

Outcomes and Processes in Economics and Anthropology

ISHA RAY

University of California, Berkeley

I. Outcomes and Processes

Economics is mainly about outcomes; anthropology is mainly about processes. (Michael Lipton)

Michael Lipton (1992), reviewing a book of essays on cross-disciplinary dialogue (Bardhan 1989), went on to argue that a true conversation—*sacra conversazione*—between economists and anthropologists has to come to terms with the distinction between outcome-focused research in economics and process-focused research in anthropology. Lipton asked: are there ways of bridging this gap, and should the attempt even be made?

Outcomes are the findings of research such as hypotheses to be tested or answers to “what if” questions from manipulating model parameters and assumptions. Outcomes in economic analysis have two characteristics (see Bardhan and Ray, in this issue): (1) they serve as predictions, saying what various actors will do in response to specific changes, given the assumptions and starting conditions of the model, and (2) they describe equilibrium solutions for the economy.

Processes are the dynamic steps through which particular outcomes may or may not be reached and through which social relations are reshaped. The outcome-process distinction, while difficult to define with precision, nevertheless seems to be understood by economists as a point of departure between their discipline and the other social sciences. For example, in their introduction to *Foundations of Human Sociality*, Henrich and colleagues say that the goal of experimental economics is to change economic theory so that “the processes determining outcomes matter as well as the outcomes themselves” (Henrich et al., 2004, 2). Anthropologists are, by and large, wary of the equilibrium-

I thank David Szanton, Jeffrey Williams, Kristi Hansen, Seemin Qayum, and Amita Baviskar for discussions and comments on this article. I am also grateful to the reviewers of this journal for their detailed comments and suggestions.

focused way of viewing the world, with its implication of satisfaction where everyone is already doing the best he or she can do.

The outcome-process divide does not mean that economists are uninterested in processes or that anthropologists think of outcomes as irrelevant. Rather, it means that research in economics is driven primarily by the need to predict outcomes while anthropology is driven by a concern with relationships, values, and power dynamics. Take the case of local common-pool resources, or the “commons.” Why do economists and anthropologists study the commons? For economists, the historical motivation has been the resolution of the commons dilemma—how can self-regarding individuals, with cooperative as well as conflicting interests, act collectively to protect their common resources (Runge 1984; Seabright 1993; Sethi and Somanathan, in this issue)? A second motivation is to affect public policy, that is, to suggest the conditions under which aggregate welfare-enhancing collective action can be fostered.

For most anthropologists, efficient management and policy advice are not the most salient concepts. In the contemporary anthropological literature on the commons, there is less emphasis on the “rules” of collective management and more on collective resistance to prevent the appropriation of lands or forests (Peluso 1992; Baviskar 1996). Another, more recent, set of concerns stems from discourse analysis in general and the knowledge-power interface in particular (Foucault 1980). Anthropologists are thus sensitive not only to what has been said but also to what has not been said, because not saying is a way of saying. Does the discourse that takes hold over allocation rules and the common good, for example, undermine or strengthen the workings of power in society? These understandings have led to critical interrogation of the dominant (and often laudatory) discourse on community-based “solutions” to common-property resources (Li 1996; Goldman 1997).

In this article, I argue that cross-disciplinary conversations would be made easier if economists were to consider how more attention to processes could refine their own work and if anthropologists were to consider the avenues through which the outcomes of economic modeling could inform their own research. The challenge for economics and anthropology is to illuminate one another while struggling with their sometimes incompatible research methods and often strikingly different research motivations.

II. Cooperative Conversations

Approaching this conversation will be easier if it is anchored by specific studies on common property. I choose community-managed irrigation water as the focal resource for the rest of this article. Through critical analysis of four papers (two economics, one institutional-anthropological, and one social-anthropological) I

ask the following three questions: (1) Can the inferences of economic models be better interpreted through anthropology-inspired questions about structure and process? (2) Can the outcomes of economic models be more precisely stated by explicitly acknowledging what is being said as well as what is not being said? (3) Can anthropologists' research be guided by results or correlations from economic models—especially counterintuitive or unexpected results—and thereby lead to new analyses?

