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Gerardo L. Munck and
Richard Snyder

Passion, Craft, and Method in Comparative Politics

INTERVIEWS WITH

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David Collier

Critical Junctures, Concepts, and Methods

David Collier has made major contributions to comparative politics through his carefully conceptualized research on authoritarianism, democracy, and corporatism in Latin America. He has also been a leading figure in the field of methodology, publishing numerous influential works on concept analysis, as well as offering a new perspective on the relationship between qualitative and quantitative methods.

Collier's early work includes quantitative cross-national research on political regimes, corporatism, and social policy, as well as an exploration of the links between regime change and public policy toward squatter settlements in Peru, which was published as *Squatters and Oligarchs* (1976). In his edited volume *The New Authoritarianism in Latin America* (1979), a landmark study in the literature on national political regimes, Collier and collaborators explore alternative explanations for the rise of authoritarianism in Latin America in the 1960s and 1970s.

His most sweeping work on Latin America, the fruit of over a decade of research, is *Shaping the Political Arena* (1991), coauthored with Ruth Berins Collier. This book, one of the most ambitious and systematic studies of Latin American politics ever published, offers an in-depth analysis of eight countries across five decades. *Shaping the Political Arena* seeks to explain contrasting regime outcomes, such as military coups against democracies, as historical legacies of how labor was incorporated into national political institutions. In addition to making a fundamental contribution to the study of politics in Latin America, the book has had a significant influence on the broader field of comparative politics. Its theoretical framework of "critical junctures" is regarded as one of the most systematic, explicitly elaborated models in the historical institutionalist literature. Moreover, the book's careful, case-based comparative analysis is viewed as an exemplar of methodologically rigorous qualitative research.

This interview was conducted by Gerardo Munck in Berkeley, California, on July 8, 2003.

A second strand of Collier's work focuses on methodology. This research addresses issues of concept formation and measurement, emphasizing the procedures used in the comparative, largely qualitative, literature on democracy, authoritarianism, and corporatism. It also discusses the wide range of tools used by qualitative researchers, explores procedures for concept formation and evaluating measurement validity that apply to both qualitative and quantitative work, and examines commonalities and differences between qualitative and quantitative methods in part from the perspective of statistical theory. These latter themes are addressed especially in *Rethinking Social Inquiry* (2004), a volume coedited with Henry Brady.

Collier was born in Chicago in 1942. He received his B.A. from Harvard University in 1965 and his Ph.D. in political science from the University of Chicago in 1971. He has taught at Indiana University, Bloomington (1970–78), and at the University of California, Berkeley (1978–present). He was president of the Comparative Politics Section of the American Political Science Association (APSA) in 1997–99, vice-president of APSA in 2001–2, and founding president of the Qualitative Methods Section of APSA in 2002–3. He was elected to the American Academy of Arts and Sciences in 2004.

Training, Intellectual Influences, and Dissertation Research

Q: You grew up in Chicago and your parents were academics. How do you think these early years influenced your subsequent academic work?

A: Growing up in the community of the University of Chicago did place me in the midst of what was then, and remains today, one of the most stimulating social science communities in the world. Many of the major names in social science of that period—in the 1940s and 1950s—lived just down the block or around the corner. I come from a family of anthropologists, and my parents were both affiliated with the Anthropology Department at the university, though both also had employment elsewhere. Robert Redfield and Fred Eggan were close family friends, and on the basis of childhood associations, I later on found it easy to place in perspective Redfield's classic study of a Mexican village (1930) and Eggan's presidential address for the American Anthropological Association (1954), in which he explored the "method of controlled comparison" in anthropology. It is curious how this methodological agenda came back to the center of my attention many years later.

I went to the University of Chicago "Laboratory School," and classmates included Stephen Stigler—son of the eminent economist—who became a prominent statistician and went on to write his magisterial *History of Statistics:* and Michael Rothschild—whom I had known since nursery school and who later did innovative work on decision making under uncertainty. Mike was coauthor of one of the papers for which Joseph Stiglitz won the Nobel Prize, as well as the purveyor of numerous quips over the years—for example, "Economics is about that part of happiness that money can buy."

Q: What are your first political memories?
A: The 1950s were certainly complicated times politically. In the first half of the decade, various colleagues and friends of my parents were subject to damaging accusations of disloyalty during the McCarthy period, and I recall my father on more than one occasion testifying on their behalf to help them retain jobs and security clearance for classified work. These family friends included a student of my father's, John Murra, who had been in the Abraham Lincoln Brigade in the Spanish Civil War, and who went on to become a prominent ethnographer of the Andes. Murra later introduced the creative concept of the "vertical archipelago" for systems of cultivation in the Andes characterized by a strong integration of cultivation at different altitudes. This became a nice piece of inventive concept formation for me to think about.

Everyone in my parents' generation within the family was actively involved in the New Deal, in part following the lead of my paternal grandfather, John Collier Sr. Beginning in the 1910s, he had been a crusader for social reform and social justice in the United States, and he later became a member of Franklin Roosevelt's brain trust. However, by the 1950s I would say that we were simply devoted "Adlai Stevenson Democrats,"[1] frustrated by the blandness of the Eisenhower years.

Q: How did you become interested in political science and comparative politics?
A: In my undergraduate years at Harvard, beginning in the fall of 1959, it was pretty easy to be interested in politics. Momentum was building for John F. Kennedy's run for the presidency, many Harvard faculty were involved in the Kennedy campaign, and at Harvard there was a contagious sense of political excitement and political possibilities. One also remembers vividly certain terrifying and tragic moments: walking through Harvard Square at the height of the Cuban Missile Crisis; exactly where I was in Widner Library when we received the news of Kennedy's assassination; and then the vivid images in the days following the killing of Lee Harvey Oswald.

For one year I was an English major—I think due to my interest in poetry

1. Stevenson was the Democratic presidential candidate in 1952 and 1956.

and in writing—but I switched to government. Samuel Beer's Social Studies 2 introduced me to Max Weber, and H. Stuart Hughes's course on European intellectual history left me fascinated by the evolution of European thought over the nineteenth and twentieth centuries. But more than anything it was my first reading of Seymour Martin Lipset's *Political Man* (1960a) that showed me how big, fascinating questions, along with a variety of specific hypotheses about politics and political change, can be addressed through astute comparisons. I found the book breathtaking. The same academic term that I read Lipset's book, I also took my first statistics course. Although *Political Man* was not very statistical, the juxtaposition of the ideas about inference introduced through the statistics course, combined with the analytic power of Lipset's book, greatly extended my horizon, and was doubtless a key early step in sparking my interest in methodology.

Other memorable experiences at Harvard included Samuel Huntington's remarkable course on political development, which I took the same semester that he published his compelling *World Politics* article, "Political Development and Political Decay" (Huntington 1965), which in turn led to his book, *Political Order in Changing Societies* (Huntington 1968). It was an exciting course. Though Huntington was not a specialist on the Third World, he had a flair for coming up with engaging arguments and hypotheses. Another invaluable course on political change was Rupert Emerson's "From Empire to Nation," which provided a masterly overview of the decolonization in Asia and Africa (Emerson 1960). I recall a key visiting lecturer in that course—Aristide Zolberg—who later was Ruth Berins Collier's dissertation chair at Chicago, and has been a career-long friend and colleague. On the side of sociology, I was fascinated by the ideas about social structure conveyed by George Homans, whose book *English Villagers of the Thirteenth Century* (Homans 1941) provided a striking example of drawing interesting ideas about social structure out of archival materials, involving a society that had existed many centuries before. Homans's famous "quarter deck" speaking style made him a memorable lecturer. I also took a course on political sociology with Talcott Parsons.

Q: In 1965 you started graduate school in political science at the University of Chicago. Why did you select Chicago?
A: Political science at Chicago periodically loses large numbers of its faculty, and then goes through an exciting period of rebuilding. I had been told that the department was in the midst of precisely such a period of rebuilding, and this advice was on target. Susanne and Lloyd Rudolph and Aristide Zolberg had just arrived, and Leonard Binder had come a couple of years before, yielding a newly fortified comparative politics group. Theodore Lowi was just arriving from Cornell and, along with the vivid ideas spinning out

of his new work on "arenas of power" (Lowi 1964), he had a powerful instinct for mentoring graduate students long before that was a standard practice—and before we even had *mentoring* as a standard term. Building on my prior course with Talcott Parsons, I studied with David Easton and gained leverage moving from the details of politics and society to more generic analysis that went well beyond these details. Outside the Political Science Department, this was the period when the "Committee on New Nations," involving such faculty as Clifford Geertz and Edward Shils, was active at Chicago, exploring the large social science agendas opened by the proliferation of new nation-states in the Third World.

Nathan Leites—a close associate and collaborator of Harold Lasswell—had come to Chicago two years before, and he was a strong influence. Although I never took a formal course from him, sitting in on his brilliant seminars taught me a lot about careful, terse argumentation. In Leites's vocabulary, the most grave—and in his view, unfortunately common—defect of arguments in political science was that they are "banal." Students awaited this epithet with dread as they presented their own ideas in seminars. I believe that this concern of Leites remains highly relevant today—as one worries that some of the most technically elegant forms of analysis in political science yield findings that are, in fact, sometimes banal.

In addition to my work with these faculty members, I had the good fortune of receiving a NIMH Training Fellowship in Social Research at Chicago's National Opinion Research Center (NORC).[2] This training program in quantitative analysis and survey research offered what was certainly modest training in methodology by today's standards. We invested large amounts of time in carrying around long boxes of IBM cards, running them through counter sorters, and receiving instruction in how—if one had unbelievable levels of tenacity—it was possible to calculate correlations, and even do factor analysis, on what by now seem like ancient Monroe Calculators. Toward the end of the time at Chicago, Norman Nie and his entire SPSS group had arrived from Stanford University,[3] so attention shifted to lugging the boxes of IBM cards over to the campus computer center in the evening and then returning in the morning to pick up the printout. By today's standards, pretty slow turnaround!

Notwithstanding these headaches, by the norms of the time this program offered a fairly substantial level of methodological training, and again extended my intellectual horizon. Further, precisely the low-tech character of the program lent itself to teaching valuable skills and topics: for example, an instinct for good comparisons, the vivid presentation of cross-tabulations, and an understanding of multi-variant relationships via the Lazarsfeld elaboration model[4]—an understanding that I believe too often does not come into sharp focus in today's methodological teaching. This NIMH-NORC experience was certainly another step toward the development of my subsequent work on methodology.

2. At the time the NIMH (National Institutes of Mental Health) was providing extensive basic training in the social sciences.

3. SPSS stands for Statistical Package for the Social Sciences, which was perhaps the first user-friendly computer-based statistical software for social scientists.

Q: What about your interest in Latin America?

A: As I noted, I come from a family of anthropologists, who have written books on Peru, Ecuador, and Mexico, and on the Americas more broadly. My father, grandfather, two uncles, and also cousins worked on Latin America. Traveling in Latin America, knowing those countries, studying them, and having a commitment to close collaboration with Latin American scholars—this was a family tradition. As a child and teenager, I had been with my family on two archaeological excavations in Latin America—my father was an archaeologist as well as an anthropologist. From these trips I had many vivid memories and impressions of Latin America, and hence the region was on my mind as a possible focus for my studies. Yet when I arrived in graduate school, Chicago had no Latin Americanist in political science, and although I audited a course from the historian Herbert Klein, it seemed implausible that I should write a dissertation on Latin America.

