

Qualitative Methods

Newsletter of the
American Political Science Association
Organized Section on Qualitative Methods

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Letter from the Editor

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With this issue, the editorship of this newsletter passes from John Gerring to myself. On behalf of the members of the section, I would like to thank John for his excellent work in establishing and editing this newsletter for the first three years of its existence. He has made it one of the best newsletters of any of the APSA sections.

Because of John's excellent work, there is little that I want or need to change in the format or content of the newsletter. I will continue to solicit pieces and symposia that reflect the interests and diversity of the membership of the section. I also see the newsletter as an outlet for work that does not fit the research article genre. This is clearly part of the mission of *Perspectives on Politics* for the association as a whole, and I see it as part of the mission of the newsletter for the qualitative methods section. I encourage you, the readership, to submit articles (not too long), symposia, op-ed pieces, and other such items to me.

Qualitative methods have been changing rapidly over the last few years. One aspect of that change has been an increase in the number of qualitative methods courses and changes in their content. Next year I would like to publish a review of qualitative methods courses. Please email me descriptions of courses that you teach (or will teach), or courses taught in your department. Please also indicate if you would like the syllabi you send to be included on CQRM's syllabus webpage at <http://www.asu.edu/clas/polisci/cqrm/syllabi.html> and we will forward them for posting.

The section sponsored or cosponsored 26 panels at the annual meetings in Philadelphia. Feedback from section officers and program chairs indicates that they were well attended, and the official statistics from APSA put the section in a three-way tie for third in mean attendance. As a result of this strong showing, for the fourth year running our panel allocation for 2007 has increased by the maximum of five. Overall, the section has had very good attendance at panels over the past few years, attesting to the interest qualitative methods has generated. Thanks go to Melani Cammett and Julia Lynch for organizing the panels this year.

Finally, I am sad to report the death of Alexander George. He was an incredible scholar, mentor, friend, and teacher to

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many of us. I need say no more since Jack Levy and Debbie Larson have written an obituary for him in this issue.

Why Quantitative Social Science Needs Interpretive Methods

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In this article I will argue for the claim that a current leading approach to quantitative social science—and economics in particular—cannot legitimately stand on its own, but, rather, must always be supplemented by strategic use of interpretive investigative methods.

The “leading approach” I have in mind is what I will call the “Mathematical-Metaphorical” (or “M-M”) approach, which entails the use of mathematical models as metaphors for social phenomena to illuminate puzzling or obscure aspects of those phenomena and their dynamics. This approach is virtually universal in economics and rational choice political science, and has expanded (and continues to expand) into other fields of social inquiry.

As I will argue below, M-M methodology can be deployed coherently only under certain conditions, and establishing that these conditions are met is not currently a necessary part of the normal business of quantitative social science. As such, analyses utilizing the M-M method in the social sciences operate under a cloud of possible incoherence. Dispelling this cloud requires establishing that the proper conditions are met for application of the M-M method, and this will necessarily involve interpretive investigative methods.

My argument will proceed in three stages. First, I will explain in more detail what the M-M method is and how it works. Second, I will establish two necessary conditions for the coherent use of M-M methodology in the social sciences. Third, I will explain why interpretive methods must be used to establish these necessary conditions.

The Nature and Functioning of the M-M Method

The M-M Method is a tool for explaining social phenomena by constructing a mathematical metaphor for these phenomena. It involves utilizing mathematics not merely as an accounting tool, but as a framework within which to interpret social phenomena. In the next section I will explain more precisely in what sense the M-M method is necessarily metaphorical. But first, in this section, I would like to elaborate on what the M-M method consists in.

Procedurally, the M-M method involves identifying a set of social phenomena to be explained/understood, constructing a mathematical model to metaphorically represent the phenomena, and then interpreting the solution dynamics of the mathematical model as possible solution dynamics of the social phenomena.

The framework I will introduce in this section—which I

will call the “Four-Part Framework” or “FPF”—is meant to capture precisely these elements. It describes four phases through which every M-M analysis must pass, namely:

1. *Delimiting*, in which the set of social phenomena under study is delimited and a research question is formed;
2. *Naming*, in which a mathematical construct meant to be analogous to the social phenomena is introduced. Significantly, the *Naming* phase produces a *catalog of correspondences* linking the elements of the set of social phenomena with their mathematical representatives;
3. *Solution*, in which the mathematical construct is brought to a solution; and
4. *Interpretation*, in which the mathematical solution and its implications are interpreted with respect to the research question.¹

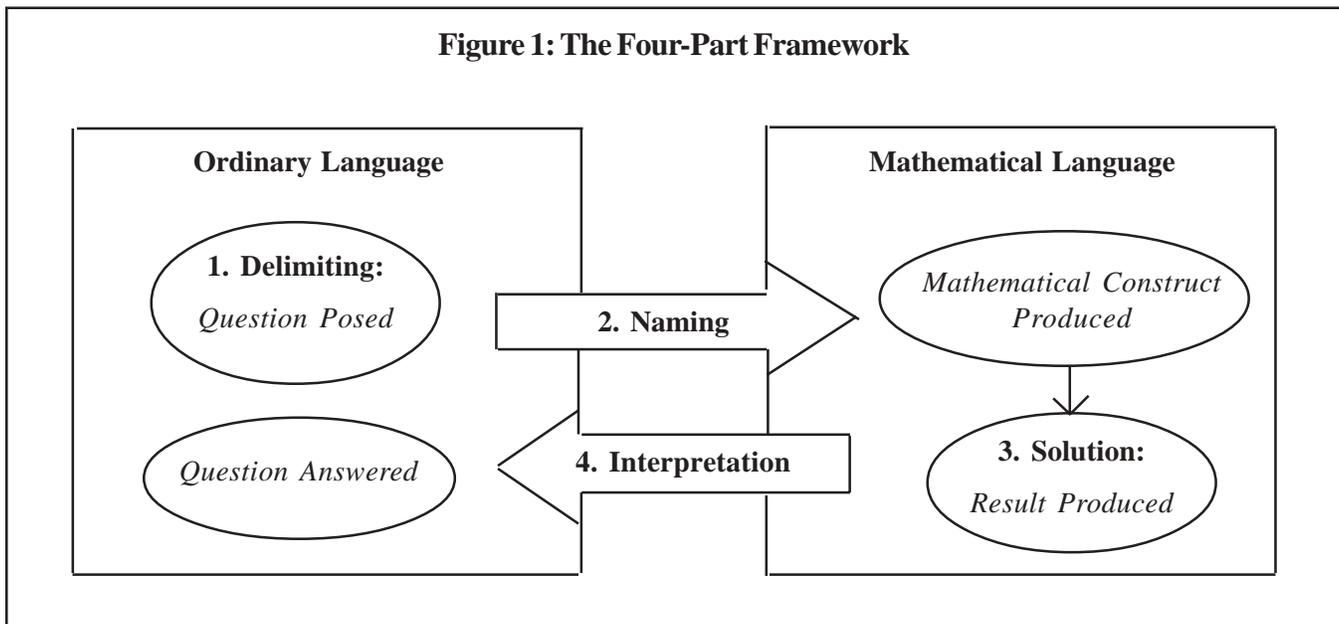
In addition to delineating the four phases of economic analysis, the FPF also highlights an important divide in M-M analysis—i.e., the divide between the realm of ordinary language descriptions (the language of ordinary usage and linguistic convention) and mathematical language descriptions. M-M analyses must twice cross this divide. The research question will be phrased in ordinary language, and this is not arbitrary or incidental: M-M analyses are meant to unravel complexities of the world as we encounter it, and description of that world must, in the first instance, be done on its own terms. The mathematical construct, on the other hand, will be articulated and manipulated using mathematical language. And in the final stage of the analysis, when the initial research question is answered, ordinary language will again be used. Figure 1 summarizes the FPF in graphical form.²

Examples of the use of M-M in social science are not hard to find, and in economics and rational choice political science it would be more difficult to find an analysis that does not utilize M-M than to find one that does. To offer just one example, from the field of labor economics, consider Carl Shapiro and Joseph E. Stiglitz’s seminal paper on efficiency wage theory, “Equilibrium Unemployment as a Worker Discipline Device” (1984).

In the *Delimiting* phase of their analysis, Shapiro and Stiglitz identify as a puzzle the persistence of unemployment. They delimit unemployment and related social phenomena as the set of social phenomena with which their analysis will be concerned:

Involuntary unemployment appears to be a persistent feature of many modern labor markets. The presence of such unemployment raises the question of why wages do not fall to clear labor markets. In this paper we show how the information structure of employer-employee relationships, in particular the inability of employers to costlessly observe workers’ on-the-job effort, can explain involuntary unemployment^[3] as an equilibrium phenomenon. Indeed, we show that imperfect monitoring neces-

Figure 1: The Four-Part Framework



sitates unemployment in equilibrium.^[4]

It is important to note that this delimitation and puzzle articulation occurs in ordinary language. The authors are referring, in the quotation above, to workers, employers, labor, unemployment, shirking, etc., *as they are understood in their social context*. Later, they will provide mathematical analogues for these concepts. But the paper is first and foremost a study of social phenomena, and we must understand the elements of the *Delimiting* phase in this light.

The *Naming* phase consists of two interrelated parts: the *catalog of correspondences* and the *mathematical construct*. The catalog of correspondences links the elements of the set of delimited social phenomena (understood in their social context and expressed in ordinary language) with mathematical analogues that compose the mathematical construct. The mathematical construct consists of these mathematical analogues, the relationships between them, and a solution concept. For illustrative purposes, I will present (in summary form) on only a portion of the catalog and construct presented in Shapiro and Stiglitz (1984):

Excerpt from the Catalog of Correspondences:

1. wage: w
2. unemployment benefit: \bar{w}
3. the minimum wage sufficient to induce employee effort – the critical wage: \hat{w}
4. effort on the job: e
5. shirking⁵ on the job: $e = 0$
6. interest rate: r
7. the probability of being detected shirking at work: q
8. the probability of being separated from one’s jobs for reasons other than being detected shirking: b
9. the probability of finding a job when one is unemployed: a

10. work-related enjoyment (also called “utility”): $U = (w - e)^{[6]}$
11. expected lifetime work-related enjoyment of an unemployed individual:

$$V_U = \frac{w + a(V_E - V_U)}{r}$$

12. expected lifetime work-related enjoyment of a shirker:

$$V_E^S = \frac{w + (b + q)V_U}{r + b + q}$$

13. expected lifetime work-related enjoyment of a non-shirker:

$$V_E^N = \frac{(w - e) + bV_U}{r + b}$$

14. The worker’s objective: $\text{Max}_e (V); V = \{V_E^N, V_E^S\}$

Excerpt from the Mathematical Construct:

Solution Concept: $\text{Max}_e (V(e, \bullet)); V(e, \bullet) = \{V_E^N(e, \bullet), V_E^S(e, \bullet)\}$ subject to $e \in [0, \infty), w \in [0, \infty)$

Note that the excerpts above are not contained in Shapiro and Stiglitz (1984) in the form I have presented. Rather, the items in the catalog of correspondences and the ultimate form of the mathematical construct are established separately in statements like the following: “We write an individual’s instantaneous utility function as $U(w, e)$, where w is the wage received and e is the level of effort on the job.”⁷ This statement establishes items 1 and 4 in the catalog above, and is part of the set of statements that establishes items 5 and 10.

The *Solution* phase consists in solving the mathematical construct. As Shapiro and Stiglitz (1984) is a theory paper, they

do not arrive at a numerical solution to the mathematical construct with actual data on interest rates, unemployment benefits, etc. Rather, they discuss certain general characteristics of a solution. One such general characteristic presented by the authors is:

(For $e \in [0, \infty)$), e will be at some positive level $e' > 0$ if and only if:

$$(5) \quad w \geq rV_u + (r + b + q)e' / q = \hat{w}^{[8]}$$

Note that this solution is entirely a mathematical statement. One can understand it completely with reference to the mathematical construct *only*, and with no reference at all to the social phenomena under study.

In the *Interpretation* phase, in order to reconnect the analysis to the question initially posed, the authors must utilize the catalog of correspondences to interpret the mathematical result of the *Solution* phase as relevant to the social phenomena identified in the *Delimiting* phase. The following statement is one example of such interpretation: I will refer to the statement as (5*):

Equation (5) has several natural implications. If the firm pays a sufficiently high wage, then the workers will not shirk. The critical wage, \hat{w} , is higher,

- (a) the higher the required effort (e),
- (b) the higher the expected utility associated with being unemployed (V_u)
- (c) the lower the probability of being detected shirking (q),
- (d) the higher the rate of interest (i.e., the relatively more weight is attached to the short-run gains from shirking until one is caught compared to the losses incurred when one is eventually caught), and
- (e) the higher the exogenous quit rate b (if one is going to have to leave the firm anyway, one might as well cheat on the firm).⁹

Note that without the catalog of correspondences, there is nothing “natural” about the implications the authors identify connecting the mathematical construct to the social phenomena under study. This is an indication that the catalog of correspondences is shouldering a good deal of work in this analysis. I will discuss this in more detail below. For now, it is sufficient simply to recognize the above statement as an example of *Interpretation*-phase activity.

Though Shapiro and Stiglitz (1984) is a theory paper, the FPF applies equally well to empirical work using the M-M method. In the case of empirical work, the FPF becomes a bit more complex. The mathematical construct may include not only the type of mathematical construct we found in Shapiro and Stiglitz (1984), but also an econometric specification based on that mathematical construct. In addition, the mathematical construct of the econometric specification will include all of the statistical machinery underlying the chosen solution concept—e.g., ordinary least squares (OLS), generalized method of moments (GMM), maximum likelihood estimation (MLE), etc. The catalog of correspondences may also become more

complex as the author searches for data variables that match the variables in the theoretical construct that is being investigated. For example, for an empirical analysis testing a theory of equilibrium unemployment, one might find that there is no direct measure of “effort” in any available data set, requiring a proxy to be found. As a result, the “effort” link in the catalog of correspondences, rather than consisting simply of “effort – e ,” might become “effort – [some other social phenomenon that will stand in for effort] – [measurement of specified phenomena meant to represent the proxy phenomenon] – [name of data variable].”¹⁰

For my purposes, the important point is that all M-M social science analyses—whether theoretical or empirical—consist of, on the one hand, a question about social phenomena, and on the other, a mathematical construct whose solution and solution dynamics ostensibly shed light on the nature and possible dynamics of the social phenomena under study.

Necessary Conditions for the Coherent Use of M-M in Social Science

But is this interpretation of the solution of the mathematical construct legitimate? To answer this question, it is instructive to split the interpretation into two steps: (i) the implications of the mathematical dynamics for the mathematical objects that make up the mathematical construct, and (ii) the implications for the social elements they are meant to represent. Or, put another way, we can split the interpretation into the implications for the right-hand-side (RHS), or mathematical, elements of the catalog of correspondences and the left-hand-side (LHS), or social, elements.

The split can be most clearly explored through an example. Recall statement (5*) from Shapiro and Stiglitz (1984). The interpretation embodied in (5*) of (a portion of) the solution of the mathematical construct can be split into the two categories mentioned above. First, it consists of an interpretation of the solution with respect to the RHS elements of the catalog of correspondences: I will refer to this statement as (5**):

The value of \hat{w} is higher,

- (a) the higher the value of e ,
- (b) the higher the value of V_u ,
- (c) the lower the value of q ,
- (d) the higher the value of r , and
- (e) the higher the value of b .

Second, it consists of an extension of this interpretation to the LHS elements of the catalog. This extension is effected by a simple reassertion of the catalog. For example, the (5**) statement

the value of \hat{w} is higher, the higher the value of e

combined with a reassertion of elements of the catalog of correspondences, namely:

the minimum wage sufficient to induce employee effort – the critical wage: \hat{w}
effort: e

produces the (5*) statement “the critical wage, \hat{w} , is higher,

the higher the required effort (e).”

The two separate (though related) activities involved in statement (5*) should each be judged according to standards appropriate to the kind of activity each of them is. The proper standards for assessing statement (5**) are the standards of proper mathematical practice.

In assessing the legitimacy of the interpretation statement (5*) there are two relevant sets of standards at work—and although both sets of standards are essential to the assessment of M-M work, one of these sets of standards is currently acknowledged as a necessary part of proper M-M social science practice, while the other is not. These standards, are respectively, (1) those of proper mathematic and econometric practice, and (2) those of apt metaphor construction.

The first set of standards allows us to assess statements like (5**). Further, these are the *only* standards needed to assess statements like (5**). We need know nothing about unemployment, wages, labor markets, or human experience to know whether or not (5**) is a true statement. To put an absurd slant on it, imagine that we replaced the LHS elements of Shapiro and Stiglitz’s (1984) catalog of correspondences with a group of randomly selected nouns. This would not change the status of (5**) one bit.

But the purpose of M-M social science analyses is to go beyond statements like (5**), to extend the implications of the solution of the mathematical construct to the LHS elements of the catalog of correspondence, e.g. to make statements like (5*). We have seen above that this requires using the connections asserted in the catalog. If there are standards for legitimate construction of these connections, then meeting these standards will be essential for analyses that utilize the M-M method. To fail to meet them would be to sever the link between one’s analytics and one’s subject matter. The mathematical construct would be irrelevant to the question at hand. As such, it is worthwhile to at least consider whether or not such standards exist, especially as there are currently no such standards generally recognized in quantitative social science practice.