In answering these questions, I will examine four papers in some detail. Pranab Bardhan's (2000) "Water Community: An Empirical Analysis of Cooperation on Irrigation in South India" (published in *Economic Development and Cultural Change*) and Isha Ray and Jeffrey Williams's (2002) "Locational Asymmetry and the Potential for Cooperation on a Canal" (published in *Journal of Development Economics*) are both empirical papers. The first is an econometric model; the second is a mathematical programming model. Robert Wade's (1987) "The Management of Common Property Resources: Finding a Cooperative Solution" (published in *The World Bank Research Observer*) is written by a political scientist; the article reaches what might be called "institutional-economic" conclusions, but it is rich in anthropological and historical detail.¹ David Mosse's (1997) "The Symbolic Making of a Common Property Resource: History, Ecology, and Locality in a Tank-Irrigated Landscape in South India" (published in *Development and Change*) is written by a social anthropologist. Mosse uses Wade's work as a foil for his own. While both of these studies are process oriented, the two reflect different research concerns and theoretical underpinnings.

A. Question 1: Can the Inferences of Economic Models Be Better Interpreted through Anthropology-Inspired Questions about Structures and Processes?

Analytical models are judged on whether their outcomes have been observed in the real world, and confidence builds in the model and its assumptions if they are deemed to have good explanatory power. At least this is the official story (Klamer 1988, 243). Occasionally models may also be judged by whether their assumptions are grounded in the real world—as Basu (2000, 246) argues they should be—but the traditional justification for accepting a model is that "it works."² New models are proposed, with new assumptions, on the argument

¹ The contents of this and other related papers were subsequently worked into *Village Republics* (Wade 1988), a book that has been enormously influential in the common pool resource literature.

² This is a reasonable way to think, but this is not always justified from within the model. Analytical models are "if *P*, then *Q*"-type statements. Logically, that tells us nothing about inferring *P* from having observed *Q*. Therefore, "the model works" means no more than "the outcomes are consistent with real-world observations."

that the older models could not explain some facet of economic life. New models are also proposed if older models “explain” a bit too much—that is, produce a plethora of equilibria, most of which are never empirically observed (see, e.g., Sethi and Somanathan 1996). However, while a large number of fake equilibria is a weakness in a model and a smaller number of more plausible equilibria is a good criterion for model selection, a unique equilibrium is not always indicative of a better model.³

Once a model has a stable outcome, for example, a unique Nash equilibrium, the economist takes that outcome and works backward to infer a plausible social process from it. However, “the basic trouble is that nature is so complex that many quite different theories can go some way to explaining the results.”⁴ Different theories may (implicitly) have particular processes embedded in them, or any one theoretical outcome could arise from any one of several processes. Some models have causal arrows explicitly specified, while others, such as the standard $m \times n$ game matrices, are quite spare. If this is so, then working back from an outcome can provide at best a partial guide to the process that gave rise to it, and at worst it is of no help at all. What, for instance, are the implications of cooperation if the conditions under which it is stable are as likely to be sustained by coercion as by consensus? Surely, for example, it matters for social policy if “resource conservation” is achieved voluntarily or through coercion by the state (Peluso 2003). What, indeed, is the meaning of such cooperation for the cooperating parties? Here is where looking with an anthropological eye at the processes that lead to specific model inferences would help to interpret those inferences and to consider alternative explanations for ostensibly the same outcomes.

Econometric models also have to contend with multiple explanations of their (unique) correlational outcomes. Take the example of Bardhan (2000)—a regression analysis of a stratified sample of 480 farmers in 48 randomly selected irrigated villages of Tamil Nadu, India. In this paper, the author conducts a correlational analysis of the conditions that are conducive to cooperation on small-scale irrigation systems. He defines three dependent variables as indicators of cooperation—the maintenance and

³ Nor does a unique equilibrium have an unambiguous interpretation. This is a point to which modelers are not sufficiently sensitive.

