

Symposium on Anthropologists' and Economists' Views on Common Resources

Methodological Approaches to the Question of the Commons

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I. Introduction

Interdisciplinary work in the social sciences is generally held to be desirable, but in practice it has proven rather difficult. This is because, while the themes studied by social scientists are often similar, the intellectual histories, the questions that are considered salient, the field research methods, the ways in which theories are applied to empirical observations, and even the approaches to systems of knowledge vary widely from discipline to discipline. As a result, economists, political scientists, sociologists, anthropologists, and geographers often fail to make their analyses and even their assumptions comprehensible to the others. Many do not even try. In this issue we put together four articles (including this one) to illustrate some of the methodological differences within the social sciences. We highlight in particular the divisions between economics and anthropology, and, anchoring ourselves to issues of the local environmental commons, we explore the possibilities of bridging some of these divisions. Economics and anthropology are seen as extremes along the social science continuum, and interdisciplinary work bridging their differences has been especially challenging.

Our goal in this introductory article is not to resolve the methodological, epistemological, and normative divides among the disciplines or between economics and anthropology. It is not clear that resolution is either possible or desirable. But understanding what is important to the other discipline, and seeing the differences in light of that understanding, is surely important for interdisciplinary work and for respectful conversation. This introduction is our attempt to reduce the oft-heard complaints from anthropologists that econ-

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omists are “often in error but seldom in doubt” and from economists that anthropologists spend forever in the field but never bring back any properly tested hypotheses.¹

II. Recent Cross-Disciplinary Conversations

In 1985 the Social Science Research Council, with the support of the Ford Foundation, facilitated an innovative workshop in Bangalore, India, that brought together economists and anthropologists to discuss and compare their analytical methods. The workshop, *Conversations between Economists and Anthropologists*, focused on diverse approaches to the measurement of economic change in rural India, such as data collection through large n surveys versus intensive village-level studies and the inability of macro surveys (favored in economics) to capture “dynamics, processes and relations” (the domain of anthropology). Some of the papers in that workshop were published in Bardhan. That first econ-anthro dialogue exposed both “unsuspected areas of potential agreement” and “legitimate rock-bottom differences” (1989, 11) and, 20 years later, remains an insightful guide to interdisciplinary field research methods.

The last 5 years have seen a revival of workshops, seminars, papers, and books focused on crossing the boundaries between economics, anthropology, and sociology. In March 2001, the “Qualitative versus Quantitative” (or “Q2”) theme was discussed in a workshop convened at Cornell University by Ravi Kanbur.² Particular attention was paid to how (and if) borrowing from “quant” methods could make “qual” methods more generalizable and comparable and to how qual could explicate relationships between variables and so introduce context into quant research.³ The journal *World Development* published a series of papers on development economics and the “other” social sciences in which John Harriss (2002), Cecile Jackson (2002), and Howard White (2002) critiqued the too-powerful role of economics in development circles and made the case that sociology, anthropology, and politics should be equal players in development policy. The dominant impressions from many of the Q2 and the *World Development* papers are that (1) cross-disciplinary work on social problems is critical and (2) the onus is mostly on the economists to change. The two most recent additions to cross-boundary conversations between economists and anthropologists are *Culture and Public Action* (Rao and Walton 2004), in which scholars across the social sciences discuss the role of culture in furthering, and

¹ The quote is attributed to the physicist Lev Landau; he was apparently denigrating cosmologists.

² The papers from the conference can be read at <http://www.q-squared.ca>.

³ This is also the theme of *Rethinking Social Inquiry: Diverse Tools, Shared Standards* (2004), edited by Henry Brady and David Collier and geared toward political scientists.

even defining, the goals of development, and *Foundations of Human Sociality* (Henrich et al. 2004), in which economists and anthropologists present new findings about human social behavior from a series of experimental games in “traditional” cultures around the world.

It would be premature to suggest that key conclusions have emerged from these conversations on how economists and anthropologists can most fruitfully collaborate with each other. On the one hand, economists have modified their behavioral premises about, say, common-pool resources, based on the results of anthropological case studies (as in Sethi and Somanathan, in this issue). Some anthropologists have gone to their field sites ready to test economists’ hypotheses on who cooperates and why and with what degree of fairness or selfishness (as in Henrich et al. 2004). On the other hand, many economists and anthropologists are still divided on their views of human agency, on what constitutes data, on how to interpret their respondents’ words, and on what constitutes an adequate explanation. As Appadurai wrote in the first *Conversations*, “At bottom, in my opinion, are not issues about sampling size, respondent error . . . though these are important issues. . . . The deeper issue is *epistemological*” (Appadurai 1989, 276).

The selection of essays in this issue represents another such contribution to this ongoing conversation.⁴ This particular endeavor is focused on methodological and epistemological approaches to the analysis of local common pool resources.

