



*Elnor Ostrom*

# A Long Polycentric Journey

Elinor Ostrom

Workshop in Political Theory and Policy Analysis, Indiana University, Bloomington,  
Indiana 47408; email: [ostrom@indiana.edu](mailto:ostrom@indiana.edu)

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## Key Words

institutional analysis, collective action, public goods, common-pool  
resources

## Abstract

In this account, I discuss my own personal journey and the efforts of many of us associated with the Workshop in Political Theory and Policy Analysis at Indiana University to develop better analyses of how institutions affect behavior and outcomes in diverse settings. First, I reflect on my experience as a young student and in my early career, primarily to encourage those who face obstacles. Then, I discuss our institutional analysis and our research on urban governance and common-pool resources, which helped me to develop more general frameworks for the analysis of complex systems over time. The frameworks have enabled us to dig into and analyze system structure, behavior, and outcomes to make and test coherent predictions and build better theory. Last, I share some ideas concerning future scholarly directions.

## INTRODUCTION

To an outside observer, my career may look rather successful at the current time. Has it always been this way? To be honest, the answer is no. My entry into an undergraduate major in political science was almost accidental. Fortunately, I had a short business career before starting my graduate program or I might have been discouraged by the advice I was given when I applied to graduate school. My research interests took me down a long and interdisciplinary path to the study of complex social-ecological systems—a path that many colleagues in political science strongly criticized.

I was deeply interested in how institutions were initially crafted, and then how they affected the incentives and outcomes of human interactions in many settings. Although the theory I participated in developing was general, most of the settings where I conducted empirical research were viewed by some in my home discipline as being irrelevant to political science. Why was I studying local governance and policing, or irrigation systems in Nepal, or peasants, or forests? A political scientist was expected to study the parliaments or bureaucracies of national or international regimes and not the design, operation, and adaptation of rule systems at lower levels.

I also participated in an extensive experimental research program in the 1980s, when laboratory experiments were not considered an acceptable method by political scientists. Then, as I dug into institutions and recognized their complexity, some of my formal-theory colleagues called me “stupid” for analyzing rule systems that involved multiple types of rules. They argued that this level of complexity was unnecessary and made analysis of institutional structures and results too difficult.

Thus, as a scholar who has addressed questions related to multiple institutions at diverse and nested levels using varied empirical and theoretical methods, I have certainly not followed a straight course, but I hope I can describe some of the paths of my journey and report on what I see for the future.

## MY PERSONAL JOURNEY AND EARLY PROFESSIONAL LIFE

### High School

Colleagues have asked what led me to become a political scientist. One answer is that I became a political scientist because I stuttered in high school. During my junior year, one of my teachers was quite concerned about my stuttering and told me to join the Speech Club. My first assignment was to recite poetry, but at the first competitive speech event, other members of the speech team kidded me for doing that “sissy” stuff. They indicated that the “real” speech team did debate. So I sat in on competitive debates, thought that looked like fun, and joined the debate team. For two years I competed against teams from many other California high schools. Learning how to debate was a great background for a scholarly career. As a debater, you learn that there are always at least two sides to every issue, because in the diverse rounds of one tournament you are likely to be assigned both sides of the debate topic, and you must be prepared to make an effective argument for whichever side you are assigned.

### College

When I arrived at the University of California, Los Angeles, I first asked if I could major in debating. They told me no. I had no idea what I wanted to do after graduation, and the freshman advisor indicated that the best major for a girl was in education so that you could get a job as a teacher. So I was assigned to education as my initial major. In my first semester, I took Political Science 101 and had an excellent Teaching Assistant who gave us an exciting course. I immediately switched and became a political science major. I also took a lot of economics and business courses. Fortunately, I did very well in my economics classes and was asked in my junior year if I would help grade the freshman economics exams. I ended up grading economics exams for a year and a half.

This job was one of many that paid my way through UCLA. I was the first kid in my family to go to college. Neither my mom nor my dad had any education beyond high school, and my mom considered college a “useless investment,” as she had received no family support after high school and had to go to work. Thus, I worked my way through my B.A. degree. In the summers I taught swimming, and during the academic year I worked 25–30 hours a week in the university library, in a local dime store, and in other miscellaneous jobs.

I graduated from UCLA in 1954 and ventured to Cambridge, Massachusetts, to help put my first husband through Harvard Law School.

### First Full-Time Job

Looking for a job in the 1950s as a female and as a new college graduate was an “instructive” experience. The first question in every interview was whether I had typing and shorthand skills. After working for a year as Export Clerk in a Cambridge electronics firm, I finally landed a position as Assistant Personnel Manager in a distinguished Boston firm that had never hired a woman for any position above secretary. I volunteered to work for several months without pay to convince them I could do the job. That turned out to be unnecessary, but I still had to prove myself repeatedly. When I returned to Los Angeles in 1957 and applied for a professional position in the Personnel Office at UCLA, I was greatly relieved to learn that I had received a strong recommendation from my Boston employer. This was particularly gratifying because I had been able to diversify the firm’s staff, previously all white and Protestant or Catholic, to include several new employees who were black or Jewish.

### Graduate School

While I was working in public personnel on the UCLA campus, I thought I should obtain a master’s degree in public administration. I took one graduate seminar per semester for a year,

decided that I liked graduate work, and began to think about pursuing a Ph.D.

Admission to the M.A. program on a part-time basis had been routine, but admission to a doctoral program and obtaining an assistantship so I could pursue full-time graduate study were far from routine. The graduate advisor in economics strongly discouraged pursuit of a Ph.D. in economics because I had so little mathematics background (due to earlier, poor academic advice), but he did approve of an outside minor in economics if I pursued a Ph.D. in political science. The graduate advisor in political science strongly discouraged me from thinking about a doctorate, given that I already had a very good “professional” position. He indicated that the “best” I could do with a Ph.D. was to teach at some city college with a very heavy teaching load. My earlier experience with finding a professional position in Cambridge led me to ignore this warning and apply for an assistantship so I could pursue a Ph.D. on a full-time basis. Fortunately, I was granted an assistantship.

Surprisingly, the Financial Aid Committee awarded four assistantships to women that year after 40 years without a woman on the faculty or as a Ph.D. student. The four of us learned mid-semester that this decision had been strongly criticized at a faculty meeting. Some faculty members were concerned that allocating four out of 40 assistantships to women was a waste of departmental resources. They feared that none of us would obtain good academic positions, which would harm the department’s reputation. Fortunately, fellow graduate students encouraged the four of us to ignore the concerns of the faculty who opposed our appointments. They also advised us whom to stay away from during our graduate program if we could.

The discipline of political science was (and continues to be) divided into the study of governments and the study of political philosophy. In political philosophy courses, one was frequently taught the lives, loves, and miscellaneous thoughts of the great thinkers rather than ways of understanding the core theories underlying the development of a discipline.

