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The Human Dimension of Comparative Research

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This book fills a void in comparative politics: the lack of a text that illuminates the human dimension of scholarship and the intricacies of the actual research process.[1] Social science research, as presented in professional publications, is cloaked by the "rhetoric of impersonality" that obscures the people who actually do the research.[2] When students read key works in the field, they seldom learn about the extra-scientific aims and motives that often drive scholars to tackle a research problem, how networks of colleagues, students, and collaborators, that is, the "invisible college," influence research, and, finally, the fits, starts, and surprises that inevitably characterize real, workaday research. We occasionally get a fleeting glimpse of such matters in the preface to a book, where the author may carefully draw back the curtain of impersonality and selectively disclose some of the twists and turns of the actual research process or the role played by his or her invisible college.[3] Still, by the time they finish their training, most students know nothing about the people who produced the books and articles they have spent years debating, admiring, and attacking. The authors of the leading works in the field are literally just names on the jackets of books and the title pages of articles.

Unless otherwise noted, all quoted material is drawn from the interviews presented in subsequent chapters of this book.

1. The lack of such a text characterizes political science, in general (though see Baer et al. 1991). Most textbooks focus on tools and methods; or, alternatively, are organized around distinct theoretical approaches and schools, often advocating one particular approach. Instead of focusing on "schools and tools," this book focuses on *people,* that is, individual scholars, drawn from across the range of approaches, schools, and generations. Along related lines, see Daalder (1997a) for a portrait of the field of comparative European politics based on intellectual autobiographies by leading figures in this area, including several of the fifteen scholars interviewed for this book.

2. The term *rhetoric of impersonality* is from Berger (1990, xix). On how interviews with scholars can serve to break through "workaday rhetoric," see Swedberg (1990, 18). See also Klamer (1984), McCloskey (1986), and Wolpert and Richards (1988).

3. Journal articles do not have prefaces and are thus even more impersonal than books.

What is wrong with this state of affairs? Is not science *supposed* to be impersonal? The value and importance of scientific research surely does not depend on who the scientist is. Students already have plenty to learn, and thus a focus on the human dimension of research, while perhaps titillating in a *People Magazine* way when juicy gossip surfaces, is arguably a waste of scarce time. Moreover, focusing on the people who produce research, especially on a small set of leading scholars, as this book does, may foster a pernicious cult of personality. Why, then, is the human dimension of comparative research something worth knowing?

First, a focus on the human dimension of scholarship helps puncture the intimidating façade that makes the authors of major works appear as "hallowed figures not easily imagined as flawed human beings" (Berger 1990, xvi). Biographical information can make the achievements of the best scholars seem attainable, which, in turn, elicits far more striving from students than a perspective that sets the leading lights atop an unreachable Mount Olympus.

Second, studying the human dimension exposes how the actual research process often diverges sharply from the stylized version presented in methodology textbooks and also in final, published products. Instead of proceeding in a linear, orderly fashion, real research, as practiced by the fifteen leading scholars on whom this book focuses, is actually laden with mistakes, false leads, and serendipitous breakthroughs. By getting beyond the rhetoric of impersonality that characterizes professional publications, a focus on the human dimension helps better align students' expectations about how research works with the realities of research.[4]

Third, a focus on the human dimension of scholarship highlights that a career in comparative politics involves far more than just mastering and applying techniques of research. A scholarly career encompasses a host of other key activities: teaching; participating in departmental and university communities as well as professional associations; interacting with colleagues, collaborators, and students; attending and organizing professional conferences and workshops; seeking funding for research; deciding whether and how to engage policy makers and other nonacademic audiences; and choosing the kinds of research projects to pursue at different stages of a career and a life. With the exception of teaching, students receive little, if any, systematic instruction about the various elements that comprise the "total package" of a modern scholarly career. Although some graduate programs offer a seminar on the professional aspect of the discipline, typically focused on publishing and job market strategies, most students learn about the non-technical elements of a scholarly career informally

4. For one scholar's effort to expose his "actual ways of working," see Mills (1959).

from their teachers. By showing how leading scholars structure their careers and balance the often competing demands of research, teaching, and service to their universities and profession, this book offers valuable insights about the range of diverse career pathways in modern political science.

The interviews presented illustrate these various advantages of exploring the human dimension of research. This introductory chapter emphasizes a further advantage: focusing on the human dimension sheds light on the skills, qualities, and habits of mind that lead to excellence in comparative research. Although the interviews with fifteen leading figures in comparative politics during the past half-century reveal no single path to excellence, no cookie-cutter template for how to become a top scholar, the best researchers share three key attributes: (1) rich life *experiences* that spark an interest in research topics and, more important, give scholars a compelling reason to care about the problems on which they work; (2) *passion* about research, often rooted in life experiences and normative commitments; and (3) a willingness to take intellectual and professional *risks*. Moreover, the scholars interviewed in this book voice a strong concern that these very qualities are in alarmingly short supply among students and professors today. Hence, a focus on experience, passion, and risk not only helps us better understand the achievements of the best researchers in comparative politics over the past fifty years, it also points to major current challenges facing the field as it advances into the twenty-first century.

It bears emphasis that experience, passion, and risk should not be seen as *sufficient* conditions for excellence in comparative research. The leading scholars have many other qualities that clearly contributed to their successes, including formidable intelligence, self-discipline, ambition, persistence, creativity, wide-ranging curiosity about politics and society, an extraordinary capacity for hard work, and maybe even luck.[5] As highlighted in the interviews, each scholar also has a distinct personality and intellectual style. Moreover, had a random sample of comparativists, rather than leading lights, been interviewed, passionate and experienced risk takers might have been found among the ranks of the less renowned or even the mediocre.[6] Still, the combination of experience, passion, and risk stands as a striking and important commonality across the fifteen scholars, and

5. Luck may be more important in determining the impact rather than the quality of research. When told that he had been lucky in his research, Louis Pasteur replied, "Fortune favors the prepared mind." See Wolpert and Richards (1988, 6).

6. Put in more technical terms, the "cases" were deliberately selected on one value of the dependent variable, because ordinary and below-average scholars were not interviewed. Also, had the N been increased by interviewing more top scholars, some might have been found to lack experience, passion, and risk taking. Although these three attributes may be neither necessary nor sufficient conditions for success in comparative research, they do characterize the fifteen leading scholars in the sample.

because the following chapters probe the specific characteristics of each scholar, this commonality is emphasized here.

The next three sections assess how a focus on experience, passion, and risk illuminates both the qualities that distinguish the best researchers and key challenges currently facing comparative politics. The fourth section discusses how exploring the human dimension of comparative research provides a stronger understanding of one of the most elusive aspects of scientific inquiry: the process of generating ideas. Finally, the chapter makes a plea against professional amnesia, arguing that the tendency of the field to dismiss older works as antiquated, even pre-scientific, robs us of powerful models of excellence and weakens our self-confidence about the achievements of comparative politics. We thus need to strengthen professional memory by knowing, teaching, and drawing inspiration from the history of our field.

Experience: Rich Lives

May you live in interesting times.—*Attributed to a Chinese curse*

The scholars on whom this book focuses draw a clear and explicit connection between their life experiences and the research problems they select. Some recount how their interest in studying politics was sparked by large-scale social trauma, for example, war, economic crisis, or political instability. Others tell how involvement in political organizing, military service, or foreign travel had a similar effect. The overall picture that emerges of the leading scholars in comparative politics is not of cloistered bookworms, but of engaged people with remarkably rich real-world experiences, especially during their formative years. This suggests a provocative hypothesis: *the quality of one's scholarship depends on the quality of one's life experiences*. Why does experience matter? How can the quality of experience affect the quality of research? First, experience infuses research with meaning and purpose. If one has lived under a repressive, non-democratic regime, as did several of the scholars interviewed in this book, then the challenge of explaining the rise and fall of these regimes is not an abstract mental puzzle, but a visceral matter of good and evil. Linkages between life experiences and research questions can thus foster the commitment and drive required to excel. Second, experience strengthens knowledge about the range of human behavior and about how the political and social worlds work. Knowledge grounded in experience serves both as a source of fresh ideas and as a basis for testing, and potentially challenging, generalizations. The interviews provide ample evidence of how life experiences generate passionate devotion to research problems as well as a reservoir of knowledge from which new ideas are drawn.