But clear and well-established standards for the proper construction of catalogs of correspondence in M-M social science do exist: they are a special case of the general standards for proper metaphor construction.

That the mathematical constructs of M-M analyses are metaphors is, I think, uncontroversial. The *Cambridge Dictionary of Philosophy* defines a metaphor as “a figure of speech (or a trope) in which a word or phrase that literally denotes one thing is used to denote another, thereby implicitly comparing the two things.”¹¹ Unless we want to view statements such as “wage is w ”—which are the constituents of catalogs of correspondence in M-M analyses—as simple category mistakes, we must conclude that the statements are meant metaphorically. What is much more controversial is the claim (which I also support) that we should understand mathematical models in quantitative social science *primarily* as metaphors. Although I do not have the space here to argue fully for this position, I would just comment that this position is not fundamentally at odds with other views of the meaning of mathematical models

in science—even, surprisingly, those views that seem directly to reject it.¹²

Regardless of the label one chooses for M-M social science methodology, it remains the case that it does its illuminating work by interpreting the solution dynamics of a mathematical model as possible dynamics of a given set of social phenomena. Philosophers of language and science have undertaken a good deal of exploration of the dynamics of this kind of illuminating process (under the heading of metaphor). In particular, Black (1962, 1993), Hesse (1963) and Goodman (1976) offer compelling accounts of the manner in which such illumination takes place, and what is necessary for it to work.

To be more precise about the process, let me first fix terms. Following Black (1993), I will call the thing being denoted in a metaphor the “primary subject,” and the word or phrase denoting it the “secondary subject.” For example, in the metaphor “man is a wolf,” “a wolf” is the secondary subject and “man” is the primary subject. Metaphors bring to light non-obvious facts about the primary subject by compelling the receiver of the metaphor to imagine the primary subject *as* the type of thing that the secondary subject is. In the example above, we are compelled to imagine man *as* a wolf. This entails, roughly, seeing man’s world through a wolf’s eyes; i.e., taking the phenomena we encounter in man’s world and interpreting them as though they were analogous phenomena in a wolf’s world. Colleagues would become rivals for power (as in a wolf pack), pursuit of goals would take on a ruthless quality (like the life and death struggle for status and sustenance in the wolf pack), etc. Another way to put this is that it entails importing the interpretive framework of “a wolf” into the world of “man.”¹³

This importation is the insight-generating mechanism of metaphor. It is powerful but also potentially problematic, because it can only work if the relevant aspects of the primary subject’s world are meaningfully organizable within the interpretive framework of the secondary subject. For example, “man is a wolf” is an apt metaphor for, e.g., describing office politics, because the phenomena associated with the human workplace are structurally similar enough to the phenomena of the wolf’s world that they can be organized by the wolf’s interpretive framework in a meaningful way. I will refer to this property of structural similarity between the phenomena of the primary and secondary subjects as “meaning-homomorphism” between the primary and secondary subjects.

When primary and secondary subjects are not meaning-homomorphic, metaphors are incoherent. For example, suppose that, in the course of discussion of man’s emotional response to bereavement, I say, “man is a waffle iron.” This is an inapt metaphor because the interpretive framework of “a waffle iron” cannot organize phenomena like emotions. As such, it cannot provide any illumination with respect to man’s emotional response to bereavement. Meaning-homomorphism is a *necessary condition* for metaphorical aptness in general. When the condition is not met, the metaphor will not make sense.¹⁴

In the case of M-M social science, the meaning-homomorphism condition translates to a need for the social phenomena under study to be meaningfully organizable by the

interpretive framework of mathematics (or, more specifically, of the particular mathematics employed in the analysis).

But what makes something “meaningfully interpretable by the interpretive framework of mathematics”? This is a very broad question—effectively, “what makes something a mathematical object?” I will scale the question down a bit by identifying some necessary conditions. My claim is that two such conditions for an entity to be meaningfully interpretable within a mathematical interpretive framework are (1) it must have “context-independent meaning” (see below) and (2) it must act always and only according to its formally defined role. Both of these conditions stem from the fact that mathematics is a formal deductive system whose symbols are tokens of formally defined types. Because mathematics is a formal deductive system, the rules of its operation are uniformly and universally proscribed throughout its realm of operation. Because its objects are tokens of formally defined types, all individual instances of mathematical types “mean” the same thing in the sense that they denote only the function that such a type performs. For example, there is an infinite number of possible vectors, but each vector can perform only the same set of prescribed actions. The same is true of variables, constants, functions, power sets, etc. (The same is not true of the elements of ordinary-language English, for example, which are capable of functioning ironically and poetically, among other things.)

Let me briefly illustrate what conditions (1) and (2) mean practically. “Context-independent meaning” means simply that any mathematical object must mean the same thing everywhere it occurs; it must maintain its meaning (a) through any manipulation to which it is legitimately subject, and (b) throughout the entire realm for which it is defined. Consider a vector $X \in R^n$. As a vector, it can perform a certain set of mathematical operations. These include scalar combinatorials and vector combinatorials (with other suitably-sized vectors), being an argument in correspondences with suitable domains, taking part in set theoretic operations, etc. And through all of these operations, X must maintain its original meaning. Concretely, X has the same meaning in all of the following statements (among others): “ aX ”, where “ a ” is a scalar; $X.Y$, where “ Y ” is a suitably-sized vector; $X = \{X_i \mid i = 1, \dots, m\}$; $F(X) = \bigwedge wX$, where “ w ” is an argument of X .

Condition (2) means simply that we do not need to know anything in particular about the token of a type in order to know how it will perform in various operations. For example, imagine that, in addition to X , there exists another vector $Y \in R^n$. Without knowing anything else about X and Y , we know that both of them can perform precisely the same actions meaningfully. Of course, they need not be identical. They can take on different values. They cannot, however, take on different roles.

In order for any particular M-M social science analysis to be coherent, then, the delimited realm of social phenomena (constituting the primary subject) must be conceivable as a composition of elements with context-independent meaning that act and interact always and only according to the laws governing their type. If this is not the case, then the dynamics

of the mathematical construct will be irrelevant to the social phenomena under study. I will summarize these necessary conditions as follows:

Necessary Conditions for the Coherence of M-M Social Science Analysis:

The delimited realm of social phenomena must be conceivable as a composition of elements

- (1) with context independent meaning, and
- (2) that act and interact always and only according to the laws governing their type.

To see what this means concretely, recall statements (5*) and (5**). In statement (5*) Shapiro and Stiglitz claim that the dynamics of the mathematical construct “naturally imply” analogous possible dynamics of the LHS elements of the catalog of correspondences represented by the construct. Imagine, though, that condition (1) above did not hold. For example, suppose we could not guarantee that the social understanding of “effort on the job” would remain constant if individuals made their labor decisions in the manner supposed in the paper. If this were the case, then equation (5) would not necessarily imply anything about “effort on the job” and its relation to other elements of the delimited social phenomena. Now imagine that condition (2) did not hold; for example, that what the authors refer to as “work-derived enjoyment” is actually an amalgam of various idiosyncratic elements of human life, only some of which have to do with the other elements of the delimited social phenomena. If this were the case, the observed behavior they associate with work-derived enjoyment could not be reliably ascribed to regular associations with the other social phenomena under study. One might be tempted to categorize this concern as a simple “omitted variable” problem, but that would be question-begging: it is not necessarily the case that the omitted influences are themselves conceivable as variables. At some point we will need to establish the plausibility of presenting the delimited social phenomena as composed of rule-following types with context-independent meaning.¹⁵

Why Interpretive Methods Must Be Used to Establish the Necessary Conditions

M-M social science must be supplemented by interpretive methods because it is not possible to establish the necessary conditions mentioned above without them. The necessary conditions are conditions on the nature and meaning of social phenomena—that is, on the experience associated with these phenomena. They are conditions that require one to consider social phenomena as they are actually experienced without presupposing that these phenomena can be meaningfully understood as conceptually modular and objectively accessible entities.

Quantitative methods alone cannot accomplish this, because they must first assume what needs to be established; namely, that the social phenomena under study are representable by mathematical objects and, therefore, are meaningfully interpretable within a mathematical interpretive framework. This

is a key point as it suggests an answer to an obvious objection. The objection is that M-M social science already has a means for assessing the aptness of its models: statistical testing using econometric methods. My response is that it is not possible to test the conclusions of an incoherent M-M analysis. If one does not establish that the necessary conditions are met, then the solution to one's mathematical construct may be irrelevant to the research question. In empirical M-M social science, the mathematical construct is typically an econometric specification (along with the chosen solution concept—e.g., OLS, GMM, MLE), and the solution to the construct includes the results of the test (e.g., point estimates, confidence intervals, power statistics). If the necessary conditions are not established, the results of these tests may not be relevant to the original research question. Establishment of the necessary conditions cannot be accomplished by empirical testing because establishing these conditions is a requirement for the coherence of presenting empirical tests as relevant to the research question in the first place.¹⁶

The necessary conditions can only be established using methods that investigate the phenomena in context. This requires understanding the interpretive framework within which the phenomena under study are organized into meanings.¹⁷ This is the only way in which we can hope to identify categories of experience (within particular social situations) stable and durable enough to be representable by mathematical constructs. Of course, there is no guarantee that such stable and durable categories will be identified, but if we proceed with M-M analysis without investigating the possibility, our analyses will be open to the legitimate criticism that they are possibly incoherent.

The challenge to M-M social science posed by the foregoing arguments, then, is to incorporate the systematic use of interpretive methods for establishing the necessary conditions identified above. Fortunately, there is a substantial and evolving body of experience in employing and assessing interpretive methods in social science—e.g., case study, ethnography, and interview methods. Further, interpretive methodology of one type or another is employed in some M-M social science already.¹⁸ What we do not currently have, however, are (1) a universal recognition that the use of such methods is a *necessary* part of good M-M social science practice, and not simply a supererogatory act or a means of tweaking modeling assumptions, and (2) an institutional framework for developing methods and standards for the interpretive groundwork necessary for coherent M-M social science. Lacking these two elements, the interpretive work that does go on in quantitative social science will necessarily be *ad hoc*.

I believe that the best means of addressing these issues would be the establishment of a new social science field responsible for the development of the methods and standards referred to above—a field that would cover the subject matter of “The Hermeneutics of the Analytic Possibilities of Social Experience (HAPSE).” The field of econometrics stands as a precedent, having worked to tailor statistical methods to the needs of economics and to establish standards for the legitimate use of such methods. Econometrics developed out of a

desire to understand social experience more rigorously and to be able to separate spurious claims from legitimate ones. A HAPSE field would address similar issues. It would allow for a more well-grounded quantitative social science by offering the means of separating incoherent analyses from coherent ones. As with econometrics, the work of the field would be both theoretical and empirical. Theoretical work would likely resemble phenomenologically minded philosophy of science and language—e.g., the exploration of the social meaning of “value” in Anderson (1993)—and would guide empirical practice. Empirical work would involve field work—e.g. case study, ethnography, interview—exploring various aspects of social experience, the findings of which could be employed by quantitative social scientists in designing their own studies. Such work is not generally undertaken in economics currently—Truman Bewley's *Why Wages Don't Fall During a Recession* (1999) is a rare example.

M-M social science needs interpretive methods in order to create a more promising match between its analytics and the nature of the object of its study. As Gadamer wrote, a method of analysis “which could be determined before even having penetrated [the object of study], is a dangerous abstraction; *the object itself* must determine the method of its own access.”¹⁹ With this in mind, practitioners of M-M social science should not view the incorporation of interpretive methods as a foray into the realm of pseudo-science but rather as a means of reassessing the proper scope and aims of quantitative social science methods according to the actual analytic possibilities of the phenomena they aim to understand.

Notes

¹ This model builds on work of Max Black (1962) and is similar to a model developed by R. I. G. Hughes (1997).

² To the extent that an analysis utilizes the M-M method, it will proceed along the lines specified in the FPF. It is important to note that the FPF need not be a literal outline of an analysis in order for that analysis to “proceed along the lines specified in the FPF.” The presentation of the FPF phases need not occur serially. Authors may, for example, include elements of their mathematical construct or their conclusions in the introduction to their paper. These are stylistic choices, and the FPF is a model of underlying structure and flow of argument, stylistic choices notwithstanding. As such, it may be necessary to extract the elements of each phase from various parts of the actual text of the analysis in order to get a clear idea of the flow of the argument as suggested by the FPF.

³ The following footnote appears in the Stiglitz and Shapiro paper on pp. 433: “By involuntary unemployment we mean a situation where an unemployed worker is willing to work for less than the wage received by an equally skilled employed worker, yet no job offers are forthcoming.”

⁴ Shapiro and Stiglitz (1984: 433).

⁵ In the context of Shapiro-Stiglitz efficiency wage theory, “shirking” is understood to mean lack of effort. As such, measures of effort can also be used to measure extent of shirking.

⁶ This is really a two-part correspondence. First, the authors associate the (ordinary-language) terms “enjoy” and “dislike” with the technical economic term “utility,” and then they associate “utility” with the mathematical term *U*: “There are a fixed number, *N*, of identical workers, all of whom dislike putting forth effort, but enjoy

consuming goods. We write an individual's instantaneous utility function as $U(w, e)$, where w is the wage received and e is the level of effort on the job. For simplicity we shall assume that the utility function is separable; initially, we shall also assume that workers are risk neutral. With suitable normalizations, we can therefore rewrite utility as $U = w - e$ " (Shapiro and Stiglitz 1984: 435).

⁷ Shapiro and Stiglitz (1984: 435).

⁸ I have modified (5) slightly from how it appears in the text of the Shapiro-Stiglitz paper. Where I have written e' in (5), they have written e . I.e., in the paper, (5) is: $w \geq rV_U + (r + b + q)e / q = \hat{w}$. I am taking the statement out of its context, and the modification is necessary to ensure that (5) here has the same meaning it does in the Shapiro-Stiglitz paper.

⁹ Shapiro and Stiglitz (1984: 436).

¹⁰ This is the case in Cappelli and Chauvin (1991), an empirical test of efficiency wage theory. The specific chain is: shirking (lack of effort) – rate of job dismissals due for disciplinary reasons – non-arbitrary measurement of such dismissals based on a specific definition of what will and what will not constitute a “disciplinary reason” – *DISL*.

¹¹ *Cambridge Dictionary Of Philosophy*, 562.

¹² To take just one recent example, Robert Sugden (2002) rejects the idea that viewing economic models as metaphors is helpful to understanding how they function, opting instead for a view that models are “credible counterfactual worlds” that help us to understand the actual world through “inductive inference” from the former to the latter. I think that Sugden is mistaken in differentiating his view of models from a metaphorical view. His “inductive inference” is nothing more or less than the projection of a mathematical interpretive framework (the organizing logic of the “credible counterfactual worlds” of economics) onto the phenomena under study (the actual world). And, as I argue below, the projection of an interpretive framework from its home realm into an alien realm is the essence of metaphor. Other views of the meaning of models in science can be found in Morgan (2002) as tools in the construction of a story about the world, Cartwright (1997, 2001) as nomological machines that produce regularities under tightly controlled conditions and Hausman (1992) as collection of statements constituting a predicate or definition of a predicate.

¹³ See Goodman (1976: 72-3) for a good discussion of the importation of interpretive frameworks.

¹⁴ See Hesse (1963: 95-6) for another example of metaphorical inaptness arising from lack of meaning-homomorphism.

¹⁵ Establishing these necessary conditions rigorously requires a good deal more space. For a more detailed argument, see Spiegler (2004), especially pp. 16-27.

¹⁶ To anticipate the kind of objection discussed in Cox (1999), I should note that, in contrast to Green and Shapiro (1994), my central claim is not that M-M social science has no empirical content. I readily acknowledge that some M-M social science may well have significant empirical content. My contention is that without establishing the aforementioned necessary conditions we simply cannot know whether or not a given theory has empirical content, irrespective of what the empirical tests tell us, because our empirical testing methodology is also M-M in nature and so its coherence, too, is dependent upon the establishment of the necessary conditions. The tests will unquestionably tell us something about the relationship between the RHS elements of the catalog of correspondences, but we need the necessary conditions to be fulfilled in order for this implication to flow to the LHS elements—and the claim of empirical content is dependent upon this extension to the LHS elements.

¹⁷ See Yanow (2003) for a useful distinction between interpretive methods and a current connotation of “qualitative methods”—i.e.,

“small-n studies that apply large-n tools.” Yanow helpfully points out that the essence of interpretive methods is ontological constructivism and epistemological interpretivism, and not necessarily small-n focus (Yanow 2003: 10)).

¹⁸ See Helper (2000) for an overview of the use of what she refers to as “qualitative” methods in economics. See also Theiss-Morse, Fried, Sullivan, and Dietz (1991) for an example of M-M social science supplemented by interpretive strategies for establishing the meaningfulness of the categories of social experience represented by mathematical constructs. I was led to this example by Stoker (2003). See also Piore (1979) for an interesting anecdotal account.

¹⁹ Gadamer (1987: 93), emphasis in original.

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Symposium: Field Research: How Rich? How Thick? How Participatory?

Introduction

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Field research remains common in comparative politics and other branches of political science. While fieldwork is more appropriate for some projects than others, leaving home base and striking out into unfamiliar surroundings is an essential way in which researchers gather new data, acquire fresh perspectives, subject seminar-room musings to cruel but necessary reality checks, and establish credibility. Yet for all our discipline's attention to methods and research design, it does relatively little to train graduate students for the practical and conceptual challenges of managing a field research project.