⁴ Crick (1988, 141). Crick—who approved of modeling but only cautiously so—was one of the most insightful of writers on the relationship between theoretical and empirical work. In the piece quoted from, he was discussing the problem of theory and modeling in biology as opposed to the physical sciences. His argument rests on the relative complexity of biological processes and their outcomes. The social world is, of course, at least as complex, with theories being at best broad generalizations with many exceptions.

condition of distributaries and field channels, the number of water-related conflicts within each of the sampled villages, and the frequency with which the agreed-upon water allocation rules are violated. He defines several independent variables—such as the number of water users per system, the extent of economic inequality, the degree of caste homogeneity, and whether the water allocation and cost-sharing rules were devised jointly by the farmers or by the village elite. The primary data collection method was the structured survey. The idea was to regress the independent variables against the indicators of cooperation to gauge the positive or negative nature of the relationships. For instance, is the number of users positively or negatively correlated with the frequency of rule violations? Is caste homogeneity positively or negatively correlated with water conflicts?

To illustrate how a model such as this one can have unambiguous and plausible outcomes that are consistent with more than one processual-structural explanation, I focus on two very interesting results. Both relate to the degree to which the elite crafted—or were perceived to have crafted—the rules of water allocation. First, the author finds that the maintenance of field channels is negatively and significantly correlated with the degree of elite rule making.⁵ This finding is accompanied by a footnoted observation that farmers were more likely to say positive things about a system where the rules were perceived to have been democratically crafted. The outcome is clear, but the implication of this outcome is unclear. Perhaps there is no “real” negative relationship between channel maintenance and the rule of elites, but the negative relationship is one of perception? That is, the responses reflected a generalized negative attitude toward elite control and a positive attitude toward participatory decision making. That is plausible. Or perhaps the correlation was real, and the negative sign is to be explained by the average farmer’s feeling of lack of ownership of the system? However, thinking about anthropological studies—many of which show that rules of community-based cooperation are regularly crafted by elites, often so for the benefit of the same elites—we can consider an alternative explanation of this outcome.

The districts of Tamil Nadu, where this study was conducted, have a number of open wells as well as bore wells, and, as elsewhere, the better-off farmers are more likely to have wells than the poorer farmers. Throughout the Deccan Plateau, shallow wells are recharged by tank and canal seepage, and since water in a private well is completely under the control of a farmer, its marginal

⁵ “Significant” here means statistically significant; it does not imply economic or otherwise substantive significance (see McCloskey and Ziliak 1996).

value can be very high (Ray and Williams 1999). In poorly maintained field channels, the water flow is sluggish rather than fast, so a higher proportion of that flow will seep into the ground (Chow 1959). If the elite did in fact craft the rules of cooperation, and if they also owned a disproportionate number of the wells, it is perfectly possible that the rules were made to emphasize water allocation shares and conflict resolution but were deliberately less particular about channel maintenance. What is more, an economic calculation might show that some of these poorly maintained elite-ruled channel systems are agriculturally more productive than some in which field channels are in good repair and the water allocation is more equitable.

The interpretation offered in Bardhan (2000), as well as the hypothetical one in this article, are compatible with the regression outcome, but they do not describe the same society and would not lead to the same policy recommendations. If the negative correlation is one of perception, then, other than to facilitate better communication within water-users' associations, there is no obvious policy intervention. If the negative correlation is real and stems from the lack of democratic decision making, then the state, or more likely, a civil society group, could perhaps foster more participatory processes. But if the elite deliberately keep channels poorly maintained so that their wells can benefit disproportionately from canal seepage, then polite policy changes are not likely to be effective—the more powerful farmers may actively resist system “improvements.”

Second, the Bardhan research finds that there is a perfect (negative) correlation between reports of rule violations and the perception of elites making the rules. What processes could have given rise to this outcome? One possible explanation—and the one suggested in Bardhan's paper—is that the elite are more likely to break rules in general because they can get away with it. However, when they make the rules, they are not as likely to break them. This is believable. But why do they not break them? Is it because, having made the rules themselves, the elite are normatively more committed to them? Maybe so.⁶ Or perhaps the rules are crafted according to elite convenience in the first place, and they are generally enforced by arguing that it is good for everyone to comply with the “community” rules (see Bourdieu 1977). In this

⁶ In my fieldwork in Maharashtra, I did observe this tendency. The irrigation cooperative that I studied had a strong elite influence. One of the three largest farmers, who had campaigned for cooperative as opposed to state control, told me that he had given up on illegal irrigation. He had supported an irrigation cooperative, so he could not now ignore the rules. “It looks bad.” I saw this on the Gediz Canal in western Turkey, too. There the allocation rules were enforced by the influential farmers, who did not always benefit from the enforcement but who enjoyed the additional status given to them as enforcers. They, too, hardly ever broke the rules.