Table 1.1. Year and Country of Birth of Leading Scholars in Comparative Politics

Gabriel A. Almond	b. 1911	United States
Barrington Moore, Jr.	b. 1913	United States
Robert A. Dahl	b. 1915	United States
Juan J. Linz	b. 1926	Germany (raised in Spain)
Samuel P. Huntington	b. 1927	United States
Arend Lijphart	b. 1936	Netherlands
Guillermo O'Donnell	b. 1936	Argentina
Philippe C. Schmitter	b. 1936	United States (raised in Europe and United States)
James C. Scott	b. 1936	United States
Alfred Stepan	b. 1936	United States
Adam Przeworski	b. 1940	Poland
Robert H. Bates	b. 1942	United States
David Collier	b. 1942	United States
David D. Laitin	b. 1945	United States
Theda Skocpol	b. 1947	United States

The fifteen scholars can be fruitfully classified according to when and where they were born.[7] As seen in Table 1.1, these dimensions divide the scholars into three groups: (1) *older Americans* born during the 1910s and 1920s (Almond, Dahl, Huntington, Moore); (2) *foreigners* born during the 1920s and 1930s (Lijphart, Linz, O'Donnell, Przeworski);[8] and (3) *younger Americans* born during the 1930s and 1940s (Bates, Collier, Laitin, Schmitter, Scott, Skocpol, Stepan). The first and second groups converge in their common experience of large-scale societal trauma: the four Americans born during the 1910s and 1920s were young adults or adolescents during the Great Depression and World War II. Two (Almond, Dahl) served in the U.S. armed forces during the war, and a third (Moore) worked for a government intelligence agency (i.e., the Office of Strategic Services [OSS]).[9] The three European scholars born during the 1920s and 1930s (Linz, Lijphart, and Przeworski) experienced the dislocations caused by World War II,[10] whereas O'Donnell, who was born in Latin America, lived through the political and economic turmoil of Argentina during the 1950s and 1960s. By contrast, the seven American scholars born during the 1930s and 1940s did not directly experience large-scale social disruption. Still, they were young

7. The criteria used to select the fifteen scholars interviewed for this book are discussed in the preface.

8. Przeworski was born in 1940.

9. The OSS was the precursor to the CIA.

10. On how the traumatic events in Europe during the first half of the twentieth century affected a whole generation of older émigré social scientists, see Coser (1984), Bendix (1986), Hirschman (1995, Part II), Gay (1998), and Dawidoff (2003).

adults in the 1960s, during the political and societal upheavals associated with the civil rights movement and the Vietnam War. As the interviews reveal, these experiences often had a strong effect on their scholarship.

Older Americans and Foreigners: The Trauma of the Great Depression and World War II

The Great Depression and World War II had a major impact on the older Americans. Gabriel Almond draws an explicit link between his scholarship and his work at the Unemployment Relief Service in the Chicago Stockyards during the Depression: "When I grew up, it was one problem after another, one disaster after another . . . I was moved by these unemployed Chicago workers who came and told you: 'My children don't have any shoes, and in the winter their feet get wet, and they get sick. Can I see my social worker so she can give me a certificate to take to a department store and they will give me some shoes?' That's what made me a kind of social scientist of Left politics at that time." As a result of these experiences and his later work in the U.S. army in Germany at the end of World War II, Almond says he "always thought of political science as dealing with very urgent and palpable evils, such as civil conflict, economic breakdown and poverty, and war" (Almond 2002, 2–3).[11]

Robert Dahl offers a gripping account of how his battlefield experiences in Europe during World War II had a decisive impact on his decision to become a scholar: "Sometime between November 1944 and May 1945, somewhere in France or Germany, it became clear to me that the things I liked to do most were read, write, and talk about ideas. So the light came on, and I decided that if I survived I would be an academic." Moreover, his wartime experiences made the topic on which his scholarship centered, democracy and its enemies, a compelling one laden with normative content: "For people like me, the real threat during the 1930s and 1940s that democracy would end, that it would be destroyed, impressed on our generation the importance of democracy. We realized that the alternatives to democracy were so much worse."

Barrington Moore, Jr.'s work as a government analyst in the OSS during World War II exposed him to an extraordinary group of German intellectuals who had fled Nazi Germany, including Herbert Marcuse, Otto Kirch-

11. In Chapter 3, Almond further discusses how the historical context of the 1930s and 1940s influenced his choice of research topics: "I have been concerned about the big problems, first the Depression, the New Deal, the war, National Socialism, fascism. Take Germany. Here's the country that invented higher education in the social sciences, where the first real social science journal was published, edited by Max Weber, going Nazi. It drove me crazy. I felt obliged to study these problems however I could."

heimer, and Franz Neumann.[12] Through his interaction with these émigré scholars, Moore learned how to use Marxist theory in historical analysis, a technique he later applied fruitfully in his most important work, *Social Origins of Dictatorship and Democracy* (Moore 1966). Moore thus concludes, "In many ways, that book was a product of my experiences at the OSS."

The research interests of the foreign scholars born during the 1920s and 1930s—Linz, Lijphart, O'Donnell, and Przeworski—were shaped deeply by the fear, uncertainty, and economic hardship they experienced during World War II and living under repressive authoritarian regimes. According to Juan Linz, who experienced the Spanish Civil War (1936–39) as a boy, "My interest first in social problems and then in politics is the result of living, practically since childhood . . . all the complex history of Europe in the interwar years from post–World War I to the Franco regime." Linz draws a connection between his Spanish background and the research questions that have commanded his interest:

> To ignore the [Spanish] Civil War and its origins, or the Franco regime, was not conceivable for a young Spaniard like me with an interest in political and social science. And who could live in the 1970s without looking at transitions to democracy? As soon as the transition started in Portugal in 1974, I quickly got my plane ticket and went several times so I could follow the democratization process by attending party meetings and rallies and by talking with politicians. What was happening in Portugal might be relevant to what was eventually going to happen in Spain, because Franco was not eternal. Because of your biography, you have a personal interest and involvement that motivate the selection of many research problems.

Arend Lijphart vividly describes the fear and deprivation he experienced as a child in the Netherlands during World War II, recalling dogfights in the sky above his town, food shortages, and a refugee from the Germans who hid in his house. In assessing the relationship between these events and his subsequent research, Lijphart concludes, "my experience during World War II made me unusually averse to violence and especially interested in questions of both peace and democracy."

Guillermo O'Donnell, who grew up in Argentina during the 1950s, was nearly arrested when the student group in which he participated ran afoul of the dictatorship of Juan Perón. Later, during the military dictatorships that ruled Argentina in the 1970s, O'Donnell was a target of threats from armed groups both on the Right and Left of the political spectrum. He sees a direct link between these terrifying experiences and his work as a social scientist: "I have done research on questions that originated in the fact that

12. See Coser (1984) for useful vignettes about Marcuse and Neumann.

we were governed by horrible regimes in Latin America and because I much preferred democracy." Adam Przeworski, who was raised in communist Poland in the 1940s and 1950s, recalls that "one's everyday life was permeated with international, macro-political events. Everything was political." Like O'Donnell, Przeworski, too, ran into trouble with the dictatorship that controlled his country, essentially forcing him into exile abroad.

As a result of their formative experiences, the core research questions on which these scholars focused—Why do democracies break down? How can stable democracy be achieved? What is the relationship between capitalism and democracy?—were not mere abstractions, but palpable, normatively charged problems.

Younger Americans: The Turmoil of the 1960s

Growing up in the United States during the 1950s and 1960s, the seven younger Americans (Bates, Collier, Laitin, Schmitter, Scott, Skocpol, and Stepan) were not exposed to the hardships of economic crisis, war, and repressive regimes experienced by their American elders and foreign-born contemporaries. They were too young to remember the Great Depression or to have served in World War II. Still, several of the younger Americans recount how the defining political events of the 1960s—the civil rights movement and the Vietnam War—sparked their interest in comparative politics. James Scott participated in the student rights movement and took part in numerous civil rights marches in his capacity as a leader of the National Student Association. Indeed, his political engagements proved a source of friction between him and the political science faculty at Yale, where he did his doctoral studies. According to Scott, "the first thing I did in graduate school was try to pass a student resolution against the Bay of Pigs, which the faculty went ape shit over and tried to stop, because they thought graduate students were professionals and should not take political positions." Skocpol describes herself as "a passionate supporter of the antiwar movement" and discusses how her undergraduate volunteer work teaching African American college students in Mississippi provided "a way to get involved in large-scale social change."