One studied Hobbes, Rousseau, Machiavelli, and Locke individually, without an effort to see how they built or did not build on one another.<sup>1</sup> The pathbreaking work of Alexis de Tocqueville and *The Federalist Papers* were rarely taught in political philosophy courses because they were thought of by many political scientists as strictly descriptive narratives of a particular period or as journalistic efforts to convince the public to support a new constitution (but see Dahl 1956; V. Ostrom 1991, 1997).

The study of government was divided up by regions or domains. One studied American government, comparative government, or international relations. Within comparative government, one focused on a continent. Geographic regions were thus more important in demarking the discipline than theories.<sup>2</sup> If one studied international relations, one focused on relationships among states. In fact, the predominant view of many political scientists was that political science was the study of “the state,” even though little agreement existed as to how to define this central concept. Titus (1931), for example, identified no fewer than 145 different definitions of “the state.” Eckstein (1973, p. 1142) commented four decades later on “one of the more peculiar facets of contemporary political studies,” namely “the remarkably numerous and various attempts by political scientists to define explicitly the nature of what they study.”

Those studying comparative government learned how to compare the structure of upper and lower houses in all major European countries, some Latin American countries, and perhaps a few in Asia. Africa was still the dark continent for most American political scientists—at least until the Peace Corps returned a

large number of future political scientists, and African Studies programs were initiated in several major universities. The role of political parties was also of major concern. One question on my comprehensive exams was: “Compare the recent history and role of the two major parties in England, France, and two countries in Latin America (of your own choice).” Another question was: “Compare the Upper Houses of Canada, Germany, and Japan.” Graduate students in political science were asked to compare, list, and describe rather than to examine the empirical evidence related to Theory A or Theory B.

## Dissertation

I was particularly lucky that my first assistantship was in the Bureau of Governmental Research. That enabled me to participate in meetings with faculty and advanced graduate students working through some of the early thinking about a political-economy approach to the study of urban government. The pathbreaking article by V. Ostrom et al. (1961), entitled “The Organization of Government in Metropolitan Areas: A Theoretical Inquiry,” was on the table several times for intense discussion. Early in my program I took a research seminar on the organization of local public economies with Vincent Ostrom.<sup>3</sup> (That was the last seminar I took with Vincent, because we started to date and eventually married.) He asked each graduate student to pick one groundwater basin located in Southern California. Our task was to examine the processes used in each basin to cope (or fail to cope) with the problem of a growing population and declining water availability while no political jurisdiction had the same boundaries as any of the groundwater basins (V. Ostrom 1962).

My assignment was the West Basin, which underlay a portion of the city of Los Angeles

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<sup>1</sup>It was not until I read Mayr's (1982) book, *The Growth of Biological Thought*, that I found a really exciting “history of thought” that looked at core concepts and how they were defined and put together into a theory, challenged by evidence or other theories, and developed over time.

<sup>2</sup>This geographic demarcation is still the predominant way of organizing departments and field examinations, with the exception of two fields in some departments—methodology and policy analysis.

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<sup>3</sup>I am eternally grateful to him, not only for his intellectual insights and encouragement over the years, but for starting Lou Weschler, Ellis Perlman, and several others of us on a very exciting path to discovery.

and 11 other cities. During the first half of the twentieth century, water producers ignored the facts that the level of groundwater underlying Los Angeles was going down and seawater was intruding along the coast. Toward the end of World War II, several municipal water departments asked the U.S. Geological Survey to conduct a major study of the area and agreed to fund one third of the study. The report detailed a grim picture of substantial overdraft and threat of further saltwater intrusion that could eventually ruin the basin for human use.

I attended regular meetings of the West Basin Water Association. It was a private association open to all water producers in the basin. What started as research for a seminar paper grew into research for my dissertation. It was great that I could drive half an hour and be in “my research site.” That proximity gave me many occasions for in-depth discussions with local participants, including state and local government officials; to spend hours in the association archive, which was fully available to me; and to study the proposals made in a diversity of arenas to try to solve this complicated political problem over time.

While I was studying a single case, I was engaged with a group of faculty and graduate students discussing several cases that had used other strategies with diverse results. Thus, as a graduate student I was beginning to understand some of the core factors that affect collective-action problems at a local level. In light of many more years of study, I now recognize that the core attributes of the West Basin problem were as follows:

1. Large *n*. Over 500 water producers were involved, ranging in scale from individual farmers using one well to municipal water utilities serving large populations.
2. Legal uncertainty. Three contending legal rules could potentially be used as “the” rule to allocate water to the various users.
3. Asymmetric interests. Producers whose wells were close to the Pacific Ocean were most exposed to the overdraft, the level of dependency on groundwater varied substantially across producers, and a legal

settlement could potentially create substantial winners and losers.

4. Lack of fit between “problem” and governmental units. All relevant governments were smaller or larger than the groundwater basin, and none had the authority to implement and enforce a solution.

These attributes characterize many situations related to social dilemmas in general and common-pool resources in particular (Baland & Platteau 2000, Berkes 1989). Because I had studied a case with these characteristics and had witnessed the development of innovative solutions over time, I was skeptical when later theoretical arguments stressed that these attributes made it highly unlikely, if not impossible, for such collective-action problems to be solved (Dawes et al. 1986, Hardin 1971, Olson 1965).

By the time I defended my dissertation, the local actors involved in the West Basin problem had designed a complex polycentric system with several positive aspects. First, producers representing 80% of the water produced had reached a legal agreement to reduce groundwater production that was enforced by the California Supreme Court, with annual reporting to all producers regarding compliance and the capacity to adjust water allocations over time if needed (Blomquist & Ostrom 2008). Second, water engineers had developed an innovative system to replenish groundwater through freshwater injection wells along the coast as well as a spreading ground inland that relied on recycled water. Third, the producers had designed a new type of special district that they asked their local representative to introduce for passage by the California State Legislature.

The system was genuinely polycentric, rather than monocentric, which has been the dominant way of thinking about political systems in the discipline. A national agency—the U.S. Geological Survey—responded to local requests and partial funding to undertake the initial study. The state of California, the Los Angeles County Flood Control District, and the Metropolitan Water District each played a

role but none was the central authority. Eleven city governments were involved—one of which was a strong hold-out in the early years of bargaining over water rights. Several large private firms were involved, including Standard Oil, who strongly urged other producers to agree to a cutback so that all could continue to use the basin.

Regular meetings of the West Basin Water Association brought this diverse set of actors together to discuss problems in an open forum. The evolved system was not a perfect solution, but it did solve many problems better than some other basins in the region and in nearby states (Blomquist & Ostrom 1985, Weschler 1968). For his dissertation, Blomquist (1992) returned to study the performance of West Basin and seven other California basins in the late 1980s, and assessed the system as working effectively over time. Continuing research on this complex governance system has shown it to be a relatively robust set of institutions (Steed & Blomquist 2006), despite some challenges due to conflict with its upstream neighbor and to a public official who used public funds for personal gain.