Then, at the end of my second year, Chicago hired Philippe Schmitter, who arrived from Berkeley amid the awe inspired by the fact that while in graduate school he had coauthored several publications with Ernst Haas (Haas and Schmitter 1964; Schmitter and Haas 1964)—who in turn later became a close colleague and friend of Ruth's and mine at Berkeley. Schmitter studied the region I wanted to study, and quite frankly, his brilliant analytic style and breathtaking framing of Latin American issues immediately made it obvious that I should work on Latin America. In his first term at Chicago, Schmitter taught a general course on Latin America, which among other things introduced ideas about non-pluralistic patterns of interest group politics that became central to his—and my own—later work on corporatism. Schmitter also anticipated by six years—clearly they were working on parallel, but at that time separate, tracks—Guillermo O'Donnell's famous book that sought to unravel the Argentine puzzle (O'Donnell 1973): a country at a high level of socioeconomic development, yet with

4. Lazarsfeld's elaboration model or formula, devised as a way of promoting causal analysis in non-experimental research, consists of a set of procedures for data analysis and causal inference that build up larger models from bivariate relationships by successively introducing control variables. That is, the principle behind the elaboration formula is to discover the explanations for an observed relationship between two phenomena (and variables) by means of introducing a third variable. See Kendall and Lazarsfeld (1950) and Lazarsfeld (1955).

patterns of regime crises quite divergent from what one would expect. This first course with Schmitter included myself, Ruth Berins, Karen Remmer—now the prominent Latin Americanist who teaches at Duke University—and a marvelous student from Brazil, Alexandre Barros, who was the first of many Brazilian students whom Schmitter brought to Chicago.

The following spring, Schmitter taught a course on comparative research methods, and this course was another factor in leading me to the kind of methodological work I have subsequently pursued. Schmitter had been strongly influenced by the intellectual ferment in comparative methodology at Berkeley in the 1960s, which included Haas's powerful use of typological methods and Ralph Retzlaff's foray into quantitative cross-national research. Retzlaff's work stimulated Schmitter to enter a period of exploring the potential for quantitative-comparative work on Latin America. That academic term my studies were agreeably disrupted by Ruth's and my marriage in March, but nonetheless this course gave me many ideas about methodology.

Q: What were some of the key books you read in graduate school at Chicago?

A: Let me respond narrowly by mentioning one line of analysis reflected in books—that came to be important to me—written around that time by researchers at Chicago. By then, scholars were reading a manuscript version of Schmitter's book on Brazil (1971). It produced a lot of excitement, not only among Latin Americanists, but also among scholars working on American politics. They saw it as giving a valuable new, non-pluralist perspective for looking at interest group politics. Besides Schmitter, three other faculty at Chicago, all of whom worked on the United States, were also concerned with this theme. One was Lowi, and another was Grant McConnell, who at that time was finishing his book *Private Power and American Democracy* (McConnell 1966). A central theme of McConnell's research focused on how interest groups are shaped by the wider systems of political power in which they are located. This can yield a system of group politics that is often highly noncompetitive, in which the group's role in representing its constituents becomes ambiguous—as does, indeed, the very definition of who those constituents are. This form of group politics is quite distinct from an image of interest groups involving the wide-ranging, competitive interaction among different groups, and with the state. With the publication of David Greenstone's book on labor politics (1969), and with the work of Lowi, McConnell, and Schmitter, it seemed that Chicago might develop a distinctive, non-pluralist school of analysis. But this did not coalesce: McConnell soon left for Santa Cruz, and Lowi went back to Cornell.

Nonetheless, this was an influential climate of discussion, and these issues later emerged in sharp relief in the Latin American field in research on corporatism. Within the framework of this discussion, we can define corporatism as a noncompetitive system of group politics, in which systems of private power, as in McConnell's framework, or of state power, as in Schmitter's subsequent work on state corporatism (Schmitter 1974, 1977), constrain competition among groups. In these contexts, the organized groups function only in part as "representatives" of their constituencies, in that they play a role of mediation between the larger system of power—whether private or public—and their presumed "constituents." This body of work—above all, that of McConnell and Schmitter—was enormously helpful in placing within a larger framework my subsequent thinking about different patterns of interest politics, as well as about different types of authoritarian regimes.

Q: What impact did the domestic situation in the United States, from the McCarthy period to the Vietnam War, have on you?

A: I commented before on the McCarthy period. Regarding the upheavals of the late 1960s, I was away from Chicago doing fieldwork during the most traumatic moments. I was not in Chicago during the tumultuous convention of the Democratic Party in the summer of 1968, and I missed the height of the protests over Vietnam and Cambodia that took place at the University of Chicago. So I was probably less wrapped up in the climate of the times within the United States than others who were in place at their home institutions during 1968–69.

However, I was also looking at these issues from Latin America, in part seeing them in the context of the hostility of the U.S. government toward the post-1968 military government in Peru. This hostility was triggered in part by the terms of the U.S. "Hickenlooper Amendment," which mandated a strong response to the expropriation of U.S. property abroad. But the reaction of many U.S. scholars working in Peru also reflected broader dismay that the United States did not respond more positively to this military government, which was after all trying to address problems that underlay the Peruvian political stalemate of the 1960s. This was a time when a great many U.S. researchers in Peru felt little sympathy for U.S. government policies on any level.

Q: Regarding your doctoral dissertation, your initial idea was to study the Peruvian congress.

A: I was intrigued by Lowi's ideas about arenas of power, according to which different political relationships crystallize around different types of public policies (Lowi 1964). And I had also been working with Duncan McRae, who was one of the first scholars to engage in the quantitative analysis of legislative roll call voting. Given my interest in Latin America, I thought I could take Lowi's ideas about the different power relationships that crystallize around different public policies, and McRae's tools for analyzing legislative behavior, and apply them to the Peruvian congress. The mid-1960s had been an interesting period of political stalemate in Peru, with a legislature controlled by APRA[5] essentially blocking President Belaunde's reform program. It seemed that studying the legislature with some new empirical tools would be a productive enterprise.

The problem was that I arrived in Peru just a couple of days after the Velasco military coup in October 1968.[6] The legislative archive had been closed by the military government, this interesting political stalemate had been resolved by military force, and in the context of the dramatic regime change, the idea of studying the legislature had become impractical, and it seemed a lot less relevant. I ended up switching my dissertation topic.

Q: This is a graduate student's nightmare, to be in the field and suddenly have to rethink the topic of a dissertation. How did you pick a new topic?

A: Well, first of all, my research was supported by a Latin American Teaching Fellowship, a program administered by Tufts University. This fellowship paid for a full fifteen months in Peru, giving me time to adjust my topic. Also, the fellowship required that I teach a course during my stay, so it put me in touch with a spectrum of people who were invaluable in helping me find a new focus for my research. In the first months of my visit, I offered jointly with the Italian sociologist Giorgio Alberti an introductory statistics course at the Instituto de Estudios Peruanos (Institute of Peruvian Studies), the leading social science research center in Peru. This institute had carried out extensive studies of the squatter settlements of Lima, thereby strongly drawing them to my attention. A series of other circumstances also encouraged my focus on the squatter settlements. At the hotel where I was first staying, I encountered a fellow Harvard graduate who was part of an international aid group working on squatter settlements. Ruth and I went out to visit squatter settlements with him a couple of times at the beginning of the stay in Lima.

I also had the good fortune that a graduate student who was just returning to the United States gave me several key articles on squatter settlements that conveyed what was then the established image—that these communities were formed as the residents illegally seized land and often valiantly fought off the police and other authorities, in order to secure precarious housing. These settlements, presumed to be illegal, were highly visible around the periphery of Lima, and their population at that time was roughly 1.5 million, approximately one-quarter of the city's population.

5. Alianza Popular Revolucionaria Americana—the American Popular Revolutionary Alliance.

6. The coup led by General Juan Velasco Alvarado was particularly noteworthy because it inaugurated a left-leaning military regime that contrasted starkly to the right-wing military regimes that dominated much of South America in the 1960s and 1970s.

The idea that began to take shape in my study was motivated by a question posed to me by a Peruvian friend early in the research process. He asked how these settlements could possibly have been allowed to form, given the degree to which they appeared to be an affront to the system of private property. Until the 1968 military coup, private property had, after all, been a crucial foundation for the power of the Peruvian oligarchy, and the widespread violation of private property around the capital appeared to threaten this foundation. My dissertation and the subsequent book (Collier 1976) sought to address this puzzle.

Initially, I encountered a few hints that, although the squatters sometimes did valiantly fight off the police, many times there were various kinds of involvement by the government, political parties, and even landowners in encouraging settlement formation. The more I looked, the clearer it became that the settlements were embedded in a wider political and economic system that strongly supported their formation. Hence, I became strongly interested in the politics of squatter settlement formation.

Further, it increasingly appeared that this support for squatter settlement formation was linked to broader choices about cultivating the political support of the poor, about social policy, and about addressing concerns over political radicalization within the squatter settlement population. Thus, squatter settlement politics became a window through which I could analyze the evolving political relationships between the state and the poor, patterns of mobilization and control, populism, and a variety of topics that subsequently have come to have wide currency in the Latin American field, and far beyond. My dissertation thus came to focus on a game of squatters and elites, with some of the elites actually being members of the Peruvian oligarchy. Hence the title of my subsequent book, *Squatters and Oligarchs* (Collier 1976).

Q: How did you reconstruct the history of squatter settlement formation in Lima?

A: My most valuable source of data was a survey I carried out to reconstruct how squatter settlements were formed. From my experience as a training fellow at the National Opinion Research Center (NORC) in Chicago and my knowledge of what was called NORC's "Permanent Community Sample," I knew that surveys could be used not just to interview citizens, but also to generate data on a variety of institutional actors. Correspondingly, I did a survey of eighty-five communities—involving both highly structured and open-ended questions, focused on early leaders in different squatter communities who had been involved in the actual episode of settlement formation. I did many of the interviews myself, and colleagues I knew through the Instituto de Estudios Peruanos helped me find skilled interviewers who, with careful supervision, applied the questionnaire in additional settlements.

A second principal source was an archive maintained by the Peruvian newspaper, *La Prensa,* which encompassed a remarkable collection of newspaper clippings and other kinds of data, including extensive information on squatter settlements. This kind of "hard copy" archive is difficult to imagine in today's world of computerized databases. I will always be grateful to my fellow graduate student, Liisa North, who secured my access to this archive. It had many hundreds of clippings and other information on squatter settlements and squatter settlement formation, and was invaluable to my research. I then connected my interviews, these data from the archive, and a wide spectrum of data from government housing offices and other sources to reconstruct the history of squatter settlement formation.

The picture that emerged showed a fascinating spectrum of different economic and political goals behind the promotion of squatter settlement formation, ranging from many direct interventions by the president and political parties seeking political support, to an initiative by a member of the economic elite seeking to launch a political career, to efforts by urban landowners to clear inner-city slums in order to develop the land as valuable real estate. These landowners would hire people to help organize the squatters for a land invasion, thereby clearing valuable inner-city land.

The answer to my friend's motivating question was thus that settlement formation was promoted by a complex network of political and economic relationships.

Q: It seems that you had a positive interaction with colleagues while conducting your fieldwork in Peru.

A: Yes. With regard to colleagues in Lima, two of whom I got to know through the Instituto de Estudios Peruanos in Lima were especially important. The leading Peruvian political sociologist Julio Cotler, and also Giorgio Alberti, became close colleagues and friends. My other great friend and collaborator was the Peruvian sociologist Sinesio López, who had long had a strong interest in the squatter settlements, and who provided an enormous amount of help in moving my research along.