Among the younger American scholars, a wide range of extracurricular experiences—foreign travel and study, service in the Peace Corps, a summer internship in Washington, D.C., military duty—shaped both their initial decisions to become social scientists and their subsequent choices of research questions. Bates took a high school trip to Africa that marked the start of a lifelong enchantment with the region: "I decided that going to Africa was the most important thing I'd ever done in my life, and I wanted a career that would get me back to Africa as often as possible." A summer internship at the Department of State during college deepened his fascination with Africa. Schmitter's interest in Latin America was piqued by studying painting in Mexico. Stepan took a six-month trip around the world after college that exposed him to puzzling cross-national differences in the relationship between politics and religion, a theme that would figure prominently in his research decades later. Moreover, Stepan's decision to write his dissertation and first book on the military's role in politics in Brazil cannot be understood apart from his prior service in the U.S. Marine Corps and work as a journalist in Latin America (Stepan 1971). Laitin's stint as a Peace Corps volunteer in Somalia proved an "exhilarating" experience that informed his research on language and politics in Africa and beyond. By broadening their horizons and generating enthusiasm about substantive political issues, the extracurricular experiences of the younger Americans served as less traumatic surrogates for the shocks of war, repression, and socioeconomic dislocation experienced by their elders and foreign-born peers, who, as the purported Chinese curse puts it, lived in more "interesting" times.

Are You Experienced?

The scholars interviewed in this book are concerned that students today lack experience.[13] Linz observes that many students "typically go from a good high school to a good college, get good grades, and then go directly to graduate school, having already majored in college in the same field in which they do their graduate work. They have never done anything else except be in the university, and that may be a drawback." Dahl notes, "My impression is that graduate students today, although many are better educated coming out of high school than I was coming out of college, lack a depth of human experience with ordinary people who aren't involved in the academic framework." Przeworski voices a similar concern:

> The people who entered graduate school during the Vietnam era had gone through quite a lot in their lives. They had intense feelings about politics, culture, and society. They usually had done something else, often political organizing, and were going back to school to reflect on their experiences, often seen as failures. Very often they were not teachable, because they were mistrustful of "positivism" and hostile to rigorous method. . . . But they deeply

13. This section embraces the spirit of the "art of mentoring" series, published by Basic Books, which is based on Rainer Maria Rilke's *Letters to a Young Poet,* and invites leaders of the arts and professions to contribute a text "meant to shape the future of their disciplines and inspire the careers of the next generation and generations after that." See, for example, Dershowitz (2001).

> cared about politics; they studied politics because they wanted to change the world. Today the situation is different. These kids, and they are kids, who are now in graduate school, by and large, have grown up in exceptionally peaceful, prosperous, and non-conflictive times. These students are smart, well educated, and eager to be taught. But they have no passions or interests. These kids absorb education and all the skills easily, but when the moment arrives when they are supposed to start asking questions, they have nothing to ask.

If, as the evidence from the interviews suggests, the quality of comparative research depends in part on the quality of the life experiences of the people who do it, then the experiential deficit noted by Linz, Dahl, and Przeworski among students today raises concerns about the future vitality of the field. What can be done about the experiential deficit? How can aspiring scholars who have not known economic crisis, war, or repressive political regimes, that is, who have had the good fortune to live in relatively *uninteresting* times, enrich their lives in ways that could enhance the quality of their work?[14]

One way to gain experience is to avoid going straight to graduate school after finishing the undergraduate degree. Taking time off after college to travel or work can help whet the appetite for comparative research. Regrettably, admissions committees for doctoral programs in the social sciences probably place insufficient emphasis on extracurricular experiences and are too eager to admit students directly from undergraduate programs. MBA programs routinely require students to spend several years acquiring hands-on business experience beforehand; social science doctoral programs might do well to adopt a similar standard. In sum, professors should strongly consider advising undergraduate students who are contemplating academic careers to "slow down!"

In addition to travel and nonacademic work, other ways to broaden one's horizons include learning languages and even reading literature. Both provide exposure to different ways of thinking, and reading literature helps attune us to variation in human behavior. Carrying out research in a foreign country provides a further way to surmount an experiential deficit, especially for students who go straight into doctoral programs after finishing their undergraduate degrees. As Bates bluntly puts it, "Fieldwork is the cure for bullshit. When you do fieldwork, you take your research problems from reality." Here, the recommendation to "slow down" merits repeating. Graduate students today often face heavy pressure to finish their doctoral degrees quickly, in five or at most six years. This pressure, which may stem largely from cost-management measures by university administra-

14. Of course, the post-9/11 period of global terrorist threat may turn out to be far from uninteresting.

tors, makes it harder to acquire experience through extended fieldwork or by spending time abroad as an exchange student. In light of the relationship this book reveals between life experiences and excellence in comparative research, efforts to shorten the length of doctoral training may prove quite costly if they cut the amount of time students with experiential deficits can spend in the field.

The process of seeking new experiences should not stop after graduate school. As Schmitter observes, "to be a good comparativist, you have to be comparative yourself. That is, you must habituate yourself to living in different cultures and being on the outside. You have to structure your life comparatively, seeking out opportunities to go to different countries." The quest for rich experiences that infuse our work with meaning and purpose, provide fresh ideas, and deepen knowledge about the range of human behavior is a lifelong endeavor.

Passion: The Emotional and Normative Aspects of Research

I am a brain, Watson. The rest of me is a mere appendix.
—Sherlock Holmes (as quoted in Grann 2004, 62)

Inspiration plays no less a role in science than it does in art.
—Max Weber (1946a, 136)

In his claim that he is just a brain, Sherlock Holmes evokes a common perception of scientific inquiry as a dispassionate endeavor carried out by brains in formaldehyde. From this perspective, emotion, feeling, and other "hot" aspects of human nature are contaminants that cloud "cool" rational judgment and thus block scientific progress. Passion has no place in science.

The interviews in this book challenge the view that scientific inquiry is a cold, heartless enterprise. Instead, the evidence from the interviews supports Weber's assertion that "inspiration plays no less a role in science than it does in art." The best scholars in comparative politics are very passionate about their work. Indeed, they often describe their research in patently emotional terms. O'Donnell sees himself as someone who "deal(s) with the kinds of real-world problems that deeply bother me when I'm shaving." He says that throughout his life he has been "obsessively concerned" with the political misadventures of his country, Argentina. Reflecting on what motivated him to continue doing research into his nineties, Almond remarks, "It's enjoyable to solve a problem. I get a thrill." Dahl observes that "for the best students the study of politics engages not just their intellects, but also their somatic systems. There is feeling, emotion." Finally, in reflecting on his experience doing ethnographic fieldwork for two years in a Malaysian village, Scott concludes, "It's very productive when you become so preoccupied with something intellectually that it occupies your waking and sleeping hours, and you're even daydreaming about it. That's a great thing for ideas."

Emotional engagement may even be *necessary* to produce excellent research (Zuckerman 1991, Ch. 6). Dahl proposes the intriguing hypothesis that the quality of our work depends on how much we enjoy it. The interviews provide evidence that pleasure matters in scientific research. Linz notes, "Each time I follow some hunch and it fits, it's interesting and pleasurable. I learn something, and, fortunately, society is paying me for having my fun." Asked what motivated him to keep working at the age of eighty-nine, Moore responded, "There's a certain amount of idealistic curiosity and intellectual pleasure that partly comes from problem solving." Stepan's description of his collaboration with Linz, which often involves late-night work sessions that, by 3:00 a.m., leave dozens of books, articles, and maps strewn like a field of debris across Linz's living room, evokes a childlike sense of play. Przeworski succinctly states, "I just like doing research."[15]

What drives the enthusiasm these scholars feel for their research? Their excitement stems partly from the pleasure they get from scholarship. Still, their passion for research is often rooted in something deeper: the conviction that the questions they study are normatively important and, hence, their work has implications for the "real world" of politics, policy, and public opinion. This conviction imbues the research enterprise with meaning, which, in turn, elicits passion.

