The Political Methodologist

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Notes From the Editor

Political science is diverse by its very nature. As such, the lines of division between political science and other disciplines often blur, and we frequently borrow methodological tools from these other fields. Of course, some of our problems can be solved with methods developed for other purposes by econometricians, psychometricians, statisticians, and even sociometricians. However, many—those that comprise the core of our discipline—often cannot. For these problems, we need an indigenous methodological subfield, capable of developing analytical tools tailored to our specific requirements. Without this capability, we are faced with a disquieting choice: We can apply ill-suited methods to core issues and draw conclusions that are very likely to be inaccurate. Alternatively, we can give up all pretensions to being an independent discipline and study only those theoretical issues appropriate to the analytical tools developed by scholars in other disciplines.

Fortunately, the last few years have witnessed some progress towards a methodology subfield in political science. A new political methodology journal has formed (see announcement below), five extremely successful meetings of the political science methodology group have been held (see list of papers below), many more political scientists have become sophisticated users of methodology, and a growing number of scholars are working on original solutions to methodological problems.

The Political Methodologist is intended to facilitate communication in this emerging subfield. This issue includes a book review, an annotated bibliography, course syllabi and exam questions, a list of papers at this past Summer's political methodology conference, a new journal announcement, and other items. Future issues will feature these topics, in addition to methodology question and answer sections, course exercises, data sets useful for class assignments, announcements of new data collections, software reviews, and other features. Please send contributions to Gary King, Department of Government, Harvard University, Littauer Center, Cambridge, MA 02138 (BITnet: gmk@harvunxw; Internet: gmk@wjh12.Harvard.Edu). We prefer submissions in plain ASCII or in $\TeX$ or $\LaTeX$ formats on MS-DOS diskettes. Subscriptions to the *The Political Methodologist* are free to members of the American Political Science Association's Political Methodology Section and are $15.00/year to others.

Thanks to Stanley Feldman, the current president of the Section, for playing an important role in bringing this newsletter to fruition. Thanks also to my staff at the Government Data Center at Harvard, especially Dwayne Bey and Hung-der Fu, for managing our desktop publishing software. Special thanks are reserved for all the contributors of this issue.   *Gary King, Harvard University.* TPM

Annotated Bibliography on Vector Autoregression
John T. Williams, University of Illinois at Chicago, John R. Freeman, University of Minnesota

Baillie, Richard T., Robert E. Lippens, and Patrick C. McMahon. (1983) "Testing Rational Expectations and Efficiency in the Foreign Exchange Market," *Econometrica* 51, pps. 553-563.

Shows how a bivariate vector autoregression (VAR) can be used to test efficient markets models. Specifically, a VAR including forward and spot exchange rates can be restricted to be consistent with a model of exchange rate market efficiency. The restriction that the forward exchange rate is an unbiased predictor of the future spot exchange rate implies restrictions on the VAR. A nonlinear Wald test rejects the market efficiency model for exchange rates. These results imply that either rational expectations is an inappropriate hypothesis for these markets or that risk neutrality assumptions are violated. This paper is most important for illustrating ways of using VAR to test rational expectations models.

Cooley, Thomas F., and Stephen F. LeRoy. (1985) "Atheoretical Macroeconomics: A Critique," *Journal of Monetary Economics* 16, pps. 283-308.

Reviews and compares VAR with the more strongly restricted approach associated with the Cowles Commission. Critically evaluates data analysis with VAR models, including the use of exogeneity testing and impulse response analysis. For example, the authors show that Granger noncausality does not necessarily imply strict exogeneity, unless additional assumptions are made about predeterminedness. Cooley and LeRoy describe VAR as atheoretical, and they ignore the way VAR's can be theoretically informed. In addition, they do not discuss new methods now being employed to restrict VAR's. The article is most useful in juxtaposing features of unrestricted VAR with those of the familiar structural equation (Cowles) approach.

Doan, Thomas, Robert Litterman, and Christopher Sims. (1984) "Forecasting and Conditional Projection Using Realistic Prior Distributions," *Econometric Reviews* 3, pps. 1-100.

Explores the use of prior distributions and of time varying parameters in VAR's. The need for restricting a VAR results from the fact that small-scale VAR models (6-10 variables) of post-war economic variables often overfit the sample observations resulting in very poor out-of-sample forecasts. The authors show that by restricting a VAR at the margins rather than using the more conventional methods of parsimonious parameterization followed by structural equation modelers, one can produce relatively accurate unconditional forecasts. In addition, conditional projections based on fu-

ture paths of endogenous variables will be more accurate if a VAR is restricted using realistic priors. In addition to providing support for the use of Bayesian VAR analysis, this paper gives a systematic and exhaustive evaluation of the variety of hyperparameters that can be used to restrict the VAR.

Fair, Ray C. (1979) "An Analysis of the Accuracy of Four Macroeconometric Models," *Journal of Political Economy* 87, pps. 701-718.

Simply compares forecasting accuracy of four macroeconometric models: 1) Sargent's classical model; 2) a six variable VAR; 3) a naive model; and 4) Fair's own structural model. This paper is important for showing that the six variable VAR performs poorly in forecasting out-of-sample data, a result which is now widely accepted by econometricians. Future work, such as that done by Doan, Litterman, and Sims (1984, see above) is oriented towards overcoming this shortcoming of VAR.

Freeman, John R., John T. Williams, and Tse-min Lin. "Vector Autoregression and the Study of Politics," forthcoming, *American Journal of Political Science*.

Reviews VAR methods especially with regard to the problem of deciding how to translate political theories into statistical models. The authors compare VAR and the more familiar structural equation approach (SEQ) with respect to epistemological underpinnings, empirical power, and usefulness in policy analysis. They show that the two modeling strategies are based on different conceptions of theory and of theory building, and that for the 4-6 variable models that we usually study, the choice between VAR and SEQ models presents a tradeoff between accuracy of causal inference and quantitative precision. In addition, VAR models have the disadvantage of being unable to incorporate nonlinear relationships as easily as SEQ models. But VAR models have the advantage of providing a more complete treatment of policy endogeneity than SEQ models. Some of these strengths and weaknesses of VAR models are illustrated in a reanalysis of the debate about the determinants of government expenditure.

Geweke, John. (1977) "The Dynamic Factor Analysis of Economic Time- Series Models," in D. J. Aigner and A. S. Goldberger, editors, *Latent Variables in Socio-economic Models*. Amsterdam: North-Holland, pps. 365-383.

Presents a dynamic factor analysis approach which uses frequency domain methods. A dynamic factor analysis representation has always proved inconvenient because methods based on the time domain require that variables and errors be independent across time. However, a spectral density matrix can be factor analyzed using conventional procedures, as estimates of the spectral density are asymptotically independent. The resulting factor model can be viewed as a restriction on a VAR, and it is appropriate for this purpose when the variables being analyzed can be con-

ceptualized in terms of an underlying dimension or dimensions. Maximum likelihood methods are introduced, and a likelihood ratio test capable of testing restrictions is proposed. If the restrictions are not supported by the data, the resulting model is a VAR.

Litterman, Robert B. (1984) "Forecasting and Policy Analysis With Bayesian Vector Autoregression Models," *Federal Reserve Bank of Minneapolis Quarterly Review* Fall, pps. 30-41.