I was also in touch with other Ph.D. students from the United States. Jane Jaquette was working on the politics of public policy formation, Liisa North was carrying out her interviewing of APRA leaders in all parts of the

country that were centers of APRA strength, Edward Epstein was also working on APRA, and Howard Handelman was in the midst of the interviewing that led to his study of the peasant mobilization that swept the Peruvian Andes in the 1960s. Abraham Lowenthal was working in the Ford Foundation office in Lima, and he was then—as he has always been—a source of astute advice and commentary about research. All of these scholars have, in the intervening decades, made important contributions to the study of Latin American politics. Finally, in a subsequent visit to Lima I interacted extensively with Alfred Stepan, who became a friend and mentor throughout my career.

Q: Did you keep in touch with your dissertation committee as your research in Peru advanced?

A: I made no return trips to the United States, and at that time one could hardly send off an e-mail message, asking for suggestions about research plans. It was also a time when long-distance calls seemed incredibly expensive. I wrote my committee a letter about my new focus, and they wrote back, in principle approving the change. Also, Schmitter was in Argentina toward the end of my stay in Peru, and I went to Argentina and discussed the refocused dissertation with him.

I recall with amusement how Chicago handled the required formal oral exam on the dissertation prospectus. Not long after my return to Chicago, when the field research was already completed, I was invited to a faculty reception at Aristide Zolberg's home. Partway through the reception, Schmitter, Zolberg, and Ira Katznelson precipitously took me into a side room and retroactively conducted the oral exam on my prospectus. What can I say? At least I didn't have an opportunity to become anxious before the exam!

Q: Apart from your research on squatter settlements, what did you learn from observing the first fifteen months of the Velasco government in Peru?

A: This was a dramatic and fascinating period in Peruvian politics. Along with the research on my dissertation, living in Peru at that time played a key role in crystallizing my longer-term research interest in regimes and regime change. During these initial years, the military government was nationalist and populist. This was intriguing from a comparative perspective, because at that time military-authoritarian governments also held power in Argentina and Brazil, and they were following a development model that was internationalist and favorable to foreign capital.

In Peru, the military seized power to break the political stalemate of the 1960s and to implement what were widely perceived as needed reforms. Just days after coming to power, the new government nationalized the

Standard Oil holding that had been a source of great dispute and scandals. This occurred on October 9, the very day we arrived. I recall riding in a taxi from the airport, listening on our tiny portable radio to a curious voice that kept repeating a statement about the nationalization—a voice that, it turned out, was that of President Velasco. Several months later the government nationalized the sugar estates of the north coast, as well as large agricultural holdings in other parts of the country, thereby breaking the back of the Peruvian oligarchy and launching sweeping agrarian reform.

The government thus quickly addressed two parts of the reform agenda that had been stalled in the mid-1960s—involving the issues that had been the focus of the original version of my research project. An elaborate system of popular mobilization was also constructed, called SINAMOS.[7] Although SINAMOS fell far short of what it was intended to accomplish—a not atypical failure with such organizations—the idea of the explicit and very public involvement of the military in popular mobilization was intriguing, and again contrasted dramatically with the experience at that time with the military governments of Argentina and Brazil.

Overall, for my thinking and learning, the experience of being in Peru during this period heightened my interest in the contrasting approaches to policy making and problem solving that emerge under different sequences of national political regimes. A key part of this learning involved the recognition that not only had the democratic regime collapsed at the end of the 1960s, in the midst of dramatic policy failure, but within a few years the military's populist and nationalist initiatives also faltered. This provided the valuable lesson that a key element driving sequences of change in regimes and governments is often policy failure, an idea that would be of recurring concern in my subsequent research.

Research on Authoritarian Regimes and Critical Junctures

Q: You eventually turned your dissertation into a book (Collier 1976). What steps did that involve?

A: In the book version of the squatter settlement project, I went even further in focusing on the connections between regime change and the evolution of policy toward squatter settlements. After completing my fieldwork at the end of 1969, I went back to Peru several times over the next six years and tracked the ongoing evolution of squatter settlements. I gave a number of talks on the project, both in Peru and in the United States, and got further ideas. There is a delicate balance between going back to the field to get enough fresh information to stimulate one's thinking and not getting so much new information that one ends up doing the whole project over again. I guess I came out reasonably well on that balance.

7. Sistema Nacional de Movilización Social—National System for the Support of Mobilization.

In looking at the connections between squatter settlement politics and broader patterns of change, I focused, for example, on a period of paternalistic authoritarianism under an antitrade union populist government, a period that we would now perhaps call neo-liberal, when elite actors sought to encourage autonomy and self-help among the squatters—an approach that anticipated by many years Hernando de Soto's well-known neo-liberal formulation in his book, *The Other Path* (de Soto 1989); a period of sweeping policy commitments during a democratic period of competitive party politics; and finally, in the post-1968 authoritarian period under Velasco, a new combination of elements, involving a focus on self-help and political control.

In this sense, I see the analysis as coming back to the themes that had motivated the initial framing of my dissertation, before I had to change the specific topic. Thus, I was concerned with how different political relationships crystallize around alternative public policies. I also see this analysis as a first step toward my continuing interesting in comparing political regimes, and sequences of regime change.

Q: How did your research interests evolve after your first project on Peru?

A: In the 1970s, I explored quantitative cross-national research as a tool for answering questions about political change. This phase of my work included one of my articles that I am most proud of, which analyzed the historical timing of the adoption of social security programs throughout the world (Collier and Messick 1975). That article, published in 1975, anticipated by many years the current proliferation of diffusion studies. I also wrote a quantitative cross-national article exploring the links between the timing of economic growth and regime characteristics in Latin America (Collier 1975)—taking some inspiration from Alexander Gerschenkron (1962).

Most crucially, it was during this period that Ruth Collier and I first received a National Science Foundation (NSF) grant to support an elaborate project collecting cross-national data on labor law. The Argentine lawyer Lila Milutín provided critical help in this effort. Obviously, labor law did not tell the whole story of state-labor relations in Latin America, but even at the level of formal, institutional data, important patterns emerged that led to our *American Political Science Review* article on corporatism that explored the dynamic interaction between inducements versus constraints in the evolution of state-labor relations (R. Collier and D. Collier 1979; see also

D. Collier and R. Collier 1977). That article, in turn, laid part of the foundation for our later book, *Shaping the Political Arena* (R. Collier and D. Collier 1991). Although *Shaping the Political Arena* centrally involved qualitative comparison, our work during this period played an essential role in pushing us toward a wider comparative view of Latin American politics, as well as generating important hypotheses that would be explored in our subsequent book.

The New Authoritarianism

Q: In 1979, you published a widely read collaborative volume, *The New Authoritarianism in Latin America* (Collier 1979). When did you first encounter the book that was the focus of your volume, Guillermo O'Donnell's *Modernization and Bureaucratic Authoritarianism* (1973)?

A: In late 1972 I learned that this book on regime change by O'Donnell was about to be published by Berkeley's Institute for International Studies.[8] A central theme of his book is the relationship between the rise of authoritarianism in Argentina and Brazil in the 1960s and the difficulties associated with a particular phase of industrial development, and this was the aspect of his thesis that received the most attention. However, other elements were also novel: his emphasis on the absolute, rather than the per capita, size of the modern sector in explaining the rise of authoritarianism; and his effort to move beyond conventional class categories to look specifically at "social roles" as being critical to regime dynamics. Specifically, he emphasized the role of technocrats as a social category, and of the popular sector, which encompassed both the working class, traditionally understood, and important segments of the lower-middle class. The authoritarian coups in Chile and Uruguay in 1973—the same year as the publication of O'Donnell's study—certainly caused many scholars to pay even more attention to his book. At the same time, these new coups raised interesting problems for certain parts of his argument.

Q: How did you convene the group of scholars for the conference that eventually led to *The New Authoritarianism?*

A: In the early to mid-1970s the Social Science Research Council (SSRC) was supporting a working group on the "State and Public Policy in Latin America," with the goal of launching a more formalized, collaborative research project. This initiative, which involved conversations among Albert Hirschman, Guillermo O'Donnell, Fernando Henrique Cardoso, Robert Kaufman,

8. See Chapter 9 for O'Donnell's discussion of *Modernization and Bureaucratic Authoritarianism.*

Julio Cotler, and myself, went through various false starts over a couple of years.

In the summer of 1975, Louis Wolf Goodman, the Latin American staff person at SSRC, phoned me and asked me to completely rethink the project, and to write a proposal that would provide a new, sharper focus for a collaborative effort. I had gotten to know Goodman a year before when I gave a talk on the squatters project at Yale, and ever since, Goodman has been a key professional colleague and friend.

I thought about Goodman's challenge for a while, and I decided it would be productive to do a project specifically centered on the arguments that O'Donnell had advanced in his book about the rise of a new form of authoritarianism. O'Donnell's thesis was an attractive focus because it raised extremely broad issues and sought to explain outcomes of enormous importance, at the same time that it pinpointed specific arguments and was formulated in such a way that it readily suggested intriguing rival explanations. My thinking in formulating the project benefited from consultations with Abraham Lowenthal and Robert Kaufman, as well as with Benjamin Most. He was a particularly talented graduate student at Indiana University, where I was a non-tenured faculty member, and from his own work on incremental budgeting he had astute instincts about the challenges of analyzing discontinuous patterns of change in regimes and in public policy.

Q: The participants in the project were a fairly diverse set of scholars, with some based in the United States and others in Latin America.[9] How was this group of participants selected?

A: Several of the participants were already involved in the prior SSRC working group, and that interaction was reinforced by the fact that in 1975 Albert Hirschman brought a number of Latin Americanist visitors to the Institute for Advanced Study in Princeton, where I was also affiliated. Hirschman strongly encouraged José Serra to bring his expertise as an economist to write specifically on a key part of the economic side of O'Donnell's arguments, and James Kurth was brought in because of his interesting focus on the timing and sectoral shifts in economic growth and their implications for political change.

Q: Edited volumes are usually little more than a collection of disparate papers. Hence, they often do not have much impact. This was clearly not the case with *The New Authoritarianism*. It had a large impact, and

9. The U.S.-based authors of the chapters of *The New Authoritarianism in Latin America* were David Collier, Albert Hirschman, Robert Kaufman, and James Kurth. The Latin American-based authors were Fernando Henrique Cardoso and José Serra, both from Brazil; Julio Cotler, from Peru; and Guillermo O'Donnell, from Argentina.

even though you had a range of authors with their own ideas, the volume held together as a coherent whole. What is the trick to pulling together that kind of edited volume?

A: The challenge is to work extremely hard, to engage in constructive but interventionist editing—including elements of what we now call "developmental editing"—and to do other things that add coherence. I included in the book my own summary of O'Donnell's framework, a summary that many people found illuminating in helping them grasp his complex arguments. This summary, along with an overview of my perspective on the wider debate, was published in *World Politics* (Collier 1978). I created a glossary that, while not aiming at the impossible goal of completely standardizing usage, made it clear how key terms were being used.

Another key element was the strong focus on O'Donnell's book, which as I just noted raised very broad issues, at the same time that it advanced very specific arguments. Indeed, I recall vividly that Guillermo was shocked at the conference that led to the volume, when he fully realized that the discussion among this set of people, some of whom were pretty eminent, was focused so specifically on his book. Further, my framing of the project—and obviously, the framing of O'Donnell's book—focused on an outcome, or dependent variable, that is, the rise of a new form of authoritarianism, on whose occurrence and dimensions the authors were in agreement. So the contributors to the volume had a well-delineated set of outcomes to explain, and that lent itself to coherence.

Shaping the Political Arena

Q: After *The New Authoritarianism in Latin America,* your next major work was *Shaping the Political Arena,* a broadly comparative study focused on the historical roots of divergent political regimes across eight Latin American countries in the twentieth century.[10] Could you discuss the genesis of this book, which you coauthored with your wife, Ruth Berins Collier? How did it link up with your previous work?