But are normative motives and goals compatible with science? According to a currently influential school in political science, "positive political economy," the answer is "no." Indeed, this school partly stakes its scientific aspirations on the claim that it studies how things *are,* not how they *should be,* which it regards as a matter for nonscientific, "normative" theory (Alt and Shepsle 1990).[16] The interviews challenge the view that positive and normative theory should operate in separate spheres: some of the most influential scholars in comparative politics self-consciously straddle positive and normative research. Dahl describes himself "as comfortably combining the normative, ethical aspects of political science with the empirical, and thus the scientific, aspects of political science." He laments that "many political scientists today unfortunately feel uncomfortable linking normative political theory with empirically grounded social science, to the detriment of both sides . . . [because] it is very hard to ask important research

15. Research is not all "fun and games." Przeworksi even notes that he finds painful the process of getting up to speed with methodological advances. Overall, the leading scholars demonstrate a remarkable capacity for hard work.

16. The notion that science should be "value-free," of course, has a long pedigree.

questions unless you define them in terms of their human value, in terms of what difference it will make if you answer them." Lijphart describes his work in similar terms:

> I see my research as starting with a normatively important variable—something that can be described as good or bad, such as peace or violence. I then proceed to investigate what produces these different outcomes. Finally, I conclude by presenting prescriptions, that is, measures that would produce the desired outcome. I don't see a tension between normative concerns and an aspiration to do science. In fact, I think a normative, prescriptive conclusion can be drawn from most empirical relationships.

As these examples suggest, the belief that one is addressing normatively important problems with real-world implications makes research meaningful, which, in turn, helps generate and sustain enthusiasm for scholarship. Moreover, it is feasible to combine a focus on research questions that are meaningful in terms of our values and moral commitments with impartiality, rigor, and objectivity in the pursuit of answers to these questions (Weber 1949, 49–112).[17] Efforts to build a firewall between positive and normative theory are thus unnecessary to achieve scientific objectivity; and because such efforts run the risk of draining the passion from research, they should be avoided.

Passion Lost: The Iron Cage of Professionalism

The scholars interviewed in this book worry that both professors and students today lack passion for their work. Skocpol observes, "I talk to a lot of graduate students who say they feel very confined. They seem to choose research questions out of a sense of duty, working on a particular topic because it is what they are expected to do to reach the next career stage. I am not sure enough people are following their noses and trusting their curiosity to lead them to a question that matters." In a similar vein, Dahl states, "Sometimes when I look at what gets published in the *American Political Science Review* I ask myself, 'Is this person really excited by that?' " Scott is concerned that too many professors and students "think of scholarship as a career, as an 8-to-5 job."[18]

17. On Weber's "dual commitment to objectivity *and* subjectivity in the social science enterprise," see Fishman (2005). See also Schluchter (1979). On how leading social scientists, including Robert Dahl, combined their normative commitments to liberalism with objective empirical inquiry in the aftermath of World War II, see Katznelson (2003).

18. Concerns about the negative impact of professionalization on the social sciences are not new. See the review of previous work on this matter in Gunnell (2004, 264–66).

What can be done about the lack of passion these scholars observe? First, professors need to do better in communicating their enthusiasm for their work to students. If professors show little passion for research and signal that scholarship is a 9-to-5 job, then students cannot be blamed for behaving this way, too. Second, works can be assigned that give students models of first-rate research by scholars who care deeply about what they study. This strategy can be seen in Bates's use of Grant McConnell's *Private Power and American Democracy* (1966), which he regularly assigns in courses. McConnell's book palpably conveys his anger about the use of public power for private advantage, which gives the reader, as Bates puts it, "a reason to care, a reason to get pissed off and join the author in the joy of the chase."

Fostering interactive communities that break the confines of a 9-to-5 routine can also help generate and sustain enthusiasm among both professors and students. Schmitter describes how most of his colleagues at the University of Chicago lived in the same neighborhood, Hyde Park, and thus frequently saw each other outside the workplace. This interaction elicited nonstop, rolling conversations and arguments, even among colleagues with quite different views about how to study politics. These conversations helped keep the faculty challenged, engaged, and excited. Skocpol notes that the study groups she joined as a graduate student had a similar effect, as did the weekly faculty-student workshops in which she later participated as a faculty member at both the University of Chicago and Harvard. These examples suggest that igniting passion in comparative politics requires that we pay closer attention to organizing our communities of learning in ways that generate excitement about research.

Finally, sparking passion requires the recognition that emotional engagement, normative commitments, and excellence in research are by no means incompatible: some of the best scholars explicitly see themselves as driven by both normative and positive concerns. Not only is it feasible to study issues we care about, it is *desirable* to study such issues. Yet in the absence of rich life experiences and normative commitments, finding a topic we care about passionately can be a tall order.

Risk: Taking Chances

In addition to rich life experiences and passion, the scholars interviewed for this book share a third quality: audacity. The best researchers in comparative politics have taken professional, intellectual, and even personal risks across three key areas: (1) how they define their relationships to teachers; (2) how they position themselves in relation to mainstream research; and (3) the kinds of questions they study.

Teachers

Defining our relationship with mentors and advisors is often a tricky matter, as we seek to balance autonomy and independence against the impulse to emulate and even imitate an admired teacher. Moreover, many teachers expect allegiance, even obedience, from students, although they may not realize or admit it. Because the support of a mentor can play an indispensable role in getting a job and successfully launching a career, challenging one's advisors can be a frightening move fraught with hazard. Still, the interviews with leading scholars provide numerous examples of this kind of risk taking.

Skocpol's very first article (Skocpol 1973), published when she was a graduate student, was a critical review of her teacher, Barrington Moore's magnum opus, *Social Origins of Dictatorship and Democracy* (1966). Stepan chose not to heed his advisors' warnings against writing a dissertation on the Brazilian military (Stepan 1971), a topic they said would prove too difficult. And Schmitter and Laitin both had the audacity to disagree openly with their teachers at the University of California, Berkeley. While taking a class with Seymour Martin Lipset, Schmitter boldly told Lipset he was wrong in arguing that political parties were the main vehicles of representation in democracies. Likewise, when Ernst Haas, Laitin's mentor, poked fun in class at Karl Deutsch's efforts to devise objective measures of human emotions, Laitin defended Deutsch against Haas's criticisms. Bates and Przeworski took a different kind of risk: they chose not to have any mentor. At Northwestern University, where he received his Ph.D. in political science, Przeworski was nobody's student. Bates, who had a similar profile at the Massachusetts Institute of Technology (MIT), says, "I basically went away to Africa, did my thing, and dropped a lot of dissertation pages on people's desks near the end."

Still, not all the scholars in this study aimed to define an autonomous position in relation to their teachers. Moore and Scott describe how their dissertations and early publications mimicked the work of their doctoral advisors. In hindsight, both express regrets about the lack of originality in their early work. In discussing his dissertation and the book that resulted from it, Scott says he aimed to "follow in [the] footsteps" of his advisor, Robert Lane, by applying Lane's theoretical framework for studying political ideology to the case of Malaysia (Lane 1962; Scott 1968). Although his first book pleased his advisors, Scott regards it as a "cheap success," because it was sharply criticized by specialists on Malaysia who found the work empirically shallow. Scott thus concludes that his first book "is [not] much worth reading." Moore takes a similarly dim view of his first academic publication, a quantitative cross-national analysis of social stratification

(Moore 1942): "I was copying my teacher [George Peter] Murdock. I regard [that article] as kind of a joke now."

These examples are not meant to suggest that graduate students should rebel against their teachers and blithely dispense with mentors. Still, a willingness to disagree with and establish independence from teachers does characterize some of the best scholars.

