. 1986. "Longitudinal Data Analysis Using Generalized Linear Models." *Biometrika* 73:13–22.

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Dyad-Hard: The Interstate Dyad-Year Dataset Creator

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Introduction

Over the last ten years, international relations scholars have devoted considerable time addressing issues of research design. Moreover, particularly in the study of war in general, and militarized disputes in particular, some nations have such a small opportunity to interact with each other that their likelihood of conflict is inordinately low. Thus, recent foci has concentrated on pairs of nation-states that have what Most & Starr (1989) call "opportunity" and "willingness" to wage conflict on each other. More specifically, the Maoz & Russett (1993) notion of a *Politically Relevant Dyad* (PRD), which considers interaction opportunity mechanisms, primarily based on geographical proximity (Most & Starr 1989, Most & Starr 1990, Cioffi-Revilla & Starr 1995), has become the *sine qua non* of theoretical case selection in quantitative studies of war and peace. Unsurprisingly, a considerable amount of the recent IR literature in general, and the analyses of wars and militarized disputes in particular, has privileged a theoretical and/or empirical focus on the dyad-year as the unit-of-analysis. I have written *Dyad-Hard*, a Stata program that generates a time-series–cross-section dataset that consists of the population of all interstate dyads (1816-1994), to advance the quantitative study of interstate dyadic interactions.

Overview

Dyad-Hard is a program that creates a STATA dataset consisting of all the dyad-years (1816-1994) for Small & Singer (1982). *Dyad-Hard* constructs the sample by using the "COW Interstate Membership Dataset (1994.1)" posted on the Peace Science Society (International) web site (`www.polsci.binghamton.edu/peace(s)/intsys.htm`). The program takes less than 10 minutes on a Pentium 90 Mhz and should yield 550,393 cases, or dyad-years.[1].

The program, at this time, can only be run from a statistical program called STATA. For more information on STATA check out the following web-site:

`http://www.stata.com`

Moreover, you will need a minimum of 16 megabytes of RAM (actually you can get by with 8 megabytes if you know how to mess around with virtual memory swapping) and about 54 megabytes of available hard-drive space.

Directions *PC* (Dos or Windows)

1. Make a directory on your hard-drive called `dyadhard`

[1] Some recent applications of *Dyad-Hard* can be found in Gates and McLaughlin (1996) and Thompson & Tucker (1997)

2. Place the self-extracting zip file *dyhard21.exe* in this directory
3. At the `c:\dyadhard` *dos* prompt, type:

   dyhard21
4. The files in the "dyadhard" directory are:

   dyadhard.ado
   dyhard21.exe
   dyadhard.hlp
   stateaga.raw
   stateagb.raw
5. Enter Intercooled STATA
6. Once in STATA, and along the appropriate path, type:
   help dyadhard

Unix

1. Make a directory in your Unix account called `dyadhard`
2. Place the zip file *dyhard21.zip* in this directory
3. Type:

   unzip *dyhard21*
4. The files in the "dyadhard" directory are:

   dyadhard.ado
   dyhard21.zip
   dyadhard.hlp
   stateaga.raw
   stateagb.raw
5. Enter Intercooled STATA
   stata -k12000
6. Once in STATA, type:
   help dyadhard

Dyad-Hard may be used and modified by anyone, but please leave my name in it at all times. Moreover, please obtain permission before redistributing this program over the internet or any other medium. I would appreciate it if you would let me know if you use this program in a paper (Please cite as Tucker 1996).

Have fun and do not hesitate to contact me if you run into any problems while trying to install and/or implement this program.

References

Cioffi-Revilla, Claudio and Harvey Starr. 1995. "Opportunity, Willingness and Political Uncertainty." *Journal of Theoretical Politics*, 7:447-476.

Gates, Scott and Sara McLaughlin. 1996. "Rare Events, Relevant Dyads, and the Democratic Peace." Paper presented at the Annual Meeting of the International Studies Association, San Diego.

Maoz, Zeev and Bruce Russett. 1993. "Normative and Structural Causes of Democratic Peace, 1946-1986." *American Political Science Review*, 87: 639-56.

Most, Benjamin and Harvey Starr. 1989. *Inquiry, Logic, and International Politics.* Columbia, South Carolina: University of South Carolina.

Most, Benjamin and Harvey Starr. 1990. "Theoretical and Logical Issues in the Study of International Diffusion." *Journal of Theoretical Politics*, 2:391-412.

Thompson, William and Richard Tucker. 1997. "A Tale of Two Democratic Peace Critiques." *Journal of Conflict Resolution*, 41:426-452.

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Why I Use Linux

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Imagine having a powerful workstation on your desk that integrates seamlessly with the campus network. Imagine having a computer that can run a long statistical process in the background while you use Netscape and work on a paper you are writing. Imagine that this same computer is serving your web pages and receiving your e-mail, and that you can access it from home or from anywhere in the world using the Internet. Imagine that this is your multitasking workstation using a 200-Mhz super-fast processor, with a high-resolution color graphical X-windows user interface. Now grab a tissue and wipe that drool off your *TPM* and brace yourself for the best part of all: the total cost of this system was only $2,500!

That's because the computer in this reverie is an inexpensive Pentium/200 running the free Linux operating system. In this article I describe Linux, identify its chief

advantages (lots) and its main disadvantages (few). What follows is less of a formal review than an unabashed attempt to convince you that Linux is worth using, or at least trying. Linux users are notorious for their zealotry, and rather than attempt to appear objective I will simply make my prejudices plain. I have been using Linux for my main computing needs for over a year now and cannot imagine using anything else—including much more expensive commercial UNIXes. So in what remains I will describe Linux and its hardware requirements, and then identify two different groups of political methodologists and why they might want or not want to use Linux.

What Linux Is

Linux[1] is a version of the UNIX operating system—a multitasking, multiuser operating system originally designed for high-powered mainframes and workstations—for systems generally known as "personal computers." Linux comes from an independent source code base, but behaves exactly like UNIX built on licensed code. In technical terms, Linux reimplements the POSIX specification with SYSV and BSD extensions; in practical terms, Linux *is* UNIX.

One of the very best features of Linux is its price. Believe it or not, Linux is maintained by a worldwide team of volunteer programmers, and distributed for free! Linux was originally written by living legend and computer folk hero Linus Torvalds in 1991 while he was a computer science student at the University of Helsinki.[2] In the past several years, Linux has become skyrocketed in popularity and sophistication. Nearly 85% of computers on the world wide web run some version of UNIX, and it is estimated that a huge proportion of those are Linux boxes.[3]

The Linux kernel—the basic operating system itself—is updated on a practically weekly basis by Linus and teams of programmers around the world. The source code is completely open, covered by the GNU Public license which prohibits selling it except for the distribution costs. This has allowed many commercial providers to bundle their own mixtures of Linux and application software, known as "distributions," but has kept the cost to a reasonable figure of between $15 and $100. And even the commerical distributions may be downloaded directly from the Internet for no charge.