A: *Shaping the Political Arena* might be called a "prequel," as opposed to a sequel, in relation to *The New Authoritarianism*—an expression suggested by Jonathan Hartlyn. Thus, the outcomes that in *The New Authoritarianism* were analyzed within a relatively short time frame were explored in much greater historical depth in our subsequent volume. But more broadly, *Shaping the Political Arena* grew out of a convergence of research interests that Ruth and I had developed.

10. The eight countries are Argentina, Brazil, Chile, Colombia, Peru, Mexico, Uruguay, and Venezuela.

My Peru book had focused on regime change. In a short period of time, Peru had experienced a striking spectrum of regime alternatives, as I already noted. After my book on Peru, I had continued to develop my interest in comparing regimes in *The New Authoritarianism,* which sought to account for a broader picture in the pattern of coups, non-coups, and regime outcomes across Latin America in the 1960s and 1970s. So, in all of my prior work, I had been centrally concerned with the comparative analysis of political regimes, which was the outcome to be explained for *Shaping the Political Arena.*

Ruth provided some of the key independent variables. Her first book, which is a remarkable comparative study of the politics of decolonization and post-independence regime change in twenty-six countries of Sub-Saharan Africa, analyzed how different patterns in the introduction of elections and party politics in the period of decolonization were linked to different subsequent trajectories of change, and specifically to the emergence of one-party versus military regimes in the post-independence period (R. Collier 1982a). This work pointed to the value of developing hypotheses about how different forms of participation, mobilization, and control can lead to quite different trajectories of regime evolution. We had begun to explore together, in our work on corporatism starting in 1973–74, some related ideas about how state intervention—involving combinations of mobilization and control, that is, contrasting patterns of inducements and constraints—shaped labor movements in Latin America (D. Collier and R. Collier 1977; R. Collier and D. Collier 1979). These ideas ended up being a central theme in *Shaping the Political Arena.*

Another key source of the puzzle that motivated our book was found in the many single-country studies of party politics, the labor movement, and regime change in the cases we were studying. We were struck by the frequency with which excellent country studies converged in identifying a key period of state building, reform, and often popular mobilization as a formative episode in each country's political history. The rise of Perón in Argentina, the first Vargas period in the 1930s and 1940s in Brazil, and the Cárdenas era in the 1930s in Mexico are obvious examples. Scholars who write on Colombia argue that the 1930s were a turning point, and many experts see the first Ibáñez period—from 1927 to 1931—as a point of inflection in Chile's political history. For each of the eight cases we studied, country monographs highlighted as a crucial watershed the episode we came to designate as the "incorporation period," a period when the state assumed a new role in society, recognized organized labor as a legitimate actor, and, in highly varied ways, sought to institutionalize this role. The first footnote in the book provides an illustrative list of studies that treat these periods as a watershed. We thought it would be an interesting challenge to take what were often inevitably somewhat ad hoc claims about these presumably key transitions, made country by country, and place them in a comparative framework, focusing on the common and contrasting patterns that helped account for distinct starting points, and different trajectories of change.

In the literature on comparative-historical research, there is considerable debate on the use of primary versus secondary sources. Both are important, but we would defend the use of secondary sources on a variety of grounds, including the following: given the large literature for Latin America on topics such as labor movements, political parties, and regime change—spanning many decades in the twentieth century—it would be a great scholarly and intellectual loss if this rich "data set" were not used in comparative research. Careful research with secondary sources specifically meets this challenge. I should add, however, that between the two of us we had also spent time in most of these countries, and we did not merely know them through the secondary material.

Q: During the time you were working on this book there was a great interest in broad comparative-historical work. How did this intellectual context affect you?

A: As we began working on *Shaping the Political Arena,* we had just moved to Berkeley, and I cannot emphasize enough how important Berkeley was in convincing me that it was feasible to carry out a complex, comparative-historical project. Perhaps we can return to this theme later in the interview. Beyond the immediate influence of being at Berkeley, Theda Skocpol's *States and Social Revolutions* (1979) had just appeared and was the focus of much discussion. Cardoso and Faletto's *Dependency and Development* (1979)—sections of which I had read in Spanish in Lima in 1969—became available in English and was being widely used in graduate seminars. Barrington Moore's (1966) *Social Origins of Dictatorship and Democracy,* which I can remember debating in the 1960s, achieved new salience and received much attention in this intellectual environment. All this provided strong encouragement to pursue an ambitious comparative-historical study.

Q: Could you discuss the research process itself?

A: The comparative analysis for *Shaping the Political Arena* evolved in an iterative fashion as we learned more about the cases. We sought to be explicit about what we meant by a critical juncture and how it was to be identified, and about what we meant by the incorporation, aftermath, and heritage periods. The book has a glossary that explores how these categories can be applied across diverse national contexts and diverse historical periods. Further, Chapter 1 lays out what we called the "Critical Juncture Framework," and I would like to think that it played a valuable role in helping to get the literature on this topic started within political science.

The central argument concerned the dialectical interplay between efforts to mobilize and/or control the labor movement in the incorporation period, and contrasting patterns of mobilization and control in the subsequent periods that we call the aftermath and the heritage of incorporation. These distinct phases, which James Mahoney (2000, 509) has subsequently labeled "reactive sequences," were demarcated by episodes of regime change, the collapse of governments, and policy failure—parallel to those that had originally struck me in Peru.

One of our key insights was that, through these sequences of change, for some cases a greater focus on control in the earlier period yielded more intense mobilization later on, whereas in other cases a greater emphasis on mobilization in the early period later yielded a greater capacity for control. These forces of mobilization and control played a key role in the period of social and economic crisis, and regime change, in the 1960s and 1970s.

We devoted close attention to considering rival explanations and weighing them vis-à-vis available evidence. We did not want to develop a lockstep argument, which claimed that the critical junctures set countries on consistent and unambiguous trajectories of change. Rather, we considered other explanations that potentially deflected these trajectories, or possibly reinforced them. We also tried to weigh carefully rival accounts, presented in country monographs, of historical episodes of major significance to our analysis. These included, for example, the question for Peru of President Leguía's relationship to the labor movement in the late 1910s and early 1920s, debates about the timing of the incorporation period in Argentina, and alternative interpretations of the 1964 coup in Brazil.

Given that we looked closely at these rival interpretations and tried to weigh the evidence with care, one of my regrets is that we did not include an appendix that explicitly discussed these rival explanations, and how we resolved them for the purpose of our analysis. Such an appendix would be a valuable addition to many works in the comparative-historical tradition.

Writing this book was a challenging undertaking, and it took much longer than we had intended. We worked on *Shaping the Political Arena* for ten years and got help along the way from a group of outstanding Berkeley graduate students, above all, James McGuire and Ronald Archer. Would I recommend that other scholars take on such a complex analysis? Well, it certainly would not be a good idea for assistant professors!

Q: What was the division of labor between you and Ruth Collier?

A: In discussing the division of labor, let me first report the insight of my Berkeley colleague Nelson Polsby. He observes that when he has coauthored research, both of the authors have done 75 percent of the work. That is a pretty good description of what happened with *Shaping the Political Arena.*

More specifically, I did the basic work on Argentina, Colombia, Peru, and Uruguay, and Ruth focused on Brazil, Chile, Mexico, and Venezuela. I had been interested in the Peru-Argentina pair for some time, specifically since my visit to Argentina while I was doing my dissertation research in Peru. There seemed to be many interesting parallels, as well as contrasts, between them. Both countries had a similar recurring dilemma concerning the banning of their labor-based party, APRA in Peru and the Peronists in Argentina, and appeared to have had parallel regime crises over several decades. Colombia and Uruguay also emerged in our comparisons as an interesting pair of cases, given the strong role of traditional parties and the specific role of these parties in the legitimation and institutionalization of the labor movement. Ruth from an early point became interested in the Mexico-Brazil comparison, and in fact published an article on that comparison early in the project (R. Collier 1982b). From there she expanded her focus to Venezuela and Chile.

Q: The book compares eight Latin American cases. Did you start out with those cases? Did you consider any non-Latin American countries?
A: We had originally planned to include Bolivia and Cuba, which in an earlier period were fascinating cases of labor populism. But it became clear that the eight cases we ended up with were enough of a challenge in terms of our time and energy. Regarding non-Latin American countries, in our initial effort to create a quantitative, cross-national data set on corporatism we scored various countries elsewhere in the world, but including them in the analysis seemed far beyond the scope of what we could possibly accomplish.

Q: Why didn't you publish a short article version of the arguments in *Shaping the Political Arena?*
A: Through the 1980s I either authored or coauthored with Ruth eleven conference papers that presented parts of the book. But we ended up not publishing any of these conference papers, because none adequately captured the overall argument. After we finished the book, we were invited to join an SSRC project with the idea of contributing a summary chapter on the book, but the project never took place. However, Ruth was more bold—and perhaps more talented—than I in writing such summaries. She published an early version of the Brazil-Mexico part of the argument in 1982, and wrote a brief summary of the book's argument in her 1993 article on the impact of internal and external factors on regime change in Latin America in the 1940s. She also introduced many of these ideas in her 1992 monograph on Mexico (R. Collier 1982b, 1993, 1992).

I should add that we did present concise summaries of the argument in the initial "Overview" chapter, as well as in the concluding chapter. Furthermore, after the initial publication by Princeton University Press in 1991, the University of Notre Dame Press reissued the book in 2002, with a splendid introductory statement by Guillermo O'Donnell. For this edition, Ruth and I wrote a three-page authors' note in which we do provide a very condensed summary of the argument (R. Collier and D. Collier 2002). Yet I have been somewhat hesitant about publishing a summary article of our book.

My hesitation about publishing a brief summary is partly connected with the kind of empirical data employed in work of this type. The evidence in major respects takes a narrative form—although, at least in our aspiration, a tightly constructed and analytically focused narrative. It simply takes a lot of space to nail down the arguments for particular countries. In our case, we covered a period spanning the first decade of the twentieth century to the 1980s. So we ended up doing a long, elaborate analysis with a strong grounding in narrative treatment, focused on the evolution of these countries through what might variously be counted as five or six historical phases. It would have been more convenient if the book could have been shorter,[11] because that would have made the argument more accessible.

Q: How was *Shaping the Political Arena* received?
A: It received uniformly excellent reviews, and I think the book stimulated many scholars to think about the critical juncture framework, to apply it in their own research, and to work out different parts of our argument for various countries. That is the kind of reception one wants to have. As mentioned before, I know that many scholars have liked Chapter 1, where we lay out the critical juncture framework. I think they appreciate our clarity in discussing critical junctures and path dependence,[12] and also our explicit engagement with rival explanations, through laying out a multivariate perspective on critical junctures. More than a few works of comparative-historical analysis are not attentive to rival explanations, and I think this is essential. Indeed, one of the members of the committee that gave the APSA Luebbert prize to the book specifically commended us for the methodological care with which the arguments were constructed.[13]

11. *Shaping the Political Arena* is 877 pages in length.

12. A critical juncture is a period of crucial change in the history of a given country or other political unit that is hypothesized to leave a distinctive legacy. Path dependence refers to the distinctive trajectories of change, within which the range of political alternatives is constrained by the way the critical juncture occurred.