For the past three years, Melani Cammett, Lauren Morris MacLean, and I co-taught an APSA short course entitled "Strategies for Field Research in Comparative Politics and International Relations."¹ A common denominator in our own research projects has been working in settings (the Middle East, sub-Saharan Africa, and China, respectively) and on topics (networks, local organizations, norms, and political perceptions) that require particular care, sensitivity, and judgment in the process of gathering data. In such circumstances particularly, *neither what is being studied nor the relationship between researcher and subjects* is always straightforward, transparent, and easily established. Up-close and direct interaction in the ethnographic tradition—trying to get as near as possible to the people from and about whom you are learning—holds the promise of particularly high validity. But what exactly does it take to achieve this result? How much time is it necessary to

spend at one's site or sites? How should a researcher present herself; what kinds of relationships should she build with her subjects? Is there any sense in which we should "partner with" or "give back" to the informants and communities that we study?

Social scientists have been grappling with these problems and questions for generations, of course. And to some extent everyone who does fieldwork comes up with his or her own unique solutions, through trial and error, judgment, finesse, and serendipity. Yet there are many general issues and approaches to be discussed and assessed. Some can be posed as *trade-offs*, and each of the three pieces below explores consequential choices that researchers face. My essay considers two prominent but also contrasting examples of field research, one based on "thick" ethnography at a single site, the other on less deeply embedded participant-observation at multiple sites. It calls for a collective effort in the discipline that would encompass and promote both modes of research within a shared framework of "site-intensive methods." Morris MacLean's piece challenges political scientists to follow the lead of other disciplines in thinking about our subjects' interests in ways that go beyond merely filling out IRB forms, by adopting more collaborative models of research. Cammett's article focuses on research in deeply divided political settings, where gaining and maintaining access to informants is often difficult and challenges to data validity can be acute. Many of her observations also apply to research on sensitive questions in less overtly divided settings and to conducting research in politically repressive places.

Note

¹ This continued a course first developed by Marc Morjé Howard,

Evan S. Lieberman, and Julia Lynch, who published their guidelines for field research in *Qualitative Methods: Newsletter of the American Political Science Association Organized Section on Qualitative Methods* 2:1 (Spring 2004), 2-15.

Site-Intensive Methods: Fenno and Scott in Search of a Coalition

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The methods that generally go under the labels ethnography and participant-observation occupy a somewhat awkward place in political science (Bayard de Volo and Schatz 2004). Our discipline lays claim to prominent, if rather isolated, examples of scholarly work based on these methods—with perhaps the two most widely read being Fenno's *Home Style* (1978) and Scott's *Weapons of the Weak* (1985). A subset of empirical researchers has always been drawn to them, going back at least as far as the immediate post-WWII generation and presumably earlier (Banfield 1958). They are discussed on the occasional conference panel.¹ They are actively employed in much exciting research today, by themselves or in conjunction with other methods (examples include Adams 2003, Allina-Pisano, 2004, Bayard de Volo 2001, Cammett 2005 and 2007, Chen 2006, Galvan 2004, Morris MacLean 2004, Roitman 2004, Schatz 2004, Straus 2006, and Tsai 2007).

And yet, these methods remain marginal. I think it's fair to say that they only occasionally crop up in methodology curricula. Even within the world of the APSA organized section on qualitative methods and the stimulating ferment that it has fostered in the past three years, they have shown up so far as a distant cousin. Very little in this vein appears on the 2006 Institute for Qualitative Research Methods (IQRM) syllabus, for example.² Two of the most important recent books on qualitative methods, while immensely useful to ethnographers and participant-observers, also seem to have been written without these approaches particularly in mind (George and Bennett 2004, Brady and Collier 2004).

What are the reasons for this marginality? Let me first mention a few obvious ones. Acquiring the skills needed to use these methods and then applying them is time consuming and costly, particularly when research is conducted in foreign-language settings. They are thus rather difficult to recommend in good conscience to the average graduate student who cares about minimizing time to completion. And, of course, other methods skills are much more in demand within the discipline as a whole.

But clearly there are other reasons, as well. Practitioners are split by apparent differences in epistemology, such as a perceived schism between "interpretivists" and "positivists," and sometimes even seem to highlight these divides rather than bridge them (Yanow 2003, Schatz 2006). There is also a reluctance on the part of many to go beyond describing what

they did in their own research and prescribe sets of general procedures for others to follow. Efforts within political science to spell out explicitly (and promote) the benefits of these methods for the building and testing of theories have so far been limited. Finally, and perhaps most importantly, there has been no real push to build a coalition behind the critical appreciation, application, and teaching of these methods.

Does marginality matter? Some people may not think so, but it seems clear that there are real costs to practitioners and to the discipline as a whole. Readers of our work lack a clear framework within which to understand, evaluate, and criticize it. It is perceived as alien, something that might be appropriate in other disciplines, such as anthropology, but is not suitable for our own. In the minds of many of our peers, it tends to be equated with a "squishy," barefoot empiricism that blunders around haphazardly collecting anecdotes. Students not only aren't trained in it, but don't even get a sense of what it is, or that it's available to them as a legitimate way to pursue answers to questions they wish to ask. Finally, all this means that less good work in this tradition is produced than would otherwise be the case, and consequently our collective understanding of politics is that much the poorer.

I believe an appropriate step forward would be for researchers who use these methods—either exclusively or in combination with other methods—to work toward building a coalition or users group within the discipline and within the broader qualitative methods section. This enterprise would start by identifying common ground in a related set of *approaches to the gathering of sources, evidence, and data*. This would, one hopes, cross-cut and set aside underlying epistemological divides.

It often feels indulgent to propose new terminology, but it strikes me that an umbrella term to bring together a set of related methods might be useful. One such term would be *site-intensive methods* (SIM), referring to the collection of evidence from human subjects within their own contexts, their interaction with which informs the study just as the researcher's own questions do. This implies the need for a deeper engagement with a site, context, locality, or set of informants than is obtained in, for instance, telephone surveys or some types of one-time interviews—though other kinds of surveys and interviews in fact require considerable stage-setting and trust-building.³ This term would subsume most of what is referred to as "ethnography" and "participant observation," and perhaps some forms of other practices, like focus groups. It would also highlight the diverse forms that this research takes, including studies that strive for a high degree of depth in a single locale, as well as those that also aim for breadth as well, and projects in which SIM is the main course, so to speak, as well as others where it is more of a side dish.

I reluctantly suggest some new term like SIM in part because I'm not convinced that other categories are up to the task of bringing together the most useful and productive group of researchers within political science. "Ethnography" on its own has the advantage of a long pedigree within the social sciences and a voluminous methodological literature, especially in anthropology. But it may have drawbacks as well.

Some versions of the anthropological model may set the bar too high in implying that months or years of immersion are required in order to obtain insights. It may imply a holistic orientation according to which the entirety of a community or locale must be comprehended in order to make sense of any one part. It should be pointed out that the meaning and practices of ethnography today are undergoing evolution and sharp debate; by no means do all practitioners see ethnography as limited to these forms.⁴ Nonetheless, the term seems to me problematic as applied to projects employing shorter stints of fieldwork and less encompassing modes of information-gathering.

“Participant observation” would also seem to be a possible umbrella term. Yet here, too, the boundaries implied by this concept may not be co-terminous with what it is we want to bring together. On the one hand, the researcher “participates” in other kinds of research, such as straight-up interviews. Conversely, to some it may imply that only through a long-term process in which the researcher becomes a part of community life can full or meaningful participation be achieved. Some may feel that this covers only a subset of the practices comprised by ethnography, while others see the two concepts as interchangeable.

Finally, there are those who favor the term “narrative methods” (Laitin 2002, 2003), but I see this as both too broad and too narrow for present purposes. As Laitin uses the term, it includes all manner of case studies, interviews, and other qualitative work. Moreover, even if unintentionally, it seems to reinforce the belittling notion that this type of research is “just a bunch of stories.” It does not do justice either to deeper forms of “thick” knowledge that might be achieved through ethnography, nor to the fact that site-based field research can be used to acquire “thin” data points as well as narratives and other types of evidence.

At the outset, I noted that two studies by Fenno and Scott might be the most widely read examples of site-intensive methods in political science. Reflecting on these books, in particular, provides an opportunity to consider what it might take to promote an initiative within the discipline that incorporates and promotes both types of research. Viewed from one perspective, they can be seen as strongly contrasting, perhaps almost polar opposites in their approach. Scott describes his project as a “close-to-the-ground, fine-grained account of class relations” (p. 41) in a Malaysian village, population 360, that goes under the pseudonym of Sedaka. Situating his methods in the ethnographic tradition of anthropologists (pp. xviii, 46), Scott states that he spent at least 14 months in Sedaka, interviewing, observing, and taking part in village life.

Fenno’s work was motivated by questions concerning the relationship between representatives and those they claim to represent: “What does an elected representative see when he or she sees a constituency? And, as a natural follow-up: What consequences do these perceptions have for his or her behavior?” (p. xiii). His approach was to spend time in the company of members of the U.S. Congress in their home districts. He famously characterizes his research method as “largely

one of soaking and poking—or just hanging around,” and situates it explicitly within the tradition of participant observation as practiced by sociologists and other political scientists; ethnography is not mentioned, as far as I can tell (pp. xiv, 249, 295). In the text of the book and its long methodological appendix, Fenno candidly and rather self-deprecatingly explains his *modus operandi* of accompanying politicians wherever they would let him tag along, building rapport, recording their remarks, and asking questions when possible.⁵ Clearly this is a much “thinner” form of engagement with a research milieu than Scott’s village study. Relative to a single-site project, Fenno traded depth for breadth, studying eighteen different representatives and thus obtaining substantial variation on characteristics such as party affiliation and seniority (pp. 253-254). The total time Fenno spent with each representative ranged from three working days to eleven, averaging six (pp. xiv, 256), and some of those days the research subject was available only part of the time.⁶

Despite their differences, these books can readily be seen as belonging to a common category. Both scholars were propelled into the field by strongly theoretical motivations—theories of hegemony and false consciousness in one case, and theories of representation in the other. In both instances, the researchers identified an empirical subject of key importance where (at least as they portrayed it) existing accounts relied on assumptions that ought to be tested or fleshed out through on-the-ground study.

Whether “site-intensive methods” or some other label proves best,⁷ the underlying idea is that practitioners of this overlapping, closely related family of methods ought to do more to talk to one another—within the political science context. We in this field are fortunate to benefit from a wide selection of examples and methodological texts from other disciplines too numerous to cite properly here. We should absolutely make the most of this material. But the development and communication of these methods within our own departments and conferences and journals is important in its own right.

The purpose would not be to try to impose a consensus on a diverse set of researchers with varying ideas and commitments concerning the philosophy of science behind what they do. Rather, it would pursue some of the following goals:

Encourage the creation of clear statements of what is meant by this research and the ways in which it can be used to address theoretical topics in our discipline. Individual researchers will differ on the meaning or status of theory, but there can be no doubt that this is the touchstone by which work is judged in political science today. The full range of purposes that can be served through site-intensive methods is beyond the scope of this essay, but they include collecting data that can be quantified and subjected to numerical analysis, gathering qualitative measures of the observable implications of hypotheses, collecting information that sheds light on causal processes, checking the plausibility and validity of theoretical claims, and refining analytic categories for use with other methods.

Relatedly, explain in straightforward terms the circumstances under which SIM is particularly valuable—indeed,

necessary—for political research. One way to think about this is that such methods are of particular value when what we’re studying is subtle (for example, relationships, networks, identities, styles, beliefs, or modes),⁸ and when what we’re studying is sensitive, hidden or otherwise kept behind barriers that require building trust or otherwise unlocking access. Scott’s and Fenno’s projects serve again as examples. In both books, the fundamental subject of study was individuals’ *perceptions* (peasants’ views of class relations and politicians’ views of their constituents). These perceptions are both subtle and, most of the time, hidden. This fact made forms of research like surveys and short interviews unworkable and necessitated strategies involving trust-building and over-time observation..

Discuss how best to teach these methods. To some, ethnography and the like may appear virtually unteachable. It may seem that each individual can learn how to do these things only by doing it him- or herself, and that the practical problems faced by students in their specific research sites will be virtually unique and impossible to prepare for in advance. But I believe this position is far too pedagogically pessimistic. It also understates the extent to which there is a set of coherent principles and techniques that underlie (even if sometimes implicitly) what ethnographers and participant observers do. This, in turn, reinforces marginality. Certainly, my colleagues and I who have co-taught the APSA short course on field research methods feel that reflecting on these methods has made us better and more efficient at using them ourselves.⁹

Develop principles for managing the trade-offs involved in site-based research. How many sites should be developed in a given project? (Would *Weapons* have been more compelling or less compelling if it had involved multiple village sites? What if Fenno had tracked just four politicians, but spent months with each instead of days?) What is the minimum, or optimal, degree of access required to obtain valid information for a particular research effort?

Identify the most fruitful ways of integrating site-intensive work into multi-method studies. In my own work on local associations in China and Taiwan, for example, I have adopted an approach that pairs the deeper insights from participant-observation and interviews with thinner but more extensive survey data.

Address the topics raised by Morris MacLean in her contribution to this issue: how not merely to obtain adequate informed consent from human subjects but possibly engage them in more active and participatory ways. Similarly, explore the particular problems discussed by Cammett here about presenting oneself and building access in divided or otherwise challenging settings.

Create a bibliography, accessible online, of work in political science that uses site-intensive methods.

It is at least as exciting to see the continued emergence of new work in this category as it is to reread classics of the field. Just as the organization of the broader Qualitative Methods section itself has been an immensely constructive step, I would urge the subset of those interested in ethnography and participant observation to think collectively about ways to pro-

mote these invaluable and closely related approaches to learning about the political world.

Notes

¹ The symposium in the Spring 2006 issue of this newsletter, “Ethnography Meets Rational Choice,” is a worthwhile example.

² <http://www.asu.edu/clas/polisci/cqrm/IQRM2006syllabus.html>

³ See Posner (2004) and Cammett’s contribution to this newsletter.

⁴ Marcus (1998), for example, provides arguments in support of multi-sited studies, albeit with reservations.

⁵ He also discusses his methods in a 1986 *APSR* article and other essays, all reprinted in Fenno (1990).

⁶ One could go on about the contrasts. Scott is, of course, particularly attuned to the voices and experiences of the subaltern, while Fenno does not conceal an often admiring sympathy for the elites that he studies.

⁷ Perhaps this category could simply be called E/PO, but at risk of confusion with the hormone erythropoietin, central to recent doping scandals in sports.

⁸ And most things I can think of in the realm of politics contain subtleties that probably deserve up-close scrutiny. At the same time, this is not at all to deny that less-intensive methods can also shed light on these topics.

⁹ To be sure, site-intensive methods are just one component of what has been taught in this course.

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The Power of Human Subjects and the Politics of Informed Consent

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One of the key challenges and rewards of doing ethnographic or "site-intensive" fieldwork is negotiating our relationship with the individuals, groups and societies we study during the process of this in-depth, close-up, on-the-ground research. Before we gain entrée as observers, and frequently participants, in the everyday lives of individuals and communities, we must consider how to minimize the risks and maximize the benefits of our research. Drawing on methodological discussions ongoing in other disciplines, I urge a reconsideration of the process of informed consent, less as an externally-imposed, bureaucratically-mandated hurdle, and more as the initial step in a more collaborative approach to research where subjects have a great deal more power.

Most of us are aware of the notorious studies from the past where the risks and benefits to human subjects were barely considered, if at all, and participants suffered a grave psychological or physical toll. Perhaps the most egregious example is of course the Tuskegee Syphilis study (1932-1972). During this study, power differentials based on race and class led the U.S. Public Health Service to continue to observe the progression of syphilis in impoverished, African-American men, despite an accepted, known cure for the disease. The cost to human subjects in this case was huge and blatantly obvious: the majority of the study participants needlessly suffered, died, and/or infected their families with the disease.

As a direct result of the media exposé of the injustices at Tuskegee, the federal government, since the late 1970s, has required all researchers to comply with federal regulations protecting human subjects. We generally accept and view these overarching principles as legitimate. But we have also experienced, personally and/or vicariously, numerous horror stories of impractical—nay impossible!—demands of well-intentioned but unyielding university human subjects committees who seem to have little understanding of the social sciences and no inkling of our particular field site contexts. Many scholars tend to think of this process of obtaining human subjects clearance from our universities and informed consent from our subjects as a bureaucratic hurdle to be overcome before the "real work" begins.

I would like to propose an alternative, perhaps somewhat controversial, viewpoint. Informed consent is more than managing to persuade our campus human subjects committees to approve a form, or a one-shot interaction in the field trying to get a respondent to participate in your survey. Instead, we should think much earlier and more broadly about how to create and communicate real benefits from our work for the affected individuals and communities. While this proposal may seem radical in political science, a growing number of scholars from a variety of other disciplines have already highlighted how a more participatory approach to research can produce improved outcomes for all involved (Kelly 2005, Kidd and Kral 2005, Dreze 2002, Chataway 2001, Reason and Bradbury 2001, Fals-Borda 1997, Park 1992). The idea is that research is a partnership between the investigators and the subjects studied. Researchers need the local knowledge and information possessed by individuals and communities, and communities may benefit from the knowledge and insight provided by the researcher. Together, they collaborate to identify the most salient research puzzles, and the most appropriate and efficient way of investigating them. In this mode, the process of informed consent begins at the initial stage of question formulation and continues through the implementation of data collection, analysis, and outreach.