Describes how Bayesian VAR forecasts and conditional projections can be used in policy analysis. The author argues in favor of using Bayesian VAR's to obtain reliable measures of a conditional forecast's uncertainty. This article is particularly useful in the way it compares Bayesian VAR methods of forecasting and policy analysis with more conventional structural equation methods. Bayesian VAR methods are better at estimation of a forecast's true accuracy than are structural equation methods, since the incremental adjustment needed for specifying conventional models serves to underestimate those models' forecast uncertainty (conventional methods of forecasting produce forecasts which appear more precise than they actually are).

Litterman, Robert B. (1986) "A Statistical Approach to Economic Forecasting" *Journal of Business and Economic Statistics* 4, pps. 1-4.

---------- (1986) "Forecasting with Bayesian Vector Autoregressions–Five Years of Experience," *Journal of Business and Economic Statistics* 4, pps. 25-38.

These articles report Litterman's experience in using Bayesian VAR for forecasting macroeconomic variables. Bayesian VAR is as accurate as much larger structural models used by commercial forecasting services, at least according to the relatively elaborate Monte Carlo procedure based on RMSE statistics that Litterman utilizes. This finding is not trivial, as forecasts provided by commercial operations can be expensive, and a Bayesian VAR forecasting model can be specified and estimated quite easily and with little expense. Though this paper focuses on forecasting capabilities, it serves to scrutinize more generally the usefulness of large-scale model- building strategies.

Litterman, Robert B., and Laurence Weiss. (1985) "Money, Real Interest Rates, and Output: A Reinterpretation of Postwar U.S. Data," *Econometrica* 53, pps. 129-156.

This is a good example of theoretically informed data analysis with an unrestricted VAR. Data on the money supply, interest rates, inflation, and output are analyzed to determine if past dynamics are consistent with a variety of monetary theories of the business cycle. Exogeneity tests are found to be inconsistent with most monetarist theories, and the moving average representations show that the data are not consistent with models where money determines output. Litterman and Weiss propose that the data are most consistent with a model where monetary policy

is designed to achieve price stability. This paper provides good examples of using exogeneity testing in the analysis of a VAR.

Runkle, David E. (1987) "Vector Autoregressions and Reality," *Journal of Business and Economic Activity* 5, pps. 437-442.

Criticizes Sims' (1980, see below) use and interpretation of unrestricted VAR models. Using a normal approximation and bootstrap procedure, Runkle shows that confidence intervals for impulse responses and variance decompositions are "huge," implying that unrestricted VAR models are not very useful for forecasting or for data analysis. Runkle analyzes data on money and interest rates to question Sims' earlier interpretations of key economic dynamics. His more general conclusion is that unrestricted VAR's may not provide much conclusive evidence about the existence of economic relationships.

Sargent, Thomas J. (1984) "Autoregressions, Expectations, and Advice," *American Economic Review* 74, pps. 408-421.

Criticizes Sims' use of VAR models for policy analysis from a rational expectations perspective. The Sims "challenge" to rational expectations critiques of policy analysis is that changes in policy do not represent changes in policy regimes, but merely changes in implementation of an existing policy rule. Sargent questions this view, arguing that the VAR approach to policy analysis exhibits internal contradictions, for instance, in assuming that policy shocks themselves will not be anticipated by economic agents. Though of primary interest to students of economic policy analysis, Sargent's discussion is useful for understanding how policy analysis with VAR finds a middle ground between conventional econometric and rational expectations approaches.

Sargent, Thomas J. (1981) "Interpreting Economic Time Series," *Journal of Political Economy* 89, pps. 213-249.

Calls for a change in the way dynamic econometrics deals with economic agents' behavior, arguing that changing behavior is a result of changing constraints. Conventional dynamic econometric practice ignores the principle that people's decision rules are influenced by constraints. This article is important in understanding Sims' argument that VAR's are useful for policy analysis in that it was many of the Sargent's concerns about changing economic constraints that led Sims to offer VAR as a solution to econometric ills. Sargent argues that more attention needs to be given to identification criteria, error processes, Granger causality, and Bayesian analysis, all of which are important under rational expectations, and all of which have been directly dealt with by VAR analysts.

Shamir, Michal. (1983) "Investigating Causal Relationships: A New Time-Series Methodology and a Political Application," *Political Methodology* 9, pps. 171-199.

Argues that political science lacks the type of theory nec-

essary for estimating conventional dynamic structural equation models. VAR is not atheoretical in its approach, as some argue, but rather it constitutes a "minimum theory" approach. Unless prior theory is accurate, structural equation models will likely be less informative than VAR. The author uses VAR to analyze the structure of Australia's political party system, using a four variable VAR including party fragmentation, ideological polarization, competitiveness, and instability.

Sargent, Thomas J., and Christopher A. Sims. (1977) "Business Cycle Modeling Without Pretending to Have Too Much A Priori Economic Theory," in Christopher A. Sims, Editor, *New Methods of Business Cycle Research.* Minneapolis: Minneapolis Federal Reserve, pps. 45-109.

Estimates and analyzes an unobserved index or factor model of the macroeconomy through factoring the spectral density matrix. Geweke's (1977, see above) paper has a more complete presentation of relevant statistical theory. This paper has a very interesting application of dynamic factor analysis. Theory is used to generate hypotheses about the number of underlying factors that are necessary to describe 14 macroeconomic variables. An important aspect of this paper is its attack on conventional econometric practice, an attack, continued by Sims (1980, see below) and Sargent (1981, see above), which argues that many exclusion restrictions necessary for estimating structural equation models in economics are disputable. Typical restrictions are based more on intuition and convenience than on economic theory, and methods not relying on these types of restrictions are needed. This paper anticipated the problems associated with estimating relatively large-scale macroeconometric VAR models, problems that have been since dealt with using other methods, like those described in Doan, Litterman, and Sims (1984).

Sims, Christopher A. (1987) "Comment," *Journal of Business and Statistics* 5, pps. 443-454.

Responds to Runkle's (1987, see above) criticism that unrestricted VAR's produce imprecise results. Results of VAR's are imprecise because the data analyst is unwilling to increase supposed certainty by making zero restrictions on parameter values. The measured uncertainty of VAR models is probably no greater than the uncertainty that econometricians actually think exists. Sims gives a distinctly Bayesian interpretation of Runkle's confidence intervals, arguing that they are best thought of as posterior distributions of the impulse responses. This article has a very good interpretation of Bayesian VAR as an antidote to the uncertainty Runkle uncovers, and the confidence bands obtained from a Bayesian restriction of the VAR are found to be much tighter around the mean response than are similar bands produced using flat priors.

Sims, Christopher A. (1986) "Are Forecasting Models Usable for Policy Analysis?" *Federal Reserve Bank of Min-*

neapolis Quarterly Review (Winter), pps. 2-16.

Examines the assumptions required for using typical forecasting models for policy analysis. Sims then advances the idea that policy analysis using a VAR is advantaged by the fact that the connection between the VAR and a reduced form forecasting model is explicit. Other structural models, even rational expectations equilibrium models, cannot easily evaluate their own probability structure because they are based on identifying assumptions which are assumed to be true, thus making impossible any accounting of the true forecast error. Identification of VAR models is achieved without the fragile assumptions necessary for identification of structural models. Rather, VAR models are identified through restrictions on the covariance matrix of disturbance terms. Sims proceeds to describe the types of restrictions that are possible, and he provides the underlying rationale for making different types of restrictions. In his example, he uses weak restrictions to identify money supply and demand equations in a six variable VAR.