13. In 1993 *Shaping the Political Arena* was awarded the Gregory M. Luebbert prize for the

Yet there are always frustrations. In the minds of many scholars, the arguments of well-known books too often get reduced to a phrase or slogan. For Barrington Moore—and thus, indirectly, for Gerschenkron—it may be "No bourgeoisie, no democracy," which was only a small part of a vast comparative panorama. For O'Donnell, it is probably "the deepening hypothesis,"[14] which is only one element in a complex, multi-variate argument. In our case, it is "labor incorporation matters." One might speculate that this simplification is in part our punishment for writing an 877-page book. However, this is a standard experience even with shorter books, as with Moore's and O'Donnell's.

Research on Concepts and Methods

Q: After *Shaping the Political Arena,* you started to publish a stream of articles on concepts and methodology. When and why did you decide to focus on methods?

A: My interest in methodology had various sources, as I have already noted, beginning early in my career. The work I did at Indiana—in part with Ruth Collier—included a significant concern with concept analysis, in that both the research on corporatism and on bureaucratic authoritarianism explored the value of working with key concepts at a more disaggregated level. And I should note that the conceptual work on corporatism grew out of a quantitative project, whereas my efforts with the conceptualization of bureaucratic authoritarianism were connected with a line of research that was primarily qualitative. I see my work on concept formation as highly relevant to both traditions.

But it was the experience of coauthoring *Shaping the Political Arena* that decisively led to a sustained period of writing on methods. That book raised many methodological questions. How was one to make viable comparisons of incorporation periods? This required a conceptualization of these episodes that yielded a plausible sorting of cases—both across countries and over time. Yet this sorting was complex, because the whole point of the analysis was that these incorporation periods occurred in different ways. Thus, the analytic categories had to accommodate both similarities and crucial differences. In a world of scholarship sometimes too sharply divided between lumpers and splitters,[15] the book thus sought a middle ground.

"Best Book in Comparative Politics" by the APSA (American Political Science Association) Organized Section for Comparative Politics.

14. This refers to a phase of industrial growth in which countries begin to produce intermediate and capital goods.

15. Lumpers tend to assume that differences are not as important as broad similarities. In contrast, splitters tend to place more emphasis on the ways in which phenomena differ.

Further, it was essential to ask whether these analytic categories were crisply bounded. Alternatively, were they ideal types, which our historical cases approximated to varying degrees? Correspondingly, were we working at a categorical, nominal level of measurement, or were our categories ordinal? Obviously, if these descriptive claims are not addressed satisfactorily, explanatory claims are on shaky grounds. Further, one may ask to what extent the causal inferences are derived from the cross-tabulation of cases presented, in the case of *Shaping the Political Arena* in the summary tables, and to what degree they rely on our in-depth knowledge of cases, involving what Alexander George calls process tracing (George and McKeown 1985, 35). I increasingly came to think that the summary tables are crucial in setting up the problem, but the fine-grained, within-case analysis is the most important source of explanatory assessment.[16]

The experience of addressing these questions led me to conclude that comparative-historical research has a weak foundation if it is not attentive to issues of method. These questions therefore pushed me to explore further basic problems of concept formation, comparison, and measurement validity.

In earlier methodological writing, there had been an explosion of interest in the comparative method in the 1960s and early 1970s, but that tradition of writing had substantially lapsed. My initial attempt to revive this tradition was my article "The Comparative Method: Two Decades of Change," in which I sought in part to extend Arend Lijphart's (1971) inventive juxtaposition of the case-study method, the small-N comparative method, and the statistical method. I strongly emphasized, as I have ever since, the need for dialogue among these methods. I recall vividly first presenting this paper at a conference at the CUNY Graduate Center in New York, a conference that led to the edited volume in which the article appeared (Collier 1991).[17] Giovanni Sartori and Gabriel Almond were both in the audience, and their enthusiasm for my presentation strongly encouraged me to push ahead with this line of methodological writing.

Q: A major theme of your work on methodology is the analysis of concepts and concept formation. What are some of the main points you would highlight from this work?

A: I have been concerned with juxtaposing two traditions of concept analysis. One, which in political science is strongly identified with Giovanni Sartori, is invaluable for bringing analytic rigor to the research—whether qualitative or quantitative—of the individual scholar. This tradition focuses

16. Within-case, in contrast to cross-case, analysis considers a case over time or across subunits.

17. A more fully developed version of this article was published as Collier (1993).

on developing carefully defined concepts; addressing the problem of overlapping concepts and of confusions in the relationship between term and meaning; achieving a sharp delineation of the crucial interaction between the elements of meaning (i.e., the intension) and the range of cases that appropriately correspond to the concept (i.e., the extension); and viewing this interaction in terms of hierarchies of concepts—as in the Weberian hierarchy in which authority is a specific type of domination, and charisma is a specific type of authority (Sartori 1970). I should especially add that this idea of hierarchies continues to be crucial to my work.

The other tradition is concerned with the complexities of concepts and conceptual change across a wider community of scholars. This approach is more centered on the recognition that the use of concepts is often confused—and follows patterns that can be detected only with strong analytic tools. Communication across different usages can be a major challenge; these problems can seriously interfere with scholarship and with the accumulation of knowledge; and the problems can potentially—but not readily—be resolved by "legislating" appropriate conceptual usage. Within political science, a key statement of this view is found in W. B. Gallie's (1956) idea of "essentially contested concepts." I have sought to address this tradition as well—indeed I am now involved in a project on Gallie's legacy—and my thinking about the complexity of concepts is also strongly influenced by what might be called the Berkeley School of cognitive linguistics, within which Eleanor Rosch was a pioneer. George Lakoff's (1987) extension of this tradition in his writing on concepts that are "radial," as opposed to hierarchical, provided a stimulating contrast with Sartori's view of conceptual hierarchies. This branch of cognitive linguistics does not argue that the use of concepts is necessarily confused; indeed, concepts routinely follow highly regularized patterns. But this approach maintains that their internal structure is complex, and that we must understand that complexity if we are to work effectively with concepts.

I pursued these themes in a series of coauthored pieces. My *American Political Science Review* article (Collier and Mahon, 1993), "Conceptual Stretching Revisited," explores the interplay between these two traditions. I had initially thought it was appropriate to view some concepts as "radial," and others not, but my subsequent work suggested that this is not a productive distinction, and that all concepts tend to have some radial elements. One example of a more productive avenue, closely related to the idea of radial concepts, is the idea of "diminished subtypes"—as in illiberal democracy—which I introduced in the article on "Democracy with Adjectives" (Collier and Levitsky 1997). These subtypes keep the analytic categories within the larger framework of debates about democracy, while recognizing that within that framework, the cases being analyzed are not fully democratic. And crucially, these types and subtypes are not related to one another within the framework of a Sartori-type conceptual hierarchy, but rather a hierarchy of what may be thought of as part-whole relations.

Other articles sought to make strong connections between problems of concept formation and issues of measurement, offering a systematized understanding of the interaction between disputes about concepts and choices about measurement. One of these pieces explored the interaction between conceptual disputes over the idea of democracy and choices about whether the concept is operationalized in dichotomous or graded terms (Collier and Adcock 1999). A subsequent article sought to offer an integrated framework, addressed to both qualitative and quantitative research, for looking at the interaction among conceptual disputes, choices about measurement, and alternative conceptions of validity—again using as a running example different studies of democracy (Adcock and Collier 2001).

Of course, the payoff of this kind of work with concepts is its contribution to improving substantive research. Much of political science is concerned with evaluating explanatory claims, yet if concepts and measures are muddled, this cannot be done in a coherent way. For example, my earlier concern with disaggregating key concepts—that is, authoritarianism and corporatism—sought to strengthen scholars' capacity to enter ideas connected with these concepts into explanatory claims. The "Democracy with Adjectives" piece similarly addressed the substantial conceptual confusion found in the literature on democratization. In this literature too many scholars are careless about definitions, and the literature has produced a startling proliferation of democratic subtypes—that is, democracy with adjectives. Such problems can paralyze efforts to assess the causes and consequences of democracy, and of different kinds of democracy. Subsequent articles also addressed issues that require central attention if causal inference is to be a viable enterprise: for example, choices about treating particular concepts in dichotomous or graded terms, as well as the need to offer a framework for establishing measurement validity in light of disputes over the meaning of concepts.

With regard to substantive payoff, I should add that articles on concept analysis written by former graduate students of mine have looked closely at how conceptual confusion can lead to problems in causal inference, including analyses focused on the concepts of institutionalization, peasant, and democracy (Levitsky 1998; Kurtz 2000; Elkins 2000). I would also note that concept analysis is now receiving more attention in political science, for example, in the work of the modeler at Rochester, James Johnson (2003).

Q: In 2004 you published an edited volume with Henry Brady, *Rethinking Social Inquiry*. What are the main arguments of that book?

A: In *Rethinking Social Inquiry* I seek—in joint authorship with Henry Brady and Jason Seawright, and in collaboration with other scholars—to offer a new view of the relationship between qualitative and quantitative methods. Let me spell out that idea in some detail.

Over the past two or three decades, the approach we call "mainstream quantitative methods"—based on regression analysis and econometric refinements on regression—has become hegemonic in some fields of political science. Simultaneously, alternative analytic tools identified with qualitative methods have been gaining importance. I have tried to play a strong role in supporting this latter development, and clearly quantitative and qualitative methods both require close attention. As the subtitle of our book puts it, in light of these *diverse tools* for political analysis—quantitative and qualitative—the challenge is to find *shared standards* for alternative approaches.

This search for shared standards vis-à-vis quantitative and qualitative approaches led us to explore the distinctive contribution of "statistical theory," understood as a broad set of tools for reasoning about evidence and inference. Whereas statistical theory might conventionally be thought of as providing the basic rationale for mainstream quantitative methods, in fact some statistical theorists are skeptical about quantitative causal inference based on observational, as opposed to experimental, data in the social sciences. In the analysis of observational data, these statistical theorists sometimes consider qualitative tools to be superior to quantitative tools for resolving certain kinds of analytic and methodological problems—or at the very least, they believe that qualitative methods make a contribution that is different in kind, but equal in importance.

A key goal in our book is to draw a parallel between "data-set observations," which are the empirical foundation of quantitative research, and what we call "causal-process observations," which are pieces of data that provide information about context and mechanism, and which contribute distinctive leverage in causal inference. In discussions of methodology, the idea of an "observation" has a very specific and special meaning.[18] We sought both to underscore the idea of an observation as the basic feature of a standard quantitative data set, and to link this idea to the insights that derive from qualitative data. Causal-process observations can be drawn from case-study research, from what Alexander George calls "process tracing"—which I already mentioned in discussing *Shaping the Political Arena*—and from what Richard Fenno (1977, 884) calls "soaking and poking."[19]

This juxtaposition of the ideas of data-set observations and causal-process observations casts in a different light, for example, earlier debates on the "many variables, small-N" problem (Lijphart 1971), as well as more recent discussions of "increasing the number of observations" as a means of enhancing inferential leverage in social science research. With a focus on causal process observations, the idea of the "N," and of increasing the number of observations, takes on a different meaning—a meaning that points clearly to the strengths of qualitative research.

18. In a standard "rectangular data set," with rows corresponding to cases and columns corresponding to variables, by conventional usage an "observation" is a row in the data set, involving all of the values for a single case.

Q: Do you see *Rethinking Social Inquiry* as a defense of qualitative methods?

A: The book is not intended to defend qualitative methods. It explores strengths and weaknesses of both qualitative and quantitative approaches, and might better be understood as an effort to "level the playing field" in relation to these alternative sets of tools.