III. Why the Local Commons?

There are four reasons for choosing the local commons as an anchor for this conversation. First, the pressures of population growth, migration, uneven market integration, social inequality, and competing claims on the same resource have gradually degraded much of the natural resource base upon which millions of poor people depend (such as forests, fisheries, grazing lands, and irrigation water). Therefore the sustainable and equitable management of such resources remains a central problem for development and environment studies.

Second, both economists and anthropologists have long records of research on what has been called *the question of the commons* (McCay and Acheson 1987). For many years after the publication of Garret Hardin’s *The Tragedy of the Commons* (1968) and Mancur Olson’s *The Logic of Collective Action* (1965), the working assumption among economists and political scientists was that self-

⁴ Two of the four papers (those by Ray and Sethi and by Somanathan) were presented in preliminary form at a workshop—“Conversations between Economists and Anthropologists II”—held in Goa, India, in August 2003.

interested individuals, without external coercion, would not act collectively to provide common goods or to protect common resources. Yet throughout this period and subsequent decades, socio-anthropological evidence came in, suggesting that poor countries were strewn with examples of what certainly looked like collective protection and collective use of local common resources.⁵ It was shown, theoretically and empirically, that norms of cooperation and trust could emerge and be sustained in local communities with a history of repeated and interlocking interactions (Wade 1988; Ostrom 1990; Seabright 1993). A rich literature on social movements developed, arguing that, in their struggles to protect their common resources, groups developed a sense of identity and collectivity (Guha 1989; Escobar and Alvarez 1992). In response to these observations, and also to experiments in social psychology and economics, new theories of cooperation and noncooperation emerged. Mainstream economic theory on the topic today has changed considerably in response to anthropological work on the commons, though there has perhaps been less trade in the other direction.

Third, there has been a resurgence of interest in local collective action and participatory development—and the common-pool resource literature is the source of much of the theorizing on, and the optimism in, such collective action and participation. The enthusiasm over “participation” unites segments of the political right, who believe that market failures can be overcome by rational individuals acting together under enabling incentive structures, and of the political left, who have grown disenchanted with the centralized state as the primary provider of development needs. New work in anthropology has critically reexamined the case for local control of the commons, or of participatory development for that matter, and has revealed the political limits and dangers of “localism” (Gadgil and Guha 1993; Li 1996; Mohan and Stokke 2000).

Finally, we hear widely expressed concerns about local common resources being eroded everywhere, or about urbanization, privatization, and globalization inevitably undermining the commons, or about community management as at best a romanticization of “communities” and at worst a cover for within-group exploitation. But these narratives of decline and degradation of traditional common-pool resources conceal the emergence of new common resources and new avenues for local collective action. In the United States, for example, the increasing numbers of neighborhood watch and community gardening groups can be seen as a bid to reclaim the local commons. In developing

⁵ See, e.g., Ostrom (1990), Baland and Platteau (1996), and the symposium on the local commons in the fall 1993 issue of the *Journal of Economic Perspectives*, edited by Bardhan.

countries, community-based drinking water sources, such as harvested rain-water (Agarwal and Narain 1997), and common sanitation facilities in crowded peri-urban areas (Hogrewe, Joyce, and Perez 1993) are proliferating. Agro-forestry experiments and research aimed at sustainable village-based livelihoods are seriously examining the interaction between ecology and community. Who claims these resources, how are they appropriated, how are they maintained, and how are they seen by different groups or users within the community? These questions are very much alive in economics as well as in anthropology, and so the commons are very much with us.

The articles in this issue provide only a small sampling of methodological approaches to the local commons in economics and anthropology. The contribution of Sethi and Somanathan illustrates the use of game theoretic analysis in understanding the conditions under which collective action on the commons can emerge and persist. The article by Mosse is a canonical example of historical-anthropological scholarship, which the author contrasts sharply with the economic-institutional approaches exemplified by the work of Ostrom (1990) and Wade (1988).⁶ Mosse cautions against a rush to cross boundaries in our eagerness to embrace interdisciplinarity, lest we end up with a fuzzy concept such as social capital. The article by Ray is explicitly bridging in that it not only seeks to explain why trade is so difficult in this era of disciplinary specialization but also explores avenues for epistemological exchange among the disciplines.

IV. The Key Dichotomies

One way to contextualize these articles is through thinking about key dichotomies that distinguish mainstream economics from mainstream cultural and social anthropology.⁷ Explicitly methodological differences between economics and anthropology include quantitative versus qualitative (referring to the nature of data and their analysis), aggregative versus particular (referring to how the data are used to illuminate social situations), and positivist versus reflexive (referring to the role of the researcher toward the data or the subjects of study). However, as both the “Conversations” and the “Q2” workshops concluded, the social sciences are most often split along epistemological lines such as: How do economists and anthropologists view human agency and individual choice? What do economists and anthropologists seek to explain? What, indeed, are the characteristics of a good—or even adequate—expla-

⁶ Ostrom and Wade are political scientists rather than economists.