Undertaking this study gave me a deep respect for individual case studies based on intensive fieldwork. If one is in a location for some time, one can talk with multiple sources about the same events, potentially check out written correspondence and other documents, analyze historical data, ask respondents about puzzles related to conflicting information, and gain a deep understanding of complex processes. As my colleagues and I stress in our forthcoming book (Poteete et al. 2010), individual case studies are a very important method for social scientists to include along with larger-*n* field studies, meta-analysis, formal models, and experimental research. None of these should be viewed as the only way or best way to do research.

When I was doing my West Basin research in the early 1960s, neither Olson's (1965) book on *The Logic of Collective Action* nor Hardin's (1968) article on the "Tragedy of the Commons" had been published. I looked at my case as an example of a difficult conflict affecting a region for

which no single government existed, which was solved as a result of the public entrepreneurship of those involved and the availability of diverse private and public arenas in which they could develop a solution. I drew largely on *The Calculus of Consent* by Buchanan & Tullock (1962) and Stigler's (1962) work on the functions of local government, as well as on Schumpeter (1942).

Since 1968, the attention paid to the work of Mancur Olson and Garrett Hardin has been massive. Hardin correctly identified the potential for everyone involved in harvesting from a common-pool resource (like the pasture he described or the groundwater basins we had studied) to try to harvest as much as they could so as to be a short-term winner, leading to long-term losses for all. That kind of conflict initially happened in West Basin. But in this case (and now I know many others), the producers were not trapped, as Hardin portrayed them. They had the legal facilities of a state court, the California Department of Water Resources, and the U.S. Geological Survey who could provide them with accurate information as well as arenas in which they could struggle together to find solutions. It was indeed a struggle, as are many collective-action problems related to the use of a commons (Dietz et al. 2003). I learned early, however, that individuals facing such problems do not always need an external authority to extract them from their tragedy. When they have arenas in which they can engage with one another, can learn to trust one another, can draw on sources of reliable data, can ensure monitoring of their decisions, can create new instrumentalities, and can adapt over time, they are frequently, though by no means always, able to extract themselves from these challenging dilemmas.

## Becoming an Assistant Professor

When Vincent was recruited to Indiana University, I was not invited to visit Bloomington, but fortunately IU did not have the kind of nepotism rule that many universities (including UCLA) had at that time, which would have precluded my eventual employment anywhere

on campus. The position did look very good for him and I enthusiastically recommended that he accept it.

Bloomington has turned out to be a lovely place. When we first arrived in January 1965, we were fortunate to find acreage in the woods surrounding the university. We spent that first semester designing a house and worked together with a crew that summer in building it. Vincent was then Editor of the *Public Administration Review* and needed help in processing submissions and reviews. IU did not provide good secretarial or Research Assistant support at that point, and so I was quite busy assisting him.

During the summer of 1965, colleagues in the Political Science Department asked if I would teach an Introduction to American Government course to be offered for freshmen that fall at 7:30 A.M. on Tuesday, Thursday, and Saturday. Because one of my field exams had been in American government, I felt relatively comfortable taking this on as a Visiting Assistant Professor, and I taught it for a year. When the graduate advisor for the department became dean, the chair asked me to become graduate advisor. The chair knew I was a little older and had a fair amount of personnel experience and thought I could handle it. I was pleased with the offer but suggested that they might not want to have a Visiting Assistant Professor as their graduate advisor. At this point, they appointed me as a regular Assistant Professor. Thus, I spent the first years of my regular appointment serving as graduate advisor during the Vietnam era, when our entry class came close to 90 students each fall. Needless to say, I was not able to start an ambitious research program in my early years as an Assistant Professor.

My early years in the profession were not unusual for women social scientists in the 1960s and 1970s. You were very lucky to be admitted to graduate school. Finding an appointment as Assistant Professor was difficult even if your dissertation was awarded an academic prize. Affirmative action policies created new opportunities and changed the professional environment. Once male colleagues became comfortable

interacting professionally with female colleagues and witnessed their research and teaching capabilities, further opportunities opened up. It is a great relief to me to witness a major change in the academic opportunities available today. Although discrimination has not disappeared, the proportion of women admitted to graduate programs, hired as Assistant Professors, and then tenured, has steadily risen over time. Formal affirmative action policies are not now invoked as frequently, since many departments recruit openly and treat women and minorities as equal applicants.

## MY RESEARCH

### First Policing Study

After I turned the graduate advisor position over to a colleague, I was able to offer a year-long graduate seminar in 1969–1970 on the theories related to urban government and the measurement of public goods and services. The Ph.D. students in that seminar included William Baugh, Richard Guarasci, Roger Parks, Dennis Smith, and Gordon Whitaker. The fall semester gave us an opportunity to review the extensive literature on urban governance and service delivery (Friesema 1966, Lineberry & Fowler 1967, V. Ostrom et al. 1961, Stigler 1962). There were two dominant approaches—metropolitan reform and public economy (V. Ostrom & E. Ostrom 1965). As we began to unpack the theory underlying these approaches, we found both posited that the size of governmental units in a metropolitan area affected the output, efficiency, distribution of costs to beneficiaries, citizen participation, and responsibility of public officials, but the direction of the posited relationship differed.

Advocates of metropolitan reform made strong policy proposals to eliminate what they called “fragmentation” of urban services. Multiple units of local government providing services were considered chaotic and thus ineffective. They had not undertaken serious research, however, to test their assumptions

and predictions (Bollens & Schmandt 1970, Committee for Economic Development 1966). When voters repeatedly rejected their proposals, the reaction was to criticize citizens for their ignorance (Hawley & Zimmer 1970, Zimmerman 1970).

Advocates of the metropolitan reform approach assumed that size of governmental units would always be positive for all types of goods and services. Scholars using a political economy approach assumed that size of governmental units would be positive *or* negative depending on the type of public good or service (E. Ostrom 1972). Those involving face-to-face delivery, such as education, policing, and social welfare, would show a negative effect of governmental unit size; those involving economies of scale, such as highways and utility systems, would show a positive effect (Hirsch 1964, V. Ostrom & E. Ostrom 1971, Stigler 1962).

Decreasing the number of governmental units within metropolitan areas was presumed to have a positive impact on all dependent variables by scholars supporting the metropolitan reform approach. In regard to citizen participation and responsibility of public officials, the political economy approach expected that decreasing the number of governments in a metropolitan area would have a negative effect and that its impact on output, efficiency, and distribution of costs would depend on the type of good or service involved.

We did have a “case” nearby that made our effort to understand these diverse approaches highly relevant. Richard Lugar, as mayor of Indianapolis, had undertaken an initial reform of the governmental structure in Indianapolis County in 1969. Unigov, as it was called, increased the power of the mayor and eliminated some of the smaller township governments. He proposed this as the first step toward a more general consolidation of all local governments within Indianapolis County, but this plan had not yet moved forward at the time of our seminar.