First and most fundamentally, we are concerned with trade-offs between qualitative and quantitative approaches. We disaggregate the qualitative-quantitative distinction into four closely interconnected dimensions—level of measurement, size of the N, whether statistical tests are employed, and whether the study is characterized by thin or thick analysis—the latter in the sense of employing detailed case knowledge. In relation to these dimensions, researchers encounter major trade-offs, with the analytic strengths of each tradition being balanced against weaknesses of the other tradition. Thus, by the definition of levels of measurement, a higher level of measurement provides more information about cases; but substantively rich typologies, utilizing case-based knowledge and categorical variables—that is, nominal scales, and thus a lower level of measurement—may provide insight of quite a different kind. Working with a large N can have enormous advantages, but it often comes at the cost of case knowledge. Statistical tests are a powerful analytic tool, but they depend on having good data and on meeting complex assumptions that underlie such tests—both of which may be hard to achieve. Thin analysis is closely connected with the second criterion, having the great advantage of permitting the examination of a large N. But, by definition, thin analysis lacks the close knowledge that is a great advantage of much qualitative research. Each approach has major strengths

19. George's process tracing is a method for identifying and testing causal mechanisms. Fenno's soaking (up information) and poking (into corners) refers to research that relies heavily on fieldwork, observation, and interviewing geared to obtaining an in-depth and close-up sense of the processes being studied.

and weaknesses, and, frankly, it is simply not appropriate for advocates of either tradition to make a special claim of analytic virtue.

Second, our goal in introducing the idea of causal process observations was in part to underscore the wider importance of a basic research procedure in qualitative analysis, but we also sought thereby to push qualitative researchers to situate their approach to causal inference in a more rigorous framework. For example, we tried to delineate carefully the differences between the two types of observations, and to encourage qualitative researchers to think carefully about the analytic consequences of introducing additional observations of each type, and also of introducing additional variables, within a given analysis (Collier, Brady, and Seawright 2004, 253 and 259).

Researchers should feel substantial humility in doing both quantitative and qualitative research. Producing meaningful and interpretable results from regression analysis can be just as hard as interpreting a case study. Quantitative methods do not have a special monopoly on analytic virtue, any more than case studies do. But I would emphasize that at this point in the evolution of political science, when I think the playing field is still not level, important parts of the discipline need to recognize more fully the limitations of quantitative methods.

Q: *Rethinking Social Inquiry* is in part a critique of King, Keohane, and Verba's *Designing Social Inquiry* (1994). How would you summarize their contribution and the relationship between the two volumes?

A: King, Keohane, and Verba's book has played an invaluable role in consolidating and legitimizing mainstream quantitative methods, the approach we have just been discussing, as well as stimulating an entirely new level of methodological debate and awareness among qualitative researchers. This is an enormous contribution. Still, we think the quantitative template that these authors seek to impose onto qualitative research has many problems, and it is hardly a panacea for the numerous challenges faced in qualitative work. To take one example, their idea of a "determinate research design" strikes us as misleading and unfortunate—even when applied to quantitative research. It implies a level of definite knowledge that we routinely do not achieve. We prefer to ask whether a research design is "interpretable" (Brady and Collier 2004, 292), a label that strikes us as far less rigid and much more helpful. Relatedly, I think many statisticians would feel that many aspects of King, Keohane, and Verba's advice about selection bias are not meaningful in a context in which analysts are not conducting their research in relation to a well-defined population. In comparative research, different combinations of cases routinely yield different findings, and if one assesses a given finding in relation to a larger N, there is no way of knowing

if differences in findings reflect substantive differences among the cases, as opposed to selection bias, conventionally understood.

One of the insightful comments I have received about *Rethinking Social Inquiry* was from a Latin Americanist who commented that the book reminded him of my earlier volume, *The New Authoritarianism*. Indeed, my goal with both projects was to critically engage a prior book that had crystallized a debate at a high intellectual and scholarly level, was sharply focused in a way that gave tight structure to that scholarly debate, yet was broad enough to have many implications beyond the immediate arguments of the book. King, Keohane, and Verba's book plays this role in relation to *Rethinking Social Inquiry*, just as O'Donnell's book did in relation to *The New Authoritarianism*. The parallel extends to the fact that both of my books include a new—and many people feel extremely helpful—summary of the prior volume, as well as a glossary of terms intended to help establish a shared framework of discussion. And with both books, I engaged in energetic, interventionist editing of the component chapters, adding—I believe—greater coherence.

I emphasize this parallel between my two books for a specific reason. Some researchers believe that the best way to advance scholarship is to take audacious positions that claim a great deal, and that give the appearance of dramatically moving the discussion forward. I find this approach unappealing. I would rather be identified with research that is fair and balanced, that addresses issues of real analytic importance, but that does not pretend to accomplish things that, in fact, it does not accomplish.

Q: How is your research on methodology related to your substantive interests?

A: Methodological work is most useful when driven by substantive questions that scholars care about. So I have tried to keep my writing on methods closely linked to themes I have long been interested in: corporatism, regime change, democracy, and recent shifts in our conceptualization of democracy—from narrower, procedural issues of regime, to broader questions regarding the character of the state, the rule of law, and citizenship. These links can be seen in my piece on corporatism (Collier 1995), the "Democracy with Adjectives" article (Collier and Levitsky 1997), the "Democracies and Dichotomies" piece (Collier and Adcock 1999), and my *American Political Science Review* piece on measurement validity (Adcock and Collier 2001). Methodological work disconnected from substantive issues can get stale. Methodologists should stay close to areas where they have a sense of interesting research questions. Connecting methodological and substantive issues also specifically motivates other scholars to pay more attention to methodology. For example, *The New Authoritarianism* raised many methodological issues, and these issues captured researchers' attention because it was a book that I think was compelling in substantive terms.

Q: Are you currently involved in other projects on methodology?
A: Henry Brady and I continue to be strongly focused on basic themes raised in *Rethinking Social Inquiry*. One major theme, which Henry is pursuing, concerns the logical foundations of causal inference and the contributions of statistical theory to illuminating these foundations. I am continuing my work on conceptualization and measurement that we have just discussed. I continue to be interested in how we can most effectively take on complex concepts, recognize their complexity, and yet ground both qualitative and quantitative research in careful empirical work about which we can make at least reasonably sound claims concerning measurement validity. Dealing with these issues through an escape into a kind of operationalism is not the solution,[20] and I am trying to work further on finding better solutions.

In a closely related initiative, Jason Seawright and I are doing research that seeks to integrate four measurement traditions among which there is virtually no communication and no mutual recognition: axiomatic measurement theory, the pragmatic approach to measurement, structural equation modeling with latent variables, and what we are calling the case-based approach to measurement. We hope through this work to take steps toward placing conceptualization and measurement in comparative politics and political science on a sounder basis.

Science and Values

Q: Do you see yourself as a scientist?
A: *Science* is a label that carries a forceful positive valence in today's world of research funding, and for that reason one has to take it seriously. I consider it a significant transition for the Training Institute on Qualitative Research Methods of Arizona State University when it went beyond a modest National Science Foundation (NSF) seed-money grant to regular NSF funding.[21] There are substantial payoffs to doing work that others consider science, and to the extent that this imposes pressure to achieve more rigor in research, I think it is all for the good.

At the same time, within the natural sciences, "science" consists of such diverse practices that it is perhaps not a helpful label. It is more helpful to

20. Operationalism is the doctrine that the meaning of theoretical terms is located in the indicators employed to measure them.

21. This Training Institute runs an annual course on qualitative methods for graduate students. On the institute, see www.asu.edu/clas/polisci/cqrm/institute.html.

claim that certain lines of work in political science respond effectively and rigorously to specific challenges of research—as in the four dimensions of qualitative versus quantitative analysis discussed above. But to label the whole enterprise too definitively, and to insist that the whole enterprise is science—or should be science—well, I think it may be useful for fundraising or propaganda, but it is not very informative. The story has it that as a result of a log-roll among the authors of the King, Keohane, and Verba volume (1994), the subtitle ended up being "Scientific Inference in Qualitative Research." As you know, I think that volume makes a large contribution, but that it makes claims about rigor that cannot be sustained. To tout these claims as "science"—well, I think that is enough said.

Q: What roles do values and normative commitments play in your own research and in the research you consider important?
A: Scholars in comparative politics are routinely motivated by normative concerns when they study authoritarianism, human rights violations, inequality, poverty, and the collapse of the rule of law. The same is true in the international relations field for researchers who study the tragedies of war and other kinds of international conflict, and in the American politics field for analysts who focus, for example, on distortions in the U.S. system of elections and legislative representation, distortions that diminish democracy. Normative issues are always present, not only in the topics we study, but sometimes—more awkwardly—in the topics we fail to study. It is essential to emphasize that these normative sources of analytic agendas are fundamental in all political research. And these normative concerns should definitely not be seen as a departure from what we would routinely think of as rigorous analysis in the tradition of empirical political science. Of course, we will encounter disputes about the normative weighting of different issues and problems; and normative agendas should not lead to predictable research findings that merely serve to validate the normative commitments of the researcher. The "value-free social science" component arises in part when this empirical research can yield unexpected, and sometimes unwelcome, findings—in the sense of Max Weber's (1946a, 147) "inconvenient facts." But we cannot for a moment imagine that we are in an enterprise that is not normatively driven.

Colleagues, Collaborators, and Students

Colleagues

Q: You taught at Indiana University from 1970 until 1978. Who were the colleagues that influenced you most at Indiana? What kind of environment was it?

A: I must underscore my great debt for the support I received for both my substantive and methodological interests at Indiana. Eleanor and Vincent Ostrom founded their now famous Workshop in Political Theory and Policy Analysis in 1973, and Dina Zinnes and John Gillespie were engaged in mathematical modeling of international conflict. Simultaneously, Indiana had a robust areas studies tradition, and I will always be grateful—for example—for the friendship of the West Europeanist Alfred Diamant. Interestingly, given my emerging interests at the time, Diamant had written a book on corporatism in Austria (Diamant 1960).

During this period—both at Indiana and nationally—there was strong intellectual support for exploring the contributions of quantitative comparative research to understanding political change. For example, at Indiana Ronald Weber in political science was working on quantitative comparisons of state politics within the United States (Weber and Shaffer 1972), and in the Sociology Department Philips Cutright was engaged in quantitative cross-national research on development (Cutright 1963). These colleagues helped spark my interest in such comparisons.

Q: In 1978, you moved to Berkeley, where you have been ever since. What was Berkeley like? How did the colleagues you encountered influence your thinking?

A: Moving to Berkeley pushed me toward a strong interest in comparative-historical work. Reinhard Bendix, who had previously been in the Berkeley Sociology Department, had moved over to political science. In 1964 he had published *Nation Building and Citizenship* (Bendix 1964), which was extremely significant for my book with Ruth, *Shaping the Political Arena,* and just after Ruth and I arrived to Berkeley, his *Kings or People* (Bendix 1980) came out, and it was widely discussed in our department. In sociology, Neil Smelser had long been interested in historical analysis, as had Victoria Bonnell, who had studied with Barrington Moore at Harvard. The two of them coordinated a faculty seminar that gave substantial attention to issues of historical analysis, and it was in that seminar that I first read Theda Skocpol and Margaret Somers's invaluable article, "The Uses of Comparative History in Macrosocial Inquiry" (1980). At the Berkeley Institute of Industrial Relations, Clark Kerr was a towering presence, and he was coauthor of the classic study *Industrialism and Industrial Man,* which gave prominent attention to the incorporation of labor into national political institutions at a critical historical moment (Dunlop et al. 1960). Harold Wilensky's bold cross-national work on welfare states underscored the feasibility of broad comparisons.