⁷ While generalizing about mainstream economics and anthropology, we do recognize that both disciplines are moving targets for which all broad-brush statements are ultimately inappropriate.

nation? (Some anthropologists would argue that they do not try to explain but rather to translate or interpret cultures.) These are foundational questions for any social science, yet they are rarely explicitly asked across disciplines, or their implications explored. We address these questions via the dichotomies of autonomy versus embeddedness, outcomes versus processes, and parsimony versus complexity.

A. *Autonomy versus Embeddedness*

The debate over whether individuals are best understood as autonomous agents within the constraints of social structures, or as products of the structures that bound their agency, is an old—some might say sterile—debate. “Men make their own history,” wrote Marx, “but they do not make it just as they please; they do not make it under circumstances chosen by themselves” (1852 /1981, 1).⁸ Who could disagree? What largely separates economists from anthropologists, then, is the question of what is a meaningful construct of agency given what we want to explain. Three particularly contentious constructs that economists have traditionally used are methodological individualism, optimizing behavior, and exogenous preferences.

For (non-Marxist) economists, the individual is the unit of analysis, and his or her purposive (rational) choices under a set of constraints are what must be explained. Agency and autonomy reside in the individual. Societal characteristics reflect the aggregated result of individual characteristics—a point of view known as methodological individualism. Methodological individualism as an analytical concept comes in several versions (Bhargava 1993; Basu 2000, 253–54), the most constraining of which have been critiqued from within economics itself (Arrow 1994). Contrary to some perceptions, methodological individualism does not imply that all social characteristics are reducible to individual characteristics—many norms and practices can emerge as the unintended consequences of thousands of uncoordinated decisions (Schelling 1978; Sugden 1989). But economics is fundamentally a social science that explains social phenomena, such as cooperation or trade, in terms of individual choices and motives.

In most economic analyses, individuals are self-regarding—they try to do the best they can for themselves given the constraints of their economic endowments, their information sets, and their tastes and preferences. In recent years economists have recognized that a person could exhibit reciprocal rather

⁸ While Marx was referring to what economists somewhat crudely will call a constrained optimization equilibrium, the historian François Furet (1981) was suggesting multiple equilibria and unintended consequences when he said that men make history without knowing which one.

than self-regarding behavior and be selfish with those who were selfish with him but generous to those who were generous to him (Rabin 1993; Charness and Rabin 2002). Nevertheless, the default assumption in much of microeconomics, and therefore in noncooperative and bargaining game theory, is that people are exclusively self-regarding. As Sethi and Somanathan explain in their article (in this issue), economists worry that frequent deviations from this assumption could open the doors to an “anything goes” mentality and ad hoc explanations.

Finally, tastes and preferences in economic analysis are exogenously given and stable. Why some members of a village community have cooperative as opposed to noncooperative propensities is not a question within the domain of mainstream economics. Methodological individualism, utility maximization, and exogenous preferences together create what might be called a thin theory of human action (Taylor 1988), but it is this thinness that gives microeconomic models their precision, parsimony, and predictive power. It enables the researcher to ask under what rules and incentives a group of self-regarding individuals would cooperate to govern the commons (Bardhan 1995) and whether or not specific asymmetries would prevent cooperation from emerging (Ostrom and Gardner 1993; Ray and Williams 2002). Much economic analysis on the commons consists of being precise about the conditions under which cooperation would emerge and be sustained, with the understanding that the propensity to defect or free ride is always present.

With few exceptions, social and cultural anthropologists find these three characteristics unsatisfactory as a comprehensive account of human agency. In particular, the notion of exogenous preferences, formed and held at the individual level, has been widely critiqued. Bourdieu famously argued that preferences reflect the inner workings of culture and power in a society, that preferences are formed just as much by the desire for social differentiation as by the inherent properties of the preferred object (Bourdieu 1979). In a similar vein, Appadurai has critiqued survey research methods that treat the household as an autonomous choice-making unit—he argues that reciprocal relationships between households are central to the choices made by their individual members (Appadurai 1989, 254). More recently, Klammer has made the case that many social values and preferences are formed through dialogue, negotiation, and learning; far from being stable, they are constantly being reassessed (Klammer 2004). For most meaningful interactions, then, the individual as the locus of given preferences is not a recognizable object of anthropological inquiry. The critique of exogenous preferences is one aspect of the broader discomfort with the economist’s individual agent. Individuals do have agency, say the anthropologists, but they are situated, embedded beings rather than auton-

omous beings who view life as a series of constrained optimization problems: autonomy in a social vacuum is not meaningful. But if autonomy is embedded, what is it embedded in?