Roger Parks had a fantastic idea for a research design to study policing in Indianapolis. He pointed out that there were three

independent, small police departments serving neighborhoods immediately adjacent to socioeconomically very similar neighborhoods being served by the much larger Indianapolis City Police Department. That gave us a natural experiment. Our study measured the performance of policing in these six urban neighborhoods using survey research and building on recent rigorous methodological studies (Wilson 1966, Wolfgang 1963). Fortunately, I taught an undergraduate honors seminar during the spring semester after the graduate students and I had worked through substantial literature on local public economies and public service measurement. The honors students wanted to do something different than a regular class, so I suggested they work together with the graduate seminar to develop a rigorous method for studying the effects of size of police department. The honors class was very enthusiastic and worked hard with the graduate students to build on the work of other scholars in developing our own survey instruments and pretesting them extensively. With Roger’s knowledge of Indianapolis and good census maps, we were able to draw a very good area probability sample of the six neighborhoods. I had no external research funds, but the Urban Affairs Center on campus supported our efforts by renting IU vans to carry us up to Indianapolis.

Thus, our first effort to develop a coherent research program to test the opposed theories of urban governance focused on the size of governmental units and not on the number of such units in a metropolitan area. Our study generated some surprising findings—at least to those scholars who presumed that larger urban governments would always produce superior public services. In contrast to their neighbors served by the Indianapolis City Police Department, households in the three communities served by their own smaller police departments faced a lower victimization rate, were more likely to call upon the police if they were victimized, received higher levels of policy follow-up, and evaluated the performance of their police department more positively (see E. Ostrom & Whitaker 1973 and E. Ostrom et al. 1973 for

a more detailed presentation of our findings and Blomquist & Parks 1995a,b for a follow-up study that found similar patterns 20 years later).

Mid-semester, several black students asked why I was studying “community control” in white neighborhoods when the issues were so important for black citizens in urban areas. I asked them if they knew of any locations where several black communities having their own police departments were located very near to a large, central city police department. They described such a setting on the south side of Chicago. I suggested that they go up and talk with the community officials of the small black cities to see if they would be supportive of a study, and if the answer was yes, I would approach the Chicago Police Department and see if I could teach an undergraduate course in IU’s Afro-American Studies program in the fall. All inquiries led to affirmative answers.

During the summer of 1970, I wrote my first National Science Foundation grant proposal to support a series of future studies using a “most similar systems” research design (Przeworski & Teune 1970) where similar neighborhoods were served by local governments of varying size. I proposed to rely on multiple measures, including survey research, analysis of internal municipal records, and a random sample of police car assignments in which we could observe police-citizen interactions first hand. Although I was a young, untenured faculty member without an extensive publication record (see E. Ostrom 1968, V. Ostrom & E. Ostrom 1965), I had demonstrated the capability of designing and carrying out a rigorous study in Indianapolis, and this was probably an important factor in my receiving a grant on my first try and publishing work that led to a positive tenure decision in 1974.

In the fall of 1970, I worked with an outstanding group of black students to conduct a study in two poor, independent black communities that we matched to three similar communities being served by the Chicago Police Department. At the time of our study, the two small communities had just a few police

officers. Their wages were low and their official cars were frequently out of service because their budgets were so limited. The Chicago Police Department had a force of >12,500 men who were paid relatively high wages. We estimated that expenditures for police service in the three Chicago neighborhoods were 14 times the amounts spent on policing in the small communities (E. Ostrom & Whitaker 1974). But despite the huge difference in spending, we found that in general the citizens living in the small cities received the same or higher levels of services compared to the residents in Chicago. Although victimization rates were similar, citizens living in the small independent communities were less likely to stay home due to fear of crime, and they agreed with statements that their local police treated all citizens equally according to the law, looked out for the needs of the average citizen, and did not take bribes (E. Ostrom & Whitaker 1974). The findings from these initial studies were consistent with the political economy theory.

In-depth case studies are strong in regard to internal validity, but the findings might reflect unique aspects of the settings studied. To test for external validity, we drew on a large survey of ~2000 citizens living in 109 cities with populations of >10,000 conducted in 1966 by the National Opinion Research Center (E. Ostrom & Parks 1973). We added data from the Municipal Year Book on city size and expenditure levels. We found a consistently positive relationship between city size and expenditure levels, but expenditure levels were not related to better citizens’ evaluations of the services provided. For example, the fear of being attacked or having one’s home broken into was positively related to city size.

We then conducted a much larger field study in the St. Louis metropolitan area, which was served by two large departments (the St. Louis City Police Department with 2200 officers and the St. Louis County Police Department with 436 officers). Within the county, two thirds of the 93 separately incorporated communities had their own police department ranging in size from <10 up to 76 full-time officers. We

developed a “most similar systems” research design to study 45 sample areas distributed across three strata of neighborhoods that were similar in wealth and age structure and were served by police departments that varied from small to large (E. Ostrom 1976a,b, Parks 1976). We found a strong and significant positive relationship between size of police department and per capita costs of police services in a neighborhood as well as the percentage of households that had been victimized. Negative relationships existed between size of department and the percentage of households that were assisted who indicated that police response was rapid, rated the job of the police as outstanding, or evaluated their police to be honest (E. Ostrom et al. 1973).

Replications of our empirical work were undertaken in Grand Rapids, Michigan (IsHak 1972), and in the Nashville–Davidson County area of Tennessee (Rogers & McCurdy Lipsey 1974). In this full set of studies, no one found a *single* case where a large centralized police department was consistently able to outperform smaller departments serving similar neighborhoods across multiple indicators.

Thus, we provided very strong support for the posited relationships of the political economy approach. For policing, increasing the size of governmental units consistently had a negative impact on the level of output generated as well as on efficiency of service provision. Why?

In our efforts to understand why smaller police departments so consistently outperformed their better trained and better financed larger neighbors, we developed the concept of “coproduction” of public services (Parks et al. 1981). This involves a mixing of the productive efforts of consumer/citizens and of their official producers. It is feasible for governments to produce highways and other physical infrastructure without the active involvement of citizens, but we observed citizens and their officials working together more effectively in the small- to medium-sized communities we studied, and this collaboration has important effects on policing. In the smaller communities, citizens take a more active role in monitoring their neighborhoods, notifying the police

rapidly when suspicious activities occur or when victimized. Systematically observing officers on duty in their patrol cars demonstrated that officers in the smaller departments also had greater knowledge about the areas they served. Thus, not only were there diseconomies of scale in the formal production of services in urban areas—as posited by the political economy approach—but human services could not be effectively produced by official agencies alone. Citizens are an important coproducer. If they are treated as unimportant and irrelevant, they reduce their efforts substantially.

### Studying Complex Urban Systems

Our initial studies of urban governance focused on the impact of the size of a government producing a service: how the size of the governmental unit affects the output and efficiency of service provision. Once we had finished a cumulative series of studies, we turned to the more difficult question of the impact of the number of governments providing a service in a metropolitan area. We were very happy when the Research Applied to National Need (RANN) division of the National Science Foundation requested proposals to study the organization of service delivery in metropolitan areas. We were of course even happier when we were funded!