Linux is a completely stable operating system. I have run it for over a year using the beta versions of the new 2.x kernel sources until these were declared "finished," and not a single time has my system locked up. I cannot make this claim for any other operating system I have ever used. Furthermore, Linux use and product support has exploded in recent years and shows no sign of slowing down. Drivers, for example, are hardly ever a problem these days, and commercial software increasingly comes in Linux versions. Perhaps once the bleeding edge, and currently the cutting edge, Linux is on its way to becoming a major established player in the operating system world.

System Requirements

Originally written to run on the Intel 386 platform, Linux now runs on a wide variety of architectures. Most common are the "x86" Linuxes, running on the Intel, Cyrix, and AMD versions of the 486, Pentium, 6x86, 5x86, and K5 chips. But Linux also runs on the Digital Alpha, the Motorola PowerPC, and Sun's SPARC platforms. Most importantly, Linux runs *well* on a variety of hardware. It ran fine for me for 9 months on an AMD 486DX/100 machine, until I upgraded to a screaming Cyrix 6x86/P150+. Linux likes 16MB of RAM and 300MB of hard drive space to run comfortably, although I prefer 32MB of RAM and 500MB of hard drive space to run it luxuriously. There are still people, however, who run Linux in terminal mode with 6 to 8 MB of RAM and around 100MB of hard drive space.

Linux now has drivers for all major hardware, including CD-ROM/CD-R drives, tape drives, sound cards, even wierd things like virtual reality helmets. It doesn't like IBM's MCA bus, but works with just about every other system architecture available.

The Linux installation procedure works just as would installing Windows 95 or OS/2 Warp. Modern distributions–such as Red Hat[4] or Caldera[5] make this process very easy. Installation involves creating a boot diskette, restarting your computer, and following answering questions about what kind of hardware you have and which applications you wish installed. This is also one of the primary advantages of Linux over other UNIXes: all of the important UNIX software you cannot live without, such as TEX/LATEX, Emacs, bash, pine, and so on are installed with the operating system and work as soon as you log in for the first time. This includes complicated things like networking, web/ftp/telnet servers, sendmail, and UUCP. Anyone who tells you installing Linux is complicated is either using an old distribution or has never been anything but a Macintosh user. (I was a devoted Mac user for 5 years, so please contain your flames.)

Several X-windows environments—the standard graphical user environment for UNIX systems—are available

[1] http://www.linux.org

[2] The pronunciation of "Linux" is the subject of tremendous flame wars. Torvalds—and most of those in the know—pronounces it "lih-nucks," rhyming with "cynics." This is because Linux is named after Torvalds and his name in Finnish is pronounced with a shorter "i".

[3] http://www.webcrawler.com/WebCrawler/Facts/Servers.html

[4] http://www.redhat.com/

[5] http://www.caldera.com/

for Linux, although by far the free version from XFree86 is the most widely used. There are also commercial versions which claim more hardware support and faster speeds, although I have been able to tell no difference. Graphics performance will depend directly on your graphics hardware. I find that an inexpensive accelerated Mach64 (PCI, 2MB) card gives me excellent performance at 1024x768 on a 17-inch monitor. Motif libraries are available commercially (although work is in progress for a free Motif clone); Openlook and several other window managers are available for free. I have been very happy with `fvwm`, the "f" virtual window manager (use your imagination), which includes a paged desktop, a configurable toolbar, pop-up menus, and behaves similarly to Motif. The latest version of `fvwm` even comes preconfigured to resemble Windows 95, complete with Start Menu and the Recycle Bin, if such be your tastes.

Why Use It?

If you are not a UNIX user, you ask, then what are the advantages of using Linux? Most political methodologists probably use one of the Microsoft operating systems and interact with UNIX only for network communications if at all. For example, let's imagine that you commonly use Microsoft Word and Excel, communicate with Eudora e-mail, and need to interface with Netware printers and volumes. Furthermore, you run Stata for your statistical needs and occasionally Gauss. Finally, you use a variety of other DOS/Windows programs that you just can't do without: Quicken, DOOM, and *EzI: A(n Easy) Program for Ecological Inference*. What benefits would Linux have for you?

Actually, Linux may have several distinct *dis*advantages that Linux has for you. First, *there are lots of DOS and Windows programs that simply will not run under Linux*. For example, the DOS version of *EzI* will not run under Linux; nor will most of the DOS/Windows software on our campus Novell servers. This said, there are ways to get around this problem. One is to use the Caldera distribution of Linux (my personal choice) for which Sunsoft's WABI is available: a Windows Emulator. WABI will run MS Office 4.3, Word 6.0, Excel 5.0, Wordperfect 6.1, Quicken 4.0 Deluxe, MS Encarta and Bookshelf, Eudora mail, and many others. In addition, Caldera's distribution ships with a Netware client written by Novell that allows printing to Netware printer queues and access to Netware volumes. A second possibility is the free `DOSEmu` DOS emulator which will run many DOS programs in Linux. The Linux team is also working on a free Windows emulator (named "Wine") but it is not yet finished.

Another way around the software problem is to keep Windows on your machine and boot up Linux only when you need to. For instance, I keep several different filesystems and operating systems on different partitions of my computer's hard drives, and use OS/2 Boot Manager to let me choose Linux, OS/2, or Windows 95 when I reboot my computer. This way I can develop DOS and Windows software on my Linux machine. Windows, and most other OSes, will run nicely in their own partitions and not even know that Linux is also there. Most other OSes cannot read Linux filesystems, although Linux can read and write DOS, Windows 95, Unixware, SCO, and NFS volumes, and read-only from OS/2 HPFS and Macintosh HFS filesystems. The majority of individual users of Linux keep some sort of dual-boot capability on their machine. In fact, this is how most people get started with Linux: testing it as an additional OS while keeping their original setup intact. This is also a great way to learn UNIX for a very small financial investment.