The operationalization of embeddedness has a rich tradition in anthropology. Polanyi (1954) argued that individuals are characterized by relationships of reciprocity rather than utility-maximizing motives and that this was particularly the case for precapitalist economies. Even ostensibly market interactions were embedded in, and inseparable from, larger social and political commitments. Geertz (1963) and Scott (1976) framed peasant societies in Southeast Asia as moral economies rather than utilitarian economies. In a moral economy, individuals act not to advance their own well-being but to make sure that resources and risks are pooled so that everyone has a part in the system. Interactions within local communities are not simply the aggregate effect of individual interests but rather the living out of shared understandings of fairness or justice. Moral economy analyses have subsequently been critiqued from within the discipline as being naïve about how power permeates the social fabric. These critics argue that what appears to be a moral economy could be, at least in part, a manifestation of long-standing inequalities or hegemonic control. Embeddedness in reciprocity is in fact embeddedness in unequal relations and multiple notions of identity and interest (Hart 1997). The very notion of the community as a unit of analysis—so central to theories of common resources and of collective management—is questionable, given multiple and overlapping intragroup heterogeneities (Li 2002).

Of embeddedness in values, commitments, power, and norms, the one intrinsically collective concept that has gained real traction in economics is that of norms. By definition, and unlike preferences or habits, norms cannot be held at the individual level. Basu (2000) makes a strong argument that economists should build norms explicitly into their models, lest they embed them unconsciously instead. He divides norms that are useful for economic analysis into three categories: rationality-limiting, preference-changing, and equilibrium-selecting (Basu 2000, 72–73). A rationality-limiting norm restricts a person from doing things, such as stealing her neighbor's newspaper, whether or not such an action would increase her utility. Preference-changing norms are those that become internalized into the utility function—the norms become preferences or cause too much guilt or shame if they are violated (see Elster 1989). Equilibrium-selecting norms help people to choose from among multiple equilibria, such as driving on the right side of the street in the United States but on the left side when in the United Kingdom. Most of the economic literature is on this third type of norm, which may or may not

benefit everyone or even anyone, but once such norms take hold no one individual has an incentive to deviate from them.⁹

How norms emerge and why they persist are two different questions. Mainstream economic analysis, true to its methodologically individualist roots, explains the emergence of norms as the aggregate (and frequently unintentional) effect of many intentional but individual decisions. For instance, Sugden (1989) shows that cooperative norms in the use of driftwood can emerge, “spontaneously” and without explicit coordination, among the users’ group. Once norms have emerged, however, they often persist because it is at least in some individuals’ interest to sustain them or in no one’s interest to diverge from them. Or norms of restraint in resource use could evolve and be stable if there are at least some members in the community who are willing to punish rule violators, even if sanctioning imposes material costs on the punishers (Sethi and Somanathan 1996). In short, norms, once the domain of anthropology, are now firmly on the economist’s agenda.

If individuals are not separable from the habits, moral commitments, or norms in which they are embedded, then the central puzzle of common property analysis is not why individuals cooperate to provide or protect a resource. Why they would not cooperate to protect a critical resource is just as much in need of explanation. In addition, common resources such as land or water are social systems with material as well as symbolic value (Bourdieu 1977), and the boundaries between the two may not be sharp. Anthropologists would thus want to go beyond economic explanations of how norms govern cooperation over common lands (e.g., McKean 1986) or how rules of water use are made or violated (e.g., Mosse 1997) toward a broader understanding of the commons as cultural systems.

The remarkable influence of Michel Foucault in contemporary anthropology has also led anthropologists to view cooperation-sustaining norms with a critical eye. Foucault (1991) argued that governance consists of certain arts and practices such as measurement, observation, and education, through which individuals become disciplined and governable subjects. The wide acceptance of these disciplinary forces circulates through society at large in the mutual enforcement of norms and of legitimate political and cultural discourses. Looking at economists’ models of repeated games on the local commons and the enforcement of cooperation through shared norms, anthropologists would cer-

⁹ It should be noted that all three norm families are considered constraints in economics—they are exogenous to the individual and they restrict her feasibility set. Norms as givens will work for social scientists with structuralist leanings but not for others. If norms are seen as dynamic, then cooperation over resources and resistance to changing property regimes would also produce, rather than merely be shaped by, commitments and norms.

tainly ask not only how these norms emerged but also how their emergence revealed the dynamics of power working through everyday practices, and how the norms enforced the status quo—in short, how norms ensured the “normality” of the ostensibly free individual. There is little room for the economist’s autonomous agent in this framework.

B. Outcomes versus Processes

“Economics is mainly about outcomes; anthropology is mainly about processes.” So begins Michael Lipton’s review (1992) of *Conversations* (Bardhan 1989) in the journal *World Development*. Lipton goes on to acknowledge that models reach their outcomes through such processes as making choices, bargaining, and so on. But these are modeled processes—economists rarely conduct empirical investigations of processes themselves. Anthropologists, in contrast, while interested in, for example, the outcomes of social relationships, are most concerned with “the structure and function of the relationships themselves” and with the processes of exchange or the exercise of power that they generate. Appadurai’s essay in *Conversations* also argues that facts about village life, such as the rights to local commons, are “relational and not merely distributional” (Appadurai 1989, 268). The implication was that empirical research in economics samples outcomes (such as the distribution of water from a common watercourse) and does not usually sample, and so may gloss over, processes (such as how relationships between lender and borrower or between rule-maker and rule-breaker in water distribution are structured and how they evolve over time).