Most prior studies describing the large number of governments producing urban services had stressed the chaos of having multiple units in the same region. Few empirical studies had been conducted to try to understand the structures that officials and citizens had created and the consequence of these structures. Given the earlier theoretical work on public service industries (Bish 1971, Bish & Ostrom 1973, V. Ostrom et al. 1961), we wanted to develop methods for measuring interorganizational structure in a consistent manner—other than the simple lists of units that scholars deemed sufficient evidence of the need for reform.

In light of discussions with police officers and chiefs over the years and in the early phases of planning the new study, we grouped the rich

set of services provided in most metropolitan areas into two types: direct and intermediate. Direct services include patrol, immediate responses to reports of traffic accidents or crimes, and investigation. While police departments are the producers of direct services to local citizens, they are the consumers of intermediate services, such as detention, basic training, criminal laboratory facilities, and dispatching. The provider of intermediate services may or may not be a police department.

Economists had long since developed rigorous methods for arraying the industry structure of buyers and consumers of specific private goods and services, but few efforts had been directed to developing methods for measuring the structure and results of the production of public goods and services. We developed a new method using service structure matrices to describe the interorganizational structure, to measure it in 80 metropolitan areas, and then to assess systematically how structure affected performance.

Despite the many police departments serving the metropolitan areas that we studied, duplication of services to the same set of citizens was rare (E. Ostrom et al. 1978a). Further, the presumed negative impact of a multiplicity of departments serving the same metropolitan area was *not* found. In fact, the “most efficient producers supply more output for given inputs in high multiplicity metropolitan areas, than do the efficient producers in metropolitan areas with fewer producers” (E. Ostrom & Parks 1999, p. 287). Reformers’ assertion that a small department should not have its own criminal laboratory turned out to be correct, but the police agencies themselves had already figured this out! Most metropolitan areas were served by only one crime lab, frequently located in a hospital or in one of the larger police departments. We developed several measures of technical efficiency and found that metropolitan areas with large numbers of autonomous patrol producers achieved higher levels of technical efficiency (E. Ostrom & Parks 1999, p. 290). However, technical efficiency is also enhanced in metropolitan areas with lower numbers of

producers providing criminal laboratory services and radio communication (both intermediate services).

By digging in to systematically study police services on the ground and gathering careful data across 80 metropolitan areas, we were able to reject the theory underlying the proposals of the metropolitan reform approach. We demonstrated in *Patterns of Metropolitan Policing* (E. Ostrom et al. 1978a) that complexity is not the same as chaos in regard to metropolitan governance (see also McGinnis 1999, E. Ostrom et al. 1978b). That lesson has been carried forth as we have undertaken further empirical studies of the governance of resource systems across the world.

## Developing an Institutional Framework

In addition to conducting research to test the metropolitan reform and political economy approaches to understanding urban governance throughout the 1970s, I offered a graduate seminar on the microanalysis of institutional arrangements. Vincent Ostrom developed a parallel seminar on the impact of institution design on democratic behavior and outcomes at a national or international level. Our seminars were offered in the Political Science Department, but our colleagues did not like our approach because we drew on economic theory as well as political thought. Graduate students were frequently advised against taking our seminars. Fortunately, enrollments, though small, were just enough to allow us to develop our approaches to institutional analysis.

In the fall of 1980, Vincent joined a year-long research effort called “Guidance, Control, and Performance Evaluation in the Public Sector” at the Center for Interdisciplinary Research (ZiF), Bielefeld University (see Kaufmann et al. 1986). I took my first sabbatical leave and joined the group in Germany for the spring semester and summer of 1981, and I returned for the summer of 1982. This was an important event in both of our intellectual journeys. It was wonderful to be with

academics from multiple disciplines—including Christopher Hood, Franz-Xaver Kaufmann, Hans-Günter Krüsselberg, Giandomenico Majone, Paul Sabatier, Reinhard Selten, Martin Shubik, and others—in a setting where one could develop ideas about institutional analysis that were not focused on one discipline alone. We could not seriously attempt to develop an interdisciplinary approach to guidance, control, and performance evaluation in the public sector without building a common language that crossed the social sciences! I was fortunate that Reinhard Selten invited me to join his game theory seminar on the Bielefeld campus. During long walks in the woods behind the campus, Reinhard and I discussed an evolving framework for institutional analysis and the centrality of game theory to its development.

Larry Kiser and I had already written a paper called “The Three Worlds of Action: A Metatheoretical Synthesis of Institutional Approaches” (Kiser & Ostrom 1982). In Bielefeld, I further developed the method of institutional analysis that provided a foundation for most of my work for the next several decades (E. Ostrom 1986b). It was a major challenge just to provide a coherent definition of “institution” for use in political science as well as economics, and to develop a common framework to study legislatures, public bureaus, markets, and the many other structures involved in a complex political economy.<sup>4</sup>

Game theory was a powerful tool that enabled one to develop mathematical models of specific situations and predict the expected behavior of rational individuals in such well-specified situations. To specify the structure of a game and predict outcomes, the theorist must posit the (*a*) number of actors, (*b*) positions they hold, e.g., first mover or row player, (*c*) set of

actions that participants can take at specific nodes in a decision tree, (*d*) amount of information available at a decision node, (*e*) outcomes that actors jointly affect, (*f*) set of functions that map actors and actions at decision nodes into intermediate or final outcomes, and (*g*) benefits and costs assigned to actions and outcomes.

I proposed that the working parts of a game could be conceptualized as the universal working parts of what Larry Kiser and I had called an “action situation.” Earlier scholars had used terms such as transactions (Commons 1924), frames (Goffman 1974), logic of the situation (Popper 1961), collective structures (Allport 1962), and scripts (Schank & Abelson 1977; see Levi 1990 for a more contemporary view). When my colleagues and I had studied police, we observed multiple types of action situations—including officer-citizen interactions on the street or in a crime victim’s home, officer-officer interactions in a patrol car, dispatcher-officer communications, city council meetings, and many others. I thought one could use a common set of structural elements to analyze many situations of interest to political economists and then explain why particular behavior and outcomes occur in some structures and not in others.

Although understanding the working parts of a game provided an excellent foundation for building a common method to analyze a variety of action situations, little foundation existed for understanding why action situations that had the same name in the literature—such as a bureaucracy, an election, or a legislature—had a variety of structures. Economists had worked out a clearer set of factors that affected market action situations, but even here, why one market had only a monopolist and another market had many producers was not always clear. Most analysis started with a specification of current structure and might examine development over time within a market but not the rules and other factors that affected market structure initially. Political scientists understood that electoral laws broadly affected the likelihood of two-party or multiparty organization and the

<sup>4</sup>In one of the most important books delving into the relationship between politics and economics during the last century (Dahl & Lindblom 1953), “institution” does not appear in the index or table of contents. When the authors discuss the concept of a “rule,” they discuss various forms of regulations and bureaucratic red tape.

strategies of politicians. They frequently argued, however, about the “best” way to model all electoral behavior (or behavior within legislatures or bureaucracies), as if there were only one structure!