case the law-abiding elite would not need to break the rules. Or perhaps different *de facto* even if not *de jure* rules are in place for the elite than for the rest—and at least some respondents in the elite-ruled villages simply internalized this difference and reflected it back in their responses. In this case, perfect rule compliance would also be reported. Or perhaps ordinary farmers are more afraid to break elite-crafted rules—thus significantly lowering the incidence of rule violation in such villages. (We do not know from the reported results which individuals are violating the rules in which particular village). These contexts, and therefore the explanations of the “same” outcome, are entirely different. Indeed, there is no *a priori* reason to assume causal homogeneity across the villages at all—inferring causation from cross-sectional data tends to force the researcher into a monocausal explanation of each outcome, which may not be justified (Collier, Seawright, and Munck 2004).

My argument in this section has been that an explanation of any model outcome is invariably about process and structure. Working back from an equilibrium solution or from a statistical correlation is helpful in understanding central tendencies and in eliminating processes that are incompatible with the model outcome. But it is of limited help in deciding which processes are at work. For this purpose, one has to interrogate the economist’s model by thinking more like an anthropologist—by carrying out “empirical investigations of processes themselves” (Lipton 1992)—so that alternative explanations can be considered.

If analyses of specific circumstances can be undertaken during the study or after the initial findings, then one possible path would be to choose a small number of cases from the larger sample for what qualitative researchers call within-case causal process analysis. This is a method by which each case is treated singly and evidence internal to the case is used to evaluate competing hypotheses (Collier, Mahoney, and Seawright 2004). Despite potential selection bias problems, cases for such studies are best not chosen randomly.⁷ Within-case analysis of a few cases with no variance on the value of the dependent variable (in this case, little rule violation) would be a better choice. The object would not be to generalize up from those detailed process analyses but, rather, with closer knowledge of at least some of the cases, to get a sense of alternative paths that might lead to the same outcome and to examine whether the

⁷ Self-selection in the sample is common in the case study method, and the traditional view of quantitative research is that this introduces “bias” in the resulting analysis. But the method of within-case analysis or causal-process analysis for a few select cases is not derivative of large-*n* analysis methods. Small-*n* samples—when chosen and used well—are not little versions of large-*n* samples; they are, as Margaret Mead once called them, “another kind of sample.”

assumption of causal homogeneity in the regression model is approximately justified.

If this last step cannot be taken, it is incumbent on the economist to interpret his or her results cautiously. Intellectual merit apart, there is a practical reason for the process-oriented interrogation of economic solutions. Without a processual understanding of cooperative outcomes, how can new institutions of community-based cooperation be fostered? The question itself explicitly assumes that community-based cooperation is desirable, because it implicitly assumes that a desirable outcome—efficient and orderly water allocation—will be the result of a desirable process. But this is not necessarily so.

B. Question 2: Can the Outcomes of Economic Models Be More Precisely Stated by Explicitly Acknowledging What Is Being Said as Well as What Is Not Being Said?

In this section I take up an issue that anthropologists consider central but economists do not explicitly take into account. In every model (and explanation) there are points that are explicitly made and others that are not made. What is explicitly said and inferred forms the basis of the model's policy recommendations. But, say critical anthropologists, what is being said and not being said, and being asked and not being asked, reveal whose interests the explicit recommendations are serving and which social structures they hold up (Li 2002). Anthropologists argue that the "celebration" of community-based natural resource management as a system that upholds the common good overlooks the micro politics of local life. Mosse (1997, 467) is critical of institutional-economic models of common property management as "sets of rules and cooperative equilibrium outcomes internally sustained by a structure of incentives." The critique is even more trenchantly expressed by Mohan and Stokke (2000, 247). They claim that the emphasis on local participation in the development arena—and in resource management—"tend(s) to underplay both local inequalities and power relations as well as national and transnational economic and political forces."⁸

In this light, I examine the implications of Ray and Williams (2002), a mathematical programming model of canal irrigation in Maharashtra, India. The paper was about conditions under which the theft of irrigation water on