A final solution to the software problem is to simply to switch to Linux versions of your old software. Wordperfect for Linux is available, although you could also switch to LaTeX, in which case you will probably soon want to start using GNU Emacs and AucTeX. Several Linux word processors and spreadsheets also exist that have no Windows versions. Most DOS and Windows statistical packages also have Linux versions (see below). This is even true for many games: DOOM, for instance. Likewise, there are native (and better) versions of Netscape, Adobe Acrobat Reader, RealAudio, MPEG movie players, drawing and painting programs (`xfig` is an excellent program not available for Windows), and network communications tools. And: the Linux versions of these are free!

A second problem is that *UNIX is not for everyone*. The power of a real command shell and the ability to customize your own kernel (the operating system software itself) come with their own costs. Many commercial vendors (RedHat, Caldera, and Slackware) provide some technical support, but you'll have to rely for assistance mostly on your own soon-to-grow expertise, publications such as the *Linux Journal* and the very helpful Linux community on the `comp.os.linux.*` groups. I installed and configured my own system in an overwhelmingly Windows/Novell environment, but it was not especially hard, and my network administrator was able to assist with the network configuration. As long as you have an ethernet connection and an IP address, you should be able to communicate just fine with your campus network. I use mail on a different UNIX host, since I sometimes (although rarely) turn my system off, but there is no reason why I could not be my own mail-receiving UNIX host. (I admit being tempted by the snob appeal of having my e-mail address be `ken@benoit.harvard.edu`.)

While installing a good commercial distribution of Linux is no more difficult than installing Windows 95, it

does require some technical knowledge, especially when your hardware is non-standard. Also, the power of UNIX generally is also the power to wreak havoc when you don't know what you're doing. One myth, however, is that using UNIX for your personal computing needs requires you to spend lots of time as a "system administrator." This is no more true than it is for any other operating system, unless you are letting multiple users access and use your system. I occasionally upgrade Netscape or add new Emacs `.el` files to my system, but otherwise never touch my basic system configuration. And since I log in as a regular user each day I have no power to destroy the system setup beyond my personal directories.

So what are the advantages of using UNIX for you? All benefits delineated in the next section apply to those making their first leap to UNIX. In addition, you will find that Linux provides a far more effective use of your computer resources than does Windows 95 or OS/2 Warp (or, if you are one of the three remaining people who use it, DOS). I found both Windows 95 and OS/2 intolerably slow on a 486DX/66 with 16MB of RAM, yet this same system hummed just fine under Linux. And I used the same software on all three platforms: Netscape, LaTeX, Emacs, and Gauss. If you are already using Emacs and LaTeX for Windows 95, and customizing your command shell with versions of `bash` and other UNIX goodies, why not just switch to the real thing? If you are a happy user of Wordperfect who uses Windows 95 and has no idea what `bash` is, then a world of opportunity awaits. You have no idea of the kinds of uses to which you could be putting your largely wasted system.

Linux for UNIX users

A different group of political methodologists that might be interested in Linux are those who already use or have just decided to use a UNIX platform. Perhaps you use an HP or Sun workstation at the office, and are looking for a home machine. Or, you are going to purchase a new system for your office and want UNIX but only have a few thousand dollars to spend. Then Linux may be perfect for you.

The advantage of Linux for you is this: it will run nearly everything you want at a much lower cost, and depending on your hardware, at greater speed.

If you already use UNIX, then having a machine that won't run Microsoft Word is not a problem for you. Linux will run all of the standard UNIX platform software you know and love. Indeed, most distributions will automatically install and configure TeX, Emacs, X-windows, all of the GNU utilities, vi, xfig, Ghostscript and Ghostview, your choice of mailer and newsreader, Netscape (depending on your distribution) and much more. This includes programming languages, libraries, and utilities such as C/C++, Fortran, lisp, assembler, Java, BASIC, make, faces, curses, gdb, xgdb, and tcl. All at no additional cost and automatically installed! The GNU C compiler (gcc) is nothing to scoff at, either: the Linux kernel itself is compiled with it. So is Aptech System's Gauss.

So what about commerical statistical packages? This is a key question for political methodologists, and has two answers. First, if you are a single UNIX user in a PC environment, then you will have to rely on the packages available for Linux. But Surprise! the list of available statistics packages probably includes the ones you use on other platforms. Linux versions exist of Gauss, Stata, MATLAB, RATS, TSP, SST, Shazam, Mathematica, and the free programs Ox and SABRE. Most of these packages now use file formats that are binary compatible with versions from other platforms. That is useful for exchanging files with colleagues or students who might use the Windows or DOS versions of these programs, for example.

Although the growing number of packages available for Linux is already impressive, a few notables are not among them. S-plus currently does not run on Linux and there are no plans for porting it. Nor are versions of SAS, SPSS, or Minitab currently available, although the Wabi port is reported to run S-Plus for Windows. But there are ways around this problem.

For the second type of UNIX user, the one connected to a network of other UNIX hosts and having accounts on these systems, it is possible to run other software remotely. If you have an account on any UNIX system that has software you wish to run, you can run them as X clients on your local machine. Your computer supplies the graphical user interface, theirs does the computing, and as long as your network connection is fast you probably won't be able to tell the difference. This is currently how I run S-plus and SAS on my machine from our remote HP network. This is also a good solution for anyone who can persuade his or her university's statistics or economics departments to give them accounts on their UNIX systems.

This strategy is also useful for computing from home, for those who work already in a UNIX environment. Linux can be configured easily with a PPP client to connect to your campus network or other Internet service provider. This permits you to browse the web, read network news, use e-mail, and—as a nice bonus that costs several hundred dollars to set up with Windows—run X-networking clients remotely. There is *no* less expensive solution to provide your PC hardware with X-terminal capabilities than Linux.

Need I Say More?

Not many people use Linux in the political methodology community, but may more should consider using this wonderful open operating system. Not only does it do nearly everything that much more expensive commercial UNIX workstations provide, but it costs less and in many cases will be faster. Linux on an Alpha 500MHz platform will provide you with the fastest microcomputer workstation on the planet, for a fraction of the cost of a machine from Sun or HP that even comes close in performance.

Some benchmark results from my own setup confirm that even a mid-range Linux system will perform comparably or better than many commercial workstations. For example, I ran the Gauss speed comparison tests from Stefan Steinhaus[6] to see how my Cyrix 6x86/P150+ ranked. To my delight its overall performance was better than that of the HP 715/100 and the Sun Sparc 5 and Ultra Sparc 170! I also ran a large *EI* estimation estimation using Gary King's software. It took 41 minutes, 55 seconds on my machine but 48 minutes, 46 seconds on our departments's HP 715/80. I also ran tests in Stata using Richard Tucker's DyadHard software for the years 1816-1900. Creating the dataset took 1 minute, 44 seconds on the HP but only 43 seconds on mine. These tests and my general experience indicates that my modest desktop using Linux offers more computing power than the HP workstation costing thousands more. I only allow myself to dream what these results would look like with a faster processor, say, a Pentium Pro 200 like the one for which Charles Franklin posted benchmark results on Stefan's page...