The Mainstream

The best researchers are prone to risk taking in a second area: how they position themselves in relation to mainstream work. Several of the scholars interviewed in this book show relentless determination to pursue their interests and passions, even when they know they are risking professional marginalization. During the 1970s, Bates found himself at the edge of the field as a result of both his substantive focus on Africa and his use of rational choice theory at a time when non-choice approaches, such as dependency and modernization theory, dominated comparative politics.[19] As he describes it, "I felt I was on the margins of the profession, and I was happy being there. I mean, I was an Africanist. You don't become an Africanist to be mainstream." Similarly, Laitin spent much of the first decade of his career indulging his fascination with political culture in Somalia, despite his clear awareness that this topic commanded little interest. Laitin tartly observes, "I had zero impact on the profession. In fact, I did not even have a substantive footnote the first twelve or thirteen years of my career . . . The only citation to my work that anyone ever made was 'Somalia is on the east coast of Africa, see David Laitin.'" As a young assistant professor, Scott chose to cultivate his passion for Southeast Asia by taking a yearlong leave of absence to study the classic historical and anthropological works on the region. He recalls the withering criticism of a colleague who admonished, "You're a knucklehead, Scott. Becoming a Southeast Asianist is a stupid waste of time. This is not where political science is headed. It's the end of your career."

A further example of risk taking in relation to the mainstream involves how scholars package research. The scholars interviewed in this book do not always follow the dominant strategy of publishing books and articles in peer-reviewed journals. For example, Linz and Stepan both tend to write hundred-page manuscripts that are too long to publish as refereed journal articles, yet too short to publish as books. As a result, much of their work appears as chapters in edited volumes, a format conventionally seen as

19. Rational choice theory did not see widespread use in comparative politics until the 1990s.

commanding less attention than books and journal articles. Moreover, one of Linz's most influential papers, on presidential democracy, was available only in an unpublished *samizdat* form for years until an abbreviated version was finally published (Linz 1990a).[20]

Questions

Another area where leading scholars take risks concerns the research questions they address. Among the scholars in this study, numerous attempts can be observed to answer *big* questions whose scope requires huge investments of time and energy in the face of an uncertain payoff. Efforts can also be seen to move on to *new* questions from the standpoint of their prior research. Some of the best researchers have a restless curiosity that drives them to seek out new problems and topics, instead of clinging safely to old ones where they have a proven record of success.

BIG QUESTIONS. Several of the scholars display a striking willingness to tackle ambitious questions: Why are some countries democratic and others not? What are the causes of revolution? What explains economic development? Addressing such questions can require an extraordinary amount of energy and patience.

Reflecting on *Social Origins of Dictatorship and Democracy* (Moore 1966), which analyzes eight countries across several centuries and took more than ten years to write, Moore sheds light on the audacity that motivated him to undertake such a daunting project: "I actually started *Social Origins* with a much more ambitious plan—an overly ambitious plan. I was going to study a wider range of countries, not just ones with an agrarian class structure, but also ones with an industrial social structure, and maybe even a couple of others." About her decision to write a doctoral dissertation that compared three major revolutions, Skocpol observes, "It was unheard of at that time for a graduate student to write a thesis about a topic as vast as the French, Russian, and Chinese revolutions. We were expected to study statistics and find a focused, 'doable' project." Finally, David Collier, who, with Ruth Berins Collier, produced an 877-page book, *Shaping the Political Arena* (Collier and Collier 1991), that explores the historical roots of modern political systems across eight Latin American countries, highlights the stamina involved in tackling big questions when he explains why comparative historical research is so often published in long books: "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 . . . It simply

20. The full paper was eventually published as Linz (1994).

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 . . . focused on the evolution of these countries through five or six historical phases."

NEW QUESTIONS. Instead of playing it safe by defending turf where they have already proven their credentials, many leading scholars challenge themselves by moving on to fresh research topics where their skills and talents have not yet been tested. This form of professional risk taking is exemplified by Huntington, who, over the past fifty years, has published widely across the three major empirical subfields of political science—American politics, international relations, and comparative politics. According to Huntington, "I wander around from field to field." To explain his peripatetic trajectory, he points to the central role of substantive problems, rather than methods, theory, or disciplinary boundaries, in driving his research: "I like to address what seem to me to be important questions—both for the real world and intellectually important issues. So I follow the trail where those kinds of questions and issues are, even if it requires moving from field to field." A similar urge to tackle new problems can be seen in the evolution of O'Donnell's research, which shifted from authoritarian regimes to transitions from authoritarianism and, most recently, to the quality of democracy. O'Donnell describes his compulsion to address new questions in the following terms: "I've had some colleagues get kind of angry and tell me that, after spending some time on some of my texts, I had already moved on to another topic. In some sense, I think this is a bad characteristic of mine. But that is something I have never been able to control. When a new theme captivates me, I abandon my children to their uncles, so to speak, and move on. That's the way my mind works." Skocpol offers another example of moving on. After producing a major book and many successful articles on the comparative study of revolutions, Skocpol shifted to a new and distinct issue: social policy in the United States. In explaining this shift, she remarks, "Soon after *States and Social Revolutions* (Skocpol 1979) appeared, I reached a point where I did not want to write about revolutions anymore. My strategy as a scholar is to define fruitful problems and use them to puzzle through theoretical issues, and I wanted to move on to new problems. I did not want to be an expert on revolutions." Finally, after spending the first decade of his career as a successful specialist on Russia, Moore transformed himself into a broadly comparative scholar. Asked what motivated him to make this move, Moore said, "I couldn't stand the idea of being a Russia specialist . . . I got interested in something else. My curiosities shifted to what emerged finally in *Social Origins* (Moore 1966): the roots of totalitarianism, liberalism, and radical revolution."

Still, not all top scholars take the risk of moving on to new research questions. Over the course of his career, Lijphart has focused steadfastly on the challenges of achieving stable democracy in plural and divided societies. Likewise, Laitin's research has centered consistently on the relationship between culture and politics. When asked to respond to the observation that most scholars in comparative politics focus narrowly on the same region or country, whereas his research spans multiple regions, Laitin offered,

> I'm the one who's very narrow. During all the years I've been doing political science research, I've largely focused on the same narrow set of questions, basically about the relationship between culture and politics, and the implications of cultural heterogeneity for politics . . . Whether my work was in Somalia, Nigeria, Catalonia, or the post-Soviet World, you can see the same questions asked repeatedly in several different ways. I've often said to other comparativists that they overestimate the costs of equipping themselves for going to a new place and underestimate the costs of studying a new issue in the same place.

The Risks of Risk Taking

Risks, by their very nature, do not always pay off. Although innovative research at the margins occasionally penetrates, and even transforms, the mainstream, the usual fate of work at the margins is to be marginalized. For example, Bates partly attributes the lack of widespread attention received by his book, *Rural Responses to Industrialization* (Bates 1976), to its reliance on rational choice theory, which set it outside the theoretical mainstream: "At that time, in the mid-1970s, comparative politics was focused on dependency theory and the Marxist critique of dependency theory. My book doesn't mention any of that and thus was far removed from the theory of the times. That's one reason the book was not taken up very strongly."

Other forms of risk taking can also be costly. Huntington notes that when one moves across fields, as he does, "the specialists in one field are generally unfamiliar with what you have done in another field. People in comparative politics think of me in terms of *Political Order in Changing Societies* (Huntington 1968) and *The Third Wave* (Huntington 1991). But they don't know anything about *The Soldier and the State* (Huntington 1957) or my book on American politics (Huntington 1981b)." Linz suggests that his penchant for writing lengthy pieces best-suited for publication as chapters in edited volumes may have lessened the visibility and impact of some of his work.[21] Finally, Schmitter recounts how his efforts to get beyond the case of Brazil, on which his research had previously focused, by doing substantial fieldwork in a new country, Argentina, generated no publications. He regards this outcome as one of his "great failures."

Even the best researchers experience disappointment and setbacks as a result of the risks they take, and the dustbin of comparative politics history is probably filled with the work of little-known and forgotten scholars who took risks that failed.

Playing It Safe: Are We Too Risk-Averse?

Although risk taking can be costly, the scholars interviewed in this book voice a concern that professors and students are too risk-averse. According to Przeworski,

> The entire structure of incentives of academia in the United States works against taking big intellectual and political risks. Graduate students and assistant professors learn to package their intellectual ambitions into articles publishable by a few journals and to shy away from anything that might look like a political stance. This professionalism does advance knowledge of narrowly formulated questions, but we do not have forums for spreading our knowledge outside academia; indeed, we do not talk about politics even among ourselves.