Sims, Christopher A. (1982) "Policy Analysis with Econometric Models," *Brookings Papers on Economic Activity* 1, pps. 107-152.

Makes the case for using VAR for policy analysis. The Lucas critique of policy evaluation is not entirely damning of econometric policy analysis if policy action can be thought of as execution of a slowly changing policy rule. If so, VAR models are capable of describing conditional distributions from the historical data, and under certain cases cautious policy-makers should be able to use this information to improve economic performance. That is, VAR models should be able to present policy-makers with the type of information they usually want: Projections of the likely consequences of policy choices and a measure of these projections' uncertainties. Sims evaluates and criticizes the Reagan administration's economic forecasts for 1982-83 as unreasonable given the historical probability structure estimated by a Bayesian VAR.

Sims, Christopher A. (1980) "Macroeconomics and Reality," *Econometrica* 48, pps. 1-48.

The seminal work in the field. Criticizes conventional econometric practice and offers VAR as a technique which can overcome problems of structural equation approaches. Past econometric practice is subject to many criticisms: 1) A priori restrictions are often made for convenience, and are not based on theory; 2) Macroeconomic models are dynamic, which makes evaluation of identifying restrictions very difficult; and 3) Rational expectations theory makes the identifying restrictions of conventional econometric models look naive. The implication of these criticisms is that an alternative statistical modeling technique must be found which does not have to be justified on the basis of "dubious" a priori restrictions. Sims offers unrestricted VAR as this solution. An eight-variable unrestricted VAR is estimated for the U.S. and West Germany. The decomposition

of forecast error variance and the moving average impulse response for these models are presented as techniques for interpreting VAR's.

Sims, Christopher A. (1977) "Exogeneity and Causal Ordering in Macroeconomic Models," in Sims, editor, *New Methods of Business Cycle Research.* Minneapolis: Minneapolis Federal Reserve, pps. 23-43.

Clarifies the use and meaning of exogeneity tests which were, at this time, relatively recent additions to econometrics. There has been much development in this area since this paper was written, much of it quite good. So this paper is a bit dated as merely an exogeneity reference. However, it is still important in recording Sims' views on the interpretation of dynamics and causal orderings, convictions which underpin Sims later work on VAR. Much of this paper is an expanded discussion of Sims' earlier finding on the causal priority of money to income.

Williams, John T. and Michael D. McGinnis. (1988) Sophisticated Reaction in the U.S.-Soviet Arms Race: Evidence of Rational Expectations," *American Journal of Political Science* 32: 968-995.

Estimates a bivariate VAR using United States and Soviet budgetary data on military spending for the purpose of evaluating a rational expectations version of Richardson's arms race. Past research is hard-pressed to find evidence of an arms race. These authors argue that this is because rational expectations decision processes mask the mutual arms race reaction. A rational expectations version of the arms race implies that the two military budget series will be causally independent in Granger's sense, but the innovations or disturbance terms of the corresponding equations will be contemporaneously correlated. This article contains a good description of "innovation accounting," where the moving average representation of the VAR is used to generate responses of the variables of the VAR to unexpected shocks or innovations in each country's military spending.

$\boxed{TPM}$

Probability and Political Science: A Book Review *Henry E. Brady, University of Chicago*

Lorenz Kruger, Lorraine J. Daston, and Michael Heidelberger, *The Probabilistic Revolution; Volume 1: Ideas in History,* Cambridge: The MIT Press, 1987.

Lorenz Kruger, Gerd Gigerenzer, and Mary S. Morgan, *The Probabilistic Revolution; Volume 2: Ideas in the Sciences,* Cambridge: The MIT Press, 1987.

Patrick Suppes, *Probabilistic Metaphysics,* New York: Basic Blackwell, 1984.

By 1800, Pierre Simon de Laplace's brilliantly success-

ful applications of Newton's theories to celestial mechanics and to the problems of heat conduction in solids had led the French physicist and mathematician to proclaim the possibility of predicting the future of the universe if he were just given the velocity and position of every particle in it. Most philosophers did not share either Laplace's mathematical skills or his conceit about the possibility of carrying out the computations involved in his vision, but Laplace's boldly deterministic view of the world was shared by philosophers as different as Comte, Kant, and Mill. As a result, determinism became ensconced in theories of scientific explantion. Causality, for example, was viewed as a deterministic process in which the same causes led inexorably to the same effects. The basic problem of science was discovering laws indicating under what conditions specific causes would lead to specific effects. The metaphysical assumption of determinism, then, led naturally to a clearcut epistemology which called for discovering deterministic laws.

The books reviewed here are about the break-down of this deterministic view of nature, and its replacement with a probabilistic perspective. This constitutes a sea-change in the way we understand the world, and its consequences are far from completely worked out. Indeed, I believe the bogeyman of determinism still haunts a great deal of political science research, and it has not yet been replaced with a coherent understanding of what we are doing. I will not pretend to offer a new and coherent perspective here, but I will review some of the problems created by determinism, the solutions offered by probabilism, and the areas where we need to do more work.

Despite its consistency with the results of Newtonian mechanics, the deterministic perspective created grave difficulties for nineteenth century students of human society. At the most basic level of understanding human action, determinism seemed to preclude the possibility of free will, and it raised complicated questions about the relationship between psychic and physical phenomena. Reductionism presented one answer to these problems, but it seemed to lead researchers to insuperable difficulties—the task of reducing human action to physical phenomena required a laborious series of reductions through psychology, biology, chemistry, and finally physics. Another answer was to look directly for deterministic social laws, but these did not seem easy to find. Economists took this second route, and the result was some impressive theories based upon the mathematical methods of Newtonian physics. Paul Samuelson's *Foundations of Economic Analysis* and Gerard Debreu's *Theory of Value* were probably the high points of this research program.

The other behavioral and social sciences have been less avowedly deterministic in their approach, but they, like economics, have maintained an uneasy relationship with determinism. On the one hand, a great deal of the philosophy of science read by introductory social science students such

as Karl Popper's *The Logic of Scientific Discovery* or Carl Hempel's *Aspects of Scientific Explanation* have emphasized deterministic laws. On the other hand, in their empirical research, students are very soon introduced to basic notions of probability and statistics. In our basic methods courses, deterministic metaphysics coexists uneasily with a probabilistic epistemology.

Teachers of methods often feel the pinch when students ask about the exact role of the error term in a regression equation. Is it simply the *imprecision* of our measurements and the *incompleteness* of our deterministic theories? Is it just the result of epistemological limitations which can, in principle, be overcome? Or is the error term at least partly the consequence of the deeper metaphysical problems of intrinsic *impredictability* in the objective processes themselves or even the *indeterminism* of causal law? Most political methodologists justify the error term to their students by talking about imprecision and incompleteness, but deep down, many of us suspect that the problem may be impredictability or even indeterminism.

The problem comes up in another, related manner as well. Suppose we are helping a colleague with a study of mass political violence in all European countries from World War II to the present. We have a good measure of mass political violence and some excellent measures of possible causes. Consequently, we suggest doing the obvious and running a regression of the pooled time-series cross-sectional data. Assuming that we have a reasonable specification, when we get the computer output, we look for statistically and substantively significant coefficients. Our colleague, however, becomes worried at this point, and she asks us why we even bother with statistical significance. After all, her data represent the universe for the time-period under study. How, under these conditions, can we interpret the standard errors?