By resisting the "superpower model of research" and allowing our human subjects the power to speak prior to the first interview, the projects we conduct will be more relevant to the individuals and communities we study. This model not only has some normative value in being fundamentally more democratic, but the voluntary and potentially more enthusiastic involvement of the study participants will undoubtedly improve

the internal validity of the data collected.

Some scholars have moved in the direction of this more collaborative approach without recognizing it when they conduct open-ended interviews or focus groups at the very beginning stages of a new project to help refine the research question to be examined. I did these sorts of preliminary investigations on a pre-dissertation fieldtrip to Ghana and Cote d'Ivoire in 1996. By listening to village residents and local government officials, I was able to narrow my topic from a broad interest in economic liberalization to a more targeted set of issues around the retrenchment of publicly provided social services in health and education. As a result of these initial contacts and refinement of my question to a priority interest in the community, my respondents were eager to participate in lengthy interviews when I returned. In addition to a 97.5% response rate, I had more confidence in the content of the data.

In my second project working with federal, state, and American Indian tribal government officials, my consultations with the various subject populations took place even earlier and were even more systematic. Instead of using feedback to narrow my own pre-determined question, I sought input before any question had been formulated. In according my subjects more power to identify what was substantively important to them, I have been able to gain entrée into multiple communities that are historically hostile to outside researchers and to each other. I cannot count the number of times that respondents have expressed their desire to have their voices heard because they felt the topic was so important. Also, despite the fact that the majority of my respondents were relatively underpaid and overworked civil servants for their respective government agencies, they almost all have spent considerably more time than initially requested providing detailed, in-depth responses to my questions. This process and my project were not atheoretical; it was simply that the communities helped identify the problem in need of theorizing.

While I am arguing for a deeper, more interactive notion of informed consent, we are still confronted with the concrete requirements of formal human subjects committees or Institutional Review Boards (IRBs). The rest of this symposium contribution is less conceptual and more pragmatic. Below, I will explore how this alternative conceptualization of informed consent might shape our everyday actions in the field. Before we continue, it is critical to understand that universities are not the only ones with IRBs. Many organizations, communities and nations have their own institutions that review and approve research studies. For instance, my study on the politics of American Indian representation in policymaking has to date required IRB approval from one university, one federal agency, two regional areas of the same federal agency, and ten tribal nations. Each of these IRBs has its own rules and procedures. We should obviously make sure that our studies are being reviewed by each of the necessary "official" IRBs in the most appropriate order. This latter point involves recognizing the potential politics involved in the sequencing of different IRB approvals. While admittedly time-consuming, my experience has been that the feedback I have received from the multiple IRB processes has only strengthened the quality of my study.

Through these ongoing IRB experiences, I have built trust with future respondents; factual inaccuracies have been highlighted before they were presented publicly; and alternate interpretations of my data have been shared.

Much of the recent writing on informed consent focuses on whether subjects truly understand the information given to them about the research project. While many IRBs stress the need to convey information at a relatively low and thus more universally accessible grade level, Hochhauser (2005) emphasizes the sheer quantity of information given to subjects. He finds that most informed consent forms give too much detailed information, creating memory overload and the impossibility of truly informed consent. Cumming et. al. (2006) emphasize how the power relationship must be shifted to a two-way flow of information to ensure participant understanding rather than a top-down delivery of information. This expands the power of the individual involved in the study from being a passive and subservient "subject" to an active participant in the process. Gold (2002) similarly emphasizes how study participants may not understand how their data may be used or become public, especially if it is part of a larger cross-national comparative project.

All of the above suggests that individuals might need more time than is typically granted to read and digest the information provided in the informed consent form. Rather than being seen as a perfunctory signature or nod of the head prior to beginning the "real work," researchers might give potential participants the form ahead of time, either in person or by sending via mail, e-mail, or fax. We might also incorporate active learning strategies from our teaching, and use active questioning to inquire whether and how the participant has processed the information. Many researchers tend to rush through the actual process of informed consent because they fear the consequences of participant refusal or rejection. But rather than jeopardizing the study, allowing time for discussion and questions can actually strengthen the rigor of the data collection process by building trust early on.

Another strategy that can help individuals understand the process of informed consent more meaningfully is if the project has been introduced publicly to the broader community. In many developing countries, no individual-level interactions can proceed until a village- or neighborhood-wide public meeting has been held where the researcher openly introduces the research team and the study objectives. Even after such a public meeting was followed by the village gong-gong beater passing "the news" at 5 a.m., my research team and I still had to dispel many myths and misunderstandings as we went door-to-door greeting and introducing ourselves (and mapping the village to do a sample). While this kind of face-to-face meeting may not always be logistically feasible in larger-scale or advanced industrialized contexts, there are often large public meetings organized for other purposes where a researcher may request the opportunity to make a brief introduction.

Since discussions of informed consent often focus on the risks of participation in a study, there is less talk about the benefits. I have argued that a more collaborative approach to

research would help promote research that would be of greater salience to participating individuals and communities. This is a way of fostering long-term benefits. But what about benefits to individual participants in the short-term? IRBs will often ask about compensation for research participants, but it is rarely, if ever, mentioned in the methodological literature in political science. Compensation is one of the most obvious ways that we, as researchers, have more power than our subjects and may create new inequalities through our actions. Even if we offer a small sachet of sugar or soap in return for the time spent in an interview, jealousies and tensions might be aroused between those individuals or communities selected to participate and those not chosen. Several researchers have described their success in taking photographs of participants as a gesture of appreciation (Chacko 2004, Price 2001) or even as an object of discussion and component of the research (Deutsch 2004). In her more autobiographical work, Gottlieb (1994) discusses how she gave back to the village communities where she has conducted long-term fieldwork by donating her book royalties toward a community development project.

Many researchers finesse this issue of compensation and simply do not consider it, arguing that it is not financially possible due to their limited resources. But there are non-monetary means of thanking or giving back to our respondents that are important to examine. First, researchers often have expertise in their subject area and beyond that may be of value to the participants. Some researchers have tutored participants' children in English or other subjects. Others have shared information about government policies and regulations that is free and publicly available but often unknown in rural or remote areas. For example, I was able to tell many Ghanaian families about the availability of free social services for the elderly or indigent at the district hospital. Many researchers have provided information about both the opportunities and constraints for traveling and studying abroad in the U.S.

Second, we can share personal information about ourselves. One way of leveling the playing field with participants is by allowing them to ask questions of us. Again, political scientists need to be more conscious of our power in the process of conducting research. Sharing power by being open and responding when appropriate to personal, or even (how dare they?!) sensitive questions can not only improve our human experience of research but also again improve the ultimate validity of the data we collect. In conclusion, I argue that we should start the process of informed consent much earlier than a few minutes before an interview is scheduled to begin. The challenges and rewards of maximizing the benefits and minimizing the risks for our human subjects is perhaps more obvious while doing ethnographic field research, where we as researchers come into repeated, more long-term, and sometimes intimate contact with our respondents. But the merits of such an approach apply to social science research of all types of design and methodology. If we begin to consciously recalibrate the power differences between ourselves and our human subjects, we may not only improve the quality of our data collection and analysis but also invigorate our own everyday lives with a new level of democracy.

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Political Ethnography in Deeply Divided Societies

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Many projects based on field research or "site-intensive methods"¹ in political science incorporate sensitive components that can hinder the researcher's capacity to gain and maintain access to sources and informants. But doing field research arguably presents particularly acute challenges in deeply divided societies, where religious, ethnic, regional, or

other types of cleavages are highly politicized and may even constitute the ostensible basis of political violence.² I encountered these challenges firsthand while carrying out field research on the relationship between faith-based social service provision and the formation of sectarian identities in Lebanon. A textbook case of a deeply divided society, Lebanon has eighteen officially recognized religious groups, and the political system institutionalizes divisions within and across the Muslim and Christian communities by allocating top leadership posts (and even civil service positions) according to sect.

This contribution to this symposium on field research describes some problems that may arise when working in a divided political context and suggests strategies for addressing them. I focus on several practical issues, including the presentation of the project and the researcher to potential informants and institutional “gatekeepers,”³ gaining access to interviewees, working with local translators and research assistants, affiliating with local institutions, and collaborating with local scholars. This is by no means an exhaustive list of potential challenges nor are the solutions suggested meant to exclude alternative approaches. Many of my observations also apply to research on politically sensitive questions in less overtly divided political settings and to conducting research in places where the enterprise of scholarly research is suspect.⁴

In any context, the initial presentation of both the research project and the researcher is critical. In divided societies, introductions are all the more crucial because potential informants are especially sensitive to how researchers will situate their group vis-à-vis others in a highly contested political space. Presentation of the research often begins well before face-to-face contact with informants or institutional representatives in email messages or letters sent from home institutions. At this stage, establishing the relative impartiality of the researcher and the research goals is key, whenever possible.⁵ In deeply divided societies, informants may view members of other groups, such as non co-religionists or non-co-partisans, as well as foreign researchers, in extreme terms: either as opponents or potential converts. Framing the project in neutral language helps to dispel expectations of hostility or sympathy, both of which can undermine the validity of interview data and the rapport needed to carry out interviews. A technical rather than overtly political description of the research goals can bolster efforts to establish neutrality. For example, presenting my research in terms of how people gain access to healthcare, schooling, or social assistance rather than how they become supporters of a sectarian organization emphasizes a more neutral yet nonetheless central aspect of my project.

Still, an image of neutrality is not always convincing. This is all the more true in divided societies in which foreign actors—such as the US, UN, EU or NATO—have played direct roles in conflict resolution (or even conflict initiation) processes. Researchers from some foreign countries, then, may face the added challenge of presenting themselves as independent of their governments. In most Middle Eastern countries, for example, it is critical for US-based researchers to stress their non-affiliation with governmental or quasi-governmental agencies be-

cause of widespread antipathy towards US policy in the region.

In divided societies, informants may view an interview as an opportunity not only to tell their side of a story but also to convince the interviewer of their perspective or to disparage other groups. The researcher may be tempted to express real (or even feigned) empathy or support to induce interviewees to share more information or facilitate access to data sources. Showing open support for an informant’s views, however, can be problematic on at least two grounds. First, it can bias responses and, second, it can backfire: word spreads—even across what appear to be rigid communal boundaries—and, therefore, it is important to be consistent in self-presentation across diverse groups.

Gaining access to interviewees is often a function of who you know, even for seemingly innocuous research questions, and the challenges are all the more acute in deeply divided societies. In political science, we often hear that “snowball sampling” (Birnacki and Waldorf 1981), or referral chains, limit generalizability and bias samples because access to interviewees is dependent on existing networks with potentially limited variance. For research on sensitive issues or in places with deep, politicized cleavages, personal referrals often provide the only entrée to respondents. Indeed, some organizations and communities are organized so hierarchically that lower-level representatives may refuse to meet with researchers without approval from above.

Researchers can initiate and pursue referral chains by asking respondents for contacts; following up on referrals mentioned during meetings; pursuing potential leads through existing colleagues; friends and acquaintances; contacting high-level representatives or public relations officers of relevant organizations and associations to solicit referrals; and perhaps even by “cold calling” interviewees. (Cold calling is often less effective given that informants are more likely to agree to be interviewed if they have assurances about the background and trustworthiness of the researcher, which is the main advantage of working through referral chains.) Depending on the topic, different referral chains may start to overlap as the research progresses. In societies with deep, politically salient cleavages, however, communal divisions often entrench separate social networks, requiring researchers to pursue multiple, parallel referral chains. (The same might hold true for research on highly polarizing issues.)

Once interviews are scheduled, concerns arise about how to pose tough questions and extract sensitive information. With a project involving research in multiple, mutually antagonistic communities, informants may be suspicious about where the interviewer’s sympathies lie and, most importantly, what will be done with information provided during the interview. Posing personal or highly sensitive questions towards the end of an interview, when a rapport is (hopefully) established between the interviewer and interviewee, increases the chances of gaining meaningful answers and of conducting a smooth interview (“Symposium” 2002).

Other choices about how to conduct the interview can improve the quality of the interaction and, hence, data col-

lected. The plusses and minuses of recording or taking notes during the interview should be considered carefully. While audio or written records of the meeting inevitably provide more complete accounts, these methods of recording responses may deter meaningful responses in the first place. Emphasizing respect for the informant's right to confidentiality or anonymity and clarifying how the informant authorizes the researcher to use the information obtained are critical to gaining and maintaining trust. When appropriate, researchers can describe specific precautions to protect the informant's identity.⁶ Finally, in a polarized society, where members of distinct communities may express extreme antipathy for one another, the researcher may find herself in uncomfortable situations. Informants may articulate racist or other offensive views whose expression would be unacceptable in the researcher's own country, institution, or community. This poses an ethical dilemma: should a researcher defend her core, moral principles by objecting to the articulation of such views, or should she repress the urge to differ in the interest of facilitating the interview and collecting information? This is obviously a personal choice, but one that should be considered in advance.

Decisions about hiring research assistants or interpreters, affiliating with local institutions, and collaborating with local scholars pose important and obvious dilemmas when conducting research in divided societies. Informants inevitably make judgments about the intentions and goals of foreign scholars based on the local institutions and individuals with whom a researcher works closely. Deep, local knowledge benefits all political science research but is essential—and not just desirable—for research in deeply divided societies. Selecting who will accompany you to interviews as an interpreter or research assistant and who will collect data on your behalf at local research institutions and government agencies requires a historically grounded understanding of local perceptions about salient political cleavages. A research assistant or translator who comes from a family, or religious or ethnic group or who has a political affiliation that is locally regarded as relatively neutral—or at least not heavily implicated in historical episodes of conflict—is often preferable.⁷ In some instances, it may be more appropriate not to have a research assistant attend meetings because her presence would overly bias or inhibit responses. Similarly, choices about collaboration and affiliation with local scholars and institutions shape a researcher's local reputation. Again, making informed decisions requires deep knowledge of local political cleavages and entrée into relevant social and professional networks.⁸

My experience in selecting a survey research firm in Lebanon raises further issues specific to working in deeply divided societies above and beyond the well-known risks of hiring a contractor to execute a survey. While interviewing the directors of several highly recommended survey research firms in Lebanon to assess their technical and logistical capabilities, a colleague who is a well-known specialist on Lebanese politics cautioned me against working with one firm because of its supposed political alignment. After meeting with the director of another firm, others warned me of its close ties to both the US government and a Maronite Christian political

movement. Regardless of the soundness of my findings, the alleged political affiliation of whatever firm I selected would inescapably color local perceptions of my research. Still, despite the risk of reduced control over the data collection process, contracting is often essential. The sheer scope of the task at hand in terms of the number and length of interviews to be conducted as well as the geographic coverage of the survey may necessitate hiring a firm to collect the data. In addition, some local research firms can match enumerators with their own religious or ethnic communities of origin, which may elicit more extensive and reliable responses from interviewees. A foreign researcher or even enumerators from other communities in the same country are less likely to put respondents at ease.

Choosing a firm perceived as politically neutral may be impossible in a deeply divided political climate, and therefore, researchers must make choices about which audiences to target. If the relevant audience is primarily the international social science community, then competing, site-specific judgments about bias may be less consequential. Regardless of the target audience, how the findings are received locally matters because of the way they may be used to inform policy decisions and because of the researcher's local reputation, which affects current and future field research prospects. This makes it imperative for a researcher to closely supervise firms contracted to execute surveys, perhaps by building in checks and incentives to improve the reliability of the data collection process.

The division of state and societal power and resources—core concerns of most political science questions—has especially high stakes in divided societies, where group leaders promote and tap into fears about the well-being and even survival of their respective communities. As a result, research in such contexts requires approaches that help to overcome the suspicions of local institutional gatekeepers and informants, whether elites or “ordinary” citizens, before data collection can even begin. Site-intensive research is often essential when working in divided political contexts or on sensitive questions for at least two reasons: first, because it enables researchers to build local credibility and establish rapport, and second, because it facilitates deep knowledge of local patterns of polarization around social cleavages or issue areas. In deeply divided societies (or in research on highly sensitive questions), close relationships with field research sites are critical not just for data collection but also for data quality, the foundation of any good research project.

Notes

¹ See Read's contribution to this symposium.

² I emphasize “ostensible” given the broad consensus that ethnic, religious or other apparently identity-based political violence is hardly inevitable and involves some combination of political entrepreneurship and framing (Crawford 1976, Fearon and Laitin 2000).

³ By institutional gatekeepers, I am referring to individuals with high-level positions who control access to the research-related resources at institutions and organizations, including documents, quantitative data, appointments with interviewees, entrées into commu-

nities, and so forth.

⁴ Scholarly research may arouse suspicions in places that lack a field research tradition or in repressive political climates. Research projects can be deemed sensitive either because the research question itself addresses touchy subjects or, even when the topic is not especially taboo, the process of extracting information may encounter reluctant respondents who fear local recriminations or reputational loss. In addition, institutional “gatekeepers” may block access to critical documentation or informants to assert their own power, protect their positions, or attempt to extract compensation from researchers.

⁵ I use the term “relative” to recognize the virtual impossibility of absolute neutrality on the part of the interviewer (Gubrium 2002, Kvale and Kvale 1996, Rubin and Rubin 2005).