Linz echoes this point, arguing that the increasing use of standardized criteria for measuring professional success, such as the number of publications in refereed journals, reduces the likelihood of innovation: "There is more and more reliance on impersonal and mechanical criteria, like publications in peer review journals, for making decisions about who should be promoted and get positions. By becoming more impersonal and more bureaucratic, the field produces standard, predictable products, but this standardization allows little room for mavericks and innovators."[22] Huntington observes that graduate students are "often very hesitant about setting forth a broad proposition." This timidity, he finds, makes graduate students far less interesting to teach than undergraduates. O'Donnell laments the passing of an era of big, daring books: "I worry that in its current drive toward methodological sophistication, political science has lost the ambition and

21. See Linz's discussion of his paper, "From Primordialism to Nationalism" (Linz 1985a), in Chapter 6. Writing papers of unconventional length is likely to be a recipe for failure given the tenure requirements that exist today, and students and untenured faculty members are cautioned against such an approach.

22. Scott makes a similar point in discussing what he calls "hyper-professionalism." See Chapter 11.

hubris of writing great books that give an account of big issues. When Moore, Dahl, or Shmuel Eisenstadt produced their major books, for example, there was a sense of possibility that you could do both methodologically self-conscious and important work on great issues. I fear this sense of possibility is disappearing."

Despite these concerns, few, including the scholars interviewed in this book, would propose universal, perpetual imprudence: not everyone can or should do high-risk research. Fierce independence is not for all; and much solid and good research has been produced through emulating and even aping mentors. Moreover, a healthy discipline may actually require a large mass of researchers doing low-risk, "normal" science: too many mavericks swinging for the fences in the hope of hitting paradigmatic grand slams is probably a recipe for disaster.[23]

Still, self-conscious steps are necessary to prevent the dominance of a herd mentality that could lead the whole field to stampede over a cliff. The leading scholars offer practical recommendations to counter the hegemony of group think. Skocpol urges students to expose themselves to a variety of faculty: "Make space for yourself by diversifying; don't apprentice yourself to just one person or approach, but to several. Learning from several different people is a good way to create an original combination." Linz advises, "Don't limit yourself by saying, 'I am in political philosophy, so I am not going to take any courses in comparative politics,' or 'I am in comparative politics, and so I will not take anything on political philosophy.' Use the best resources of your department broadly." Scott stresses the importance of reading widely: "Just as the health food people say, 'You are what you eat,' you are as an intellectual what you read and whom you're talking with. And if you're just reading in political science and only talking with political scientists, it's like having a diet with only one food group. If that's all you do, then you're not going to produce anything new or original. You're just going to reproduce the mainstream. If you're doing political science right, then at least a third of what you're reading shouldn't be political science." Lijphart proposes that young scholars hedge their bets by keeping one foot anchored in the mainstream while stepping outside it with the other: "The trick is to build on existing research without being bound by it, to work within the paradigm but also to think outside it."

Finally, it may be more appropriate, and certainly more prudent, to take risks after tenure. As Moore wryly remarks, "Tenure is a great thing. It allows you to be as much of a damn fool as is humanly possible." Though a field dominated by damn fools is surely not desirable, greater effort to exploit

23. On the key role in generating scientific progress of "traditionalists" who, in contrast to self-conscious innovators, "enjoy playing intricate games by pre-established rules," see Kuhn (1977, 237).

the remarkable freedom tenure gives for taking intellectual risks will help keep comparative politics vibrant and exciting.

Stirring the Comparative Imagination: Creative Hypothesis Generating in Comparative Research

A focus on the human dimension sheds light on one of the most elusive aspects of scientific inquiry: the process of generating ideas. Textbooks and courses on methodology center mainly on the issue of *testing* ideas, yet usually offer little insight about the prior matter of how one *generates* ideas worth testing in the first place.[24] Likewise, professional publications rarely include discussions of how ideas emerge. Because the interview format allowed an exploration of how leading scholars actually do their research, the material presented in this book opens a valuable window on the process of formulating good ideas. As discussed above, the interviews show that rich life experiences provide fertile ground for generating new ideas. Yet experience is not the only path to insight in comparative research. Scholars spend a large share of their time reading; and books, journals, and newspapers all play an indispensable role in the development of ideas. Moreover, directly observing political and social interaction is also an important tool for creative hypothesis generating. The interviews highlight five methods that help spark the comparative imagination of the leading scholars: (1) "bibliographic sleuthing," that is, hunting for untapped sources in libraries and bookstores; (2) following current events; (3) critical engagement with contemporary works; (4) reading, and rereading, the classics of political and social theory; and (5) real-time observation of political action.[25]

Bibliographic sleuthing, which involves searching, even haphazardly, in libraries or bookstores, can lead to the serendipitous discovery of works that provide new insight.[26] For example, while rummaging in a used bookstore in Rio de Janeiro, Schmitter found an obscure book written in the 1930s that triggered his insight that the system of interest representation in Brazil could be conceptualized as "corporatist."[27] Similarly, Skocpol discovered an old, forgotten book on social insurance in the United States in the early 1900s, which argued that Civil War pensions were a major social policy that

24. This imbalanced focus on hypothesis testing, as opposed to hypothesis generating, is not unique to political science and sociology. See McGuire (1997). On "tricks of the trade" for doing creative social science research, see Becker (1998).

25. This is not an exhaustive list of strategies for stimulating the comparative imagination. Still, this list contains the main strategies discussed by the fifteen scholars on which this book focuses.

26. Bibliographic sleuthing can be done increasingly on the Internet.

27. Manoïlesco (1934). For Schmitter's account of this incident, see Schmitter (1997b, 289–90).

would soon lead the United States to surpass Europe in the public provision of social benefits.[28] According to Skocpol, "When I read this, it made me curious, because the mere empirical assertion that, in 1913, a lot of government social spending was going on that amounted to de facto old age pensions cut against the grain of the whole literature that saw the United States as a laggard in social provision. I was skeptical at first . . . but I decided to look into the matter, because I had a hunch it might lead to something." Skocpol's hunch proved correct, and her probing resulted in a novel argument: the United States was actually a precocious welfare state, not a laggard behind European countries. This argument, in turn, played a pivotal role in her book *Protecting Soldiers and Mothers* (Skocpol 1992).

Following current events by reading newspapers and magazines can also serve to stimulate new ideas. Huntington says that reading "about what's going on in the world" plays a fundamental role in his research. He recounts how his observation of chaos, anarchy, and corruption across developing countries in the 1960s, "when everybody was talking about modernization and development," led to the insight that "there [was] more political *decay* out there than political development. And so I wrote *Political Order in Changing Societies* (Huntington 1968)." Reading about current events can work in tandem with bibliographic sleuthing in the formation of new ideas. While reading the newspaper in Switzerland one day, Schmitter saw an article about the role of the Swiss Milk Producers' Association in the annual price-fixing mechanism for milk.[29] He noticed that this regulatory framework bore a remarkable resemblance to the corporatist systems of interest intermediation he had previously studied in Brazil and Portugal. This realization led him to the library in search of material on Swiss interest group politics, where he discovered an unpublished dissertation from the 1930s on Swiss corporatism. As a result of his newspaper-inspired trip to the library, Schmitter saw that the concept of corporatism could be applied not only to authoritarian countries, but also to democratic ones. This insight anchored his influential article, "Still the Century of Corporatism?" (Schmitter 1974), as well as subsequent works that further elaborated the corporatist model of interest group politics as an alternative to the pluralist model.

Critical engagement with contemporaries is a further way to generate new ideas. Laitin describes how research on the relationship between culture and politics by contemporary scholars like Harry Eckstein, Aaron Wildavsky, and Arend Lijphart provided a compelling foil against which he developed and refined his own ideas: "I was going after Harry Eckstein from

28. Rubinow (1968). Rubinow's book was originally published in 1913.
29. In addition to Chapter 10, see Schmitter (1997b, 291–92).

the very beginning. I was arguing against Eckstein's congruence theory, which posited a kind of direct mapping from one realm—culture—on to another—politics (Eckstein 1966). In contrast, I said that there was no necessary connection between the cultural and other realms, between say religion and politics . . . My views also went against Lijphart and also against almost everyone who had been writing on culture." Laitin's critical engagement with the work of these interlocutors helped him formulate his idea that culture both shapes and is in turn shaped by political choices. Similarly, Skocpol notes that arguing against "mistaken others" plays a key role in the process of developing her own ideas: "I have always worked out what I was thinking by critiquing work done by others. What gets me excited is seeing that someone else is partly right and partly wrong . . . My major projects have always been launched with a sense of argument against a received wisdom or an interlocutor, especially somebody important whose work I respect."