Our usual answer is, of course, quite clever. We argue that these countries are really only a "sample" of possible European countries after World War II and that even with the independent variables fixed, the dependent variable could have had different values for each country in each year. Indeed, the other factors that could have caused these different values are captured in the error term of our equation, and these factors are no problem as long as they are uncorrelated with the included independent variables. This sounds good, but it leads our colleague to an immediate question: Does this mean that her results are really only applicable to this hypothetical ensemble of possible European countries? How can she generalize to other situations? What has she gained by trading a descriptive purpose which ignores the standard errors for a scientific purpose which takes them into account? I don't think there is an easy answer to this problem. One approach, of course, is to name the problem and call it "external validity." But naming is not solving. Another approach is to say that the results can

be applied to other, similar sets of countries. But how do we know when another set of countries is similar? If we had a deterministic theory, the answer to this question would be simple. We would know when one "system" was similar to another by just checking to see whether all of the relevant variables had the same values. With a probabilistic theory, however, we must be able to say something about the similarity of the unobserved error terms as well as observed independent variables. This is no easy task.

The volumes by Kruger et al. and Suppes are interesting precisely because they suggest that these problems are not unique to the social sciences. In fact, they crop-up in psychology, physiology, biology, and even modern physics. In all of these areas, there has been a "probabilistic revolution" since 1800 which has changed the way we do science. The introductory section of *The Probabilistic Revolution, Volume 1: Ideas in History* tries to describe exactly what type of revolution has occurred. There are major chapters by Thomas Kuhn, I. Bernard Cohen, and Ian Hacking, and one could not ask for a better set of authors to discuss this question. Nevertheless, the discussion is somewhat confusing.

Kuhn recapitulates and refines his famous theory of "scientific revolutions," but he does not apply it directly to probabilism. Cohen presents a somewhat distinct notion of a "revolution in science," and he argues that there was no such thing during the gradual growth of probability theory from the 18th century to the present. However he does believe that there has been a revolution in the *application* of probabilistic methods. Hacking, the author of a superb book on *The Emergence of Probability Theory*, agrees with Cohen that there was a revolution in application but no Kuhnian scientific revolution. But he goes on to argue that there was a change in:

> ...ontology, in our vision of the world in which we live.... The taming of chance and the erosion of determinism constitute one of the most revolutionary changes in the history of the human mind. I use the word "revolutionary" not as a scholar but as a speaker of common English. If *that* change is not revolutionary, nothing is.

To complete the confusion, Chapters Four through Nine suggest that there *was* a Kuhnian revolution in probability theory during the early twentieth century when the Laplacean notion of equiprobability as the basis for operationalizing probability was replaced by the Von Misean notion of long-term frequencies. At least all the authors are agreed that something happened, although they are not sure whether it was a change in the understanding of probability theory itself (a Kuhnian revolution), in the application of probability theory to the world (Cohen's description), or in the understanding of the world (Hacking's argument).

Personally, I believe that all three things have happened

but that the most important by far was the change in our understanding of the world. This change may be most apparent in Lorenz Kruger's discussion of how the famous physicist James Clerk Maxwell reconciled determinism and probabilism. As the inventor of statistical mechanics, Maxwell was one of the first physicists to be confronted with this problem. His approach was to argue that in complex systems there will be many cases where events involving very small amounts of energy can create enormous differences in the outcomes. Although Maxwell remains agnostic about whether or not these small triggering events are in principle unknowable, he does stress that they make it very difficult, and maybe impossible, to construct theories in which the same causes always lead to the same effects. Hence, we must have probabilistic theories which allow for the possibility that somewhat similar causes lead only to somewhat similar effects.

This presents the problem of identifying similar systems. I think that this is the same problem that confronted our colleague who was studying mass political violence in Europe. Maxwell, and other physicists who worked on thermodynamics, solved this problem by describing some of the molar characteristics which similar systems had to satisfy. Perhaps there is a similar solution to our problem. Indeed, in an interesting contribution, Stephen Stigler argues that the problem facing nineteenth century social scientists was that of "categorizing data into homogeneous groups, that is, groups for which the major influential factors could be considered constant and residual variation was seen as due to haphazard accidental causes (page 289)." Galton, Edgeworth, and Pearson developed a partial solution to this problem by using "some coordinates of multivariate data as, in effect, determining the categorization of others." They showed that "multivariate data could be analyzed conditionally, given some of the coordinates as controlling or classifying variables (page 291)." This is not a complete solution to the problem because it still does not tell us exactly how to think about the error term, but it is a good start.

Subsequent chapters elaborate upon these and other themes. Ivo Schneider presents the fascinating story of how Augustin Louis Cauchy, the father of modern analysis, sought simultaneously to clean-up mathematical derivations and to reduce what he thought were the extravagant claims made for mathematics in areas such as the social sciences. One of his targets became Laplace's (supposed) claim that ordinary least squares could be applied to data no matter what the distribution of the error term. Cauchy searched for a counterexample, and he discovered the Cauchy distribution which had neither mean nor variance (nor moments of any order).

In a series of chapters on the collection of statistics, the authors show how the development of the modern state went hand in hand with the development of statistics, and how the ideology of the state, in turn, often affected the way these statistics were collected and uses to which they were put. In the second volume, there are chapters on the role of probability theory in psychology, sociology, economics, physiology, evolutionary biology, and physics. Many of these chapters are quite specialized, but they can be enjoyed by the uninitiated if one is willing to skip over some relatively technical passages.

The two volumes of *The Probabilistic Revolution* provide very stimulating reading, but they lack a consistent point of view and they do not reflect recent thinking about the relationship between science and probability. The book by Patrick Suppes on *Probabilistic Metaphysics* provides both a unified perspective and an up-to-date discussion of the issues. Suppes is especially well-acquainted with mathematical approaches to the social sciences, and he has contributed to measurement theory, logic, mathematical psychology, theories of rationality, and many other areas. It is, then, something of a surprise when he begins his book by listing the following propositions:

1. The fundamental laws of natural phenomena are essentially probabilistic rather than deterministic in character. 2. Our conception of matter must contain an intrinsic probabilistic element. 3. Causality is probabilistic, not deterministic, in character. 4. Certainty of knowledge—either in the sense of psychological immediacy, in the sense of logical truth, or in the sense of complete precision of measurements—is unachievable. 5. The sciences are characteristically pluralistic, rather than unified in language, subject matter, and method. 6. Language learning and performance in their phonological, grammatical, semantical, and prosodic aspects are intrinsically probabilistic in character. 7. The theory of rationality is intrinsically probabilistic in character (page 10).

Suppes goes on to elaborate upon each of these propositions in very well-written and compelling chapters.