Thus, I thought the next important step was developing a common language for examining the underlying structure of diverse action situations. From our earlier work on the nature of goods, it was obvious that part of the underlying structure came from the biophysical world. From fieldwork in regard to groundwater as well as policing, I knew part of the underlying structure came from the community background of those participating. In addition to the biophysical and community foundation for an action situation, I wanted to better understand the relationship between the formal and working rules that were involved and the resulting action situations where behavior led to outcomes. This led to thinking about seven types of rules that potentially affect the seven parts of an action situation. I later outlined these (E. Ostrom 1986a) as follows:

1. Position rules that specify a set of positions and how many actors hold each one;
2. Boundary rules that specify how actors are to be chosen to enter or leave these positions;
3. Authority rules that specify the actions assigned to a position at a node;
4. Aggregation rules (such as majority or unanimity rules) that specify how the decisions of actors at a node are to be mapped to intermediate or final outcomes;
5. Information rules that specify channels of communication among actors and what information must, may, or must not be shared;
6. Scope rules that specify the outcomes that can be affected; and
7. Payoff rules that specify how benefits and costs are to be distributed to actors in positions.

To illustrate the usefulness of analyzing the rules that underlie action situations, I

chose to examine several important and extensively studied models of the bargaining between elected and public bureaucratic officials over the budget-output combination to serve their citizens. Downs (1957), Niskanen (1971), Romer & Rosenthal (1978), and McGuire et al. (1979) all made different predictions about the equilibrium outcome in a bargaining game. At the time, controversy existed as to whose model was correct. My analysis showed that they were *all* correct, given the differences in the underlying rules of each model.

The four models differed in regard to whether they assumed that boundary rules allowed more than one bureau to bargain with elected officials, whether the authority rules gave the bureau chief or the elected officials control over the agenda, and whether the aggregation rules specified the budget level would revert to the status quo or go to zero if no agreement was reached. Downs predicted that the equilibrium outcome of the bargaining game would be optimal for the median voter; Niskanen predicted that the equilibrium would be the largest budget-output combination capable of winning a majority in an all-or-nothing vote; Romer & Rosenthal predicted an equilibrium providing the median voter with at least as much as the status quo; and McGuire et al. predicted that allowing multiple bureaus to enter the bargaining would lead to an equilibrium that would produce the highest net value for the electorate. Our own empirical studies of cost functions for policing in metropolitan areas gave empirical support to the models predicting better outcomes for the voters when multiple bureaus were involved (Parks & Ostrom 1981). I was happy that I could demonstrate that digging under competing models to examine the specific rules assumed by scholars explained why the predictions made for the “same” bargaining situation differed so widely. Some of my public choice colleagues were, however, very upset as they interpreted my derivation of seven types of rules as adding far too much complexity to the study of institutional arrangements.

Defining seven broad types of rules that affected the working parts of a game or action

situation was not the last effort I devoted to understanding rules. Having been strongly influenced by the work of John R. Commons, I had thought it was obvious that the distinguishing characteristic of rules was whether they defined an action or an event to be required, forbidden, or permitted. As one studied the broad institutional literature, however, “rule,” “norm,” and “strategy” were defined in confusing and overlapping ways. Sue Crawford and I spent several years reading widely regarding how scholars had defined these concepts, and we found ourselves in a “Tower of Academic Babel.”<sup>5</sup> We then took the initiative to develop a cumulative syntax that would enable future work on institutional analysis to have a common foundation (Crawford & Ostrom 1995). These efforts at a more general theoretical understanding were very useful when I returned to more intensive research on salient environmental questions.

## Returning to the Commons

Empirical studies were providing evidence of the capacity of local users to solve problems of the commons, contrary to the prediction of Garrett Hardin (Berkes 1985, 1986; McCay & Acheson 1987; Netting 1972). Still, it was generally believed to be impossible for resource users to overcome such problems. Concern over this disjunction led to the creation of a National Research Council (NRC) committee to study diverse common-pool resources (National Research Council 1986).

Back when I wrote my dissertation on how the water producers of the West Basin solved their common-pool resource problem, I had not realized I was studying a common-pool resource problem, nor that it was widely considered insoluble. By the time I was asked to join the National Research Council Committee

on Common Property Institutions, I was ready to turn to the challenge of trying to understand why some users overcame the tragedy of the commons that they faced, while others let themselves be dragged down and destroyed valuable resources. A major activity of this NRC committee was to bring scholars from diverse disciplines together in working groups to discuss their own empirical studies. Given the diversity of variables stressed by different disciplines, the committee asked Ronald Oakerson to present the Institutional Analysis and Development (IAD) framework and lead the planning sessions to enable scholars to discuss their cases using the same framework (Oakerson 1986).

It soon became known that scholars from multiple disciplines had studied at least 1000 cases related to diverse resources in many regions of the world. To gain some knowledge from these unorganized assets, William Blomquist, James Wunsch, Edella Schlager, S.Y. Tang, Sharon Huckfeldt, and I slowly developed the Common-Pool Resource (CPR) database using the IAD framework. Trying to develop a sizable and reliable dataset on how users had related to fisheries, forests, irrigation systems, and other resources turned out to be a substantial challenge. Authors from different disciplines recorded information about variables that were thought to be important in their discipline, but ignored other variables that were not perceived as relevant. Several years were devoted to screening cases to assess the quality and extensiveness of data collected, to record those cases with substantial information, to check with case authors when feasible to improve data quality, and to undertake careful analysis. Our collaborative efforts yielded insights related to irrigation (Tang 1991, 1992), fisheries (Schlager 1990), and cross-sector analysis (Schlager et al. 1994) to formally model relationships and conduct experimental studies to test specific hypotheses (E. Ostrom et al. 1992, 1994; E. Ostrom & Gardner 1993; Weissing & Ostrom 1991).

Comparative analysis using the CPR database also supported systematic conceptual developments related to the concept of

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<sup>5</sup>See tables 1 and 2 in Crawford & Ostrom (1995) for the rich array of terms that scholars have used and how their definitions overlap. Levi (1988) discusses how difficult it is for rulers to enforce rules on subjects who do not share normative commitments that the rules specify actions that they must do, must not do, or may do.

property rights. Property rights had been defined by many scholars as existing only when users had the right to sell (alienate) their harvesting rights to others (Demsetz 1967). In this view, users who could not sell their rights to others, in effect, had no property rights. Instead, Schlager & Ostrom (1992) found that user rights of access, withdrawal, management, exclusion, and alienation were all important rights and were generally cumulative. It is not possible, for example, to exercise rights of withdrawal without rights of access. This led to a new conceptual terminology for analyzing bundles of rights within a hierarchy of possible rights. An authorized user only has rights of access and withdrawal. A claimant has these rights plus management rights. Proprietorship adds rights of exclusion. Ownership encompasses the full set of rights. Examples from the CPR database illustrated each bundle of rights and demonstrated that users did not need alienation rights in order to manage a resource sustainably. This conception of property rights is now accepted by scholars who have studied diverse property-rights systems around the world (Brunckhorst 2000, Degnbol & McCay 2007, Paavola & Adger 2005, Trawick 2001).