Another way to stir the comparative imagination concerns *classic works* of political and social theory.[30] These classics play an important role in the intellectual life of leading scholars in comparative politics. Dahl sees himself as having engaged throughout his career in what he calls an "imaginary dialogue" with Plato, Rousseau, and Marx. Przeworski observes, "Reading classics of political theory is extremely important to me. It is a source of hypotheses, historical information, and great ideas." Schmitter offers: "For me, engaging the classics is almost automatic. I start by thinking about the nature of the problem on which I want to work, and then I ask myself, 'Who's said something about this?' Sometimes it is simply a matter of having these classic works in your head, having read them . . . My first instinct is to go through my own memory of what I have read in political thought."

Finally, Linz notes, "Whenever I start working on something, I usually look to see whether Weber has anything to say on that theme." To show how he draws ideas and inspiration from the classics, Linz recounts his use of Weber's concept of sultanism to study personalistic dictatorships, such as those of Anastasio Somoza Debayle in Nicaragua and Rafael Leonidas Trujillo in the Dominican Republic.[31] Because the degree of cronyism, nepotism, and unbridled discretion enjoyed by the ruler was so extreme in these cases, Linz felt uncomfortable classifying them in the same category as

30. For an insightful essay on the role of classics in modern social science, see Merton (1996a). It bears emphasis that not all the scholars interviewed in this book gain inspiration from reading the classics. Indeed, several (e.g., Laitin and Lijphart) say that the classics have little influence on their research.

31. Linz's initial formulation of the sultanistic regime concept is in Linz (1975). See also Chehabi and Linz (1998a).

regimes like Francisco Franco's in Spain and Antonio Salazar's in Portugal. According to Linz,

> Weber makes a distinction between a traditional, legitimate form of patrimonialism, on one hand, and the corruption of patrimonialism into sultanism, on the other. When I reread Weber's section on patrimonialism, I thought, "That's exactly what I am looking for!" Then I reformulated Weber's concept in a modern way by specifying indicators of sultanism, like nepotism, cronyism, and the private appropriation of power and wealth.
>
> You have questions in your own mind that you want to address, and sometimes you read the classics and say, "Well, that's an interesting insight, it illustrates what I was groping for." So, the more you read and the more you know, the better.

Real-time observation of political action is a further technique for generating fresh ideas. Scott describes how living in a Malaysian village for two years enabled him to conduct rolling interviews with peasants that helped him see the "subterranean forms of resistance to hegemony, such as desertion and foot-dragging, underneath the placid surface of the village."[32] Scott also emphasizes that "politics is everywhere," not just in the distant and exotic setting of "the field," and he offers a fascinating example of observing political interaction among passengers while riding on a train from New York City to Washington, D.C. Schmitter also highlights the value of observation, noting that his efforts to form new concepts are often stimulated by talking to political actors and listening closely to the words they use to describe what they do. Laitin's discussion of watching a Catalan national dance, the Sardana, while doing fieldwork in Barcelona offers an especially vivid example of how observation can help trigger new ideas:

> When the people perform the Sardana they put their little bundles of possessions in the center and dance around them. So, they developed an urban dance that enabled them to protect their property the whole time they were dancing. And they have to count a fairly large number of steps . . . I saw them counting their steps with their lips, though trying to hide it because you're not supposed to show it.
>
> Thousands of tourists have seen the Sardana; it happens all the time, and the dance itself is relatively boring. But to me it was inspirational, and I asked myself a very simple question. "Here I am in the most bourgeois city I've ever lived in, with a commercial bourgeoisie that goes way, way back, which developed an urban form of culture in which they can protect their property while dancing. And they count! It's the fundamental commercial function to

32. The results of this research were published in Scott (1985).

> count." Then I asked, "Why are people who are so rational and so calculating pushing a linguistic movement that would increase their communicative capabilities by zero? You would think the Catalans would be on this gigantic learn English campaign, which would be tremendously more useful for their commercial dealings. Why are they pushing this language, Catalan, which, if successfully promoted, will allow them to communicate with no more people than they presently communicate with, and which will have no communicative payoff whatsoever?" And I just walked through the town for the next two or three days, sort of like a zombie, asking and re-asking that question to myself.

Watching the Sardana made it easier for Laitin to see that the tools of game theory, especially the concept of coordination games, offered a powerful and fruitful way to explain why people participate in language movements that do not serve their material interests.[33] Laitin concludes, "this insight from Barcelona pushed my research program for quite a while, in utterly new directions. Fieldwork has that excitement for me."

Just as rich life experiences, passion, and risk taking are no guarantee of becoming a leading scholar, hunting for obscure books, perusing the newspaper, critically engaging contemporary authors, reading the classics, and making observations are, of course, not sufficient to formulate important ideas. After all, many social scientists read the newspaper and follow current events, yet few produce works with the impact of Huntington's *Political Order in Changing Societies* (1968) or Schmitter's "Still the Century of Corporatism?" (1974). And many people do fieldwork and make real-time observations, yet few achieve the level of insight seen in Scott's *Weapons of the Weak* (1985). Moreover, as Weber (1946a, 136) reminds us, "Ideas occur to us when they please, not when it pleases us."[34] Hard work, discipline, and perhaps a measure of luck are also necessary to develop good ideas, as is intelligence, especially the capacity to recognize an important question, puzzle, or lead when it arises.

Although factors like luck and intelligence are difficult, if not impossible, to control, there may still be ways to increase the probability of developing new ideas. The evidence from the interviews underscores the importance of openness to the possibility of surprise combined with the curiosity, confidence, and drive to follow a hunch. Moreover, by mastering the literature so that we have a firm grasp of the "conventional wisdom," we may be able to enhance our ability to notice puzzling new information. For exam-

33. On coordination games, see Schelling (1980).

34. Weber (1946a, 136) further notes, "ideas come when we do not expect them, and not when we are brooding and searching at our desks. Yet ideas would certainly not come to mind had we not brooded at our desks and searched for answers with passionate devotion."

ple, had Skocpol not understood that the standard view cast the United States as a welfare laggard, then she probably would not have seen that the book she serendipitously discovered through bibliographic sleuthing made an argument that cut sharply against the grain. And her fortuitous discovery of this book still might have led nowhere had she lacked either the curiosity to pursue the lead or the skepticism and confidence to question received wisdom.

While there is no magic formula for sparking the comparative imagination, the interviews in this book suggest that rich life experiences and the various methods of creative hypothesis formation discussed here are important aspects of the process of generating good ideas.

Against Professional Amnesia

Despite the important role that older works, especially the classics, play in inspiring some of the best scholars, the field of comparative politics has a feeble professional memory.[35] Few works have been written about the history of comparative politics, and students are rarely taught this history.[36] Indeed, graduate students are often discouraged from reading older works, which are routinely seen as passé and even "pre-scientific." Why is a weak professional memory a reason for concern? Does not scientific progress require amnesia? According to Weber (1946a, 138), "In science, each of us knows that what he has accomplished will be antiquated in ten, twenty, fifty, years. That is the fate to which science is subjected . . . Every scientific 'fulfillment' raises new 'questions'; it *asks* to be 'surpassed' and outdated." Claude Bernard asks pointedly, "What use can we find in exhuming worm-eaten theories or observations made without proper means of investigation?" And A. N. Whitehead offers the dictum, "A science which hesitates to forget its founders is lost."[37]

From this standpoint, the problem with comparative politics is too *much,* not too little, professional memory. The field has been too hesitant to

35. On the importance of professional memory—and its weakness in comparative politics—see Almond (1990, 23–29 and Part II).

