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1 Introduction: critical comparisons

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Why compare?

The social sciences today are torn apart by a tension between two desires: to richly describe the world, showing its complexity and variability, and to robustly model the world, showing its relationships and regularities. We argue in this volume that engaging in comparisons of a few, well-understood cases reduces this tension. We offer, in effect, a case study of an encounter between two quite different disciplines, political science and anthropology. As students of society and culture, we found that we shared a stake in discovering processes and mechanisms underlying social phenomena, and that we found small-scale comparisons critical to that effort. And yet as participants in different disciplinary traditions, we continue to debate among ourselves about how best to compare, about how to interpret the patterns perceived, and about the ultimate goals of social research.

In a series of conferences and other exchanges, a collection of political scientists and anthropologists engaged in comparative study decided to put on the table what connected us and what divided us. Though a diverse lot – our objects of study run from ritual wailing to trade union disputes to agrarian transitions – we recognized in each other the dual commitment to understanding things both in their detail and in their general implications. We included no formal modelers or atheoretical monograph writers. All of us were engaged in comparative analysis of one sort or another, but some were also highly critical of much current comparative work.

We did, admittedly, approach our encounters with some fears – about disciplinary imperialisms, or about the Other's predilections for reductionism or mindless description. In truth, none of the worries have entirely been quashed, but they have been quelled, perhaps, as we have discovered that, yes, we do quite different things, and, indeed, that such is the point of the encounters. Here we wish to show and tell how these encounters ought to enrich comparative studies for social scientists generally as they have for us as a group.

In our discussions we noted a discordance between the richness of current comparative studies in our disciplines and the narrowness of how such work is described or prescribed in handbooks and review articles. Take two recent volumes (both discussed more fully below). King, Keohane, and Verba's *Designing Social Inquiry* (1994), a masterful prescriptive text in political science, delineates a set of requirements for valid "scientific inference" that effectively reads out of social science all comparative work designed to do anything other than test (or perhaps generate) causal hypotheses. By contrast, Holy's edited *Comparative Anthropology* (1987) argues that anthropological comparisons today are designed mainly to locate culturally specific meanings, and relegates to a "positivist" past all efforts to study social regularities. The gap between these two visions could be evidence that political scientists and anthropologists have absolutely nothing to say to one another – or it could be a sign that these summations are missing something of critical importance.

We began this project betting that the latter conclusion was the correct one, and we now think we were right. We prejudiced our experiment against finding agreement by bringing together, in the same room, political scientists whose work drew on rational choice theories with anthropologists whose work was highly concerned with the culturally specific. What we found we shared was a sense that the world's complexity demands some respect even as we try to understand or isolate processes and mechanisms.

This shared commitment to describing empirical richness and accounting for it has led us to critique and try to innovate beyond current ways of doing research in our own disciplines. For example, those of us who isolate a single set of motives or interests for modeling purposes (and only some of us do that) seek to retain in the analyses the specific processes and mechanisms characterizing each case. Sometimes doing so has required creating new ways of presenting material, as in the "analytical narrative" used by Margaret Levi and her colleagues. Conversely, our descriptions are shaped by efforts to understand processes and mechanisms – how and why things got to be the way they are. This effort, too, has required new ideas, as in the thesis advanced by Fredrik Barth and Greg Urban that variation both within and across cultural boundaries should be explained by reference to similar mechanisms. In both cases we are supplementing and critiquing standard images of what strategic models or cultural accounts can be.

It is, we argue, comparison that leads us to this critical use of our disciplinary tools – critical use, and not "application" of prefabricated tools (nor, for that matter, abandonment of social science). In this volume we show this more than tell it – we believe that exemplifying is more effective than prescribing – but we do also, here and in the other chapters, reflect explicitly on the value and limitations of particular kinds of comparisons. In design as well as in presentation, the volume is inductive, bottom-up, case-based, rather than deductive, prescriptive, law-giving. It offers the reader a set of examples to ponder, argue with, and perhaps draw from in planning comparative components for his or her own research.