These volumes and other recent work should make us reexamine what we are doing in our elementary methods courses. I believe that they lead to a number of interesting conclusions. First, it is no accident that modern game theory suggests that rational people will often use "randomized" strategies which involve choosing courses of action based upon some probability distribution. This procedure is often disquieting to those schooled in deterministic metaphysics, but it seems completely natural to those who view chance as basic. More generally, our social theories must presume that randomness is a fundamental part of human action and behavior. Second, the error terms in our equations are only partly a measure of our ignorance. They are also a measure of the essential indeterminateness of the world. "Rational expectations" theory in economics has recently begun to recognize this fact, and we should recognize it in our own modeling. Third, and this is the least well worked out result of the modern approach, random-

ness is paradoxically closely related to complexity. Far from being simple, randomness is quite complicated. At the moment, not much more can be said about this, but it seems likely that some important insights may follow from having a better grasp of the relationship between randomness and complexity. Among other things, the relationship suggests that when we are dealing with complex social phenomena we must have only modest hopes for the results of social science. In short, the Laplacean vision is probably unattainable. |TPM|

New Political Methodology Journal Forms

The University of Michigan Press will publish a new journal of political methodology, sponsored by the Section. (Thanks to John Jackson and Jim Stimpson for their efforts at writing proposals and negotiating an agreement.) This journal, called *Political Analysis*, will promote the development and dissemination of innovative scholarship in the field of methodology throughout political science. Methodology includes statistical methods, modeling, measurement, research design and other topics related to the conduct and development of analytical work. Innovate work on these topics is currently being done in virtually all subfields of the discipline in a manner that is making methodology a generic part of political science and not specific to each subarea. For example, innovative use of structural equations, ordinal and nominal variables, event counts, measurement problems, cooperative and non-zero sum game theory, dynamic models, and set theory is exists throughout political science. *Political Analysis* invites submissions that make original contributions to empirical methods, measurement, modeling, or any other topic that furthers analytical research and its application in the social sciences. The primary criteria will be originality and importance to other researchers regardless of subfield. Emphasis should be on showing how improved methods lead to different substantive conclusions rather than on presenting new methods for the sake of methodology. Submissions will be reviewed anonymously. Substantially discounted subscription rates will be available to Section members.

Please send submissions to James A. Stimpson, Editor, *Political Analysis*, Department of Political Science, University of Iowa, Iowa City, IA 52242. |TPM|

The Sixth Annual Political Methodology Conference

Next Summer's political methodology meeting will be hosted by the University of Minnesota in July. Details will

be mailed to all Section members and will appear in a future issue of PS. Money for travel and a small per diem is usually provided through our National Science Foundation funding. Please send paper proposals to Stanley Feldman, Department of Political Science, University of Kentucky, Lexington, Kentucky 40506-0027. Advanced graduate students are also encouraged to apply. |TPM|

Agenda of the Fifth Annual Political Methodology Conference

14-16 July 1988, University of California, Los Angeles

Thursday

John Freeman, "Systematic Sampling, Temporal Aggregation, and the Study of Political Relationships"

Nathaniel Beck, "Effects of Time Aggregation on Estimation"

John Williams, "Dynamic Factor Analysis Using Frequency Domain Methods"

Michael Goldfield, "Proportional Hazard Models"

Friday

Robert Erikson, Michael MacKuen, and Jim Stimson, "Modeling Changes in Popular Political Evaluations"

Walter Mebane, "Popular Evaluations of Economic Conditions and Policy in the United States, 1978-1985"

Mark Kamlet, "A Strategic Model of the Budgetary Process"

Gary King, "Vanishing Marginals and Materializing Seats-Votes Curves: Modeling Uncertainty"

Saturday

Henry Brady, "Sense and Nonsense in Dimensional Analysis"

Stanley Feldman and John Zaller, "Frame of Reference and the Survey Response"

Doug Rivers, "Choice Theoretical Models of Voting"

John Zaller, "Information Flow in Partisan Elections." |TPM|

Final Exam Questions *Gary King, Harvard University*

Part I.

In 1978, McCleskey, a black man, was convicted of killing a white police officer in Georgia and was sentenced to death. He appealed his death sentence to the Supreme Court (*McCleskey v. Kemp*, April, 1987), arguing that Georgia's criminal justice system discriminates against blacks. To support this claim, he offered a statistical analysis (called the Baldus Study) as evidence. Baldus et al. regressed the

death sentencing rate on race and a large number of control variables. They argued that, since the coefficient on race was large and statistically significant, Georgia's criminal justice was biased against blacks.

1. Specify the complete model implied in the paragraph above. Identify the parameter of interest; describe how it is interpreted and its probable value.

The Court found the Baldus Study insufficient to demonstrate unconstitutional discrimination and upheld McCleskey's death sentence. Because of this decision, McCleskey will likely be executed in Georgia's Electric Chair. Listed below are several of the reasons the Court found the Baldus Study unconvincing. For each of these, identify (a) the formal statistical problem underlying the Court's complaint, (b) the consequences of the problem, and (c) your substantive conclusions about the Court's argument and judgment.

2. "In certain cases, the study lacked information on...race." In these cases, Baldus et al. randomly assigned values to this explanatory variable.

3. The court noted "the instability of the various models. Even with the 230-variable model, consideration of 20 further variables caused a significant drop in the statistical significance of race. ...this undermined the persuasiveness of the model that showed the greatest racial disparity, the 39-variable model."

4. The court found that "the high correlation between race and many of the nonracial variables diminished the weight to which the study was entitled."

5. The court noted "the inability of any of the models to predict the outcome of actual cases. ...statisticians use a measure called 'r^2' to measure what portion of the variance in the dependent variable (death sentencing rate, in this case) is accounted for by the independent variables of the model. A perfectly predictive model would have an r^2 of 1.0. A model with no predictive power would have an r^2 of 0. The r^2 value of Baldus' most complex model, the 230-variable model, was between 0.46 and 0.48. Thus, ...the 230-variable model does not predict the outcome in half of the cases."

Part II.

1. Define each algebraically. Then distinguish among them theoretically: $V(y)$, $V(\epsilon)$, $V(b)$, $V(X)$. Assume the classical regression model.

2. "In the classical regression model the right-hand side variables are often called the *independent variables*, the columns of X are assumed to be *linearly independent*, and the disturbances are assumed to be *independently distributed* random variables." The word "independent" is used in three different ways in the above sentence. Explain each use of the word. What would be the meaning and the consequence, in each case, of "non-independence?" (Johnston, 1972)

Part III. True or false. Explain your answer.

1. An important assumption of the classical regression model is that the disturbances have zero expectation. One way to check if this assumption holds is to see if the mean of the residuals is zero in the sample.

2. Suppose that the expected value of y were *really* a linear function of X. Then in the sample, there would be a perfect fit. TPM

ECPR Summer School at University of Essex *Nathaniel Beck, University of California, San Diego*

Why send your students to dreary Ann Arbor for the summer when they can do a summer methodology sequence at the European Consortium for Political Research's Summer School at the University of Essex in the heart of beautiful East Anglia. Would you rather be an hour from Detroit or an hour from London? Would you rather pubcrawl in Wivenhoe or Ann Arbor?

The school offers a wide range of courses, ranging from the elementary to the advanced (see my syllabus below). There are three two week sections, and students can mix and match from a variety of courses. Last year there were, in addition to my advanced course, advanced courses in time series and LISREL. There was also a substantive course on economics and elections (taught by Mike Lewis-Beck). All courses have a large hands-on computer component using micro-labs and a large number of data sets available through the ECPR.