Where did I put that tissue?

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A Live Report from Linux-land: What a New User Should Expect

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Not long ago, I became interested in simulation models that take into account the interaction of large numbers of individuals. I did some research and discovered that, at the Santa Fe Institute, Chris Langton and colleagues were developing a program called SWARM, which held out the promise of a general simulation language for multi-agent systems. To my lament, SWARM was built for UNIX systems, and I did not have a Sun Workstation or DEC Alpha Workstation. SWARM had been successfully installed on the Linux operating system, however, and I was intrigued to know more about it.

This essay describes some of the observations I made during my personal journey from the DOS/MS-Windows world to the world of Linux. I installed the Linux operating system on my home computer and ran some small simulations. The hard disk still holds a copy of Windows 95, and I expect it will stay there because I've not been able to wean myself from a few of the more useful Windows programs. But, after restarting the computer under Linux, I can run programs designed for the UNIX operating system, a powerful multi-tasking, multi-user system. And the real beauty of it is this: the UNIX operating system is very expensive, but Linux is free. That's right! I downloaded it from the internet for free (without violating any copyrights!).

It is not easy to install Linux. Even though recent package 'distributions' of the operating system and software have made it easier, beginners should expect a fairly high number of frustrating hang-ups. During the break-in period, the user will often wonder whether the user or the system is being broken-in. I have often suspected that the truth is some of each. It is not effortless to go between Windows and Linux-it is necessary to restart the computer to do so. While going back and forth, one might become confused by the simplest conventions. Linux uses forward slashes (/) in directory paths, but MS-DOS/Windows uses back slashes (\).

The hardship is justified if one needs to run programs designed for the UNIX environment. The Linux operating system has other sorts of appeal. The user is not limited by the kinds of artificial restrictions that have so regularly plagued MS-DOS programmers and users. There is no 640KB memory ceiling to be negotiated, for example. Linux, like the UNIX systems that it emulates, is a multitasking system designed to run many programs at once (true pre- emptive multitasking, much better than Windows '95). Beyond the technical convenience, however, another important benefit of Linux is purely emotional: users gain a feeling of freedom, autonomy, and self- determination. A Linux system offers users the power to adjust and tinker with the 'look and feel' of their computer.

My primary purpose in this essay is to describe what Linux is, why it is useful, and to provide a sketch of the installation and usage of the system. The presentation is aimed at an experienced MS- DOS/Windows user, not programmers. I don't write with the background of a computer programmer[1]. Rather, my knowledge of computing is almost totally learned by the seat-of-the-pants in the practice

[6] http://www.uni-frankfurt.de/ stst/gaussst.html

of social science. I hope to help others make informed decisions and save time and effort when they consider installing Linux.

What is Linux?

First things first. How do you say that? I've heard many pronunciations of "Linux." The person who created the Linux kernel, Linus Torvalds, pronounces it with his native Swedish accent in a way that sounds like "Lin'nux" (rhymes with "cynics"). If you want to hear him say it, browse my Linux page: http://lark.cc.ukans.edu/ pauljohn/linux/pronounce-linux.au. Some people say "Line'ux " to make it rhyme with the long-i in the American pronunciation of the name "Linus". Whichever method of pronunciation you choose, people will know what you mean!

Linus Torvalds was 23 year-old college student at the University of Helsinki when he began investigating the possibility of creating a 'UNIX-clone' operating system for the personal computer. The Linux operating system creates an environment in which one can run UNIX programs on the same personal computer that was probably purchased with the intention of using a Microsoft operating system. Linux provides the 'kernel' of the operating system, which means it manages the 'low level' operations that go on inside a computer: it gives programs time on the central processing unit, and it communicates with the hard disk, memory, and video display. In 1991 he announced his design and invited programmers around the world to join in the effort to expand the system's capabilities. Linux has all of the appealing features of the Unix operating system. It transparently manages the activities of many programs and users. Unlike DOS, programs running under UNIX are not plagued by any artificial limits on the amount of RAM that can be accessed. UNIX has native support for TCP/IP (internet protocol) as well.

Linux responds in the same way as UNIX to commands, but it is legally and financially a separate thing from UNIX and companies that sell UNIX. One might refer to properties of the "UNIX/Linux" operating system, but the legal distinction should always be kept in mind. There are many different copyrighted UNIX operating systems (and kernels) sold by competing companies. There is a continual search for an industry-wide agreed-upon standard, but still there are minor differences among versions of UNIX (people who are in the know call them 'flavors' and use the plural 'Unices'). Readers might have heard of companies like Sun Microsystems (which sells Solaris, a UNIX OS) and Digital Equipment (which sells Digital UNIX, previously known as OSF1). Linux, on the other hand, is provided for free to users under the terms of the GNU General Public License, which is administered by the Free Software Foundation. The GPL is a long document, but basically it endorses the concept of freely-available software. If someone uses and improves a GPL-ed program, the GPL requires that the resulting source code be available without charge. To emphasize the difference, the FSF homepage uses the term "copylefted" to describe the legal status of such software. "Copyleft says that anyone who redistributes the software, with or without changes, must pass along the freedom to further copy and change it." (http://www.fsf.org/copyleft/copyleft.html). In addition to the Linux kernel, other vital programs, such as the GNU's editor "emacs" or the GNU compiler for the C/C++/Objective-C language family, are available under the GPL.

What is Linux like, though? How does it work? It may not be possible to describe an elephant to someone that has never seen a four-legged animal. If someone has never seen a computer, perhaps they will not be able to imagine what Linux is. But, if someone has seen a dog, one can describe the elephant by contrast (e.g., it is much larger, not so fuzzy, with a really long, tubular snout). I would pursue a similar strategy in describing Linux to someone who has used Microsoft DOS (disk operating system) and Microsoft Windows. For reasons that historians might debate, the Linux/UNIX operating systems have many similarities with MS-DOS and MS-Windows. (Since the UNIX operating system existed first, the readers might judge for themselves which one is a pale imitation of the other.) MS-DOS is like the UNIX console mode, or terminal screen. The user sees a 'shell prompt.' (A shell prompt is called a 'command prompt' in MS-DOS.) Commands can be typed in, results may be written on the screen or into files. Instead of typing "dir" to see a list of files, in Linux one types "ls." Programs run in the console may use color and graphics, in the same way that DOS programs do-that is, with some difficulty. The Linux/UNIX systems also offer a graphical-user-interface (GUI), which is called the X Windows System, or X for short. Anyone who has found that a program runs only in MS-DOS, and not MS-Windows, or only in Windows, and not MS-DOS, will feel completely at home in the UNIX/Linux world. The same basic rule holds: Programs designed for the GUI (X) run only under the GUI, while some programs written for the nonGUI (the console) run under the GUI as well.