⁶ Such precautions may include coding interviews and maintaining a separate list of informants or refusing to reveal the names and other identifying information of informants.

⁷ Some local candidates for research assistantships or interpreter positions may regard themselves as out of the fray because of their personal political views, but others will invariably pigeonhole them. I experienced this while conducting field research in Lebanon, where my research assistant was a secular, Western-educated development consultant, yet my informants regarded her as a Sunni Muslim from a prominent Beirut family.

⁸ This can include to units *within* larger institutions, such as universities. For example, after several extended research trips to Lebanon, I found that the School of Public Health at the American University in Beirut not only housed impressive faculty, affiliated researchers, and students, but also enjoyed a solid reputation among NGOs, government agencies, and the scholarly community in Lebanon as scientifically rigorous and politically non-aligned.

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The Hermeneutic Foundations of Qualitative Research

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This article is the result of reflection that emerged while conducting qualitative field research on nationalism and exclusion in Portugal.¹ The problem I confronted was when to stop interviewing. Stated more precisely, I was seeking an answer to the question of when one has collected enough empirical data to support or reject one’s hypotheses. This initial problem led me to a rather old discussion on the difference between natural and human sciences that has characterized German academic life for many years—in fact, since the early 19th century—producing some more heated phases of academic dispute, known as the *Positivismusstreit* in the 1930s and the 1960s.

Hans Georg Gadamer’s *Truth and Method*, first published in German in 1960, stands out in this dispute as one of the major works to advance the discussion about the epistemology of human and social sciences. Gadamer makes an important contribution to the endeavor of providing a scientifically solid ground on which to make valid and reliable claims about the subject of their analysis: human action, transmitted or not in the form of written text. His main argument is that any human action and any utterance is meaningful and requires interpretation. The interpretation of a speech or a text must necessarily start from the concrete historical and cultural position of the researcher and on the cognitive and linguistic tools available to him or her. These, in turn, are determined by one’s own cultural, linguistic, and historical context and background. Understanding others therefore first requires understanding yourself. The more a researcher becomes aware of his or her own situatedness, the more (s)he can compare it to that of the researched other and initiate a process of systematic understanding via comparison and tentative overlapping of two different cognitive systems.

This process of understanding others is potentially endless, as one can expand the horizon of meaning further and further. Gadamer’s work does not offer a direct answer to the problem of when to stop collecting data; it does, however, allow for a deduction drawn from the more general arguments presented. I argue that one of the implications that follow from Gadamer’s hermeneutics is that integrating a clearly defined research design and a concrete research question into the interpretative process controls the potentially endless process of interpretation. In other words, although a full and complete understanding of others is impossible, the answering of a research question about a well-defined aspect of their lifeworld is not.

In order to elaborate these arguments, I will first lay out the intellectual and historical contours of *Truth and Method*. Secondly, I seek to explain Gadamer’s theory of the hermeneu-

tic circle, and thirdly, I will apply it to the question posed above, namely, when to stop interviewing.

Gadamer's Truth and Method

Georg Gadamer's *Truth and Method*, first published in 1960, was written in order to provide a solution to the problem of interpreting historical texts and to the broader problem of understanding historical utterances, a problem solved unsatisfactorily by Schleiermacher and Dilthey. Gadamer's work succeeds in providing an epistemological grounding for the interpretation of historical texts and speech.

As pointed out by Hans Albert during the German *Positivismusstreit*, the major weakness of the interpretative approaches that dwelled on the Schleiermacher tradition was that understanding of meaning expressed in speech or text required empathy. But empathy does not provide a solid enough grounding for reaching reliable and inter-subjectively verifiable generalizations. How then, can we reach a reliable interpretation of historic texts? This is the question Gadamer sets out to answer. By elaborating on this problem, Gadamer provides answers to many related problems, such as how to understand and interpret the utterances of others in the first place and how to proceed methodologically in order to reach a reliable interpretation of these utterances. I will first elaborate on these two questions.

How to Understand and Interpret the Utterances of Others

We are born into institutional settings that predate our entry. As human beings, we constantly have to learn and interpret the world, and more specifically the institutions that surround us, in order to understand and interpret correctly the meaning of actions and utterances with which we are confronted. In the case of my research on nationalism and minority rights in Portugal, it became clear that contemporary reality is the product of historical forces and as such the product of meaningful human interaction and struggle. Past relations of domination and control influence the ways individuals with different ethnic and religious backgrounds interact in contemporary Portugal. Portugal's ambivalent situation towards Europe is equally influenced by the century-old struggle for recognition and respect from the much richer and influential northern European states.² Historical continuities also emerged when analyzing the prevalent nationalist discourses in Portugal. An ongoing threat of early 20th century scientific positivism that had tried to demonstrate the "naturalness" of belonging of certain people to certain places, justified with reference to their essentialized cultural characteristics, lurked through many of the contemporary statements and discourses I collected about nationalism and the position of minorities.³ During my initial time in Portugal, I was unable to "place" the statements and interviews I recorded into their context of relevance. Although I understood all the words that my informants told me, their statements didn't make much sense to me, because I had yet to become familiar with their context.

To complicate matters even further, cognitive systems are closed systems (Luhmann 1985: 404 f), and it is by no

means self-evident how communication across different consciousnesses is even possible. Social interaction relies almost exclusively on language as a means to communicate, but language communication is inherently threatened by misunderstanding. The main reason for this is that utterances are made to convey *meaning*, and hence require interpretation. Chances for misinterpretation are accordingly high, first because language does not always overlap 100 percent with the intended meaning and secondly because perception is structured by the cognitive constitution, or consciousness, of the receiver, i.e., that any information is filtered, sorted out, and changed by the receiver. Some information might not be recognized as information at all and might simply not be processed, and some information will be changed in the process of perception in order to fit the available frame of reference and to "make sense." In my case, I had to first become familiar with Portugal's colonial history before I was able to correctly situate the statements of most of my informants. I also needed to become familiar with ways the Portuguese interact on a daily basis and with the taken-for-granted and normalized ways they reproduce their everyday reality in order to be able to reach a more adequate interpretation of the statements and behaviors I observed and recorded.

All these complications have led many social scientists to abstain from the reconstruction of such slippery concepts as motivation, meaning, intention, etc., and focus their efforts on observable and measurable outcomes on the one hand, and on one strong motivational pattern on the other, namely rational self-interest. This endeavor has produced some very strong and extremely useful theories able to explain much human behavior, but their very elegance is the cause for their heuristic limitation, as they do not allow for an assessment of human behavior if and when it is irrational and not, or not predominantly, motivated by the urge to maximize profits. In addition, a true understanding of meaningful human interaction cannot be achieved through these methods, and issues of causality and validity necessarily follow because we are left in the dark about such important factors in human action as motivation. Merely measuring actions and observable outcomes of social interaction therefore provides an "under-socialized" (to use the term coined by Mark Granovetter, 1985) view of meaningful social interaction. Furthermore, as Gadamer notes, "the individual case does not serve only to confirm a law from which practical predications can be made. Its ideal is rather to understand the phenomenon itself in its unique and historical concreteness" (Gadamer 1994:5).

The Problem of Objectivity

According to the widely accepted research guidelines provided by King, Keohane, and Verba (1994), all social inquiry must be reliable and replicable, and therefore guided by a systematic and transparent methodology. Such procedures should ensure that findings are verifiable or falsifiable by others. Although this seems a self-evident requirement, it bypasses a whole set of problems and complications, the most serious being the assumed neutrality of the researcher that is required in such an approach. When addressing this problem,

Gadamer dwells on Edmund Husserl, who argued that “The naiveté of talk about ‘objectivity’ which completely ignores experiencing, knowing subjectivity, subjectivity which performs real, concrete achievements, the naiveté of the scientist concerned with nature, with the world in general, who is blind to the fact that all the truths that he acquires as objective, and the objective world itself that is the substratum in his formulas is his own *life construct* [emphasis in the original] that has grown within him, is, of course, no longer possible, when *life* [emphasis in the original] comes on the scene” (quoted in Gadamer 1994: 249).

Gadamer himself argues that “the theme and object of research are actually constituted by the motivation of the inquiry. Hence historical research is carried along by historical movement of life itself and cannot be understood teleologically in terms of the object into which it is inquiring. Such an “object in itself” clearly does not exist at all. This is precisely what distinguishes the human sciences from the natural sciences” (Gadamer 1994: 285). Others, like Jürgen Habermas, have followed in this tradition, arguing that “everyday experience ... is, for its part, already symbolically structured and inaccessible to mere observation” (Habermas 1984: 110).

The criticism raised by these authors is that the researcher cannot escape from the social reality that he or she is analyzing. Being part of a symbolically structured world implies that one is not free in one’s relationship to this world and that the very perception of the world around us is influenced and structured by our belonging to it. The questions we ask and even the facts we perceive have been handed down to us by tradition and we cannot escape from its influence.

That, per se, is not a problem, according to Gadamer, as long as we are aware of our own cultural, linguistic, and historical, in addition to our racialized and gendered, backgrounds⁴: “All that is asked is that we remain open to the meaning of the other person or text. But this openness always includes our situating the other meaning in relation to the whole of our own meanings or ourselves in relation to it” (Gadamer 1994: 268). The worst would be to not be aware of one’s own background, not making it explicit in one’s account of a given analysis, and instead pretending that one stands above the observed reality, analyzing it “neutrally.” Such an attitude must necessarily lead to unreflected and undisclosed bias and distortion, and it leaves the reader without a means of taking the position and situatedness of the researcher into account.

The Positioning of the Researcher and the Fusion of Horizons

Several theoretical insights influence our ways of perceiving, understanding, and explaining reality. At the minimum, any researcher must assume a critical and self-reflective posture towards the reality of the study and towards oneself as a researcher. There is no escape from the fact that the researcher shares some of the categories and some patterns of perception, interpretation, and explanation with the subjects of her/his study. Furthermore, one’s own scientific activity is necessarily part of the ongoing process of reconstructing the world through meaningful interaction and potentially influences the

very outcomes one wants to observe and understand. The only way to achieve more reliable findings about any given social reality is to include one’s own historical, cultural, gendered, and racialized background and situatedness into the analysis and make it explicit to the audience of one’s work. This positioning of the researcher within the object of his or her research is driven by an understanding of the world as socially constructed through meaningful interaction.

Gadamer points out that understanding requires that the inquiring subject become self-aware of his or her traditions, backgrounds and institutionalized ways of seeing and interpreting the world. According to Gadamer, this means realizing “that the interpreter’s own thoughts too have gone into re-awakening the text’s meaning. In this the interpreter’s own horizon is decisive, yet not as a personal standpoint that he maintains or enforces, but more as an opinion and a possibility that one brings into play and puts at risk, and that helps one truly to make one’s own what the text says. I have described this above as a “fusion of horizons” (Gadamer 1994: 388).

Ultimately, then, understanding others requires self-understanding first, where self-understanding is a necessary, although not sufficient, condition to understanding others and their verbal or textual utterances. In the words of Gadamer: “*All such understanding is ultimately self-understanding* [emphasis in the original]. Even understanding an expression means, ultimately, not only immediately grasping what lies in the expression, but disclosing what is enclosed in it, so that one knows one’s way around in it. Thus it is true in every case that a person who understands, understands himself, projecting himself upon his possibilities” (Gadamer 1994: 260).

Accordingly, part of the task of understanding others is self-analysis. Concretely, the researcher must analyze his or her interpretative patterns and prejudices in an effort to become aware of them so that they can be considered and addressed, or at least integrated into the analysis as limiting factors. Gadamer explains: “Thus it is quite right for the interpreter not to approach the text directly, relying solely on the fore-meaning already available to him, but rather *explicitly to examine the legitimacy—i.e., the origin and validity—of the fore-meanings dwelling within him* [my emphasis]” (Gadamer 1994: 267).

In the case of trying to reach an adequate interpretation of the statements made by my informants in Portugal, I soon realized that being a white male of German nationality helped me gain initial insights from white Portuguese citizens who oftentimes counted on my benevolent understanding when making racist statements about Africans, Brazilians, and blacks (“pretos”). Such unrestrained talk allowed for important initial insights into the social and racial hierarchies of Portuguese society because it demonstrated how far everyday language had incorporated discriminatory value judgments. But being who I am also limited my access to the lifeworlds of excluded groups and my understanding of historical processes. It took me months to grasp the oftentimes very subtle references and value judgments conveyed in everyday speech and interaction, and even after a year of sharing the lifeworld of Lisboans, I still confronted situations where I did not know how to inter-

pret correctly what I saw or heard. Counterbalancing my status as an outsider was the fact that I speak Portuguese with a strong Brazilian accent, which goes back to several years spent in Brazil. As a result, I was perceived by most Portuguese as a Brazilian immigrant and as such I had the chance to share some typical “immigrant experiences,” such as being barred from entering certain bars and being attended badly in several shops, restaurants, libraries, and even by university personnel.

The more general questions arising from such experiences are if and how understanding is possible in the first place, and how correct and reliable interpretations can be achieved without falling into the highly speculative and voluntaristic statements that hard-minded scientists have been so eager to criticize. This is the question that Gadamer sets out to answer with his hermeneutics.

Hermeneutics

When analyzing a text or speech, we make use of the frames of reference that we have at our disposal, anticipating the meaning of the whole by reading or listening to a part. Whenever we seek to understand, we reach forward and come to more or less adequate conclusions about the meaning of an utterance. We cannot, and should not, avoid this method of advancing understanding through anticipation. What differentiates scientific understanding from popular understanding is that the first is systematic and less willing to readily accept the first conclusions reached. Scientific understanding, according to Gadamer, instead requires a going forth and back, a constant questioning of achieved conclusions, a suspension of already achieved insights until more information has corroborated the findings, and an openness to revise one’s findings in the light of contradicting evidence. Gadamer explains that “A person trying to understand a text is prepared for it to tell him something. That is why a hermeneutically trained consciousness must be, from the start, sensitive to the text’s alterity. But this kind of sensitivity involves neither “neutrality” with respect to content nor the extinction of one’s self, but the foregrounding and appropriation of one’s own fore-meanings and prejudices. The important thing is to be aware of one’s own bias, so that the text can present itself in all its otherness and thus assert its own truth against one’s own fore-meanings” (Gadamer 1994: 269).

Gadamer further explains that, “Within the concrete conditions of his own historical existence—not from some position suspended above things—he [the historian] sets himself the task of being fair. “This is his fairness, namely that he tries to understand” (§91). Hence Droysen’s formula for historical knowledge is “understanding through research” (§8). This process implies both an infinite mediation and an ultimate immediacy. Only in ceaseless research into the traditions, in opening up new sources and in ever new interpretations of them, does research move progressively toward the idea (Gadamer 1994: 215).

For Gadamer, “Methodologically conscious understanding will be concerned not merely to form anticipatory ideas, but to make them conscious, so as to check them and thus acquire right understanding from the things themselves”

(Gadamer 1994: 269). He quotes Heidegger to demonstrate that all understanding is anticipation, arguing that “Heidegger describes the circle in such a way that the understanding of the text remains permanently determined by the anticipatory movement of fore-understanding. The circle of whole and part is not dissolved in perfect understanding, but, on the contrary, is most fully realized. The circle, then, is not formal in nature. It is neither subjective nor objective, but describes understanding as the interplay of the movement of tradition and the movement of the interpreter. The anticipation of meaning that governs our understanding of a text is not an act of subjectivity, but proceeds from the commonality that binds us to the tradition. But this commonality is constantly being formed in our relation to tradition. Tradition is not simply a permanent precondition; rather, we produce it ourselves inasmuch as we understand, participate in the evolution of tradition, and hence further determine it ourselves” (Gadamer 1994: 293).

It is important to highlight that Gadamer does not demonstrate the impossibility of understanding others, nor does he advance a postmodern position, where any interpretation is as good and valid as any other. Instead, Gadamer seeks to demonstrate that there is indeed one correct way to interpret a given speech or text and that to reach this correct reading one must follow a systematic methodology. When addressing the question of how to avoid misinterpretations, Gadamer contends that “all that is asked is that we remain open to the meaning of the other person or text. But this openness always includes our situating the other meaning in relation to the whole of our own meanings or ourselves in relation to it” (Gadamer 1994: 268). The only limitation to this claim is that any interpretation is as historically situated as the text it sets out to understand and any interpretation is therefore open to different future interpretations and revisions, much in the tradition of scientific work as defined by Kuhn and Popper. In his hermeneutics, he instead lays out a way how to reach reliable interpretations of text and speech.

During my own research, my interpretation of the meaning of a statement changed as I became more familiar with the background and context of the speaker, allowing me to reach better and more accurate interpretations over time. A statement, such as “I am a Portuguese citizen but at the same time I am not a Portuguese citizen,”⁵ made by a black Portuguese, initially did not make much sense to me, but once I became more familiar with Portugal’s colonial history, I was able to understand what this interviewee tried to convey to me.

Method

As explained above, Gadamer’s basic insight is that any utterance is historically situated and cannot be understood without also understanding the historical context in which it is produced and by which it is constituted and made possible. Dwelling on the work of Edmund Husserl and Martin Heidegger, Gadamer argues that we approach any utterance, be it in the form of speech or text, by anticipating its meaning according to our own frame of reference, or what Luhmann calls our system-own creation of sense. When listening to others, we try to understand their meaning by categorizing

their utterances into our existing framework of sedimented patterns and stereotypes. In order to reach an understanding of the meaning as it was conveyed by the speaker, we must therefore start a process of not only understanding a single utterance, but the whole frame of reference of the speaker, her way of making sense, and her categorizations and stereotypes. We must, in the words of Gadamer, understand a person's lifeworld so that we can understand where a single utterance is coming from, where "life-world means the whole in which we live as historical creatures" (Gadamer 1994: 247).