Students come from all over Europe, and seem quite motivated. Few American students attend the school and David Sanders, the director, is anxious to attract more. He is willing to offer fairly favorable tuition rates to Americans. For more information, write Dr. David Sanders, Director, ECPR Summer School, Dept. of Gov., University of Essex, Wivenhoe Park, Colchester, CO4 3SQ ENGLAND. TPM

An ECPR Methodology Syllabus

Nathaniel Beck, University of California, San Diego

Introductory Comments

A wide variety of models are considered. Some are quite complex and others fairly straightforward. I expect that people will pick and choose where they wish to put their effort. Some models you may simply wish to store away for future investigation, while others may be directly relevant to current research. In any event I will try to talk around some of the mathematical complications so that everyone can get the flavor of all of the models. I will also go into some mathematical detail where necessary; it is not necessary that everyone follow all of the mathematical details. I also expect

people to get their hands dirty with most of the models, so that even if you cannot follow all of the mathematical niceties you can still get some sense of what is actually going on.

I will attempt to provide computer exercises using real political data where possible. I have several programs and several datasets set up on the micros. You may feel free to use other packages and other datasets; ideally many of you will be using your own datasets.

There are a large number of readings assigned. Some of the readings are quite difficult and some are quite easy. The critical readings that I expect everyone to do are in boldface. I will discuss the nature of the readings a day or two ahead so you can have some idea of what to do (or not to do).

Our task for the two weeks is not set in stone. I am quite willing to modify the set of topics and readings to take into account class interests. The constraint here is whether I have anything useful to say and whether we can find useful readings on the spur of the moment. (I also ignore important topics, such as measurement issues and univariate time series that were well covered in other courses this summer.) Please do not hesitate to suggest modifications of this syllabus.

Also, we are not in a race. If we need to slow down to ensure understanding, we will do that. Topics started one day may lop over into the next day. The purpose of these two weeks is not to give you a compendium of techniques but instead to introduce some ways of thinking about complicated models so you can figure out how to deal with other complicated models that will come up in your research.

We will, in general, meet for two hours in a relatively formal setting each morning. (I assume we will take a short break after an hour.) We shall then adjourn for a more informal session in the computer lab. That hour will also be available for more informal consultations. I will be around many afternoons, so if you wish to discuss things after lunch please just schedule an appointment a day or so ahead of time.

I presume that most of you know (at least) the material covered in 2F88. We will quickly go over some of the topics covered there, but it would be nice if you were seeing these things for the second time. (For those who were not at Essex for the second session, I would like to assume that you have gone through at least one statistics course that got you at least through multiple regression.) Most of the course deals only with single equation systems, so you need not be familiar with the estimation of simultaneous equation models. I also presume familiarity with 2A88. (Again, for those who were not here during the second session, it would also be nice if you knew some calculus and something about matrices.)

Topics and Reading

Week I (Cross Sections)

Aug. 8 A recapitulation of things I hope you might know, and a brief introduction to some things you might not. An introduction to maximum likelihood estimation.

You should look over chs. 1-7 of Kmenta, some of which you might find hard. (Chapter 6, on maximum likelihood, is critical.) Also Hanushek and Jackson, chs. 1-4 and appendices.) Also chs. 1-4 of King.

Aug. 9 and 10. Maximum likelihood. Maximum likelihood estimation of the standard regression model. Applications to count data and limited dependent variables. Choice and random utility models.

Kmenta, 11-1, 11-5, 11-6. King, ch. 5. Hanushek and Jackson, ch. 7. Harvey, ch. 3 (sections 1 to 4). Madalla, ch. 2, 3 (skim).

Aug. 11 and 12. Models of selection bias. Issues of truncation and censoring. King, ch. 9 (skim 8 first). Madalla, ch. 6-9. Achen (all).

Week II (Time Series)

Aug 15. Basic issues of time series estimation. OLS, GLS and ML estimation of time series models.

Read Hanushek and Jackson, ch., 6. Kmenta, 8-3, 11-4. Harvey, ch. 3 (section 5) and ch. 6. King, ch. 7 (sections 1 and 2).

Aug. 16 and 17. Alternative theoretical specifications. Read King, ch. 7 (sections 3 to 6); Beck (Alt. Dyn. Structures, Estimating Dynamic Models...); Harvey, ch. 7 and 8. Hendry, et al., chapter in Handbook.

Aug 18. Testing and model selection.
Read Harvey, ch. 5.

Aug. 19. A strategy of modelling. A brief introduction to Bayesian interpretation. Read Hey, ch. 4-8. Look at portions of Leamer book that are in English and not Greek. Read Pagan and Mizon chapters in Hendry and Wallis (ch. 5 and 6).

References

Achen, *The Statistical Analysis of Quasi-Experiments* (U. of California)

Beck, Estimating Dynamic Models is Not Merely a Matter of Technique (*Political Methodology*)

Beck, Alternative Dynamic Specifications (Xerox)

Beck, Kalman Filter Models (Xerox)

Hanushek and Jackson, *Statistical Methods for Social Scientists* (Academic)

Harvey, *The Econometric Analysis of Time Series* (Phillip Allan)

Hendry and Wallis, *Econometrics and Qualitative Economics* (Blackwell)

Hey, *Data in Doubt* (Blackwell)

Judge, et al., *The Theory and Practice of Econometrics* (2nd) (Wiley)

King, *Unifying Political Methodology: The Likelihood*

Theory of Statistical Inference (Xerox of ms. forthcoming from U. of Cambridge)

Kmenta, *Elements of Econometrics* (2nd) (MacMillan)

Leamer, *Specification Searches* (Wiley)

Madalla, *Limited Dependent and Qualitative Variables in Econometrics* (Cambridge)

Grilliches and Intrilligator, *The Handbook of Econometrics* Chapters: Leamer, Model Choice and Specification Analysis, Judge and Bock, Biased Estimation, Engle, Wald, Likelihood Ratio and Lagrange Multiplier Tests, Hendry, et al., Dynamic Specification. [TPM]

Alternative Approaches to Political Research: A Syllabus *Herbert M. Kritzer, University of Wisconsin, Madison*

General Objectives

This course is intended to acquaint students with a wide variety of research methods and styles used by students of political phenomena, concentrating on those approaches that are essentially nonquantitative in orientation. Other than where necessary for purposes of comparison, the seminar will not cover standard quantitatively oriented topics such as survey design, experimental and quasi-experimental research, or statistics. The particular topics to be discussed include a variety of approaches, some positivist in orientation, some nonpositivist, but all involving an empirical emphasis.

The material in the course is divided into four parts:

1. Approaches to assessing research methods. 2. Issues of design in qualitative research. 3. Methods of data collection. 4. Styles of analysis.

While the course does not necessarily presume a working familiarity with standard, quantitatively oriented methodologies, those students who can draw on such a background may be advantaged in the course because those standard methodologies can be used as a baseline for evaluating the strengths and weaknesses of the alternative approaches that are the concern for this semester. I am not suggesting that quantitative methods are in any way better, simply that to judge one method, it must be compared to some other method.

There will be four guiding themes for the seminar:

1. The rhetorical element of research, analysis, and presentation. 2. Probabilistic (stochastic) versus deterministic explanation. 3. The search for patterns. 4. The elimination of alternative explanations.

My hope is that these themes can focus our consideration, and comparison, of the methods that will be discussed in the coming weeks.

While there is a "how-to-do" element in the course, I am a firm believer that one only learns "how to do" research in

a particular style by actually do it (in a structured situation where substantial feedback and assistance is readily available). For many of the research approaches we will discuss, there is no way that they could actually be carried out in the kind of course context this will be. Consequently, the emphasis will be on the kinds of opportunities, problems, and advantages offered by different methods—the kinds of things that can be learned from the experiences of others rather than from ones own experience.