Aside from its ability to easily manage large amounts of memory and powerful CPUs, the Linux/UNIX operating system has important features that distinguish it from MS-DOS. Perhaps most importantly, a Linux/UNIX system is a multi-user system. When the system is installed, the first user created is the 'root' user, also known as the 'superuser.' This is the system administrator, the user who has the responsibility for maintaining software on the system. The superuser is the only one with permission to install most

programs or run programs that affect the way the system operates. The superuser can create other users and designate their rights. Each user gets a 'home' directory, e.g., a person named fred might have a home directory named /home/fred. Each user is free to customize the way the Linux system responds to commands (more on that later). Each file has an owner, the only person that is allowed to control access to that file by other users. The owner can assign 'permissions' which say whether the file can be read, written-over, or executed by the owner, a member of the owner's group, or any user on the system. One of the most vital pieces of advice to a new Linux user is this: Never run anything as root unless you absolutely have to. One should log onto a Linux system as an ordinary user, and if necessary, log on temporarily as the root/superuser by using the command "su root." This prevents traumatic accidents because ordinary users are not allowed to delete vital system files.

Only the most courageous do-it-yourself user will try to build a Linux system from the ground-up, even though the source code is available. Most people will use a "distribution" that includes the Linux kernel along with lots of other programs that are standard parts of Linux/UNIX. As far as I know, every UNIX operating system has a C compiler, for example. Linux systems typically include the GNU C compiler. Any user can type in a computer program, use that freeware compiler to turn it into an executable, and then run the program. Distributions are getting bigger, including more utilities and handy programs. Whereas installation was once fraught with difficulties that might drag out over days or weeks, now one can use a CD from a distribution to install Linux in a day or so. In the present state of refinement, the major difficulties will be encountered in the preparation of the PC for installation of Linux (more on that later).

Why consider Linux?

1. *Some programs are available only for UNIX.* The simplest, most obvious reason to consider Linux is software. If you don't have a personal workstation, you need Linux to run UNIX programs. There are plenty of programs that are written for UNIX that have not been ported (that's the word they use to refer to redesign) for Windows. Many scientific programmers work in a UNIX environment, so their programs run in UNIX. Windows versions might be nonexistent or delayed. I want to run the program called SWARM, and although plans exist to create a Windows NT version, in the here- and-now, Linux is the feasible option.

2. *Linux can be less expensive.* Suppose you can choose between upgrading a PC to run Windows NT or Linux. Linux is definitely less expensive, both in terms of hardware (it requires less memory) and software. If one cares to, the Linux operating system can be obtained for free. It is not shareware. It is free. One can download a distribution of Linux from the World Wide Web. Documentation is free, too, on the Web. The biggest site for Linux related material is Sunsite (http://sunsite.unc.edu). In the directory called /pub/docs/HOWTO, one will find "HOW-TO" files that address many different aspects of the Linux system, ranging from installation of the system and software to details about specialized hardware. In the directory /mdw, one will find the homepage of the Linux documentation project. This will offer hypertext versions of many HOWTO files, as well as handy tip sheets and links to various Linux sites.

In bookstores or computer stores, one can find manuals with titles such as Using Linux (Tackett and Guner, 1996), which will provide CD-ROMs with a distribution of the Linux operating system. Such a book will probably be handy, but CD-ROM is likely to be out of date. The Linux kernel changes rapidly, as both major upgrades and minor fixes are created. For example, in 1997, the kernel was upgraded and "modularized." While Linux is running, the kernel can automatically start-up a module that allows the use of a particular printer or tape-backup drive. Modules allow the kernel to stay small- using minimal system resources-and still support diverse equipment. Very valuable updates, such as support for modules, imply that one should obtain a Linux distribution directly from a company that advertises on the internet.

There are a number of companies that sell Linux distributions. In the early years of Linux, the most complete package including the Linux OS and various programs was the Slackware package. More recently, companies such as Red Hat and Debian have come into existence to sell distributions of the Linux operating system.

I have experience with the Slackware and Red Hat distributions. I tried Slackware because it was on a CD that came with a manual. Within days, I discovered that it was outdated. At that point, I spoke with a computer consultant who said "forget Slackware. Get Red Hat. That's what Linus Torvalds uses." That turned out to be good advice. The Red Hat company makes available on the Web a full distribution of Linux, including many freeware programs. I downloaded about 150 megabytes of compressed software (but did not install it all). They make available a manual that one can download and print out. If I were to start over, I would have spent the $50 to order the newest Red Hat Linux package direct from the company. That would make the installation easier, provide a printed manual, and it would give me the right to bother them with silly questions. (So, if you are keeping track, I've spent about $100 so far.)

The main advantage of Red Hat Linux is the "RPM" system. The Red Hat Package Manager keeps each program

or utility in a file with the RPM extension. The user can then use a program called RPM to install and remove packages. It keeps track of dependencies among programs, and will not let the user delete a program that is vital for other programs and it also will not install programs unless all of the software upon which they depend is also installed. The package system offers upgradeability, the easy way to add new versions and yank out old ones. The Red Hat company maintains a WWW site where users have contributed RPM packages for a wide variety of software. The Slackware distribution boasts its own package manager, but in my experience it was not nearly as powerful. I've not done in-depth research on the Debian distribution to make a statement about its relative power.

The Red Hat distribution will come with its own installation instructions. Those instructions are tailored for Red Hat and supercede any general HOW-TO information obtained elsewhere on the web.

3. *Linux is nice to use.* There are handy little features that make Linux fun to use. For example, Linux supports long file names. Always has. But typing long file names can be tedious. A Linux user only needs to type the first few letters of a filename, however. By touching the tab key, the user causes the operating system to automatically search the current directory and complete the filename. Consider another handy feature. Linux remembers commands. If one needs to repeat a command that was issued a while ago, just press the up-arrow key repeatedly. The system will cycle through previous commands, showing them one after the other. Unlike MS-DOS, in which such commands are difficult to revise, Linux allows the arrow keys to move the cursor without erasing characters, so revising commands is extremely convenient.