In order to reach a valid understanding of the intended meaning of an utterance, one must therefore start a process of gathering information about the historical, political, and institutional context under which a particular utterance is made. The end-result of this process is reached when the researcher is able to understand and explain why the single utterance in question was made and how it relates to its specific context, or stated more precisely, when the researcher is able to grasp the lifeworld of the person who made the statement and place the single utterance into its context.⁶ Concretely, the researcher must engage in a process of moving back and forth between any single piece of information received and the context into which this piece is embedded. In the words of Gadamer, "the meaning of the part can be discovered only from the context—i.e., ultimately from the whole... Fundamentally, understanding is always a movement in this kind of circle, which is why the repeated return from the whole to the parts, and vice versa, is essential. Moreover, this circle is constantly expanding, since the concept of the whole is relative, and being integrated in ever larger contexts always effects the understanding of the individual part" (Gadamer 1994: 190).

By understanding another person's lifeworld we get familiar with it and make it our own, which is the only way of reaching an understanding of an external consciousness, as we cannot assume the identity of another person and perceive the world through her eyes and senses. The most we can do is reproduce a system of reference that overlaps enough with the other person's to allow us an understanding of her way of creating sense and interpreting the world.

Understanding requires that the inquiring subject becomes self-aware of his or her traditions, backgrounds and institutionalized ways of seeing and interpreting the world. According to Gadamer, this means realizing "that the interpreters own thoughts too have gone into re-awakening the text's meaning. In this the interpreter's own horizon is decisive, yet not as a personal standpoint that he maintains or enforces, but more as an opinion and a possibility that one brings into play and puts at risk, and that helps one truly to make one's own what the text says. I have described this above as a "fusion of horizons" (Gadamer 1994: 388).

Interpretation of texts and speech can therefore be analytically separated into two separate realms. On one side, the researcher has to analyze the text or speech by putting it into its historical and cultural context, by connecting the part to the whole, as it were. This procedure will lead to a gradual expansion of the horizon of understanding, allowing the researcher to gain more and more insight into the meaning of a given text

or speech. This movement is not to be confounded with reconstructing the original meaning of a text, as this would also be a futile and naïve endeavor, because no complete reconstruction of all the relevant factors will ever be possible, nor is it required. Instead, and this is where the second component of the analysis becomes relevant, by including one's own horizon into the analysis, one can reach a sufficient understanding of the text or speech in question that is grounded in one's own historical situatedness. "This is how I understand it now," and "my understanding is the most complete understanding possible" are the two statements that any researcher should aspire to reach.

The same is true for cultural distance. Cultural distance does not foreclose the possibility of understanding someone from another culture. The task of understanding him or her correctly does not also require "going native" and reconstructing the underpinnings on which current statements rest. This would be a naïve and futile endeavor. As human beings we all share a common ground of constructing our world in ways that are meaningful to us. The use of language is at the core of this possibility, as our meaningful constructions of reality rest on language as the main means of this construction, and language also constitutes the main vehicle of communicating across generations and cultures. Understanding requires, then, becoming aware of the differences in the historical construction of meaning, and it necessarily proceeds by way of comparing one's own cultural background with that of the object of inquiry. No understanding is possible without a conscious integration and explicitation of one's own background, and understanding of other cultures is reached by going back and forth between one's own interpretation of reality and facts and that of the examined other. In Gadamer's words, "To understand what a person says is, as we saw, to come to an understanding about the subject matter, not to get inside another person and relive his experiences" (Gadamer 1994: 383).

One problem arising from this conception is that, because the circle of relevance is constantly expanding, this process is potentially endless. This is indeed the case for the unstructured inquiries we make in our everyday lives, but this problem does not present itself in scientific research, which is theory driven. Theory restricts the domain of research and allows for a priori sorting out of relevant from irrelevant variables, without foreclosing the possibility to discover and integrate previously unknown variables, as long as this occurs in a structured manner. "Incompleteness of description is not a deficiency as long as the choice of descriptive expression is determined by a theoretical frame of reference" (Habermas 1988: 159).

Language

Language is the medium through which understanding can be reached: "For you understand a language by living in it—a statement that is true, as we know, not only of living but if dead languages as well. Thus the hermeneutical problem concerns not the correct mastery of language but coming to a proper understanding about a subject matter, which takes place in the medium of language... Thus we do not relate the other's

opinion to him but to our own opinions and views” (Gadamer 1994: 385).

Language bears the inherent possibility for such a process; as Habermas explains: “the idea of coming to rationally motivated, mutual understanding is to be found in the very structure of language, it is no mere demand of practical reason but is built into the reproduction of social life” (Habermas 1989: 96). Language allows us to become familiar with the lifeworlds of others. Familiarization means considering these lifeworlds by integrating them tentatively into our own frame of reference and way of making sense of the world. Lifeworld, as I use it here, is best defined as “represented by a culturally transmitted and linguistically organized stock of interpretive patterns” (Habermas 1989: 124).

Implication: When to Stop Interviewing

The hermeneutic circle provides an answer to the question of when to stop researching, and, as a corollary, when to stop interviewing. The premise for accepting this answer lies in recognizing that any scientific research must start with theory, as only a theoretical framework allows for the separation of realms for systematic inquiry. First, we have to define what we are interested in; then our theory will tell us what variables we have to look for and how we suppose they relate to the phenomenon in question. In other words, the hypotheses we formulate allow us to determine what is relevant to our inquiry. It is important to remember that theories cannot be true or false, but they can generate heuristic models that allow us to pose useful questions and formulate useful hypotheses about reality, by artificially separating one realm of reality from the totality of the social world. Theories reduce complexity in order to be able to make fruitful causal statements about reality. As Kenneth Waltz (1979: 5) has reminded us, theory is not equal to reality but a heuristic construct operating on a higher ontological level. Theory allows us to separate a realm of relevance and identify those variables we will consider in our inquiry.

Once we have separated a realm for our inquiry, we can start the process of gathering data, where speech acts are considered part of the data to be collected. In addition to collecting speech acts, we must contextualize this data with other information relevant to the speaker(s) so that we can reach an understanding of her lifeworld and situate her speech. This is achieved by going forward and back between the specific and the general, the concrete speech act and the political, historical, psychological, and, in general, institutional context in which the speaker and the speech are embedded.

This conception leads us to gather empirical data up to the point when each single new piece of information “makes sense,” i.e., it complements the logical structure of the lifeworld we are exploring. Each new interview must relate to and ultimately confirm what we already have found out, in a positive or negative way, and little by little we construct a contextualized understanding of the single speech act in question, which allows us to interpret each new piece of information and locate it within the horizon of meanings that constitute the context or lifeworld of the speech and the speaker and the realm we have separated for our inquiry.

If we come across information that “does not make sense,” i.e., utterances or data that cannot be explained by the frame of reference we have already elaborated, we are forced to revise this framework by gathering more information and amplify our perception of the lifeworld in question until each single piece of information “fits in” and can be explained by it. The hermeneutic circle closes when all the gathered pieces of information complement each other, forming a closed whole or, in the words of Gadamer, when the specific and the general complement each other and form a heuristic whole. Once this stage is achieved, each new utterance and new piece of information “makes sense” and fits into the already achieved understanding and interpretation of available data and its relationship to the lifeworld in question. Any new information can be explained and understood from within its context, even apparently deviant information. Once this point is reached, the process of interviewing is finished, as at this point in time new information would only confirm what has already been found and merely add unnecessary layers of validity to the findings.

In practical terms, interviewers will reach a point where all the new information they receive confirms the insights and explanations already achieved. One of the main understandings I gained when researching the ethnic bias and the exclusive construction of Portuguese nationhood was that Portuguese citizens of African descent were treated as foreigners in their everyday interactions with traditional, white Portuguese citizens. After some exploratory research, I was also able to formulate the hypothesis that the Portuguese state actively supported and disseminated the conception of nonwhite Portuguese citizens as foreigners by strategically financing those studies that dealt with foreigners and immigrants, while at the same time not supporting any research that dealt with ethnic minorities. The formulation of these hypotheses allowed me to collect data, in the form of interviews, to confirm or refute my arguments. I was then able to interpret correctly what an interviewee meant when he said, “I am a Portuguese citizen but at the same time I am not a Portuguese citizen.” Once I had reached this level of familiarity with the Portuguese way of collectively imagining an ethnically biased definition of nationhood, all the new information I was able to collect started to make sense and “fell into place,” i.e., it complemented the overall and more general explanation I had reached. At this point, my research could stop, because all the new evidence I collected only added additional layers of validity to my argument. The hermeneutic circle had closed.

Conclusion

One of the central insights flowing out of Gadamer’s work is that understanding requires that the researcher be part of the social reality he or she is trying to understand. Gadamer demonstrates that it is naïve to try to assume a neutral or objective position toward social facts. As human beings we are already born into a world that has been structured and invested with meaning. Even more, our actions constantly influence the world around us. Max Horkheimer explained, “the world which is given to the individual and which he must ac-

cept and take into account is, in its present and continuing form, a product of the activity of society as a whole. The objects we perceive in our surroundings—cities, villages, fields, and woods—bear the mark of having been worked on by man. It is not only in clothing and appearance, in outward form and emotional make-up that men are the product of history. Even the way they see and hear is inseparable from the social life process as it has evolved over millennia. The facts which our senses present to us are socially preformed in two ways: through the historical character of the object perceived and through the historical character of the perceiving organ. Both are not simply natural; they are shaped by human activity, and yet the individual perceives himself as receptive and passive in the act of perception” (Horkheimer 1972: 200).

Parting from this insight, Gadamer constructs a philosophical grounding of understanding that is rooted in interpretation. He then develops the rules which allow for a systematic and method-driven interpretation, namely, through following the ins and outs of a “hermeneutic circle.” I suggest in this article that the logic that drives this methodology of interpretation can be expanded and applied to the very practical question of when to stop interviewing. Accordingly, the short answer is “when the hermeneutic circle is closed and all the new and partial information obtained fits into the broader explanatory context, forming one coherent whole.”

Notes

- ¹ Reiter 2005.
- ² Léonard 2000, Vol. 4: 524 ff.
- ³ E.g., Gilroy 2000: 281 f.
- ⁴ As pointed out, e.g., by Sandra Harding (1993).
- ⁵ Interview conducted on 10 June 2003 in Lisbon. My translation.
- ⁶ The German *nachvollziehen* captures this process with more accuracy, as it implies a temporal dimension of “after acting,” different from imitation, but also more precise than “understanding.”

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In Memoriam: Alexander L. George, 1920—2006

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Alexander L. George died of a stroke on August 16 in Seattle. George was a towering figure in the international relations field, and he made pioneering and enduring contributions to case study methodology, foreign policy decision-making, political psychology, and the study of deterrence, coercive diplomacy, and crisis management. George was a former president of the International Studies Association, a key figure in the distinguished international relations faculty at Stanford University for over three decades, and a valued teacher and mentor of countless students at Stanford and throughout the discipline.

George was born in Chicago on May 31, 1920. He did his undergraduate and graduate work at the University of Chicago and received his PhD in Political Science in 1958. He served as a research analyst for the Federal Communications Commission during World War II and then as a civil affairs officer in Germany after the war. George was an analyst at the RAND Corporation from 1948 to 1968, and became director of its social science department. He moved to Stanford University in 1968, where he taught until he retired in 1990. He was emeritus at Stanford until 2006, when he moved to Seattle. George was professionally active until the very end. His short book *On Foreign Policy: Unfinished Business* was published a month before his death.

George’s first book, *Woodrow Wilson and Colonel House* (1956), written with his wife Juliette, had a major impact. It is widely regarded as one of the very best psychobiographies ever written, and is still in print after fifty years. George and George were concerned as much with the methodology of psychohistory as with the substance. They looked for pat-

terms in Wilson's interactions with others over the span of his life, rather than trying to reduce the former president's many achievements to unobservable neuroses and childhood traumas.

As a result of his work on decision-making and strategic interaction at RAND, George grew increasingly impatient with abstract theory and more interested in the tasks of constructing policy-relevant theory and making historical experience more relevant to the needs of policymakers. George wanted to open the "black box" of decision-making and examine the perceptions, interpretations, and calculations of political leaders. He believed that these variables could not easily be captured by large-N statistical studies, and that their analysis required instead the intensive study of individual cases. George thought, however, that existing case studies in international relations were atheoretical and lacking methodological rigor, and that they did not contribute to the development of theory or the cumulation of knowledge.

These considerations led George to develop the method of structured, focused comparison to combine the advantages of comparing numerous cases with the more intensive study of variables in individual cases. The method was *structured* in that the same questions were applied to each case and *focused* in that the case studies were selective in their treatment of theoretically relevant variables. George's overall goal, as he said repeatedly for three decades, was to convert descriptive narratives into analytic narratives. Another goal was to develop *contingent* generalizations that would identify the scope conditions for theoretical generalizations, thereby helping policymakers as well as theorists by facilitating the identification of the different conditions under which particular strategies were likely to be effective.

George applied these ideas to *Deterrence in American Foreign Policy* (1974), which he coauthored with Richard Smoke and which won the Bancroft prize for American History and Diplomacy. Substantively, George and Smoke rejected simplified models of deterrence and developed a more nuanced theory of deterrence, one that included a specification of a number of distinct paths through which deterrence might fail. Methodologically, in addition to demonstrating the utility of structured focused comparison, the book also highlighted the interplay of theory and evidence: the investigator began by specifying a theory, then tested the theory against the historical record through focused comparison, and then went back to modify the theory based on the evidence.

George's belief in the mutually beneficial relationship between theory and history is reflected in his collaboration with the historian Gordon Craig, both in a co-taught course at Stanford and in the book on *Force and Statecraft* that grew out of the course. This theme is also reflected in a number of edited volumes that brought together scholars with particular expertise in various historical cases. These volumes include *The Limits of Coercive Diplomacy* (1971, with David Hall and William Simons), *Managing U.S.-Soviet Rivalry: Problems of Crisis Prevention* (1983), *U.S.-Soviet Security Cooperation* (1988, with Philip Farley and Alexander Dallin), and *Avoiding War: Problems of Crisis Management* (1991).

Each of these volumes was guided by another theme, as well—the importance of developing theory that was useful for policy-makers. George became one of the leading proponents of policy-relevant theory, and developed his ideas more fully in *Bridging the Gap: Theory and Practice in Foreign Policy* (1993). George's commitment to policy-relevant theory was closely tied to his longstanding belief that statecraft informed by such theory could help promote a more peaceful world.

George continued to develop case study methodology in influential essays on "Case Studies and Theory Development" in 1979 and 1982 and in his annual graduate methodology seminar at Stanford. At the same time, he became more and more interested in judgment and decision-making. In 1969, George published a still-influential article in *ISQ* on the operational codes of political leaders. George began with Nathan Leites' analysis of the Bolshevik operational code, eliminated the psychoanalytic component and focused on cognitive dimensions, and generally tried to put operational code analysis on sounder social science footing. George argued that individual belief systems are anchored in philosophical beliefs about the nature of politics and conflict and in instrumental beliefs about the efficacy of alternative strategies for advancing one's interests. George's framework has served as the basis for many subsequent studies of the operational codes of political leaders.

The analysis of individual decision-making was further developed in George's *Presidential Decisionmaking in Foreign Policy: The Effective Use of Information and Advice*, which included an analysis of the political context within which decisions are made and an analysis of alternative management styles used by different American presidents. His "multiple advocacy" model (APSR, 1972) was an influential contribution to the literature on proposed correctives to pathologies in organizational decisionmaking.

This interest in individual and group decisionmaking fed back into George's ongoing efforts to develop case study methodology. He argued that the best way to study the impact of beliefs on foreign policy—and to assess their causal impact and rule out spurious inferences and alternative interpretations—was by tracing their impact on the various stages of the decision-making process. This led George to incorporate *process tracing* into his methodology.

George continued to develop the within-case method of process tracing, along with the congruence method and structured focused comparison more generally, in his decade-long collaboration with his former undergraduate student Andrew Bennett, which culminated in *Case Studies and Theory Development in the Social Sciences* (2005). This award-winning book refines George's earlier work, grounds it more thoroughly in the philosophy of social science, makes important advances in George's earlier ideas on "typological theory," argues for the complementarity of case study methods and quantitative and formal methods, and provides a useful "how to" manual for students. George also played a role in the development of the Consortium on Qualitative Research Methods and the training institute, and served as

the first president of the Consortium.

Among George's numerous awards were the MacArthur Prize (1983) and the National Academy of Sciences Award for Behavioral Research Relevant to the Prevention of Nuclear War (1997).

George was a valued colleague and unparalleled mentor. Many have benefited from George's detailed and incisive comments, always handwritten in his own inimitable (and microscopic) style. Many scholars have kept their earlier drafts just to reread George's comments.

George is survived by his wife Juliette, daughter May L. Douglass and son-in-law John Douglass, son Lee L. George, and grandchildren Julie and Ben.