On the conceptual side, Hanna Pitkin's compelling analysis of ideas such as representation, justice, reification, and utility set a high standard for careful conceptualization (Pitkin 1967). Ernst Haas's work was an intellectual anchor in efforts to rigorously conceptualize politics across a wide range of nations and historical periods, and Kenneth Jowitt's (1992) work on Leninism was a model for creative concept formation. I attended Jowitt's lectures on Leninism, and invited him to my methodology class to give his basic Leninism lecture as an exemplar of creative work with concepts.

Also, a few years after we arrived, Berkeley hired Gregory Luebbert in political science. Around the time *Shaping the Political Arena* was crystallizing as a book project, Luebbert was doing closely parallel historical work on Western Europe (Luebbert 1991). He was a key colleague. We began working on our books independently and did them in quite different styles. But, in certain substantive ways, the arguments are quite similar. There was cross-fertilization between the projects through many conversations with Luebbert, through joint seminars, and through extensive interaction with graduate students. Tragically, Luebbert died in the late 1980s in a whitewater boating accident. His manuscript was almost finished, and Giuseppe Di Palma made a crucial contribution in pulling it together. I had the bittersweet experience of writing a preface for the book with Seymour Martin Lipset, with whom Luebbert had worked at Stanford. Luebbert's death was an incredible loss for the discipline and for the Berkeley department, and I feel that we never regained that ground until we hired Paul Pierson from Harvard in 2004.

Overall, Berkeley was a setting where comparative-historical work was valued, and there was a sense that it was possible to do such work in a way that was conceptually innovative, analytically rigorous, and at the same time close to the cases. These criteria set a standard that we tried to meet in *Shaping the Political Arena.*

In discussing intellectual and institutional support for *Shaping the Political Arena,* let me mention two other debts, the first to Sanford Thatcher, then of Princeton University Press, which published our book, and now director of Penn State University Press. For several decades he has been the leading figure, in the world of publishing, in terms of support for the Latin American field, and dozens of scholars—including the Colliers—owe him a large debt for his dedication, patience, insight, and professionalism. In our electronic age, editors and book publishers still matter a lot. In addition, I also have a large debt for the intellectual support provided by the remarkable community of faculty, postdoctoral scholars, and graduate students at the Kellogg Institute for International Studies at the University of Notre Dame. During the past twenty years, Kellogg has played a singularly exciting role in supporting the kind of normatively informed and rigorous comparative social science that I care about.

Turning to the past ten years, I would underscore Berkeley's importance as the setting in which we ultimately wrote *Rethinking Social Inquiry.* In two successive years, Henry Brady and I jointly taught a methodology course in the late 1990s, which in turn pushed us to develop new perspectives on methods and on methodological debates. The Berkeley statistician David Freedman was a source both of broad ideas about the application of statistical theory and of meticulous help with editing—so meticulous that he once threatened to edit the menu at a restaurant where we were having dinner. In this setting, a number of graduate students were stimulated to pursue advanced training in statistics and methodology, and from among these students Jason Seawright ended up being coauthor of major portions of *Rethinking Social Inquiry.* Henry Brady has been a strong supporter of the Institute on Qualitative Research Methods, coming for two days every year to talk about our book and to address issues of causal inference, conceptualization, and measurement.

Collaborators

Q: You have done a lot of collaborative research. Why do you collaborate?

A: Well, I said before that in coauthoring with Ruth Collier, we both did 75 percent of the work. With that much effort going into the research, the result is likely to be of better quality. Correspondingly, the collaboration that went into not only *Shaping the Political Arena,* but also *The New Authoritarianism* and *Rethinking Social Inquiry,* produced something that was more than the sum of the parts. Further, I have had the good fortune of spending most of my academic career at Berkeley. We have extremely good students—and I dare say that Ruth and I are pretty well known in the department for pushing our students hard, so they come out of Berkeley with many newly acquired skills and talents. I have written extensively with graduate students: it makes my work better, and it launches them on a strong research trajectory.

Many scholars who are committed to collaborative research, and who do not have the good fortune of such good graduate students, may be more oriented toward writing with colleagues at other institutions, and this can work just as well. One of the striking features of academic life in the United States—as opposed, I think, to most other countries in the world—is that there are hundreds of universities and colleges where scholars can maintain an active research program, along with their teaching. This enriches scholarship for all of us, and for any given researcher it yields many opportunities for collaboration with scholars at other institutions.

In my experience, collaboration and coauthoring is a way to pool knowledge and combine different skills. For example, one contributor might know qualitative methods and the other might know quantitative methods. The quantitative person will argue, "No, you can't say it that way or everyone will know it's wrong." The qualitative person will say, "But that's not how qualitative researchers think about the problem. If you say it that way, they won't believe you at all." In working on *Rethinking Social Inquiry,* I was collaborating with scholars whose knowledge of statistical theory went far beyond mine. The statistician David Freedman, though not a coauthor, was a key intellectual contributor to the project. I once asked him what could motivate him to collaborate with someone like me, who does not have a broad knowledge of statistical theory. He responded that it is because I have "sechel"—Yiddish for "common sense," or "smarts." Perhaps it is a nice thing that at this late date in the evolution of the social sciences, a prominent statistician should think that common sense is important.

Students

Q: What is your approach to training graduate students, to their selection of dissertation topics, to coauthoring with them, and to supporting their professional development after their Ph.D.s?

A: Ruth Collier and I collaborate closely in training and mentoring students. Over the years we have routinely co-chaired dissertation committees, and we both work very, very hard at it. I should add that this question within your interview—indeed, an important part of this interview—should be seen as being as much about Ruth's work as about mine.

The way students pick a dissertation topic varies a lot. Some students go out on their own and select topics completely different from what Ruth and I do. We learn a lot from those dissertations. Other students focus on topics that are extensions of themes we have worked on. It varies a lot, and it is good to have both.

On coauthoring with graduate students, I do this extensively. It serves as a research apprenticeship, and gives them writing experience. I also have often given sustained, intensive feedback on graduate student articles that virtually amounts to coauthorship, but in situations where I want the student to get all the credit for the article. Hence, I am not listed as coauthor. Regarding involvement in professional development after the Ph.D.—well, let me simply say that Ruth and I work very hard indeed in supporting the intellectual and professional development of our former students.

Institutional Initiatives

The Comparative Politics Section of the American Political Science Association

Q: During the past several years, you have been actively involved in several capacities in the American Political Science Association (APSA). I would first like to discuss your role in the Comparative Politics Section, of which you were president during 1997–99. What is your impression of the section?[22]

A: The field of American politics is well represented in APSA, scholars in international relations have the International Studies Association (ISA), and area studies associations are essential for some comparativists. But, until the Comparative Politics Section was started in 1989, comparativists had no overarching organization, giving the organized section a crucial role. Soon after it was launched, the section quickly became the largest organized section of APSA, by a wide margin. It has a fine newsletter that started at the University of Washington, prospered at UCLA, and is now prospering at Notre Dame. The section sponsors a large number of panels at the annual APSA meeting, for which immense numbers of applications are received, and the colleague who each year assumes the role of program organizer deserves a lot of credit. The section also gives four annual prizes—I was fortunate to have the opportunity to secure the endowment that supports the book and article prizes named after Gregory Luebbert. Overall, the section is a valuable and intellectually vital organization.

Q: What did you seek to accomplish as president of the Comparative Politics Section from 1997 to 1999?

A: I tried to ensure that the diverse interests of the membership were well represented. In appointing committees and taking initiatives, I sought balance among modelers, quantitative researchers, scholars engaged in qualitative comparative research, and those with strong, analytically grounded area studies backgrounds. I put a lot of effort into running a high-quality, eclectic, balanced section, and I thought it worked well. A broad constituency of first-rate scholars was willing to participate actively in the section.

I also sought to use my four letters from the president in the section's newsletter to raise issues I felt merited close attention, and my goals as section president may be summarized by noting ideas that I presented in these letters. I first revisited the earlier debate about the comparative method (Collier 1998a), emphasizing the interconnectedness among alternative methodologies—a theme to which I have returned many times. Another letter was devoted to comparative-historical analysis and underscored the degree to which it has become a well-institutionalized current in the discipline, with a large number of exciting books having been published in the 1990s (Collier 1998b). I frankly think my letter on this latter topic helped to crystallize a new round of discussion focused on this approach. As with *Shaping the Political Arena,* this letter underscored my concern with the methodological underpinnings of comparative historical analysis and with achieving higher standards of analytic rigor in such studies.

22. For the perspectives of two of Collier's predecessors as president of the Comparative Politics Section of APSA, see the interviews with Robert Bates and David Laitin in Chapters 14 and 16.

In another letter I discussed the types of scholars, and the types of field research, that succeed in "extracting new ideas at close range," a phrase I adopted from the sociologist Alejandro Portes (Collier 1999a). I sometimes think we make too much of the deductive side of political analysis, and fail to recognize that a good deal of the most creative work comes from scholars who possess powerful analytic skills, and who are immersed in the reality of politics such that they "see" new political processes and structures in a novel way, based on deep experience with the cases they are studying. If they are really good at this kind of research, they may succeed in focusing our attention on "novel emergent processes," to use a phrase suggested by my Berkeley colleague Paul Rabinow, which will subsequently be the research focus for dozens of other scholars. To my mind, Guillermo O'Donnell's work is an exemplar of this approach.

Relatedly, I addressed the debate concerning the relationship between area studies and broader analytic agendas in the field of comparative politics. I argued in this letter that it had sometimes incorrectly been presumed that area studies had collapsed at the Social Science Research Council (SSRC) in the mid-1990s, when SSRC abolished its traditional area studies committees. In fact, at a key transition point in the evolution of SSRC, when the president of the Council Kenneth Prewitt played a lead role in eliminating these committees, he at the same time strongly underscored SSRC's commitment to area-based knowledge. I pointed out in my letter that at this juncture, SSRC funding for area-based dissertation research in fact was substantially increased.

In my final letter, I wrote about the challenge of building a disciplined, rigorous center in comparative politics (Collier 1999b), again underscoring the idea that between the area studies tradition, on the one hand, and research based on formal modeling and/or advanced quantitative methods, on the other, we must ensure ample room for qualitative-comparative research that is rigorous, and that should command full respect and prestige within the discipline. Many colleagues felt that I expressed those ideas in a way that was useful and productive for re-centering the Comparative Politics Section.

The Qualitative Methods Section of the American Political Science Association

Q: You have been the driving force behind the recent formation of the Qualitative Methods Section of the American Political Science Association (APSA).[23] What was your goal in starting this new section?
A: I have great admiration for my colleagues who created the Political Methodology Section of APSA, which has successfully institutionalized the subfield of quantitative methods. They have converted *Political Analysis*, which used to be an annual publication, into an important journal. Gary King played a key role in leading this initiative, Neal Beck did a splendid job as the first editor, and Robert Erikson has now taken on that role with impressive success. The degree to which they have successfully defined what it means to be a political science methodologist, and the criteria that must be met for scholars to be appointed in faculty positions that correspond to "methodology" in Political Science departments in the United States, are markers of a successful episode of academic institutionalization.

Yet I have been convinced for some time that the focus on methods in political science has been excessively tilted toward quantitative methods. This group of colleagues successfully appropriated the label *political methodology*, but their enterprise has almost entirely focused on quantitative methods. Not unrelatedly, for many years in quite a few departments, training in qualitative methods has not been available at all. Scholars who wanted to teach qualitative methods were not allowed to do so, or could do so only within the field of comparative politics, and not through what was accepted by their colleagues as a "real" course on methodology. For many years, I saw the need for a broader vision of methodology that was more eclectic, encompassing qualitative methods as well. We needed to do for qualitative methods what our quantitative colleagues had done for their branch of methodology. Qualitative methods are, in important respects, a foundation of the discipline—Andrew Bennett and collaborators have written a compelling article emphasizing this point (Bennett, Barth, and Rutherford 2003)—and this intellectual current needs to be strongly represented in our graduate teaching and, correspondingly, in the discipline and in APSA.