Three concepts underlie these essays: comparisons, processes, and mechanisms. *Comparisons*, we argue, are at the heart of the matter for social science. We argue specifically for the value of controlled, or "small-n" comparisons of a few cases (or, as in David Laitin's chapter, a few sets of contrasting pairs). "Four plus or minus one" seems to capture what "a few" means in practice. Why comparisons, and why smallish ones, is detailed below, but the main message is that comparing several cases allows us to distinguish the important from the unimportant (or the relevant from the irrelevant, or the related from the contingent), and that limiting the number of cases allows us to deal more adequately with the complexity of social life.

We choose cases according to the questions we ask and the assumptions we make about this "complexity." When we study such "big" events as revolutions, trade union disputes, or enlistment in large standing armies (as in the projects by Margaret Levi and Miriam Golden), we may only have a few cases to start with, and the strengths and weaknesses of the analysis will depend to a great extent on the kind of information available about each (as Levi discusses).

We may decide to limit the scope of comparison to a region, or a type of society, to limit the differences between cases. This strategy of selecting closely related cases may be the result of different logics. We may, for example, be trying to control for shared features so as to isolate those elements that lead to a specific outcome, as in David Laitin's and Barbara Geddes' studies. Or we may be trying to study the variation and change in a cultural form across related societies, as in Greg Urban's and Fredrik Barth's studies. These pairs of studies start from very different questions – What are the general causal relations here? what are the specific processes of change here? – but they both depend on comparisons of closely related cases in order to find answers.[1] We may also decide to choose quite different cases so as to see if postulated relations hold up in very different contexts. David Laitin combines these two approaches; he uses the "most different case" strategy to see how well his hypothesis holds up once he has initially tested it from a "most similar case" approach.

We use comparisons not for their own sake, but because we find that they allow us to understand better *processes and mechanisms*, the how and why, narrative and explanation, of social phenomena. Mechanisms are specific patterns of action which explain individual acts and events; when linked they form a process. As developed in political theory (see Elster 1987), they are intended to apply over a wide range of settings, and they generally refer to psychological predispositions. For example, someone might continue to keep and repair an old automobile despite the likelihood of additional costly repairs because he or she figures that a lot has already been invested in the car. This mechanism, the "tyranny of sunk costs," may also keep spouses together who would otherwise separate because they cannot accept the fact that investments in the relationship have been in vain. This mechanism is both general, in that it can be applied to a wide variety of cases (cars and spouses), and specific, in that it can be used to explain why a particular event occurs.

A mechanism approach to explanation does not, however, seek a high degree of predictive power, nor does it aim at the creation of general laws. Sometimes spouses do break up, and other mechanisms ("the grass is greener," for instance) may be at work. "If p then *sometimes* q" is the closest to a prediction that can be made within this explanatory framework. The political scientists writing in this volume by and large adopt this approach, seeking a finer-grained account of several phenomena rather than a general law. This methodological choice, sometimes associated with rational choice theory (Johnson 1996), distinguishes them from other political scientists seeking predictive power through the use of a variable approach (see King, Keohane, and Verba 1994). It also brings them closer to the anthropologists in the collection (most of whom would otherwise see little affinity between their work and that stemming from rational choice) in their emphasis on understanding particular processes rather than generating highly simplified propositions about the general relationship among two or more variables. Indeed, in his concluding chapter, Bowen argues that in all the chapters the authors make their point most convincingly when they offer microhistorical accounts of processes, and often contrasts of processes across societies, rather than static comparisons of cases.

The political scientists included here are interested fundamentally in discovering mechanisms that lead people to undertake certain courses of action under certain conditions. Margaret Levi, for example, has as her main goal understanding the mechanisms that lead people to enlist (or not enlist) in armies. But she also constructs an analytical narrative of each country case, tracing specific macro-level pathways. Further, she tells another process story of building the model out of earlier work on taxation, looking for a very different domain against which to hone the model further, and then gradually building up knowledge of each case. (Levi thus chose her topic following a "most different case" strategy, and then compares cases of similar countries.)