The George family has suggested that memorial gifts be sent to the Alexander L. George Book Award Fund for an annual award presented by the International Society of Political Psychology (ISPP), or to the Camphill Communities California, where Lee George resides. Contributions to the book fund should be addressed to the Alexander George Fund, c/o ISPP at the Moynihan Institute of Global Affairs, 346 Eggers Hall, Syracuse University, Syracuse, NY, 13244. Contributions to Camphill Communities California may be sent to P.O. Box 221, Soquel, CA, 95073.

Griffin, Roger, Werner Loh, and Andreas Umland (eds.) 2006. *Fascism Past and Present, West and East: An International Debate on Concepts and Cases in the Comparative Study of the Extreme Right*. With an afterword by Walter Laqueur. Stuttgart, Germany: ibidem-Verlag. \$34.90 paperback.

Book Notes

Book descriptions are excerpted from publisher's web sites.

If you would like to recommend a book to be included in this section, email Joshua C. Yesnowitz, the assistant editor of QM, at jyesnow@bu.edu.

Ackerly, Brooke A., Maria Stern, and Jacqui True (eds.) 2006. *Feminist Methodologies for International Relations*. Cambridge: Cambridge University Press. \$29.99 paperback.

Why is feminist research carried out in international relations (IR)? What are the methodologies and methods that have been developed in order to carry out this research? *Feminist Methodologies for International Relations* offers students and scholars of IR, feminism, and global politics practical insight into the innovative methodologies and methods that have been developed—or adapted from other disciplinary contexts—in order to do feminist research for IR. Both timely and timeless, this volume makes a diverse range of feminist methodological reflections wholly accessible. Each of the twelve contributors discusses aspects of the relationships between ontology, epistemology, methodology, and method, and how they inform and shape their research. This important and original contribution to the field will both guide and stimulate new thinking.

Carpenter, Jeffrey P., Glenn W. Harrison, and John A. List (eds.) 2005. *Field Experiments in Economics*. Greenwich, CT: JAI Press. \$116.00 hardcover.

Experimental economists are leaving the reservation. They are recruiting subjects in the field rather than in the classroom, using field goods rather than induced valuations, and using field context rather than abstract terminology in instructions. This volume examines the methodology of field experiments, and offers a wide array of applications of field experiments. The methodological issues revolve around the ability of field experimenters to ensure the same degree of control that lab experimenters claim. The applications cover issues such as risk and time preferences of the Danish population, savings decisions of the Canadian working poor, differences between the social preferences of American students and workers, the effect of educational vouchers on American school children, and differences in bargaining behavior across nations. This volume serves as an introduction to the issues and applications of this new area of experimental economics.

In the opinion of some historians, the era of fascism ended with the deaths of Mussolini and Hitler in 1945; yet the academic debate about its nature is as far from resolution as ever. Besides, a number of developments since 1945 make it ever more desirable that politicians, journalists, lawyers, and the general public can turn to "experts" for a heuristically useful and broadly consensual definition of the term. The novel post-war phenomena include the emergence of a highly prolific European New Right, the rise of radical right populist parties, the flourishing of ultra-nationalist movements in the former Soviet empire, the radicalization of some currents of Islam and Hinduism into potent political forces, and the upsurge of religious terrorism. This book brings alive the intense controversy the topic has generated, while suggesting valuable heuristic strategies for resolving it. Twenty-nine academics, mostly German but including several prominent experts working in English, were invited by the journal *Erwaegen Wissen Ethik* to engage with an article by Roger Griffin, one of the most influential theorists in the study of generic fascism in the Anglophone world. The result is essential reading for all those who realize the need to provide the term "fascism" with theoretical rigor, analytical precision, and empirical content. The book will change the way in which historians and political scientists think about fascism, and make the discussion on the threat it poses to infant democracies like Russia more incisive not just for academics, but for politicians, journalists, and the wider public.

Gubrium, Jaber F. and James A. Holstein (eds.) 2002. *Handbook of Interview Research: Context and Method*. Thousand Oaks, CA: Sage. \$155.00 hardcover.

Interviewing has become the leading window on the world of experience for both researchers and professionals. But as familiar as interviewing is now, its seemingly straightforward methodology raises more questions than ever. What is the interviewer's image of those who are being interviewed? Who is the interviewer in the eyes of the respondent? From where do interviewers obtain questions and respondents get the answers that they communicate in interviews? How do the institutional auspices of interviewing shape interview data? Drawing upon leading experts from a wide range of disciplines to address these and related questions, the *Handbook of Interview Research* offers a comprehensive examination of the interview at the cutting edge of information technology in the context of a challenging postmodern environment. Encyclopedic in its breadth, the *Handbook* provides extensive discussions of the conceptual and methodological issues surrounding interview practice in relation to forms of

interviewing, new technology, diverse data gathering and analytic strategies, and the various ways interviewing relates to distinctive respondents. The *Handbook* is also a story that spins a particular tale that moves from the commonly recognized individual interview as an instrument for gathering data to reflections on the interview as an integral part of the information we gather about individuals and society.

Kennedy, Craig H. 2005. *Single-Case Designs for Educational Research*. Boston: Allyn & Bacon. \$56.80 cloth.

Single-Case Designs for Educational Research provides up-to-date, in-depth information about the use of single-case experimental designs in educational research across a range of educational settings and students. Each chapter reviews state-of-the-art content in a writing style that is accessible to graduate-level students. Examples of research methods are drawn from contemporary educational literature and address a range of student populations and educational issues.

Lawson, Tony. 2003. *Reorienting Economics*. London: Routledge. \$43.95 paperback.

In this wide-ranging, thought-provoking book, Tony Lawson further advances the basic thesis of his much-acclaimed *Economics and Reality* that social theorizing, and in particular economics, need to give a more explicit and systematic attention than hitherto to considering the nature of their subject matter. Formally put, the author continues his call for an ontological turn in social theorizing. Tony Lawson finds the discipline of economics to be in a none too-healthy condition, and demonstrates that the problems arise largely because of a widespread tendency of economists to make stipulations on methods independently of considerations of context or conditions of analysis. In addressing this situation the author argues for a radical reorientation of discipline to allow a more pluralistic forum, one that is accommodating of ontology and critical thinking among much else. The emphasis on pluralism is basic to Lawson's argument. Far from being a polemic against the currently dominant set of economic practices, this excellent book sustains the thesis that if economics is to be saved from itself, it is highly desirable to seek, where feasible, a continual dialogue between all interested parties.

The coverage of *Reorienting Economics* includes commentaries on the current state of economics, the nature of ontological theorizing (including the nature of its consequences), possibilities for social explanation, the scope of economics, evolutionary thinking, the contribution of heterodox traditions (including feminist economics), post-Keynesianism and old institutionalism, the historical process whereby economics came to take its current orientation, and much else. As with Lawson's previous writings, *Reorienting Economics* will be of interest not only to economists but also to philosophers, the variety of social theorists, and indeed anyone interested in understanding the current state of social theorizing and contemplating how it might be improved.

Schramm, Sanford F. and Brian Caterino (eds.) 2006. *Making Political Science Matter: Debating Knowledge, Research, and Method*. New York: New York University Press. \$24.00 paperback.

Making Political Science Matter brings together a number of prominent scholars to discuss the state of the field of Political Science. In particular, these scholars are interested in ways to reinvigorate the discipline by connecting it to present-day political struggles. Uniformly well written and steeped in a strong sense of history, the contributors consider such important topics as the usefulness of rational choice theory; the ethical limits of pluralism; the use (and

misuse) of empirical research in political science; the present-day divorce between political theory and empirical science; the connection between political science scholarship and political struggles, and the future of the discipline. This volume builds on the debate in the discipline over the significance of the work of Bent Flyvbjerg, whose book *Making Social Science Matter* has been characterized as a manifesto for the Perestroika Movement that has roiled the field in recent years.

Trachtenberg, Marc. 2006. *The Craft of International History: A Guide to Method*. Princeton: Princeton University Press. Paperback \$19.95

This is a practical guide to the historical study of international politics. The focus is on the nuts and bolts of historical research—that is, on how to use original sources, analyze and interpret historical works, and actually write a work of history. Two appendixes provide sources sure to be indispensable for anyone doing research in this area. The book does not simply lay down precepts. It presents examples drawn from the author's more than forty years' experience as a working historian. One important chapter, dealing with America's road to war in 1941, shows in unprecedented detail how an interpretation of a major historical issue can be developed. The aim throughout is to throw open the doors of the workshop so that young scholars, both historians and political scientists, can see the sort of thought processes the historian goes through before he or she puts anything on paper. Filled with valuable examples, this is a book anyone serious about conducting historical research will want to have on the bookshelf.

Tilly, Charles. 2006. *Why?* Princeton: Princeton University Press. Cloth: \$24.95.

Why? is a book about the explanations we give and how we give them—a fascinating look at the way the reasons we offer every day are dictated by, and help constitute, social relationships. Written in an easy-to-read style by distinguished social historian Charles Tilly, the book explores the manner in which people claim, establish, negotiate, repair, rework, or terminate relations with others through the reasons they give. Tilly examines a number of different types of reason giving. For example, he shows how an air traffic controller would explain the near miss of two aircraft in several different ways, depending upon the intended audience: for an acquaintance at a cocktail party, he might shrug it off by saying, "This happens all the time," or offer a chatty, colloquial rendition of what transpired; for a colleague at work, he would venture a longer, more technical explanation, and for a formal report for his division head, he would provide an exhaustive, detailed account.

Tilly demonstrates that reasons fall into four different categories: Convention, Narratives, Technical Cause-Effect Accounts, and Codes or workplace jargon. He illustrates his topic by showing how a variety of people gave reasons for the 9/11 attacks. Tilly also demonstrates how those who work with one sort of reason frequently convert it into another sort. For example, a doctor might understand an illness using the technical language of biochemistry, but explain it to his patient, who knows nothing of biochemistry, using conventions and stories. Replete with sparkling anecdotes about everyday social experiences (including the author's own), *Why?* makes the case for stories as one of the great human inventions.

Yanow, Dvora and Peregrine Schwartz-Shea (eds.) 2006. *Interpretation and Method: Empirical Research Methods and the Interpretive Turn*. Armonk, NY: M.E. Sharpe. Cloth \$89.95

This book demonstrates the relevance, rigor, and creativity of interpretive research methodologies for political science and its various subfields. Designed for use in a course on interpretive research methods, or as a second text in a course in which the instructor seeks a balance between positivist and interpretive approaches, the book situates methods questions within the context of broader methodological questions—specifically, the character of social realities and their know-ability. Exceptionally clear and well-written chapters provide engaging discussions of the methods of accessing, generating, and analyzing social science data, using methods ranging from reflexive historical analysis to critical ethnography. Reflecting on their own research experiences, the expert contributors offer an inside, applied perspective on how topics, evidence, and methods intertwine to produce knowledge in the social sciences.

Article Notes

Achen, Christopher H. 2005. "Let's Put Garbage-Can Regressions and Garbage-Can Probits Where They Belong." *Conflict Management and Peace Science* 22:4 (Winter), 327–339.

Many social scientists believe that dumping long lists of explanatory variables into linear regression, probit, logit, and other statistical equations will successfully "control" for the effects of auxiliary factors. Encouraged by convenient software and ever more powerful computing, researchers also believe that this conventional approach gives the true explanatory variables the best chance to emerge. The present paper argues that these beliefs are false, and that without intensive data analysis, linear regression models are likely to be inaccurate. Instead, a quite different and less mechanical research methodology is needed, one that integrates contemporary powerful statistical methods with deep substantive knowledge and classic data-analytic techniques of creative engagement with the data.

Bennett, Andrew and Colin Elman. 2006. "Complex Causal Relations and Case Study Methods: The Example of Path Dependence." *Political Analysis* 14:3 (Summer), 250-267.

This article discusses the application of qualitative methods in analyzing causal complexity. In particular, the essay reviews how process tracing and systematic case comparisons can address path-dependent explanations. The article unpacks the concept of path dependence and its component elements of causal possibility, contingency, closure of alternatives, and constraints to the current path. The article then reviews four strengths that case studies bring to the study of path dependence: offering a detailed and holistic analysis of sequences in historical cases, being suitable for the study of rare events, facilitating the search for omitted variables that might lie behind contingent events, and allowing for the study of interaction effects within one or a few cases.

Bennett, Andrew and Colin Elman. 2006. "Qualitative Research: Recent Developments in Case Study Methods." *Annual Review of Political Science* 9, 455-476.

This article surveys the extensive new literature that has brought about a renaissance of qualitative methods in political science over the past decade. It reviews this literature's focus on causal mechanisms and its emphasis on process tracing, a key form of within-case analysis, and it discusses the ways in which case-selection criteria in

qualitative research differ from those in statistical research. Next, the article assesses how process tracing and typological theorizing help address forms of complexity, such as path dependence and interaction effects. The article then addresses the method of fuzzy-set analysis. The article concludes with a call for greater attention to means of combining alternative methodological approaches in research projects.

Caren, Neal and Aaron Panofsky. 2005. "A Technique for Adding Temporality to Qualitative Comparative Analysis." *Sociological Methods & Research* 34:2 (November), 147–72.

As originally developed by Charles Ragin in *The Comparative Method* (1987), qualitative comparative analysis (QCA) has been used extensively by comparative and historical sociologists as an effective tool for analyzing data sets of medium-N populations. Like many other methods, however, QCA is atemporal and obscures the sequential nature of paths of causation. QCA ignores the order of events by treating combinations of attributes as though they occur simultaneously rather than as unfolding over time. While preserving the essential strengths of QCA, the authors present a modification that is capable of capturing the temporal nature of causal interactions. This modification involves a hybrid of Boolean algebra and sequence analysis to create a parsimonious set of solutions. This technique is referred to as temporal qualitative comparative analysis, or TQCA.

Chernoff, Fred. 2004. "The Study of Democratic Peace and Progress in International Relations." *International Studies Review* 6:1 (March), 49-78.

This essay argues that the field of international relations has exhibited "progress" of the sort found in the natural sciences. Several well-known accounts of "science" and "progress" are adumbrated; four offer positive accounts of progress (those of Peirce, Duhem, Popper, and Lakatos) and one evidences a negative assessment (Kuhn). Recent studies of the democratic peace—both supporting and opposing—are analyzed to show that they satisfy the terms of each of the definitions of scientific progress.

Cooper, Rachel. 2005. "Thought Experiments." *Metaphilosophy* 16:3 (April), 328-347.

This article seeks to explain how thought experiments work, and also the reasons why they can fail. It is divided into four sections. The first argues that thought experiments in philosophy and science should be treated together. The second examines existing accounts of thought experiments and shows why they are inadequate. The third proposes a better account of thought experiments. According to this account, a thought experimenter manipulates her worldview in accord with the "what if" questions posed by a thought experiment. When all necessary manipulations are carried through, the result is either a consistent model or a contradiction. If a consistent model is achieved, the thought experimenter can conclude that the scenario is possible; if a consistent model cannot be constructed, then the scenario is not possible. The fourth section of the article uses this account to shed light on the circumstances in which thought experiments fail.

Ebbinghaus, Bernhard. 2005. "When Less is More: Selection Problems in Large-N and Small-N Cross-national Comparisons." *International Sociology* 20:2 (June), 133-152.

The problem of case selection is a crucial but often overlooked issue in comparative cross-national research. The article discusses methodological shortcomings and potential solutions in selecting cases. All comparative research of social entities, whether quantitative or qualitative, faces the problem of contingency, the fact that the potential

pool of cases has been pre-selected by historical and political processes. In large-N cross-national studies, the use of inference statistics is problematic since random selection is rarely given and the cases represent a highly stratified set. In small-N case studies, however, the selection of cases is a deliberate choice based on the theory-driven comparative method. The epistemological and methodological problems of both comparative approaches are discussed and evaluated.

Elman, Colin. 2005. "Explanatory Typologies and Property Space in Qualitative Studies of International Politics." *International Organization* 59:2 (April), 293-326.

Explanatory typologies are powerful tools in the qualitative study of international politics. They are likely to be most valuable when scholars systematically apply shared techniques. This article provides an account of analytic steps used in working with typologies, and an accessible vocabulary to describe them. These analytic steps are illustrated with concrete examples drawn from prominent versions of offensive structural, defensive structural, and neoclassical realism. Five forms of cell compression—rescaling and indexing, as well as logical, empirical, and pragmatic compression—are considered, along with the drawbacks associated with each. The expansion of a partial typology and the rediscovery of deleted cells are also discussed. Finally, the article considers the potential drawbacks of a typological approach, and argues that scholars must be mindful of the risks of reification and of relabeling anomalies.

Gerring, John. 2005. "Causation: A Unified Framework for the Social Sciences." *Journal of Theoretical Politics* 17:2 (April), 163-198.

This paper offers four main arguments about the nature of causation in the social sciences. First, contrary to most recent work, I argue that there is a unitary conception of causation: a cause raises the probability of an event. This understanding of causation, borrowed from but not wedded to Bayesian inference, provides common semantic ground on which to base a reconstruction of causation. I argue, second, that rather than thinking about causation as a series of discrete types or distinct rules, we ought to reconceptualize this complex form of argument as a set of logical criteria applying to all arguments that are causal in nature (following the foregoing definition), across fields and across methods. Here, it is helpful to distinguish between the formal properties of a causal argument and the methods by which such an argument might be tested: the research design. Sixteen criteria apply to the former and seven criteria apply to the latter, as I show in the body of the paper. In summary, causation in the social sciences is both more diverse and more unified than has generally been recognized.