Course Organization

As is obvious from the course title, the structure of the course is a seminar. This places a substantial burden on students to come to the sessions prepared to talk. I do not plan to lecture, and I view the course as a learning experience for myself as well as for the other participants. While in preparing the seminar, I drew upon a variety of methodologies that I have in fact used at one time or another (believe it or not, reputations notwithstanding, I have done a fair amount of nonquantitative, nonstatistical research and writing), many of the methods to be discussed are not in my repertoire.

Starting with the third meeting of the class, the session will begin with a two person "debate" (lasting 30 to 40 minutes). One person will make a case for the advantages, strengths, etc. of the research method covered in the week's reading; the second person will critique the approach, considering its weaknesses and disadvantages (the proponent will then have the opportunity for rebuttal). A sign-up sheet for topics will be circulated at the first session, and assignments will be posted on my door (and then distributed at the second class); the students for a given week are encouraged to work together in preparing for the "debate," particularly since the assignment of who is "pro" and who is "con" will be made by the flip of a coin at the beginning of the session where the presentations are to be made.

Requirements and Grading

In addition to "debate" participation, grading in the course will be based upon seminar participation and written assignments. During the semester there will be two 6-8 page papers; each of these papers is to be a critique, with an emphasis on a methodological evaluation of substantive conclusions, of a minimum of two pieces of research dealing with the same substantive question (i.e., a critical look at how the same question can be approached empirically in different ways); students are encouraged to draw upon work in their own substantive fields in preparing these discussions. In addition, there will be a final examination at the time listed in the Timetable; the exam will consist of a single essay question to be distributed at the last seminar session.

Readings

As the assignments make clear, readings are drawn from a combination of methodological discussions and exemplary studies from the research styles covered. The reading load is

heavy, and the number of books involved is large; students will probably want to be selective in which books they buy and what they choose to read in the Dean Room. For many of the book-length examples of various approaches, I have not indicated specific page numbers; students will probably want to read selectively in those works.

The following books should be available at the University Bookstore:

Adler and Adler, *Membership Roles in Field Research*
Agar, *Speaking of Ethnography*
Bloch, *Historian's Craft*
Cohen, *Theater of Power*
Douglas, *Creative Interviewing*
Erikson, *Childhood and Society*
Horowitz, *Courts and Social Policy*
Kaufman, *Forest Ranger*
Kirk and Miller, *Reliability and Validity in Qualitative Research*
Lane, *Political Ideology*
Liebow, *Tally's Corner*
Manning, *Semiotics and Fieldwork*
Punch, *Politics and Ethics of Fieldwork*
Rakove, *Don't Make No Waves, Don't Back No Losers*
Shapiro, *Courts*
Skocpol, *States and Social Revolutions*
Smelser, *Comparative Methods in the Social Sciences*
Sundquist, *Dynamics of the Party System*
Van Maanen, *Tales of the Field*
Yin, *Case Study Research*

*Ordered as "recommended"

Course Outline

PART I: APPROACHES TO ASSESSING RE-SEARCH METHODS

1. RHETORIC AND RESEARCH
Reading: Gusfield, The Culture of Public Problems, pp. 51-108 McCloskey, "The Rhetoric of Economics," Journal of Economic Literature 21 (June 1983), 481-517. Davis & Hersh, "Rhetoric and Mathematics," pp. 53-68 in Nelson, Megill, & McCloskey, The Rhetoric of the Human Sciences: Language and Argument in Scholarship and Public Affairs. Bazerman, "Codifying the Social Scientific Style: The APA Publication Manual as Behaviorist Rhetoric," pp. 125-144 in Nelson, Megill, & McCloskey, The Rhetoric of the Human Sciences: Language and Argument in Scholarship and Public Affairs.

Other items of interest: Nelson, Megill, and McCloskey, The Rhetoric of the Human Sciences: Language and Argument in Scholarship and Public Affairs. McCloskey, The Rhetoric of Economics. Edmondson, Rhetoric in Sociology.

2. VALIDITY AND INFERENCE
Reading: Cook & Campbell, Quasi-Experimentation: Design & Analysis for Field Settings, pp. 1-94. Kirk and Miller, Reliability and Validity in Qualitative Research. Schauer,

"Causation Theory and the Causes of Sexual Violence," 1987 American Bar Foundation Resarch Journal 737 (Fall 1987).

Other items of interest: Weller and Romney, Systematic Data Collection. Marshall and Rossman, Designing Qualitative Research.

PART II: ISSUES OF DESIGN IN QUALITATIVE RESEARCH

3. THE CASE STUDY APPROACH
Readings: Yin, Case Study Research: Design and Methods. Kaufman, The Forest Ranger.

Other items of interest Lipset, "The Biography of a Research Project: Union Democracy," pp. 111-139 in Hammond (ed.), Sociologists at Work. Schuck, Agent Orange on Trial.

4. EXTENDING THE CASE STUDY: THE COMPARATIVE APPROACH
Reading: Smelser, Comparative Methods in the Social Science. Horowitz, The Courts and Social Policy.

Other items of interest: Ragin, The Comparative Method: Moving Beyond Qualitative and Quantitative Strategies. Przeworski and Teune, The Logic of Comparative Social Inquiry. Barber, Presidential Character. Janis, Groupthink. Cooper, Hard Judicial Choices. Feeley, Court Reform on Trial. Levin, Urban Politics and the Criminal Courts.

5. CROSS-NATIONAL COMPARISON
Reading: Kohn, "Cross-National Research as an Analytic Strategy," 52 American Sociological Review 713 (1987). Lijphart, "Comparative Politics and the Comparative Method," 65 American Political Science Review 682 (1971). Shapiro, Courts.

Other items of interest: MacIntyre, "Is a Science of Comparative Politics Possible?" in Against the Self-Images of the Age: Essays on Ideology and Philosophy. Frendeis, "Explanation of Variation and Detection of Covariation: The Purpose and Logic of Comparative Analysis," 16 Comparative Political Studies 255 (1983). DeFelice, "Causal Inference and Comparative Methods," 19 Comparative Political Studies 415 (1986).

6. HISTORICAL ANALYSIS
Reading: Bloch, The Historian's Craft. Braudel, On History, pp. 25-82. Sundquist, Dynamics of the Party System. Mills, Sociological Imagination, 143-164.

Other items of interest: Barzun and Graf, The Modern Researcher. Thompson, The Poverty of Theory. Murphey, Our Knowledge of the Historical Past. Stinchcombe, Theoretical Methods in Social History. Walton, Reluctant Rebels. Irons, The New Deal Lawyers. Irons, Justice at War.

7. COMPARATIVE HISTORY
Reading: Skocpol, States and Social Revolutions.

Other items of interest: Skocpol, Vision and Method in Historical Sociology. Moore, Social Origins of Dictatorship

and Democracy. Abrams, Historical Sociology.

PART III: METHODS OF DATA COLLECTION

8. FIELD RESEARCH–GENERAL ISSUES

Reading: Fetterman, Ethnography: step by step. Liebow, Tally's Corner. Punch, The Politics and Ethics of Fieldwork. Geer, "First Days in the Field," pp. 372-398 in Hammond (ed.), Sociologists at Work.