These convenient features are available from the start. Others can be accessed when desired. For example, I like color-coded file listings. Each element in a listing of a directory can be color-coded to make types of files easy to spot. Text files are white, but executable files are bright green. Sound files are purple. Subdirectories are blue. The optional color listing is obtained by adding a flag to the ls command, so it becomes "ls -color". If the user wants to make such an option permanent, it can be done by editing a "hidden" file called .bashrc that is (probably) in the user's home directory. Files that start with a period do not show up in a listing unless the user types "ls -a". (That's the only sense in which they are hidden.) The .bashrc file is just one of many configuration files that are just waiting for the user to add a personal touch. These files are typically scripts that are accessed when programs run. No inscrutable interface or menuing system is needed. A simple text editor is all one needs to have virtually total control over the colors that appear on the screen or the locations that the computer looks for programs.

Like many new users, the customizability of Linux is as much a source of confusion as promise for me. One source of frustration was the term "shell," which is absolutely integral to the understanding of the user interface. I first heard the term when I had trouble with my account on the DEC UNIX system that I use to read e-mail and run SAS jobs. The consultant asked "What shell are you running?" I must confess I was baffled. It never occurred to me that everything I did was occurring within a program that received my commands, conveyed them to the kernel of the OS, and then conveyed results back to me. That is a "shell." MS-DOS, with its command line and such, is a shell. The big difference in Linux/UNIX is that there are many shells, some old, some new, and each of them uses its own terminology and each can be specially tailored to the user's preferences. Almost all new Linux systems will use the "bash" shell, short for "Bourne Again Shell." This is a modernized version of Stephen Bourne's original UNIX shell. There are others, of course, such as the C-shell or the Korn shell. In MS-DOS, one can write a batch file-a list of commands-and make it executable by attaching the suffix .bat. In Linux/Unix, such files are called "shell scripts" and they are quite easy to prepare (see, for example, Matthew & Stones, 1996).

4. *Each individual has a personal duty to resist the evil empire.* I was mad at Microsoft even before I knew of Linux. Problems with configuration of MS-DOS games that needed more than 640KB of RAM (like Wing Commander II or Front Page Sports-NFL Football) drove me crazy. I got more upset when I noticed the indiscriminant consumption of hard disk space by MS products. Witness the bloating of Microsoft Word. When I started with Word in 1985, it ran off a single 5 inch floppy disk. Word for Windows '95 takes more than 15 megabytes. No wonder cynics call it "bloatware." In an effort to throw-in a function for every conceivable purpose, they created a monster. The same is true of other MS products as well. Recently, I wanted to run some programs written in C++. I ended up using Linux and the GNU compiler, which is clean and effortless. I went that route after I saw that Microsoft Visual C++ was so cluttered with bells and whistles that it demanded 50 megabytes of hard disk space.

The MS approach really perturbed me when I started studying the revised version of Windows 95 (the so called OSR2, or Win 950B). If you install the first version of Win '95, you are allowed the option of saying NO when asked if you want to install software for the Microsoft Network. Even if you say no, the installation drops a directory called \Program Files\The Microsoft Network on your hard disk. OSR2 does the same, but it also installs several megabytes

of software for Compuserve, Prodigy, and AOL without permission. There's no way within the add/remove programs regime to get rid of unwanted files like that. I suspect that the average user has no idea the hard disk is crowded with them. (Lately I've noticed companies like Acer and Compaq are following suit, selling PCs with 50 or 100 megabytes of promotional videos hidden on the hard disk.)

But with OSR2, I found the shocking development that the installer slaps Microsoft's Web browser "Internet Explorer" (IE) onto the hard disk without asking. That bothered me because I prefer Netscape's Navigator and don't want to carry the deadweight of IE. Unlike other Windows features, such as Wordpad or the calculator, this program does not show up in the add/remove panel that would allow a root-and-branch removal. One can simply delete the IE directory, but not without some concern over whether a necessary program might be disabled. If one buys Norton Utilities II for Win '95, one will be dismayed to find that it also checks the system to find out if you've gotten rid of Internet Explorer, and reinstalls it. Why would Microsoft be so insistent on jamming IE down my throat? It is obvious that they want to discourage me from using a web browser from the leading competitor.

I understand the argument that pluralism is not necessarily a good thing in the computer world. The diversity of operating systems may create inconvenience. However, these costs are far outweighed by the advantages of competition–rapid program development and low cost. In Linux, I have much more freedom to pick-and-choose programs that I want to run, and almost all of them are free. I've never seen a Linux program that tried to edge out a competitor by taking up disk space that was intended for it.

Is Installation a Hassle?

The installation process was a terrible hassle for me. The experts might say, given recent improvements, it should take an hour or two to install, but I'd bet that those estimates are made for users who know what they are doing. New users will take wrong turns, get confused, read the wrong document, and waste time.

The first step is to ignore the boy-scouts in the Usenet groups who advise the new user to try to compile Linux and other software for themselves from scratch. (Look in the hierarchy comp.os.Linux.* for such opinions). Instead, buy a Linux distribution that includes the OS, lots of software, and good instructions. After comparing distributions, I chose Red Hat. Second, buy your distribution directly from a company that advertises on the internet. This will guarantee you the newest version.

The installation process for Linux has been simplified somewhat through the past two years. Still, some fundamental details must be attended to. First, one must find hard disk space for the Linux system. The minimum space needed is about 100 megabytes, but I would recommend a gigabyte or so. If Windows is currently using up all of the hard disk space (that's typical), one must either install another hard disk (get the same brand as possible to simplify installation) or shrink the amount of disk space devoted to Windows. Microsoft does not make this easy. Straightlaced consultants advise a person to wipe out the whole hard disk and then create a new partition where Windows can be reinstalled. One creates a diskette from which to boot the MS- DOS operating system, and from that disk the FDISK program is run to destroy the old Windows partition (and everything on it) and create a new, smaller partition. Since reinstalling Windows is a major hassle, one can try programs that can nondestructively shrink a partition. I had excellent experience with a program called Partition Magic 2, which ran in DOS–off a floppy disk–in a simple and transparent way. There is a freeware alternative called FIPS, but I have not tried it. I cannot testify from experience that these programs work on the newest MS file system, VFAT32, but they worked fine with VFAT (that's what the first version of Win '95 uses).