### **The Struggle to Write *Governing the Commons***

We were vigorously coding and analyzing the data in the CPR database, when I had a unique opportunity placed on my agenda. Doug North heard a presentation I made on institutional analysis and the development of institutions for common-property regimes. He was very enthusiastic and urged me to write a book for the series that he and Jim Alt were editing for Cambridge University Press. Then, Jim Alt and Ken Shepsle asked me to give a series of five lectures at Harvard that would become a draft manuscript of this book. This invitation was extended in the fall of 1986, before I spent another spring sabbatical at the Center for Interdisciplinary Research (ZiF) in Bielefeld, Germany. Reinhard Selten had invited a fascinating group of social scientists and biologists to examine

how game theory could be applied to understand patterns of interactions and outcomes in both biological and social systems.<sup>6</sup>

Having a sabbatical and these kinds of invitations all at the same time seemed a sign that I should write a book about what general lessons we could learn from individual case studies and our statistical analysis of particular questions. I dreamed of analyzing the rules that we had coded in our meta-analysis to find those that were generally associated with long-term success. I spent weeks and weeks rereading cases, writing them up, redoing statistical analysis, and thinking that I was a dope for not being able to identify regularities in the specific property rights of the successful cases. Finally, it dawned on me that I should drop the goal of identifying the *specific* rules that tended to generate success. Perhaps what I needed to do was move up a level in generality and try to understand some more general institutional regularities among the systems that were sustained over a long period. I did not even know what I should call those regularities, but the idea finally flashed that one way of talking about them would be as “design principles.”

I did not think that the irrigators, fishers, forest dwellers, and others who had invented and sustained successful common-property regimes over several centuries had these principles overtly in their minds. Not all artists have training in art and know the design principles that they actually use in painting an outstanding work of art. I thought of these institutional regularities as underlying design principles that one could draw out from the cases of long-sustained regimes. The next task was to compare them with failures and to assess whether the failures were characterized by any of the same principles. If they were, of course, the principle would not provide a meaningful distinction between durable and unsuccessful

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<sup>6</sup>The first seminar after my arrival was entitled “Why Sex?” Obviously, I attended that one. I learned to my amazement that having two sexes, rather than just one, is an inefficient way of reproducing. The question of the seminar was serious and being studied by many outstanding biologists.

systems. But the comparison showed that the cases of failure were not characterized by these principles.

After several months of struggle and giving presentations in Bielefeld, I gave the first set of lectures at Harvard in April 1988. Colleagues there had vigorous responses. I remember some wonderful exchanges with Ken Shepsle, Jim Alt, and Bob Putnam. Fortunately, Doug and Jim gave me plenty of time to revise the initial draft before submission. I had drawn on a diversity of case studies beyond my own groundwater institutional development. I sent all of the chapters out to the authors of the original case studies so they could tear them apart. I did not want to base an argument on a misreading of someone else's case study.

As I sent the book in to Cambridge University Press, I was quite uncertain as to whether the design principles would be looked upon as a crazy set of ideas or as a discovery of underlying regularities.

It now appears that the struggle was worth it. Many scholars have read *Governing the Commons* (E. Ostrom 1990) and found that the robust, self-organized systems they studied in the field were characterized by many of the design principles described in the book, and that failures were not. Thus, most of the evidence is supportive of the design principles I laid out in 1990. Several visiting scholars and graduate students at IU's Workshop in Political Theory and Policy Analysis (see "Establishing an Environment to Enhance Coproduction of Knowledge," below) have developed a database to record information about the studies they located in a systematic search of the literature. They identified 111 empirical studies of irrigation, inshore fisheries, pasture systems, and forests with information about the design of the local institution. In a recent paper, Cox et al. (2009) find that only 10% of the studies are not supportive of the usefulness of the design principles to help explain long-term sustainability. Many authors did offer good suggestions about small ways of improving them. Cox and coauthors have made several useful reformulations and cautioned readers not to use the concept

of a "design principle" as being equivalent to a "design blueprint."

## Further Research on the Commons

In addition to the early research we did on self-organization and the robustness of common-property regimes to manage common-pool resources, colleagues associated with the Workshop have been active in further studies of irrigation resources and now of forest resources. We used the coding forms that we had developed to do meta-analysis for the CPR database as a first draft of a relational database to study forestry institutions and the forests themselves around the world. We did not want to focus only on resources that were managed by communities. We also studied systems that were government owned and managed, privately owned and managed, co-managed, community managed, or not managed at all (open access).

We created a network of Collaborating Research Centers located in a dozen countries. Each center has chosen its own set of forested areas for intensive study. We are building on a lesson learned long ago in our first policing study: Coproduction of knowledge is necessary. An international project in which American scholars dash out to study a variety of locations overseas, and then return home, is not likely to be successful. One needs a collaborative network where everyone uses the same underlying logic and data-entry forms for collecting and entering data. Everyone in the International Forestry Resources and Institutions (IFRI) network lives in the country under study and has a long-term interest in improving the sustainability of forests and of the people who rely on them.

Some of the results from earlier research have been reenforced. We found that it is possible for local users of a forest to self-organize and develop their own set of rules for managing a forest. Some groups try and fail while others succeed. Others start a system that collapses over time. The crucial and strong finding is that it is not impossible for resource users to

self-organize, as Hardin earlier asserted and many scholars still believe.

Further, we found some government forests that operate well and are able to sustain forest conditions in good shape over time. We found some private forests that work well and also some comanaged forests. In other words, it is not the general type of forest governance that makes a difference but rather how a particular governance arrangement fits the local ecology, how the specific rules are developed and adapted over time, and whether users consider the system to be legitimate and equitable. Further, multiple studies have found that one of the most important factors affecting the likelihood of sustainability is whether the users themselves engage in day-to-day monitoring of activities in the forest (Coleman 2009, Coleman & Steed 2009, Gibson et al. 2005, Hayes 2006). The importance of user monitoring amazes some scholars. It is consistent, however, with our understanding from field and experimental studies that building trust that others are generally following agreed-upon rules is essential for sustaining rule conformance over time and sustaining the resource itself.

### **Establishing an Environment to Enhance Coproduction of Knowledge**

In addition to studying coproduction in the field over the years, we find it extremely important to engage faculty and students in their own coproduction activities. The Collaborating Research Centers discussed above are one example. An earlier important step to enhance serious discussion of institutional theories was undertaken in the early 1970s with the establishment of a Colloquium series to meet on Mondays at noon during the academic year. Faculty and graduate students in anthropology, business, economics, geography, law, political science, and occasionally other disciplines came together for serious discourse about the structure of diverse political economies, the incentives they generated, and the patterns of outcomes. The Colloquium had met 902 times by June 2009. The discussions enabled us to develop our theoretical insights,

tighten our research designs, and apply successfully for grants.