Outcomes in economic analysis have two characteristics—they serve as predictions (including predicting backward to understand changes that took place in history), and (when possible) they describe equilibrium points in the economy. Many anthropologists acknowledge that prediction is valuable in thinking about social change and that the sharp predictions of economics make it more influential in policy circles than the “softer” social sciences. But they are concerned that economists’ assumptions and models are too simple to be socially useful or that prediction of a phenomenon under a given set of constraints is too readily conflated with justification of an existing institutional set-up (see Bardhan 1989, 238). Yet others argue that in situations of rapid social and economic change, only the obvious can be predicted. Whether prediction is or is not an explanation or whether understanding the process is as important as predicting the outcome are both questions that relate to the nature and purpose of explanation in the social sciences. What makes for

a good explanation is too big a question to address here.¹⁰ We concentrate here on causal explanations, which are important both in economics and in anthropology.¹¹

Causal explanations draw upon repeated empirical observations of the event and its supposed cause, as well as upon theories of the underlying mechanisms or structures that supposedly produce the explained event. In economic theorizing, the causal arrow from cause C to event E is clearly specified. It is explicitly built into the model specification, and the model (at least in theory) stands or falls or wobbles on the basis of the accuracy of its predictions. Attributing causation in a regression analysis is a more complex matter—real data naturally create real problems. The causal arrows are not specified in statistical models; they have to be inferred from the strength and significance of the correlation between the dependent variable and the relevant independent variables. Of course, correlation on its own, however strong, cannot pass for causation. Because of the complexity of real-world data (and because of most researchers' reliance on secondary data), the most common problems econometricians struggle with are sample selection, endogeneity or reverse causality, and omitted variable bias. Here economists' attempts at determining causes through hypothesis testing have in recent years become much more rigorous, particularly through creative use of instrumental variables and other identification strategies and through random evaluations of interventions.

Social and cultural anthropologists explain social phenomena primarily by way of the case study method.¹² These case studies are well equipped to—and often do—investigate causal processes directly. An anthropologist's case study could include a small number of cases, compare two cases, or even conduct within-case analysis of a single case of interest (Ragin 1987). On the one hand, the few-cases method restricts the researcher's ability to generalize beyond his or her study site. On the other hand, anthropologists generally have a better insight into the wellsprings of human behavior, since they regularly live with the respondents, observe their practices, participate in some fashion in their daily lives, and can ask people why they took some action.¹³ When the contributors to *Foundations* discovered that their respondents rou-

¹⁰ A good introductory text is Little (1991). A set of classic readings in the field can be found in Rosenberg (1988).

¹¹ Some anthropological explanations such as symbolic interactionism are noncausal in nature.

¹² The term *case study* could imply that the case in question belongs to a family of cases with similar or generalizable characteristics. There are anthropologists who view their work as explaining what is particular or unique about a situation and who therefore reject the *case* terminology.

¹³ David Szanton points out that the immersion in the field that is often a rite of passage in social and cultural anthropology is itself a form of embeddedness.

tinely undermined the predictions of bargaining theory, they were able to ask them explicitly about their motives. It was thus discovered that the way the games were played mirrored everyday interactions among the players (Henrich et al. 2004). There are also cases where many alternative causal paths may lead to the same outcome (sometimes called the *equifinality problem*), and the case study method may be better equipped to handle these. Some political scientists use what George (1979) calls *process-tracing*, which focuses on an analytical narrative of sequential processes in a causal chain within a particular case (and not on correlations of data across cases).¹⁴

While anthropologists are thus better at telling us how a variable mattered to the outcome, economists are often better at measuring how much it mattered. One creative way to combine the strengths of the two disciplines is through participatory econometrics (Rao 2002). This approach includes participatory appraisals (by the researched), focus group discussions, participant observation (by the researcher), and structured surveys in the design of which the respondents participate. While labor- and skill-intensive, participatory econometrics is likely to yield better insights into causal processes than traditional econometric methods and to be more generalizable than traditional case study methods.

The contribution by Ray (in this issue) is also focused on how to combine the strengths of the two disciplines. The author asks if and how (1) the inferences of economic models can better be interpreted through anthropology-inspired questions about structure and process and (2) the research of anthropologists can be informed by results or correlations from economic models. The argument in the article is that an explanation of the findings of a model is invariably about process and structure. Therefore, without a processual and place-based understanding of cooperative outcomes, new institutions for cooperation cannot be fostered. In addition, anthropologists pay attention to the silences of their respondents as well as to what they say. In that spirit, one must ask if models that abstract from caste or influence or self-respect implicitly suggest that these factors are less important and that village-level cooperation can take place largely on the basis of economic and ecological factors. Finally, anthropologists are critical of economic models for their simplicity and excessive faith in quantifiable outcomes. But, every now and then, economic models surprise with counterintuitive results or nonobvious correlations, and

¹⁴ Case studies are also prone to errors of selection bias, reverse causality, and omitted variable bias, though these errors are rarely explicitly addressed in the studies. In general, the problems of inference in both the quantitative and qualitative traditions arise from social scientists' use of observational data rather than controlled experiments, and therefore rival hypotheses are always difficult to eliminate.

such results should be an invitation to anthropologists as well as economists to investigate social processes that were hitherto not anticipated.