Q: Is this initiative in qualitative methods linked to other organizational efforts?
A: Yes. It is the result of a collective momentum that involves a number of people, and went through a series of steps. One should not seek to form a new APSA organized section without a strong organizational, as well as intellectual, foundation. Part of our initiative began with my efforts to transform the earlier Committee on Conceptual and Terminological Analysis (COCTA)—which was closely identified with Giovanni Sartori. This committee was a "Related Group" within APSA, and it was Research Committee No. 1 within the International Political Science Association (IPSA). First we expanded the activities of this committee; then we renamed it, giving it the broader name of the Committee on Concepts and Methods. This remains today the Research Committee No. 1 of IPSA. Subsequently, we expanded this to be an Organized Section of APSA, gathering a thousand signatures in support of the new section, and our group quickly became one of the larger sections within the association. These efforts were reinforced by the initiatives of Alexander George, Andrew Bennett, Colin Elman, and myself in forming the Consortium for Qualitative Research Methods, which has sponsored a highly successful training institute at Arizona State University every January. This institute now draws eighty students annually, and it is taught by a rotating set of about twenty faculty who come from universities across the United States. I should reiterate that important legitimation for the institute was provided when it began to receive NSF funding.

Q: What are the potential gains from having a qualitative section of APSA?
A: If you wish to persuade the political science profession that a particular analytic or methodological perspective should be taken seriously, having a successful organized section is a valuable step. If the section is strikingly successful, as ours has been, it is a particularly good step. Almost immediately after the section's formation, it came to rank among the larger organized sections, and we have seen a surprising increase in our panel allocation at the annual APSA meeting. Having a large number of panels in turn serves as a vehicle for encouraging qualitative, comparative, and small-N research and is a valuable stimulus to scholars and graduate students who work in these areas. Our panels are extremely well attended: at the 2005 annual APSA meeting, we came in second among the forty-six divisions in our average panel attendance. We now award three prizes for outstanding contributions that either develop or apply qualitative methods—a book prize, an article prize, and a paper prize—and those play a valuable role in recognizing good work and calling attention to new developments in the field. We have an excellent newsletter, *Qualitative Methods*, with a strong editor, John Gerring, and I think a lot of good communication is taking place through the newsletter.

23. Collier was the founding transitional president of the APSA Organized Section on Qualitative Methods.

Q: What is the relationship between the Qualitative Methods Section and the Political Methodology Section of APSA?

A: I have good relations with many of the scholars who have played a leading role in the Political Methodology Section. We consulted with them extensively when we formed our section. At one point, we discussed the option of being part of their section. Various senior members of the already existing section thought this was not a good idea. When they first formed their group they were poorly institutionalized and felt they needed to do things on their own for a while, without anyone looking over their shoulder. I think the qualitative methods people today feel much the same way.

Let me underscore again a key point to which I already referred above: the inconvenience—one might say the irony—that they had preempted the name *political methodology,* committing them to a broader scope of methodological offerings than they in fact delivered. For a number of months when we were forming the new section, I resisted the label *qualitative methods,* feeling that this name framed our enterprise too narrowly. As I have already emphasized, I see major parts of my methodological work as being highly relevant to quantitative research. I thought about alternative terms such as *integrative methodology* or *eclectic methodology*, but those were hardly good names for an APSA organized section. In the end, given that the *political methodology* label had indeed been preempted, I acquiesced and accepted the name *qualitative methods*. For the purposes of this interview, it seems simpler to refer to qualitative methods, but what I have in mind is this more eclectic view of methodology, of the kind reflected in *Rethinking Social Inquiry.*

Related to this issue of labels, I think a small number of colleagues have thought that our initiative has been divisive, in that it appeared to separate out the qualitative component from the overall enterprise of methodology. To be perfectly frank, if any of these initiatives is to be called divisive, it is the original formation of the Political Methodology Section. The broad label was thereby appropriated, but by-and-large, only the quantitative aspect of methodology has been addressed.

Achieving coherence in work on an eclectic view of methodology that emphatically includes qualitative tools is necessarily going to be a gradual process. We are building a well-defined set of people who are reading and commenting on each other's work, gaining insights from one another, and publishing in leading journals. Another sign of progress is the greatly increased number of qualitative methods courses—or integrative methods courses—being taught in graduate programs across the country. Along the lines of the arguments in *Rethinking Social Inquiry,* it is also essential to have many qualitative methodologists who have strong training in statistical theory, which as I emphasized before can provide valuable underpinnings for both qualitative and quantitative methods. I think we are making progress, but we still have some distance to go. Yet looking at this in a longer perspective, at no point in the history of the political science discipline has the development of qualitative methods received the kind of systematic attention it is currently receiving.

Achievements and the Future of Comparative Politics

Q: Looking broadly at the field of comparative politics, has there been an accumulation of substantive knowledge over the past thirty or forty years?

A: Yes. I see entire bodies of valuable literature on political parties, party systems, and electoral regimes. We have enormous breadth and historical depth of knowledge about democracy and authoritarianism, and more broadly about the dynamics of different types of national political regimes. The growing literature on path dependence has provided new leverage in systematizing insights about continuities and discontinuities in political institutions, and the literature on ethnic conflict is impressive. The Mahoney and Rueschemeyer volume *Comparative Historical Analysis in the Social Sciences* (2003) provides a detailed account of long-term advances in research on social policy, revolution, and democracy and authoritarianism, and another chapter that was written for that volume, but unfortunately was dropped for lack of space, provides good insights into the cumulative expansion of our knowledge of European state building (Mazzuca 2001). Thus, in many areas, we know much more than we did a few decades ago.

Q: You have been a strong proponent of comparative-historical analysis. What, in your view, is the place of historically oriented work in political science, a discipline that is more focused on contemporary issues?

A: It is exciting to study the events that are unfolding in the present. But that kind of focus can sometimes involve a short-term perspective that is vulnerable to analytic mistakes. What is the purported comment of Mao on the legacy of the French Revolution? I understand that he said: "It is too early to tell." It is hard to identify the optimal time frame for good research, but sometimes the time frame is too short. For example, in the excellent and stimulating literature on democratic transitions that emerged two decades ago, some scholars hypothesized that the mode of transition would strongly influence subsequent regime dynamics. I think that idea has been substantially discredited, and a crucial problem may have been that analysts did not have a sufficiently clear historical framework for assessing what kind of transition is likely to leave a distinctive legacy.

I am convinced that macro-comparative analysis—often called comparative-historical analysis in political science and historical sociology within that discipline—has a critical analytic contribution to make. The concern with the micro-foundations of politics in the past decade or so is an invaluable analytic advance. Yet it is equally important to understand the macro-settings in which micro-foundations make a difference. I think we have achieved enormous analytic gains in understanding these macro-settings.

Q: What is your view about how the ongoing methodological debates in political science will unfold, and should unfold?
A: One aspect of this picture is the continuing, and possibly expanding, importance of mainstream quantitative methods. Parts of this tradition exhibit a strong impulse toward the technification of analytic tools. In many respects this technification has been productive, and has given us new leverage in addressing a wide spectrum of substantive problems. Yet we warned on the final page of *Rethinking Social Inquiry* that this technification can go too far (Collier, Brady, and Seawright 2004, 266). It can become an end in its own right and can cause scholars to lose sight of simpler tools that may offer greater analytic leverage. Further, the complex training required for using more advanced quantitative tools may absorb time and energy that could otherwise be devoted to gaining essential substantive knowledge of the topics under study.

The tradition of statistical theory on which we draw in our book, while of course itself very technical, sometimes provides grounds for arguing that simpler analytic solutions are better. Further, one of the most interesting reviews of *Rethinking Social Inquiry*, "Beyond the Linear Frequentist Orthodoxy" (2006), written by the highly respected quantitative methodologist Philip Schrodt, takes a far more harsh view of regression-based research than we do in *Rethinking Social Inquiry*, and he also points to the potential contribution of simpler tools. We need to recognize that the strong words of caution in *Rethinking Social Inquiry*, focused on the elaborate tradition of regression analysis and econometric refinements on regression, by no means reflect an isolated position. These are very serious issues for the future of comparative politics and political science.

This issue of balance among alternative methodological traditions is insightfully addressed by Robert Keohane in his essay on "disciplinary schizophrenia" (Keohane 2003). He discusses the greater ease of professional credentialing in what he calls the "technical-specialization" track in graduate training, focused on quantitative and formal research and on what is often a quite narrow definition of the research question, as opposed to a much

broader track,[24] which is concerned with case-based, sometimes historical knowledge; with diverse methodologies; and with achieving rich substantive insights that cut across subfields of political science. Keohane argues that the technical-specialization track lends itself, to a greater degree, to a stream of articles in mainstream journals, giving scholars a readily definable record that, for example, yields a great advantage in tenure reviews. In contrast, scholars who pursue the broader track often follow a career trajectory in which it may take longer to develop an impressive professional record. Keohane sees this imbalance as reflecting a kind of disciplinary schizophrenia, and he speculates that political science may disadvantage itself, and inappropriately narrow the breadth of knowledge produced by the discipline, by shifting too far in the direction of the technical-specialization track.

Given these issues, where do we stand today? I think that faculty job recruitment in the field of political methodology still simply means mainstream quantitative methods. It is true that for searches in comparative politics and international relations, candidates with broad methodological skills that combine quantitative and qualitative tools are sometimes particularly welcome. I think this mix of skills should also be a requisite for political methodology positions in general—and specifically not just a mix of skills that, while appearing to incorporate and embrace qualitative methods, in fact marginalizes it.

Let me highlight one more point about how I think methodological debates should unfold. Generating meaningful regression coefficients and interpreting them appropriately can be just as hard as, if not harder than, making sense of a case study. It is time for important parts of the discipline to move beyond the view that quantitative methods have a special monopoly on analytic virtue. To the extent that this occurs it will be a welcome change, and it is a much needed change.

Conclusion

Q: To conclude, what advice can you offer graduate students entering the profession?
A: I would return to Keohane's discussion, just noted, of the tension between the narrower technical-specialization track in graduate training and the broader track, which encompasses a greater degree of case-based, often historical knowledge, diverse methodologies, and rich substantive insights

24. Keohane calls this the "contextual knowledge" track, but this description of the track follows his presentation.

that cut across subfields of political science. For students entering comparative politics and political science, pursuing the technical specialization track may be an attractive option because it appears to offer a more secure professional trajectory, and there is not the slightest doubt that scholars following this track have made major contributions.

Yet students should also recognize the professional opportunities associated with the broader track. Journals have become much more receptive to articles on qualitative methods, and many excellent journals welcome well-crafted articles based on small-N comparisons that may be focused on contemporary or on historical cases. Doctoral dissertations based on comparative-historical analysis are definitely feasible, and can lead reasonably promptly to a book. New organizational initiatives—such as the Institute on Qualitative Research Methods discussed above—have proved invaluable for creating networks of graduate students and younger scholars who share a commitment to maintaining a more eclectic version of our collective enterprise.

Hence, pursuing a broader line of work in graduate school is perfectly compatible with moving fairly quickly toward a strong professional record. This is obviously not to say that young scholars who follow the technical-specialization track are not making an important contribution. They definitely are. But we will be far better off if we address Keohane's concern with disciplinary schizophrenia through deliberate efforts—in graduate training, in hiring, and beyond—to maintain and protect multiple paths toward good scholarship.