The final critical decision concerns the method that will be used to start Linux. Most distributions assume the user will install LILO, a program that is written onto the Master Boot Record of the hard disk. When the computer is booted, the user will be asked whether Linux or Windows is to be started. I was cautious about this because I had heard that some people damaged their Windows partition by installing LILO. There are two alternatives. One is to use the boot disk to enter Linux by restarting the computer. That is inconvenient. The alternative is a program called "loadlin" that is included-but curiously not documented-in most distributions. Loadlin is an MS- DOS program that sits in a directory called c:\loadlin on my PC. In that directory there is a copy of the Linux kernel, which by convention is called zImage (on some systems it is vmlinuz). To start Linux, I restart in MS-DOS mode, then type these commands:

```
cd c:\loadlin
smartdrv /c
loadlin zImage root=/dev/hda4 ro
```

These commands can be put in a batch file for convenience. This restarts the computer in Linux. I've learned that there is another major advantage of loadlin that people with computers with Plug-and-Play technology ought to take into account. PNP causes trouble for Linux systems because the DMA and IRQ assignments might be difficult to ascertain. If you use loadlin, the computer keeps track of those

assignments and Linux can use them. Until I did this, I was not able to use my PNP sound card while in Linux.

What Can You Do "Out of the Box"?

When Linux is first started, the user sees a prompt to logon, and when that is finished, the screen will show the "shell" prompt for the bash shell. The commands mv, rm, cp, are Linux/UNIX commands to move, remove, and copy files. The commands mkdir and rmdir will create and remove directories.

Every Linux distribution that I know of will have the staple UNIX programs, such as the mail program "pine," the editors emacs and vi, the C compiler, some game programs, and other utilities. Even if a PC is not in a network, it still has a mail facility. Why? So the system can send mail to the root user, of course. For example, the Red Hat distribution installs a program called crontab, which runs system maintenance jobs at predesignated times, perhaps weekly or monthly. Cron updates indices of manual pages, for example. If something goes wrong when the crontab program tries to run, it sends an email to the address root@localhost. If one logs on as root, and starts the email program pine, those error messages can be viewed.

New users will get their feet wet in the console. After the user is able to copy files, move them, rename them, edit them, and the like, the next major challenge is X windows setup. Linux distributions typically include the free version of the X Windows server known as XFree86. It is called a 'server' because it controls access to shared resources, such as the display screen, keyboard, and mouse. The programs that are sharing the resources, the 'clients,' are the programs we use to take advantage of the graphical user environment. The current (brand new!) version is 3.3. The main chore in installation is to set hardware specifications in a file called XF86config. The XF86config is a big file where the user can read about the various specifications for fonts, mouse type, video card, monitor refresh rates, resolution, and the size of the virtual desktop. That file will typically be found in the directory /etc or /usr/X11R6/X11. The boy scout in me says that every user ought to open up that file and edit it like a soldier. But the consultant in me would tell the user to try to use the configuration program Xconfigurator. That program will take the user, step-by-step, through a selection of settings. That will write the XF86config. The user starts the X Windows program by typing startx, which is in fact a list of commands that tell Linux to start the GUI and access various start-up specifications, including XF86Config. If X does not start, it means some settings in the XF86Config file are wrong. Users should be cautious about editing settings because there is the possibility of damaging the monitor by improperly setting refresh rates.

Setting up Xfree86 is a nightmare for many people who have hardware that is not yet supported. If the video card in the machine or the monitor is not recognized by Xfree86, the alternative is to get a different video card or to buy a commercial X Windows server, such as Accelerated-X or Metro-X. Either of these will cost about $100. In my experience, these commercial X Windows servers are considerably more simple to setup.

Once X starts up, what will the user see? One or two 'xterm' windows will be opened automatically. These will look like consoles, except that they are inside windows that can be stretched or dragged like windows in other programs. They have little buttons on top that can close them or shrink them to icons. (Come to think of it, they look like MS-DOS boxes in Windows '95). Commands can be typed inside these xterm windows. Beyond that, it's hard to say what the appearance of X might be. The appearance depends on something called a 'window manager.' The window manager will set the color scheme, the style of the windows, and it will also create method for launching programs that it knows about. Red Hat version 4.1 includes a number of window managers and uses the one called fvwm95 by default. This window manager looks a lot like Windows '95, featuring a start menu button and taskbar. Someone who is a Windows '95 user may feel at home, but there are other window managers that can be tried. My favorite is the Afterstep window manager.

There are some programs that are not distributed with the Red Hat package, but they are available in RPM format on the Red Hat site. In my opinion, any new X user's first step ought to be to get a copy of the newest version of a program called TKdesk. TKdesk is the like the Windows Explorer and it will save the user much trouble. For example, files obtained on the WWW are often 'tarred' and 'GNU- zipped.' If a person can remember the UNIX command to untar and unzip the file, no problem. If you have TKdesk, however, you don't have to remember all that syntax. Built into TKdesk are scripts that execute these common commands.

Problems that Drive New Users Crazy

I have an interest in operating systems, but most users do not. They would rather just write their papers and read their mail without knowing anything about how the OS works. Unless one has an administrator who is in charge of installing software and maintenance, some knowledge of the OS is necessary, whether one is an MS-DOS or Linux user. It is definitely more time consuming to manage a Linux system. My experience is that many of the problems that burn up time are trivial matters that can be remedied easily with a little warning.

In that line, I've prepared a list of problems that might crop up.

After a few incidents like this, I've concluded that one ought to check for documentation in a logical sequence. First look at the Red Hat manual, then the Red Hat website for tip sheets or to peruse the archives of the Red Hat mailing lists. Then check the readme file that comes with the program, then the man page that comes with the program, and so forth. When I was trying to configure dial-up networking (point-to-point protocol, or ppp), I was very frustrated. The HOW-TO for PPP is available on Sunsite, of course, but it is a rather complicated document. Most of it is designed for network administrators who are creating terminal servers, not for people who want to dial up. I stumbled through the Web for half-a-day, downloading documents and programs that had hints about setting-up dial-up access. After I screamed about this, the experienced users shook their heads. On the Red Hat Web site there is a tip-sheet giving step-by-step instructions. All that one needs is an installation- specific set of commands. To save others from this, I created a manual of my own (http://lark.cc.ukans.edu/ pauljohn/linux/PPP-Guide.html).

We Are Still Working On That (or, "That's a Feature, Not a Bug!").

The beauty of the Linux culture is that the source code is freely available, so anyone who wants to invest the time can try to fix a glitch. This is completely different from the MS-DOS world, where only the 'official' MS employees can fix things. The flipside, however, is that there are fewer 'gate-keepers' that stop mistakes or bugs from sliding through. But they are usually caught quickly, fixed, and made available for download.