For the anthropologists, both processes and mechanisms are desired objects of knowledge, but the better understanding of a particular process may be deemed more important than the uncovering of general mechanisms. Fredrik Barth's and Greg Urban's projects both involve redirecting comparative studies from the arrangement of predetermined cultural objects to the study of the processes by which forms are changed and transmitted. Ancillary to their studies, but mentioned by both as additional desiderata, is the uncovering of mechanisms that produce variation. Barth, in particular, seeks to link his fieldwork to studies of general cognitive mechanisms by which people forget and change information.

Although we find the two disciplines converging toward a renewed attention to controlled comparisons, each has its own quite distinct genealogy.

Anthropology

Anthropology exhibits continued nervousness about executing comparisons at all. When Robert Barnes (1987: 119) complains that "anthropology is permanently in crisis about the comparative method," it is the legacy of "the Comparative Method" that is at fault. This "Method" dates from the nineteenth century, and in particular from Lewis Morgan's (1871) philological studies and E. B. Tylor's (1889) cross-cultural comparisons, which he called the study of "adhesions." It is what Barth and Urban refer to as the museum approach to anthropology: isolating cultural traits and rearranging them according to such universalistic criteria as types of social structure or the relative complexity of tools.

The main traditions of American and British anthropology developed in large part as reactions to this acontextual isolation of traits. Boas and his cultural anthropology students in the United States emphasized the holistic properties of particular cultures; the founders of British social anthropology emphasized the interconnection of statuses and norms in particular societies. Yet both also engaged in comparisons of related societies or cultures. In the 1930s and 1940s Fred Eggan developed the term "controlled comparisons" to characterize studies of social variation and change in Native American societies of the southwest United States

(Eggan 1966). Regional comparisons were also used to generate and test ideas about processes, such as the rise of social stratification in the Pacific (Sahlins 1958), the development of witchcraft in Africa (Nadel 1952), or, returning to the US southwest, the development of personality through child-rearing practices (Whiting 1954). Sometimes regional comparisons were developed as contrasts, to show how different things could be along some axis within a region, as in Mead's (1935) contrasts of neighboring Melanesian societies.

Large-scale comparisons continued to be refined and expanded in the 1940s and 1950s, leading to today's "cross-cultural" method of universalistic comparisons based on a standard sample of cultures. This method typically focuses on the co-occurrence of social and cultural traits, sometimes using multiple regressions to explain the particular distribution of a feature such as "high women's status" or a certain residence rule (see Burton and White 1987).

By the 1970s and 1980s, comparative studies had been eclipsed by renewed emphases on interpretation and meaning. Large-scale cross-cultural analyses came in for particular criticism for their emphasis on traits over context, and their universalistic framework of bounded "cultures." First, critics argued, taking traits (such as "residence rules") as fixed features of cultures risks losing from the analysis many of the interesting features that good ethnography provides, including contextual determinacy (for example, residency choices depend on resources), and variation in local understandings (for example, genealogical ties to a co-resident can be reckoned in more than one way). As anthropologists turned more and more to the interpretation of local meanings, this criticism seemed increasingly telling.

Secondly, comparing across a universe of bounded, putatively independent "cultures" risks losing sight of the processes by which variation is created. The elements of a culture change over time and vary over space precisely because they have a dynamic interrelationship which can be causal and meaningful. Even in pursuit of the general hypotheses sought by practitioners of large-scale comparisons, regional variation can be a better source of data because of the control on certain variables (Mace and Pagel 1994). Although cross-cultural research has enjoyed a recent upsurge in interest, it is rarely even consulted by the majority of anthropologists; many consider it to have produced little of clear value, as recently noted by two of its major practitioners (Burton and White 1987).

Regional comparisons also have been neglected in the theoretical literature from the 1970s onward; Allen Johnson (1991) reviewed such studies and concludes that they have had little impact on the discipline as a whole. This neglect is probably due to the combined critiques of both cross-cultural comparisons and of ethnography itself. Work labeled "post-modern" has questioned the validity of all ethnographically produced knowledge (Clifford and Marcus 1987) and has further directed theoretical discussions away from comparisons. As the editor of a volume on comparative studies put it, "the line between comparativists and non-comparativists . . . is probably more sharply drawn than ever before" (Holy 1987: 9).