36. A recent survey of graduate syllabi and reading lists for comprehensive exams revealed no items on the history of comparative politics among the canonical works of the field (i.e., works assigned by more than one-third of the thirty-two departments in the sample) (España-Nájera, Márquez, and Vasquez 2003). The virtual absence in comparative politics of works about the lives and contributions of leading scholars is a further sign of the field's weak professional memory. By contrast, sociology and economics have been more attuned to the biographies of their founders and leading lights.

37. The Bernard and Whitehead quotations are taken from Merton (1996a, 28 and 33). Similarly, Thomas Kuhn argues, "Science destroys its past." Quoted in Dryzek and Leonard (1988, 1249).

forget its founders, as seen by the large amount of time students spend reading antiquated "classics" from the 1960s, 1970s, and 1980s in proseminars and in preparation for comprehensive exams. Progress in the field requires that we purge these outdated works from the curriculum and replace them with recent, cutting-edge research.[38] A book like this one, which focuses retrospectively on authors of many of these older works, is useful, at best, for a course in the history of science and, at worst, poses a barrier to the advancement of science. There are no classics in science.[39] Ignorance of the past of comparative politics is not just bliss, it is necessary for the health of the field.

This book disagrees strongly with the idea that professional amnesia is desirable. First, a fundamental principle of modern science is that scientists should know and acknowledge prior work on the topic of their research. According to Robert Merton (1996a, 27), the "rationale for this is as clear as it is familiar: ignorance of past work often condemns the scientist to discovering for himself what is already known." Hence, professional amnesia is antiscientific. Dahl offers a good example of the perils of professional amnesia when he notes his frustration with the lack of progress since the 1950s that he discerns in the study of what is arguably the central subject of political science—power. "Fifty years later, I see people use the word and concept *power* as if we were back where we started. Even elementary distinctions going back to Max Weber—such as the distinction between power and authority, or legitimate power—seem to have been forgotten. So perhaps we've not only failed to progress in the study of power, we've actually gone into reverse." Professional amnesia can also lead to the problem of *misspecified ignorance,* that is, identifying false gaps in knowledge that would have been revealed as such had the researcher thoroughly reviewed prior work.[40] Professional amnesia poses a threat to progress in comparative research because it prevents us from benefiting from past accomplishments and increases the risk of both repeating past mistakes and reinventing the wheel (Almond 1990, 7–8).

Second, by expunging classic works from the curriculum, professional

38. An extreme variant of this position can be seen in Auguste Comte's "principle of cerebral hygiene." As Merton (1996a, 29) puts it, "[Comte] washed his mind clean of everything but his own ideas by the simple tactic of not reading anything even remotely germane to his subject."

39. One study of the "half-life" of journal articles found that in physics and biomedical journals there were virtually no citations to works older than ten years (Baum et al. 1976). Of course, professional amnesia is not always driven by scientific aspirations. It often results from the efforts of new generations of scholars to assert their independence from previous ones.

40. On Merton's concept of "specified ignorance"—the recognition of "what is not yet known but needs to be known in order to advance the pursuit of knowledge"—see Sztompka (1996, 11).

amnesia robs us of inspiring models of intellectual excellence. Reading the classics allows us to watch great minds at work. O'Donnell's description of Weber illustrates the point: "To see [Weber] think through a problem, to see how his mind works, is very instructive . . . He is my model of intellectual power." Classic works are indispensable tools for cultivating standards of taste and good judgment (Merton 1996a, 31–32). Moreover, as highlighted in the interviews, reading the classics serves as an important way to generate new ideas.

Third, by producing ignorance about what comparative politics has accomplished, professional amnesia contributes to a crisis of confidence about the field. Laitin argues persuasively that political scientists should take greater pride in the many achievements of the discipline over the past fifty years.[41] This, of course, requires that we first *know* what the field has accomplished, which is obviously not possible without a strong professional memory.[42]

Finally, while "a serious student of physics . . . can safely ignore the original writings of Newton, Faraday, and Maxwell,"[43] and, likewise, a biologist does not need to read Darwin's *On the Origin of Species,* no serious student of political order can ignore Hobbes and Huntington, no serious student of democracy can ignore Aristotle, Schumpeter, Dahl, and Lijphart, and no serious student of revolutions can ignore Tocqueville, Moore, and Skocpol. Comparative politics is defined by a fundamental continuity in what is worth knowing; and this continuity differentiates the social from the natural sciences. This core of perennial questions and themes gives classic works an enduring vitality in the social sciences.[44]

The past holds the key to our field's identity. If professional amnesia severs our connection to the history of our field, then comparative politics will be soulless, condemned to perpetual envy and imitation of other fields and disciplines with a stronger sense of where they come from, and, hence, who they are.

Conclusion

Focusing on the human dimension sheds light on key aspects of comparative research. It reveals that the best researchers have rich life experiences, are passionate about scholarship, and take risks. It offers fresh insight about

41. See Chapter 16 and also Laitin (2004a).

42. On the inextricable link between professional history and identity, see Dryzek and Leonard (1988).

43. M. M. Kessler, as cited in Merton (1996a, 24).

44. The existence of an enduring core of themes and questions in comparative politics is emphasized especially in the Dahl and Laitin interviews in Chapters 5 and 16.

how to generate new ideas. Finally, it illuminates major challenges facing comparative politics. Because the quality of comparative research depends in good part on the quality of the life experiences of the people who do it, the experiential deficit observed by leading scholars among students today raises concerns about the future vitality of the field. *Steps should be taken to ensure that students and professors, too, find ways to enrich their lives by regularly stepping outside the academic framework.* Passion about research is in jeopardy because of the widespread tendency for professors and students alike to regard scholarship as just a 9-to-5 job. *To avoid this iron cage of professionalism, enthusiasm for research as a "calling" should be cultivated and rewarded, which requires acknowledging that emotional engagement and normative commitments are compatible with, and even necessary for, excellence in scientific research.* Professionalism threatens to squelch risk taking and creativity. *Incentives for innovation should thus be strengthened to prevent the hegemony of a herd mentality.* Finally, professional amnesia is depriving us of powerful models of intellectual excellence and weakening our self-confidence about the achievements of the field. *We need to improve professional memory by knowing, teaching, and drawing inspiration from the history of our field.*

To conclude, the following recommendations for aspiring scholars can be drawn from the examples offered by the fifteen leading comparativists interviewed in this book:

1. Get off the academic track and gain real-world experience by working or traveling before you go to graduate school. This will make you a better social scientist by helping infuse your research with meaning and purpose. It will also provide a stronger foundation of knowledge about the range of human behavior, which can serve both as a source of fresh ideas and as a basis for testing generalizations.
2. If circumstances do not permit you to take time off before graduate school, then doing a fieldwork-based dissertation is probably the next best way to gain experience. Consider extending the amount of time you spend in the field. Fieldwork provides an indispensable empirical grounding for comparative research, helps hone skills of observation, and should be seen as a lifelong investment that will inform your research over the course of your career, even if you never do fieldwork again.
3. Study with faculty who are enthusiastic and excited about their research and do not see scholarship as just a 9-to-5 career. Have fun doing your research, because the more enjoyment and pleasure you get from it, the better it will probably be.
4. Build strong, interactive communities with other students and with your professors that get beyond the confines of the classroom and for-

mal training. Interaction outside the classroom in study groups, workshops, and even social gatherings can help strengthen your enthusiasm for research.

5. Do not be afraid to let normative commitments shape your selection of research problems or to explore the normative implications of your work. This will nurture your passion for research. But do not let normative commitments blind you to "inconvenient facts" that do not support your position.
6. Take measured risks. Enroll in courses that excite you, even if they are offered by professors in other subfields and departments. Know and master mainstream research, yet try to stand with one foot outside the mainstream. Do not apprentice yourself to a single professor, but gain exposure to a variety of faculty with different perspectives. As you advance and get tenure, you can afford to take greater risks.
7. Look beyond professional fashions and fads by paying attention to classic and older works and also to the wisdom of senior scholars. See yourself as part of a field with a distinguished lineage reaching back to antiquity.

Combined with recent important advances in the methodological training of students, a stronger focus on experience, passion, risk, and professional memory holds the promise of new generations of comparativists whose achievements match, and even surpass, those of their most illustrious predecessors.