One of the strengths of anthropologists' concern with process, and the detailed analysis that the case study approach requires, is the ability to explain how power operates within a society and the multiple ways in which it manifests itself. Economists are also interested in understanding power relations, and much work on the effect of inequality on cooperation on the commons has been done by economists. But economists usually model power asymmetries as a standing condition, operationalize them as measurable inequalities, and then work through their consequences for the relevant economic agents. This leads them to overemphasize the material benefits and costs of asymmetry, and to underemphasize the symbolic and disciplining dimensions of power, where power and authority are regularly articulated through commons institutions.

Anthropologists, drawing on social theory as well as field observations, have brought a much richer understanding of power to the literature on common pool resources. First, as we mentioned, power has symbolic as well as material dimensions, which have to be revealed in the course of observation and analysis (Li 2002; Mosse, in this issue). Second, an understanding of power is incomplete without an understanding of the resistance that oppression can generate, and the history of common resource struggles is replete with such resistance. From struggles to retain the right to use common forest resources in Indonesia (Peluso 1992), to the protests to stop the displacement of tribal people from their traditional lands along the Narmada River (Baviskar 1996), to the mass squattings and land take-overs of the landless in Brazil (Wright and Wolford 2003), the exercise of power has generated collective actions that can be understood only as dynamic movements and processes—not as predictable outcomes, not as rules of management, and certainly not as equilibria.

Finally, power is not only the ability to make someone do something that is not in the doer's interest, which is what economics can analyze. It is also, at its most subtle and perhaps most pervasive, the ability to frame social policy and the terms of public discussion such that the powerless do not even recognize their powerlessness (Lukes 1974). The ascendance of critical social theory has brought the language and framing of particular issues into the core of current anthropology. The discursive turn in anthropology reflects the influence of poststructuralism, whose starting point is not the objective truth but rather the multiple and coexisting interpretations of social and environmental problems. In this framework, "truths are statements within socially produced discourses rather than objective 'facts' about reality" (Peet and Watts 1996, 13). The ways in which different groups and individuals represent and use con-

cepts¹⁵ such as “protection of the commons” and the politics of such representations, become the foci of analysis. How (only) some environmental problems such as soil erosion or acid rain come to be seen as “problems,” and how these problems are framed for policy purposes is an active research area (Hajer 1995; Leach and Mearns 1996).

Anthropological research in the wake of critical theory undermines the naturalness of familiar categories by revealing how all such categories and regimes are socially constructed and, by so doing, undermines the regimes of power that naturalize these categories. By rejecting the “community” or the “local” as preexisting starting points, for example, Gupta and Ferguson (1997, 67) argue that the researcher is free to explore the feelings, dynamics, and processes that go into “the construction of space as place and locality in the first instance.” The policy and the political implications of either accepting or interrogating these categories are sharply different. Many economists, at least those who are reflexive about their discipline, would probably agree that “the way a question is framed reveals the kind of accommodation being reached” (Dasgupta 1998, 10), but framings and discourses as instruments of social control are far from central to economic analysis. Such uses of power can only be uncovered through process analysis, and as of now they are squarely in the anthropologists’ corner.

The article by David Mosse shows how collective action over tank irrigation in Tamil Nadu, India, is as much an issue of allocating water as it is of maintaining or resisting social power. The article discusses an earlier study by the author that compared two villages, both short on water, where one was very cooperative but the other had no collective action practices. His study led him to believe that water needed to be treated as an institutional whole, that the separation of economic-political and religious-cultural spheres was not meaningful. “My interest was in relationships and processes; but I was also concerned with outcomes,” he explains.

Mosse started with traditional institutional-economic questions such as, why did farmers in some villages cooperate to manage scarce resources, while in others, crucial resources were neglected? At first blush, institutional-economic explanations of variation in collective action in terms of risk and individual incentives made sense to him. But his deeper probing into the processes of exercising power shows that the economic models overlooked the structures of caste authority and the cultural construction of irrigation systems.

¹⁵ In anthropological writings, these questions are often phrased in a somewhat disembodied or agentless manner: How does this issue get represented? How does it get used? How does discourse get reproduced?