And yet comparative work thrives at the heart of the discipline, particularly at the level of collaborative efforts to understand better the nature of variation and processes within regions. Controlled, regional comparisons are more widely accepted in anthropology than are universalistic ones, because they preserve a relatively high degree of contextual specificity while moving beyond the boundaries of specific societies or cultures. Much of this kind of research has been intended mainly to point out regional variations on a theme, as in a collection of studies of eastern Indonesia social organization (Fox 1980) that points to the widespread symbolic importance of the house and of the "flow of life." Similar comparativist studies of culture areas can be found for Africa (Parkin 1980), South Asia (Yalman 1967), and lowland South America (Rivière 1984). More rarely do these anthropologists identify mechanisms generating variation within the area. Barnes (1980), for example, compares marriage payments across a number of eastern Indonesian societies not only to show variations on a theme but also to argue that a causal relationship holds between the degree of trade, the levels of bridewealth demanded, and the consequent difficulty of completing payments and converting an uxorilocal marriage to a virilocal one. Mandlebaum (1988) describes the widespread ideas and practices leading to the seclusion of women across south Asia, and then explains variation in these ideas and practices by reference to women's labor participation (see also Miller 1981).

Regional analyses have been perhaps most central to studies of New Guinea societies, where they also have achieved a noteworthy theoretical sharpness. An earlier emphasis on identifying subregions by the preponderance of particular diagnostic features (for example, competitive feasting, intensive sweet-potato cultivation) has yielded to more recent studies (for example Godelier and Strathern 1990; Knauft 1993) that emphasize the ways in which different questions (for example, the spread of social organizational forms, the development of stratification) will highlight different possible configurations of subregions. Thus an initial concern with mapping of cultural forms has been succeeded by a new focus on examining the processes that generate variation (Barth

1987; Hays 1993). This change in comparative strategies is often associated with the work of Fredrik Barth, and Barth's chapter for this volume focuses on ways to study generative social processes by comparing social forms within and across cultural boundaries. Because the same set of processes may develop variation within and across cultural boundaries, this approach takes cultural forms, and not bounded cultures, as the units of analysis.

For anthropology, the emphasis in this volume on process and mechanisms recalls much of the original purpose of undertaking controlled comparisons. Eggan's studies in the US southwest were a rebuke, albeit disguised, to the scientistic claims of his teacher A. R. Radcliffe-Brown that such societies had no history and that they therefore could only be understood in terms of the functional consequences of particular social forms. The comparison of neighboring societies, combined with what was known of early history, was intended precisely to reintroduce historical processes and mechanisms of change into social anthropology. What is different about today's work is in part the emphasis on comparisons of more highly interpreted realms of culture, such as the ritual wailing explored by Greg Urban and the Islamic rituals studied by John Bowen, and longitudinal analyses, as in Johnson's work. The goal of these and other analyses is understanding the processes by which cultural forms are learned, transmitted, and transformed.

Political science

Unlike anthropology, political science contains a subfield devoted to comparative studies. Arend Lijphart's much-cited 1971 article, "Comparative Politics and the Comparative Method," contains a view of the evolving role of small-scale comparisons within that subfield. Lijphart wrote (1971: 685): "If at all possible one should use the statistical (or perhaps even the experimental) method instead of the weaker comparative method." The strength of small-scale or "small-n" comparisons, Lijphart continued, lay in their ability to help create coherent hypotheses in a "first stage" of research. A statistical "second stage" would test these hypotheses "in as large a sample as possible."

Twenty-five years later, while some comparative research is conducted in the manner Lijphart recommended, much is not (see Collier 1991). In fact, the methodological coherence and division of labor envisioned by Lijphart has never developed. On the contrary, one might say that the sub-discipline of comparative politics has become either remarkably diverse or terribly fragmented, depending on one's perspective.[2]

Furthermore, as exemplified by the work and arguments of the political scientists below, small-scale comparisons are no longer a second choice to statistical approaches, nor are they simply used to generate hypotheses as a "stage" in the research process. They are used for both theory-building and theory testing, and they form a complete research program in their own right. In order to understand the continued prominence and even resurgence of these controlled comparisons in comparative politics, it is necessary to understand both the disillusionment with other research approaches, and the innovations in small-scale comparison.