Gill, Christopher J, Lora Sabin, and Christopher H. Schmid. 2005. "Why Clinicians are Natural Bayesians." *British Medical Journal* 330 (May), 1080-1083.

Two main approaches are used to draw statistical inferences: frequentist and bayesian. Both are valid, although they differ methodologically and perhaps philosophically. However, the frequentist approach dominates the medical literature and is increasingly applied in clinical settings. This is ironic given that clinicians apply bayesian reasoning in framing and revising differential diagnoses without necessarily undergoing, or requiring, any formal training in bayesian statistics. To justify this assertion, this article will explain how bayesian reasoning is a natural part of clinical decision making, particularly as it pertains to the clinical history and physical examination, and how bayesian approaches are a powerful and intuitive approach to the differential diagnosis.

Hargrove, Erwin C. 2004. "History, Political Science and the Study of Leadership." *Polity* 36 (July), 579-592.

This essay addresses the interrelationship of individual skill, organizational structure, and political context and seeks to establish the possibilities of theoretical generalization concerning political leadership. Working at the interstice of what might be called "theoretical history," the author reflects upon his own previous contributions to the literature and those of his collaborators. These studies have dealt with an analysis of one institution, the Tennessee Valley Authority, over time; of a group of similarly placed officials whose careers spanned different organizations and/or contexts; and of presidents and prime ministers in the US and UK within a limited historical context. The author concludes by proposing additional research in the form of "small studies" across time and space that may yield new perspectives on possibilities for political leadership in the contemporary world rather than predictive political science.

Katz, Aaron, Matthias vom Hau, and James Mahoney. 2005. "Explaining the Great Reversal in Spanish America: Fuzzy-Set Versus Regression Analysis." *Sociological Methods & Research* 33:4 (May), 539-573.

This article evaluates the relative strengths and weaknesses of fuzzy-set analysis and regression analysis for explaining the "great reversal" in Spanish America. From 1750 to 1900, the most marginal colonial territories often became the region's wealthiest countries, whereas the most central colonial territories often became the region's poorest countries. To explain this reversal, five competing hypotheses are tested using both regression and fuzzy-set methods. The fuzzy-set analysis reaches substantively important conclusions, finding that strong liberal factions are probabilistically necessary for economic development and that dense indigenous populations are probabilistically necessary for social underdevelopment. By contrast, the regression analysis generates findings that are not meaningful. The article concludes that fuzzy-set analysis and regression analysis operate in different "causal universes" and that greater attention should be granted to the causal universe occupied by fuzzy-set analysis.

Pahre, Robert. 2005. "Formal Theory and Case-Study Methods in EU Studies." *European Union Politics* 6:1, 113-146.

EU studies have long been dominated by case studies, although formal theories have made substantial inroads into the field over the past decade. This article examines the advantages and challenges of combining these two methods, with an analysis grounded in both the philosophy of science and research design. Looking at the philosophy of science highlights several practices and principles shared by formal theorists and case-study researchers. The logic of discovery in both methodologies prizes studies of puzzles that can be found in a single case. Both are inclined to retain a research program even in the face of acknowledged anomalies. In the area of research design, many formal hypotheses take functional forms that cannot be reduced to linear, additive models. Most of these hypotheses lend themselves easily to case study methods. Thus, although research design has provided an avenue for applying the logic of statistical inference to qualitative research, case studies retain independent value.

Plumper, Thomas, Vera E. Troeger, and Philip Manow. 2005. "Panel Data Analysis in Comparative Politics: Linking Method to Theory." *European Journal of Political Research* 44:2 (March), 327-354.

Re-analyzing a study of Garrett and Mitchell ("Globalization, government spending and taxation in the OECD," *European Journal of Political Research* 39:2, 145-177), this article addresses four poten-

tial sources of problems in panel data analyses with a lagged dependent variable and period and unit dummies (the de facto Beck-Katz standard). These are: absorption of cross-sectional variance by unit dummies, absorption of time-series variance by the lagged dependent variable and period dummies, mis-specification of the lag structure, and neglect of parameter slope heterogeneity. Based on this discussion, we suggest substantial changes of the estimation approach and the estimated model. Employing our preferred methodological stance, we demonstrate that Garrett and Mitchell's findings are not robust. Instead, we show that partisan politics and socioeconomic factors such as aging and unemployment as expected by theorists have a strong impact on the time-series and cross-sectional variance in government spending.

Ragin, Charles. 2006. "Set Relations in Social Research: Evaluating Their Consistency and Coverage." *Political Analysis* 14:3 (Summer), 291-310.

Because of its inherently asymmetric nature, set-theoretic analysis offers many interesting contrasts with analysis based on correlations. Until recently, however, social scientists have been slow to embrace set-theoretic approaches. The perception was that this type of analysis is restricted to primitive, binary variables and that it has little or no tolerance for error. With the advent of "fuzzy" sets and the recognition that even rough set-theoretic relations are relevant to theory, these old barriers have crumbled. This paper advances the set-theoretic approach by presenting simple descriptive measures that can be used to evaluate set-theoretic relationships, especially relations between fuzzy sets. The first measure, "consistency," assesses the degree to which a subset relation has been approximated, whereas the second measure, "coverage," assesses the empirical relevance of a consistent subset. This paper demonstrates further that set-theoretic coverage can be partitioned in a manner somewhat analogous to the partitioning of explained variation in multiple regression analysis.

Schneider, Carsten Q. and Claudius Wagemann. 2006. "Reducing Complexity in Qualitative Comparative Analysis (QCA): Remote and Proximate Factors and the Consolidation of Democracy." *European Journal of Political Research* 45:5 (August), 751-786.

Comparative methods based on set-theoretic relationships such as "fuzzy set Qualitative Comparative Analysis" (fs/QCA) represent a useful tool for dealing with complex causal hypotheses in terms of necessary and sufficient conditions under the constraint of a medium-sized number of cases. However, real-world research situations might make the application of fs/QCA difficult in two respects—namely, the complexity of the results and the phenomenon of limited diversity. We suggest a two-step approach as one possibility to mitigate these problems. After introducing the difference between remote and proximate factors, the application of a two-step fs/QCA approach is demonstrated analyzing the causes of the consolidation of democracy. We find that different paths lead to consolidation, but all are characterized by a fit of the institutional mix chosen to the societal context in terms of power dispersion. Hence, we demonstrate that the application of fs/QCA in a two-step manner helps to formulate and test equifinal and conjunctural hypotheses in medium-N comparative analyses, and thus to contribute to an enhanced understanding of social phenomena.

Smith, Kevin. 2005. "Data Don't Matter? Academic Research and School Choice." *Perspectives on Politics* 3:2 (June), 285-299.

School choice has attracted much attention from academic policy analysts. Their data analyses, however, have become entangled in the politics surrounding debates over market-driven reform rather than serving as foundations for utilitarian policy making. High-profile research programs generate an increasing number of empirical studies, which are often valued for how well they fit ideological preferences rather than how useful they may be in offering practical guidance on workable and politically feasible strategies. In this paper I assess what supportable policy message(s) can be extracted from the large research literature on choice and vouchers.

Tickner, J. Ann. 2005. "What Is Your Research Program? Some Feminist Answers to International Relations Methodological Questions." *International Studies Quarterly* 49:1, 1-21.

Methodological issues have constituted some of the deepest sources of misunderstanding between International Relations (IR) feminists and IR theorists working in social scientific frameworks. IR theorists have called upon feminists to frame their research questions in terms of testable hypotheses. Feminists have responded that their research questions cannot be answered using social science explanatory frameworks. Deep epistemological divisions about the construction and purpose of knowledge make bridging these methodological divides difficult. These epistemological standards lead feminists to very different methodological perspectives. Asking different questions from those typically asked in IR, many IR feminists have drawn on ethnographic, narrative, cross-cultural, and other methods that are rarely taught to students of IR. Drawing on a range of interdisciplinary scholarship on feminist methodologies and some recent IR feminist case studies, this article analyzes and assesses how these methodological orientations are useful for understanding the gendering of international politics, the state and its security-seeking practices, and its effects on the lives of women and men.

Wedeen, Lisa. 2003. "Seeing Like a Citizen, Acting Like a State: Exemplary Events in Unified Yemen." *Society for Comparative Study of Society and History* 45:4 (October), 680-713.

This paper explores three counter-intuitive understandings of the relationships among state sovereignty, democracy, and nation-formation. First, whereas contested elections may require "strong" states and national coherence, other forms of democratic activity, such as widespread political activism and lively public debates, may exist because state institutions are fragile and affective connection to nation-ness only mildly constraining. As we shall see, the fragility of the state and the vibrancy of civic life mean that the regime's exercise of power is both blatant and intermittent. Second, common experiences of moral panic may be just as effective as, or even more capable than, state spectacles in generating a sense of passionate belonging to the imagined community of the nation. Third and relatedly, experiences of national belonging may actually be shared in the breach of state authority—in the moments when large numbers of people, unknown to each other, long for its protection. Or put differently, Yemen demonstrates how events of collective vulnerability can bring about episodic expressions of national identification. This essay is devoted to elaborating these arguments while narrating the events that bring them to the fore.

Weyland, Kurt. 2005. "Theories of Policy Diffusion: Lessons from Latin American Pension Reform." *World Politics* 57:2 (January), 262-295.

What accounts for the waves of policy diffusion that increasingly sweep across regions of the world? Why do many diverse countries adopt similar changes? Focusing on the spread of Chilean-style pension privatization in Latin America, this article assesses the relative merit of four theoretical explanations that scholars of diffusion have proposed. As the principal mechanism driving innovations' spread, these approaches emphasize external pressures, emanating especially from international financial institutions; the quest for symbolic or normative legitimacy; rational learning and cost-benefit calculation; and cognitive heuristics, respectively. The article assesses which one of these frameworks can best account for the three distinctive features of diffusion, namely, its wavelike temporal pattern, its geographical clustering, and the spread of similarity amid diversity. While several approaches contribute to understanding policy diffusion, the analysis suggests that the cognitive-psychological framework offers a particularly persuasive account of the spread of pension reform.

Woodruff, David M. 2005. "Commerce and Demolition in Tsarist and Soviet Russia: Lessons for Theories of Trade Politics and the Philosophy of Social Science." *Review of International Political Economy* 12:2 (May), 199-225.

Leaders of commodity-exporting states will sometimes push exports even when world prices are declining, if export receipts allow access to international capital markets. This article demonstrates that such state-mediated ties between commodity and capital markets shaped the politics of foreign trade in tsarist, and then Soviet, Russia. It also refutes an alternate, group-centered explanation of the same historical cases proposed in Rogowski's *Commerce and Coalitions*, pointing out serious empirical errors and oversights. These empirical problems have methodological roots. Searching for universal "laws," rather than sometimes-relevant "mechanisms," limits the consideration of counterhypotheses to those that apply to a whole universe of cases, rather than a subset of them. Because such counterhypotheses serve to determine which data are relevant, their exclusion weakens the empirical tests to which proposed laws are subjected. Thus, the ambition for generality may cause scholars to become inadvertently too generous to the theories they seek to test.

Announcements

2006 Giovanni Sartori Book Award

Recipient: Alexander L. George and Andrew Bennett, *Case Studies and Theory Development in the Social Sciences* (Cambridge, MA: MIT Press, 2005).

Committee: Gary Goertz, Univ. of Arizona (Chair); Tullia Falletti, Univ. of Pennsylvania; Aaron Schneider, Univ. of Sussex

Citation: *Case Studies and Theory Development in the Social Sciences* represents not only a synthesis of the work that George and Bennett have conducted over the years (and decades, in the case of George) but also a significant advance in the methodology of case studies. The volume covers a wide range of topics, from the role of case studies in theory development, to the techniques of process tracing, to the methodologies of typological analysis.

Chapters discuss the issues surrounding the advantages and disadvantages of process-tracing, along with a systematic discussion of how to apply this method and conduct case study research. In addition

to the historical-process dimension, the volume's chapters on typological theory show how cases can be incorporated into cross-sectional research.

Philosophy of science is extensively treated, as well. This is important because often courses take up the philosophy of science implicit in large-N statistical methods and explicitly defended by King, Keohane, and Verba. George and Bennett contend, instead, that process-tracing and typological theorizing are powerful methods for testing theories about causal mechanisms and for developing contingent generalizations about the conditions under which these mechanisms operate.

It is not surprising that the book has almost immediately upon publication become a standard in various research design and qualitative methods courses. The committee expects it to have a direct impact on the quality of qualitative research that appears in the coming years.

2006 Alexander L. George Article Award

Recipient: George Thomas, "What Dataset? The Qualitative Foundations of Law and Courts Scholarship." *Law and Courts* 16:1, 5-12.

Committee: Cecelia Lynch, Univ. of California, Irvine (Chair); Ted Hopf, Ohio State Univ.; Kenneth Shadlen, London School of Economics and Political Science

Citation: The committee examined a highly competitive field of 15 nominated articles. We chose this piece by George Thomas because it is a closely argued exposition of critical issues involved in case selection in the field of law and courts scholarship. It is also rich in methodological insights for any subfield. The article links the case "method to a systematic concern with research design," the goal of the Alexander George Award, in that it reviews a significant body of empirical work on law and the courts while moving forward our understanding of the relationship between theory and evidence, concept formation, and case construction, as well as selection and "test" evaluation. Thomas's piece is elegant and eloquent in pointing out how the empirical debates in the law and courts subfield are related to methodological biases and notions of "science."

Thomas directly addresses many of the central concerns involved in qualitative research. He uses recent research on a variety of substantive issues, including civil liberties and the role of the courts, to show how in-depth work that begins from well-informed (read: inductively formed) questions produces conceptual and theoretical breakthroughs in law and courts scholarship. For example, critical insights that have challenged the counter-majoritarian thesis could not be obtained without recent in-depth case research which demonstrates that Congress often "invites" the Courts to settle ambiguous language and, hence, policies.

Thomas uses these and other works to argue persuasively that beginning our research with one or a few well-constructed cases can be more productive than starting with a randomly constructed (and hence putatively "objective") dataset. While not ignoring the benefits of research based on such datasets, Thomas also provides evidence for his insistence that the qualitative/inductive method is equally "scientific." He makes the point, which bears repeating, that neither cases, concepts or theories are "prefabricated," that even quantitative researchers rarely begin from the entire universe of potential cases, and hence that the refinement of theory and prior theoretical and substantive knowledge are symbiotic components of the scientific enterprise.

2006 Sage Paper Award

Recipient: Giovanni Capoccia and R. Daniel Keleman, "The Study of Critical Junctures: Theory, Narrative, and Counterfactuals in Institutional Theory."

Committee: Audie Klotz (Chair), Syracuse Univ.; Lucan Way, Temple Univ.; Daniel Ziblatt, Harvard Univ.

Citation: Capoccia and Keleman address a concept widely—but loosely—used in qualitative research. In particular, path dependency arguments, a key element in historical institutionalism, are built upon the notion of a critical juncture. Yet too little attention has been paid to theoretical and methodological issues. First, Capoccia and Keleman question what makes the confluence of certain outcomes a "juncture," and then they ask why some junctures should be considered "critical." In response, they suggest guidelines, such as the need for counterfactual arguments to bolster analytical claims, the recognition of unsuccessful attempts at change as evidence of critical junctures, and the significance of levels of analysis for demarcating key factors and deci-

sions in the construction of historical narratives. Their illustrations from comparative politics and international relations should resonate with a wide readership. The committee anticipates that this nuanced paper, and its published version, will have considerable impact on theoretical arguments in historical institutionalist scholarship and the application of qualitative methods more generally.

The committee also recognized the valuable insights of four other papers (in alphabetical order): Thad Dunning, in "Improving Causal Inference: Strengths and Limits of Natural Experiments," brings attention to the potential for greater use of experimentation as a complement to qualitative and quantitative research. Both Mona Lena Krook, in "Comparing Methods for Studying Women in Politics: Statistical, Case Study, and Qualitative-Comparative Techniques," and Candice Ortobals, in "N = 5 to 20: A Challenge to Feminist Research?" explore the intersection of gender analysis and medium-n cases, with implications for research on many other issues. Paul Pierson, in "The Costs of Marginalization: Qualitative Methods in the Study of American Politics," offers a refreshing and compelling critique that raises the question of whether American political science is fundamentally blinded by its reliance on a particular set of methodologies.

Qualitative

Methods

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Qualitative Methods is edited by Gary Goertz (tel: 520-621-1346, fax: 520-621-5051, e-mail: ggoertz@u.arizona.edu). The assistant editor is Joshua C. Yesnowitz (e-mail: jcyesnow@bu.edu). Published with financial assistance from the Consortium for Qualitative Research Methods (CQRM) and Arizona State University's Institute for Social Science Research (ISSR). Opinions do not represent the official position of CQRM or ISSR. After a one-year lag, past issues will be available to the general public online, free of charge, at [http://www.asu.edu/clas/polisci/cqrm/Qualitative MethodsAPSA.html](http://www.asu.edu/clas/polisci/cqrm/Qualitative%20MethodsAPSA.html). Annual section dues are \$9.00. You may join the section online (<http://www.apsanet.org>) or by phone (202-483-2512). Changes of address take place automatically when members change their addresses with APSA. Please do not send change-of-address information to the newsletter.