I recently had an experience with Redhat 4.2 that reminded me of the frequent revisions of software. In the Redhat 4.2 package, the RPM system was upgraded to version 2.3.11. It worked fine, except when I asked the RPM system to verify all installed software, the rpm program crashed with a message "Segmentation Fault." There is an RPM mailing list and web page within the Redhat site. I learned that RPM had been upgraded to version 2.4.3. If one browses the mailing list enough, one will find comments about how to install the new versions, but, to my surprise, instructions are not included with the new versions of rpm (which are available on ftp://ftp.rpm.org). After some guesswork, I installed 2.4.3, which solved the segmentation fault, but then it generated all kinds of new errors when installing packages. The authors announced that most errors could be ignored or worked-around, but the most fundamental problem was unaddressed. RPM packages created with version 1 of the RPM software could not be installed with version 2.4.3 of the RPM package! Well known commercial software, such as Red Hat Motif, could not be installed without reinstalling version 2.3.11. A few weeks later, after a few revisions, RPM version 2.4.6 arrived, which can install version 1 packages, does not crash, and produces only the occasional error message.

People who have only been end-users of MS products are probably horrified by experiences like that. But people who have been using Windows '95 for a while have seen exactly the same evolution. The only difference is that bug fixes from MS seem to emerge much more slowly and with more baggage on them.

Final Words

There are some Windows programs without which I cannot live. I'm used to word processing and news reading in Windows and I suspect I would have a hard time going one-hundred percent into the Linux system. The rapid growth of commercial offerings for Linux will probably address these issues. A version of Word Perfect for Linux is available, as are office products like the Applixware Suite from Red Hat and Star Office, which is included in the Caldera office package. It seems to me that the main long-term blockages against a total conversion to Linux would be compatibility with old data bases and document exchange with other users. The frequent changes in format for MS-Word documents may add some fraction of functionality, but they seem designed to block interoperability of MS-Windows with other operating systems. Another problem is that the UNIX versions of popular software, such as Mathematica, is more costly for UNIX than Windows.

My interest in Linux was originally driven by a very practical need to run software designed for the Unix operating system. As I have learned about Linux, I've experienced the joy of its power (particularly multi-tasking) and customizability. There have been many hang-ups. In retrospect, I was nave to think that the transition from one OS to the other would be easy. But, all-in-all, the transition was tolerable.

If a colleague wants to run a program written for the UNIX OS, there's no reason to discourage them from installing the Linux operating system. After the hard disk is partitions, Linux does no damage to the partitions that house other operating systems.

Footnotes 1. At the risk of sounding like grandpa at a family picnic, the first computer I can remember using was a Honeywell that took punch card input. While in graduate school, I used IBM mainframe computers with operating systems known as Wylbur, Music, and CMS. The terminals that were used to access these systems were replaced by personal computers in the mid 1980s. The IBM mainframes

(and their operating systems!) were phased out, and we were introduced to the UNIX operating system which ran on less-expensive, but more powerful computers that replaced the mainframes. I learned some UNIX at that time because, as DOS users, we were able only to telnet to the UNIX system and run UNIX programs in terminal mode. In 1993, I learned about the Winsock, which allows Windows software to access mail or files on a UNIX system without noticing that they are reaching across operating systems, so I forgot most of the UNIX commands that I had learned.

References

Abrahams, Paul W. and Bruce R. Larson, *Unix for the Impatient*. 1996. New York:Addison-Wesley.

Greenfield, Larry. 1996. *The LINUX User's Guide*. Linux Documentation Project.

Matthew, Neil, and Richard Stones. 1966. *Beginning Linux Programming*. Birmingham, U.K.: Wrox Press.

Tackett, Jack Jr. and David Gunter. 1996. *Using Linux, 2ed*. Que.

Abrahams, Paul W. and Bruce R. Larson, *Unix for the Impatient*. 1996. New York:Addison-Wesley.

Paul Johnson is an associate professor of political science at the University of Kansas. His research interest lately lies in the computer modeling of the interaction of rational actors, with applications mostly in his substantive specialty, interest group politics.

Electoral Studies Launches Methodology Section

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"Electoral Inquiry" is a new section of *Electoral Studies* designed for papers dealing with research methods relevant to the study of voting and elections. Papers making original methodological contributions, and ones presenting expository treatments of existing, but "cutting-edge," methods, are welcome. Wherever possible, papers should illustrate methodologies with worked empirical examples. *ES* editors encourage authors to make data sets available via their Web sites for replication and teaching purposes and, as appropriate, to include discussions of the statistical software they use for estimation purposes. What kinds of papers are we looking for? Think *AJPS* Workshop is the short answer – yes, imitation really is a sincere form of flattery! The *ES* review process is anonymous and two reviewers are used for each submission. Every effort is made to ensure a prompt review process. Want to learn more? See the ES Homepage (www.psci.unt.edu/es/), or contact Harold Clarke, Joint Editor, Electoral Studies (phone: 817-565-2247; fax: 817-565-4818; email: hclarke@vm.acs.unt.edu).

Proposals Invited for 1998 Political Methodology Summer Meeting

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We invite proposals to attend or participate in the 15th annual summer meeting of the Society for Political Methodology (a group separate from but nearly coincident with the Political Methodology Section of the APSA). This year, we will be at the University of California, San Diego, July 22-26, 1998. Those who have participated in the past have viewed these meetings as among their most lively and intellectually engaging academic experiences. Graduate students and faculty interact closely together over the continuous formal and informal sessions. We hope you will propose to attend, give a paper, or appear as a discussant. Graduate students are expected to participate in our poster session. Participation and attendance at the conference is by invitation only, but we plan to expand invitations substantially this year.

Those proposing papers or posters should describe their research. Everyone applying should include a curriculum vitae. Please send applications by 1 April 1998 to Robert Erikson, Chair, Annual Meeting Selection Committee, Department of Political Science, University of Houston, 4800 Calhoun Road, Houston, Texas 77004, Email: **pols79@uhupvm1.uh.edu**.

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Subscriptions to *TPM* are free to members of the APSA's Methodology Section. Please contact APSA (212 483-2512) to join the section. Dues are $8.00 per year.

Submissions to *TPM* are welcome. Articles should be sent to the editor by e-mail (nagler@wizard.ucr.edu) if possible. Alternatively, submissions can be made on diskette as plain ascii files sent to Jonathan Nagler, Department of Political Science, University of California, Riverside, CA 92521. LaTeX format files are especially encouraged; but the editorial staff of *TPM* is able to handle almost any format. The deadline for submissions for the next issue is April 30, 1998. See the *TPM* website [http://wizard.ucr.edu/polmeth/tpm/tpm.html] for the latest information.

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