During the 1950s, political science moved away from describing the legal-formal aspects of political systems towards a more behavioralist approach. Substantively the field was dominated by the issue of "development." The Social Science Resource Council Committee on Comparative Politics became the most influential institutional actor helping to create from the late 1950s to the early 1970s a large literature on development. Many of the works produced in this era put forth universalistic typologies and chronological models: developing nations could, and would (and should), follow the Western path toward democracy with the help of institutions and processes already witnessed in the United States.

By the late 1960s, however, faith in the universalistic processes that work toward outcomes of social justice was shaken by events throughout the world. Developing countries did not follow the expected paths, and Vietnam was a disaster. The last great grand synthesis of the field, Huntington's *Political Order in Changing Societies* (1968), reflected the original developmentalists' loss of optimism. The Social Science Resource Council Committee on Comparative Politics was disbanded. The backlash against the developmentalists produced a whole new set of general models. Dependency theory, corporatism, and bureaucratic-authoritarianism are the most well-known and direct responses to the perceived failures of the developmentalist approach.

However, these general models proved inadequate in explaining the complexity of modern politics: Asian newly industrialized countries (NICs) produce booming economies while other developing economies flounder; military regimes fade from Latin America while fundamentalist revolutionary regimes appear elsewhere; communist regimes fall but former communists win elections; mass ethnic killing in Rwanda and the former Yugoslavia occur simultaneously with peaceful change in South Africa and the Middle East; and so on. As complexity increased, two dominant approaches, model-building at the level of grand theory and large-scale statistical studies, went into relative decline.

The focus of the political comparativists in this volume is less on sweeping general models and more on explaining better-defined phenomena. Miriam Golden explains a set of labor actions in industrialized states; Barbara Geddes explains bureaucratic reform in Latin America; David Laitin isolates a set of conditions explaining nationalist violence; Margaret Levi's work explains variation in conscription policies and responses in several Western states. Following William Riker (1990), Golden describes her choice of topic and scope by asserting that "a narrow focus to attain a proper solution is a better research strategy than a broad focus that fails to generate conclusive results. By narrowing the focus of the phenomena under study, we reduce the trade-off between analytic rigor and empirical accuracy." An increasing number of comparativists have come to agree with this argument.

While large-scale studies are still prevalent in comparative politics, faith in cross-cultural and cross-national statistical study has diminished with increased awareness of problems associated with conceptual "stretching," unreliable measures, and improper specification of domain and units.[3] As Sartori (1970) has pointed out, the very concepts used to define independent and dependent variables often translate across societies only with the greatest difficulty. As more cases are included in a given study, the basic concepts are often "stretched" to incorporate them, sometimes to the point of meaninglessness. Furthermore, heightened appreciation of cultural difference has generated skepticism of statistical measures. For instance, does the gap between expected income and actual income really measure relative deprivation in both France and Indonesia? Does "income" have the same meaning and relevance in both societies? When does the social scientist know which cases belong in the sample if knowledge of cases is superficial (as in most large-scale studies)?

In addition to some of the more intractable methodological problems involved with large-scale statistical studies, some scholars are not satisfied with the very nature of the explanation that such work provides. Rather than simply identifying probabilistic relationships between sets of variables, many comparativists would rather work to identify the nature of causal linkages among parts of a process. The work of David Laitin (see chapter 2) comprises such an effort.

Many of today's political comparativists are skeptical of the abilities of general models and large-scale statistical work to capture the complexity of their subject matter; however, they remain committed to social science methods that allow for generality. Skepticism has not produced the desire to do purely descriptive and highly specific work. Margaret Levi speaks for many comparativists when she writes in chapter 8 that "an overemphasis on specificity . . . obscures the commonality among