The Workshop in Political Theory and Policy Analysis, named by Vincent Ostrom, was established at IU in the 1973–1974 academic year. We had had the good fortune of working with several craftsmen in building our home and most of the furniture in it, as well as building a log cabin on the Manitoulin Island on Lake Huron in Canada. We learned to respect the creative but rigorous artisanship that occurs in a good workshop. Our vision for this research center was that graduate students should learn good artisanship as they worked together with faculty from diverse disciplines using multiple methods to design and carry out well-crafted research (see Aligica & Boettke 2009, V. Ostrom 1980). The Workshop celebrated its thirty-fifth anniversary this past year with a self-organized conference involving 144 scholars from 27 countries: “Workshoppers Around the World: What Lessons Have We Learned?”

### **The Next Book**

As I write this essay during the summer of 2009, I have been finishing a book called *Working Together: Collective Action, the Commons, and Multiple Methods in Practice* with my coauthors, Amy Poteete and Marco Janssen, for publication by Princeton University Press (Poteete et al. 2010). We discuss how individual case studies, meta-analyses of multiple cases, large-scale comparative field-based research, formal theory, experimental research, and new methods for combining theory and agent-based models have improved understanding of collective-action theory as applied to the study of common-pool resources. It has been a massive task to bring these diverse strands of research and methods together, but if the external reviews of the manuscript are any indication of future reaction to the book, the effort will help move our understanding ahead. Further, we hope very much to tear down some of the artificial walls that separate the users of diverse methods even within a single discipline, let alone across disciplinary boundaries.

## The Next Project

In 2007, Marty Anderies, Marco Janssen, and I organized a special feature of the *PNAS*, “Going beyond Panaceas” (E. Ostrom et al. 2007). I developed a framework for analyzing relationships within social-ecological systems that could provide the foundation for a diagnostic theory. During the summer of 2009, “A General Framework for Analyzing Sustainability of Social-Ecological Systems” was published in *Science* (E. Ostrom 2009). This enabled me to reach a broad audience of scholars across disciplines to update a nested, multilevel framework for analyzing theoretical questions related to how multiple users and uses of resource systems embedded within governance systems affect the sustainability of social-ecological systems. Scholars in multiple universities in Europe and the United States and colleagues at IU are working with me to develop this framework further in a project we call a Diagnostic Ontology for Analyzing Social Ecological Sustainability (DOSES). DOSES will become the common framework used by this international community of scholars for analyzing water systems, pastoral systems, fisheries, and urban infrastructures, as well as the continued study of forest systems through the IFRI network. DOSES imbeds the IAD framework as a crucial central part, but overtly includes a diversity of key ecological measures to complement the social and institutional measures derived from IAD.

## INTELLECTUAL CHALLENGES FOR INSTITUTIONAL THEORY IN THE FUTURE

Although institutional scholars have made great advances since the 1960s, there is obviously much more to be done. One of the major challenges is to continue unpacking the factors that affect action situations. The DOSES project will tackle this question for social-ecological systems, and I hope other scholars will address this challenge in other policy arenas. Young scholars should now have substantial opportunities to develop rigorous databases early in

their careers that will enable them to study complex social-ecological systems over time.

A second major challenge is further development of the theory of individual choice that can be used in a variety of institutional settings. When fallible, learning individuals interact in frequently repeated and simple situations, it is possible to model them as if they had complete information about the variables relevant to their decisions. In highly competitive environments, we can make the further assumption that the individuals who survive the selective pressure of the environment act as if they are maximizers of a key variable associated with survival in that environment (e.g., profits or fitness) (Alchian 1950). When individuals face a relatively simple decision situation where institutions generate accurate information about the variables relevant to a particular problem, that problem can be adequately represented as a straightforward, constrained maximization problem. The most fully developed, explicit theories of individual choice compatible with the IAD and DOSES frameworks—game theory and neoclassical economic theory—involve extreme assumptions, such as unlimited computational capability and full maximization of individual net benefits. When analyzing private good markets in a setting where property rights are well defined and enforced at a relatively low cost to buyers and sellers, theories of market behavior and outcome based on complete information and maximization of profits predict outcomes well.

Many of the situations of interest in understanding local public goods and common-pool resources, however, are more complex, involve greater uncertainty, and lack the selective pressure and information-generating capabilities of a competitive market (E. Ostrom 2005). Therefore, one needs to substitute the assumption of bounded rationality—that persons are “intendedly” rational but only limitedly so—for the assumptions of perfect information and utility maximization used in axiomatic choice theory (see E. Ostrom et al. 1994, ch. 9; Simon 1965, 1972; Williamson 1985). Information search is costly, and the information-processing capabilities of human

beings are limited. Individuals, therefore, often must make choices based on incomplete knowledge of all possible alternatives and their likely outcomes. All individuals may make mistakes in choosing strategies designed to realize a set of goals (V. Ostrom 1986). Over time, however, they can acquire a greater understanding of their situation and adopt strategies that result in higher returns. Reciprocity may develop rather than strictly narrow, short-term pursuit of self-interest (Hyden 1990, Oakerson 1993).

Thus, one is not faced with a rigid choice between modeling individual behavior as done in classic economic models or adopting a model of individuals who have preferences for outcomes for others and always use reciprocity. We need to develop an underlying theory of human behavior in diverse settings so that we can begin to predict how individuals who interact in one type of setting will or will not gain trust and reciprocity compared to another type of setting (E. Ostrom 1998, 1999; E. Ostrom & Walker 2003; Walker & E. Ostrom 2009). It is frustrating for many institutional scholars that we are no longer able to use *one* model of individual choice in every type of action situation of interest to scholars. We are slowly gaining a better understanding, however, of how the structure of a situation and various attributes of the users (e.g., whether they can communicate, exit or enter voluntarily, know one another, gain information about past behavior, etc.) interact to generate situations where reciprocity and trust grow or deteriorate (Poteete et al. 2010, ch. 9). More work is needed, however, and I encourage young scholars to engage this important challenge.

There are many challenging questions for young scholars to address today. Some

theories and techniques for addressing them are currently available and will be improved over time. A rich set of opportunities exists for further work built on a much firmer foundation than existed half a century ago. I am optimistic about the future for three reasons. First, there are now many more opportunities for women and minority scholars owing to major institutional changes in admissions, hiring, work assignments to junior faculty members, and tenure decisions in American universities. Second, interdisciplinary scholarship is now more feasible than it was. The National Science Foundation has established several programs to support research that focuses on the structure and outcomes of social-ecological systems over time undertaken by social and biophysical scientists. Journals including *Conservation and Society*, *Ecology and Society*, *International Journal of the Commons*, *Journal of Economic Behavior and Organization*, and *Policy Studies* are both strongly interdisciplinary and well respected. Some political science departments still insist that articles considered for tenure decisions must be in top political science journals, but that policy is also changing over time to recognize the importance and relevance of interdisciplinary as well as disciplinary research. The third reason for my optimism is the broader recognition that complex institutional arrangements are not automatically chaotic. Ecologists and biologists long ago learned that they were studying complex phenomena composed of many parts at multiple levels and that their challenge was to unpack the complexity in order to understand it. Our challenge as social scientists is to harness knowledge about complex systems (Axelrod & Cohen 2000) and not simply to call for their simplification.

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