The author argues that anthropological research did not necessarily explain outcomes any better than the economics research. But how those outcomes were produced and the social-historical processes involved were crucial to the understanding of what people actually did in practice and why they did it. For example, his social-historical analysis revealed that local common resources, contrary to the premises of economic models, were not primarily managed to maximize economic utility and ensure accountability but rather to minimize social conflict and to enhance the prestige of existing leadership.

C. *Parsimony versus Complexity*

We have just shown that the explication of the multiple ways in which power works in a society is a strong suit for anthropology. Many economic models allow social structures and cultural norms to emerge from millions of disaggregated individual decisions, with no explicit role for power in the emergence. Each individual choice may be reasonable, but together the choices may create an inefficient, unjust, or downright awful society. Dasgupta further claims this feature as an achievement of modern economics, “because it does not rely on postulating predatory governments, or thieving aristocracies, or grasping landlords. This is not to deny their existence, but you don’t need an intellectual apparatus to conclude that a defenseless person will be robbed if there is an armed robber bent on robbing her” (Dasgupta 1998, 12). In a similar spirit, discussing von Thünen’s pioneering work on agricultural land use, Krugman shows that a complex theory of power and history was not needed to explain how land was allocated in the von Thünen model—the assumption of self-interested behavior and strategic interaction was sufficient to allow the spatial pattern of land use to emerge (Krugman 1995, 75). The point that we do not need a particular assumption to explain a particular outcome is an expression of the principle of parsimony, also known as that of Occam’s Razor.¹⁶ If there are two theories with equal explanatory power, we should choose the one with the fewer assumptions. This has been a guiding principle for model building in the physical sciences.

It may not, however, be reasonable to assume that simplicity provides an insight into a particular society, which is a historically evolved system, with layers of change and modification building upon what was already there before it. This is the argument against parsimony that Francis Crick (1988, 138) makes with respect to biology: “While Occam’s Razor is a useful tool in the physical sciences, it can be a very dangerous implement in biology. It is . . .

¹⁶ Named for William of Occam (also spelled Ockham), an influential philosopher and theologian, born in the village of Ockham in Surrey, ca. 1285.

rash to use simplicity and elegance as a guide in biological research.” So why has parsimony been embraced by economics, which is not, after all, a physical science?

The first and most obvious reason is that economics looks for patterns in economic life that, while not universal, are widely generalizable. If, despite differences in culture, norms, and values, a similar enough set of behaviors can be observed in many places and over time, then a small set of simple assumptions may be sufficient to explain them. The most critical element of parsimony has been the assumption of the self-regarding, choice-making individual—usually but not always simplified to a utility-maximizing agent. This one assumption, allied in modern economics to strategic interaction, has given economics its theoretical generalizability and practical policy relevance. This assumption is seriously being questioned by experimental and behavioral economists, but even here they look for systematic departures from the canonical model so that, for example, other-regarding behavior can be formalized and utilized for suitable generalizations.

The article by Sethi and Somanathan (in this issue) is a good example of a parsimonious theory of collective action on the commons. It explicitly lays out the assumptions of the model and how these depart from prior models, makes a set of predictions about cooperative equilibria and when these will occur, and ends with policy prescriptions. The article starts with a classic question: what makes cooperation on the commons more likely? One way to produce cooperative equilibria in traditional models of self-interested behavior is to make resource extraction a repeated game where the future matters. However, the authors point out, infinitely many equilibria that exhibit different degrees of resource exploitation are supported by these models. Prediction becomes impossible with an embarrassment of equilibria—at least, impossible without ad hoc assumptions.

Now the authors introduce their own model, which departs from traditional models in two ways. First, it assumes a stringent form of bounded rationality. Second, it assumes that individuals are not simply selfish—they believe in reciprocity. From these and a few other very simple assumptions, the model predicts that cooperation is more likely when communication is cheap, public good provision is sufficiently productive, effective punishment opportunities are available at sufficiently low cost, and group size is large, *ceteris paribus*. This last prediction is interesting—many orthodox models predict that large groups are less likely to foster cooperation.

The second and less obvious reason for parsimony in economic theory is the modeler’s aesthetic sense. Parsimonious theories explain many observations with few assumptions, and this feature has come to be regarded as elegant.

The conventional argument in all the social sciences, including economics, is that empirical tests are the final judges of whether a theory or hypothesis is a good one. Of course, the conditions under which the hypothesis holds—the *ceteris paribus* condition that is ubiquitous in economics—should be as precisely specified as possible, so the tests conducted are relevant ones. However, there are disagreements among economists about how to test particular theories, about whether in a particular case the *ceteris paribus* condition was (approximately) met, about model specification, and so on. All the social sciences have running debates about what the data show, and, as a result, more than in the natural sciences, several competing and conflicting theories and hypotheses coexist within each discipline. In these circumstances, despite official agreement on the importance of empirically informed theorizing in economics, if there appear to be trade-offs between elegance and relevance, parsimony is likely to be the guiding principle (Klamer 1988, 245).