Other items of interest: Agar, Speaking of Ethnography. Werner and Schoepfle, Systematic Fieldwork. Warren, Gender Issues in Field Research. Rynkiewich and Spradley, Ethics and Anthropology: Dilemmas in Fieldwork. Whyte, Learning from the Field: A Guide From Experience. Murphy, Getting the Facts. Johnson, Doing Field Research. Marcus and Fischer, Anthropology as Cultural Critique. Cohen, "Anthropology and Political Science: Courtship or Marriage," pp. 29-48 in Lipset (ed.), Politics and the Social Sciences. de Tocqueville, Democracy in America. Rubinstein, City Police. Gans, The Urban Villagers. Punch, Policing the Inner City. Greenhouse, Praying for Justice: Faith, Order, and Community in an American Town.

9. NONPARTICIPATING OBSERVATION

Reading: Barnard, Research Methods in Cultural Anthropology, chapt. 8. Garfinkel, Studies in Ethnomethodology, chapt. 2. Karp, "Observing Behavior in Public Places: Problems and Strategies," pp. 82-97 in Shaffir, Stebbins, and Turowetz, Fieldwork Experience: Qualitative Approaches to Social Research. Fenno, "U.S. House Members in Their Constituencies," 71 American Political Science Review 883 (1977). Sarat and Felstiner, "Law and Strategy in the Divorce".

Other items of interest: MacCannell, Nonviolent Action as Theater: A Dramaturgical Analysis of 146 Demonstrations. Bales, Interaction Process Analysis. Speier, How to Observe Face-to-Face Communication: A Sociological Introduction. Goffman, Asylums.

10. PARTICIPATING OBSERVATION

Reading: Barnard, Research Methods in Cultural Anthropology, chapt. 7. Adler and Adler, Membership Roles in Field Research. Becker, "Problems of Inference and Proof in Participant Observation,: 23 American Sociological Review 652 (1958). Rakove, Don't Make No Waves, Don't Back No Losers. Van Maanen, "Fieldwork on the Beat," pp. 103-151 in Van Maanen, Dabbs, and Faulker, Varieties of Qualitative Research.

Other items of interest: Jorgensen, Participant Observation: A Methodology for Human Studies. Filstead (ed.), Qualitiative Methodology: Firsthand Involvement with the Social World. Witte, Democracy, Authority, and Alienation in Work: Workers' Participation in an American Corporation. Carp, "The Behavior of Grand Juries: Acquiesence or Justice?" 55 Social Science Quarterly (1975). Flood, The Barristers Clerk. Thorne, "Political Activist as Participant Observer," in R.M. Emerson, Contemporary Field Research.

11. SEMI-STRUCTURED INTERVIEWING

Reading: Douglas, Creative Interviewing. Barnard, Research Methods in Cultural Anthropology, chapt. 9. Lane, Political Ideology. Bumiller, The Civil Rights Society: The Social Construction of Victims.

Other items of interest: Spradley, The Ethnographic Interview. Morgan, Focus groups as Qualitative Research. McCraken, The Long Interview. Converse, Conversations at Random. Lane, Political Life. Gaylin, Partial Justice.

12. WORKING WITH QUALITATIVE DATA

Reading Strauss, Qualitative Data for Social Scientists. Kritzer, "The Dimensions of Laywer-Client Relations: Notes toward a Theory and a Field Study" 1984. American Bar Foundation Research Journal. 409.

13. TEXTUAL ANALYSIS/LANGUAGE ANALYSIS

Reading: Edelman, Political Language, pp. 1-55. Manning, Semiotics and Fieldwork. Ricoeur, "The Model of the Text: Meaningful Action Considered as a Text," pp. 73-102 in Rabinow & Sullivan (eds.), Interpretive Social Science: A Reader. Zagoria, The Sino-Soviet Conflict, 1956-1961, pp. 24-35. Cohen, Theatre of Power.

Other items of interest: Atkinson and Heritage, Structures of Social Action: Studies in Conversational Analysis. Maynard, Plea Bargaining. O'Barr and Conley, "Lay Expectations of the Civil Justice System," 22 Law & Society Review 137 (1988).

14. PSYCHOANALYTIC STUDIES

Reading: Lasswell, Power and Personality, pp. 1-77, 269-319. Greenstein, Personality and Politics, pp. 63-93. Erikson, Childhood and Society.

Other items of interest: George & George, Woodrow Wilson and Colonel House. Erikson, Gandhi's Truth. Erikson, Young Man Luther. Wolfenstein, The Revolutionary Personality.

15. RHETORIC AND RESEARCH REVISITED

Reading: Van Maanen, Tales of the Field. Taylor, "Interpretation and the Sciences of Man," pp. 25-71 in Rabinow & Sullivan (eds.), Interpretive Social Science: A Reader.

Other items of interest: Mitchell (ed.), The Politics of Interpretation. Ricoeur, Hermeneutics and the Human Sciences: Essays on Language, Action, and Interpretation. Hookway and Pettit (ed.), Action and Interpretation: Studies in the Philosophy of the Social Sciences. Bryman, Quantity and Quality in Social Research. Simons and Melia (eds.), The Legacy of Kenneth Burke.

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Other Topics of Potential Interest

A. GENERAL ISSUES IN NONQUANTITATIVE RESEARCH

Rabinow & Sullivan, Interpretive Social Science. Van Maanen, Qualitative Methodology. Patton, Qualitative Evaluation Methods. Webb et al., Nonreactive Measures in the Social Sciences. Glaser and Struss, The Discovery of Grounded Theory: Stategies for Qualitative Research.

B. CULTURAL ANALYSIS

Geertz, The Interpretation of Cultures. Geertz, Local Knowledge. Rogin, "Ronald Reagan, The Movie" and Other Episodes in Political Demonology. Merelman, Making Something of Ourselves.

C. COMPARATIVE THEORIES

Allison, Essence of Decision. Gormley, "Alternative Models of the Regulatory Process," 35 Western Political Quarterly 297 (September 1982).

D. ETHNOMETHODOLOGY AND EXISTENTIAL SOCIOLOGY

Leiter, A Primer on Ethnomethodology. Garfinkel, Studies in Ethnomethodolgy. Douglas, Investigative Social Research. Mehan and Wood, The Reality of Ethnomethodology. Douglas and Johnson (eds.), Existential Sociology.

E. INSTITUTIONAL AND ORGANIZATIONAL ANALYSIS

Mohr, Explaining Organizational Behavior. Hall, Governing the Economy.

Evans, Rueschmeyer, and Skocpol (eds.), Bringing the State Back In. Zysman, Governments, Markets, and Growth. North, Structure and Change in the American Economy. March and Olsen, "The New Institutionalism: Organizational Factors in Political Life," 78 American Political Science Review 734 (September 1984). Smith, "Political Jurisprudence, the 'New Institutionalism.' and the Future of Public Law," 82 American Poltical Science Review. 89: (March, 1988)

F. LEGAL ANALYSIS

Mermin, Law & the Legal System. Smart, The Ties that Bind. Carter, Reason in Law. Cardozo, The Nature of the Judicial Process. Merryman, The Civil Law Tradition. David, French Law. Hart, The Concept of Law. Downs, The Nazie of Skokie. Cooper, Hard Judicial Choices. Griffiths, The Politics of the Judiciary. TPM