Parsimonious explanations are not particularly favored in anthropology. There are two important and related reasons for this—the role of the anthropologist in her research and the methodological philosophies of major schools of anthropology. Anthropology as a discipline has a history of being concerned with non-Western, noncapitalist economies, with a mission to explore the particular and the unique and to translate other “lifeworlds” into social scientific discourse. There was time when this mission was not especially progressive, let alone emancipatory—rather, it served to cement colonial stereotypes or exoticize other cultures (see, e.g., Asad 1991). Today, however, the role of the anthropologist in research is conceived in a more complex way than that of the economist. For example, an empirical economist adopts the role of a neutral observer when in the field, gathering data about her subjects while remaining at all times a dispassionate outsider. Some anthropologists are in this category, but an increasing number are not willing to admit the possibility of a wholly neutral position. The attention to the formation of the subject at the intersection of power and knowledge have made researchers conscious of the asymmetries implicit in conducting surveys and interviews, which then purport to represent their respondents to the wider world. Thus these researchers see themselves as empathetic rather than neutral observers, as interpreters of speech and action “from the inside,” or even as partners in their respondents’ aspirations and struggles (Blaikie 2000, 52).

Moreover, the epistemological position of major schools of anthropology is not to focus just on the seen and heard, but to look for hidden meanings, to listen for the unspoken, to interpret culture from the insider’s perspective (Geertz 2000)—in short to “make strange the familiar.” The traditional concern of anthropology with the particular and the unique has also made the gene-

ological approach of Foucault (1980, 1997) especially influential in this discipline. The genealogical approach argues that societies change through a series of power struggles and that there is no overarching or predictable trajectory to this unfolding. There are no universalizable evolutionary laws and no grand theory of change as such. The methodological consequence of this framework is that the role of the social scientist is to reveal the contingent course that has shaped a society and, through this method, to contest notions of necessary orders and structures.¹⁷ This is a very different project from that of economics—if anything, the project is to complicate rather than simplify, question the unquestioned, and be wary of neat and tidy parsimonious explanations.

The difference between a parsimonious and a complicating approach has had enormous consequences for the role of economics and anthropology in policy circles. In formulating causal explanations, the parsimony principle leads economists to insulate the effect of one variable, controlling for others, so that they can measure its direct effect. Anthropologists throw into the analysis a much larger set of factors to capture the essential multidimensionality of action—without telling us what the effect of each factor by itself will be. A cause may never be attributable to one factor, and the symbolic and the material may be considered inseparable in judging effect. The economists' approach is needed if we want to use the research results to guide policy advice. We would want to know about the impact of a particular policy that largely has an impact on one variable (e.g., property rights). We could legitimately argue that too much inseparability and too much multidimensionality would make policy advice impossible and could lead to an accumulation of possibly relevant factors, without providing clues about how to sort the accumulated evidence.

Anthropologists acknowledge that policy advice requires simplifying assumptions and generalizable conclusions, and detailed analyses of complex situations are not conducive to either.¹⁸ But they could legitimately point out that monocausal analyses have frequently been too simplistic and have had unintended consequences when applied as policy. Policies are implemented in unequal social, cultural, and economic structures, and these inequalities and their impacts are more complex than policy analysts realize (Rao and Walton 2004, 360) or even want to know. Simplification for the sake of policy could lead to new ways of social control (Li 2002). A focus on rule making in sharing water could overlook the point that rules are not followed as such; rather, they

¹⁷ In contrast, we may note that while sociologists do consider Foucault to be a key social thinker, the structuralist roots of the discipline have made him far less central to sociology than to anthropology.

¹⁸ However, we have found anthropological studies where generalization much beyond the study site has implicitly occurred, without acknowledgment of this practice.

show how public behavior should be represented (Mosse, in this issue). And parsimonious explanations of central tendencies could lead to the further marginalization of the already marginal, particularly in terms of learning about omitted variables. As Rao (2002, 7) argues, much can be learned from having “tea with an outlier.”

V. Conclusion

In this essay we have argued that some of the key barriers to interdisciplinary work between economists and anthropologists are differences of methodology and epistemology—in what the two disciplines consider important to explain, and how they evaluate the criteria for a good explanation. The essay is an introduction to three articles, on economics, anthropology, and the question of the commons, that illustrate some of these differences and that suggest both the potential and the pitfalls of trying to bridge these methodological gaps. Our goal in this article was not somehow to resolve the differences. Rather, we were motivated by the belief that understanding what is important to the other discipline, and seeing the differences in the light of that understanding, is important for interdisciplinary work and for respectful conversation. We have highlighted three dichotomies that are emblematic of some of these differences: autonomy versus embeddedness, outcomes versus processes, and parsimony versus complexity. A discussion of dichotomies is, of course, just one possible opening into a conversation between economists and anthropologists. We hope our discussion leads at least some economists and anthropologists to reexamine the assumptions and modes of analysis that prevail within the disciplines and to open up new conversations in new directions.

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