⁸ While sympathetic to the need to listen to the unspoken as well as the spoken, as it were, I should point out that many model-oriented papers on irrigation do have extended discussions on locally and historically specific forms of cooperation, on the characteristic of irrigation guards and "water-turners," and on the sometimes antagonistic and sometimes tolerant attitudes of the state to local cooperation. This is true of the work of Elinor Ostrom (Ostrom 1990; Ostrom, Gardner, and Walker 1994) and Robert Wade (1987, 1988)—two of the most influential economic-institutional scholars of irrigation.

canal systems—officially called “unauthorized irrigation”—could be controlled through farmer cooperation. Williams and I hypothesized that locational asymmetry on even small subsections of the canal system was a key determinant of who was likely to cooperate and who was not likely to cooperate. In a 30-farm model of a watercourse, we gave the farmers an equal propensity to steal extra water, but we made the stealing opportunity a function of the farmer’s location. Upstream farmers are generally better positioned to siphon off extra water when needed, so our hypothesis was that they would resist a farmer-run cooperative. We assumed that each farmer was profit maximizing, and we derived a model map of the watercourse showing the (endogenous) dividing line between those who would want a cooperative with enforceable rules of water allocation and those who would prefer the ruleless free for all. We tested this model empirically by interviewing 67 farmers in two villages where an irrigation cooperative had recently been voted into existence,⁹ and we compared the model map of procooperative votes to the actual village-specific one. In both cases, there was a clear pattern of downstream votes in favor of a farmer cooperative—therefore against Irrigation Department (ID) control of the canal—and upstream votes against.

I now reexamine this model in the light of what it left out. Any economist knows that a model leaves things out—otherwise it becomes intractable and yields no useful outcomes. When I wrote the code for the program, I isolated the effect of location by leaving out wealth, caste, and power inequality. Our question was, with or without these heterogeneities, does location have an independent role in the farmer’s attitude toward cooperation? But in the world outside the model, wealth, caste, and power influences were present. Was our paper, then, an example of a model with strategic silences and omissions, which were then not explicitly acknowledged in the policy recommendation sections? I illustrate the point with three examples from my fieldwork in Maharashtra.

First, several Muslim farmers were located downstream, and the model “predicted” that they would vote for a farmer-run cooperative on the basis of their locational disadvantage. And, in fact, they were overwhelmingly in favor of it. But here are the words of one of the three leaders of the cooperative movement, himself a well-off Muslim farmer: “Basically, Madam, everything here goes by community. I wanted this cooperative, I did not like having to take water unlawfully. I saw that it was creating conflicts, and this was bad for the whole village. So I campaigned for the cooperative. Within a few days all the Muslims had signed up for it.”

⁹ Both villages were on the same watercourse.

Second, caste loyalties, or lack thereof, also made a difference to how specific farmers voted. For example, two quite prosperous Marwari brothers in this Maratha- and Mali-dominated region—one upstream and one downstream—both voted in favor of local-level cooperation. Therefore, one of these votes, that of the upstream brother, would have been incorrectly predicted by our model.¹⁰ Here is what that troublesome farmer said: “See, these people are all decent people. We have never felt that we didn’t get water because we were outsiders. My own water flow was good, my groundnut yields are the best, ask anybody. But sometimes there were fights on the canal.¹¹ And when there is a fight, well, we just don’t have the numbers. For outsiders, laws that everyone has to follow are better than not having proper laws.”

Third, several of the smaller farmers had felt humiliated by the arbitrary and arrogant behavior of the Irrigation Department canal inspectors (who used to allocate the water prior to the formation of the cooperative). Many of those farmers were located at the bottom two-thirds of the watercourse, and, as predicted by our model, they had voted overwhelmingly in favor of replacing the Irrigation Department inspectors with local inspectors. They said that they were always facing water shortages on account of water theft and general mismanagement. But what is more, they reported: “To the ID’s man, I was not even an insect; I was dirt. He wanted to see me on my knees, begging for water, calling him *Bhausahib*, *Bhausahib* [respected brother]. But why should I do it? As soon as those society people came to me I said, I don’t need any explanations, you just show me where to sign my name.”

My point in revisiting this model is not that it was “wrong” in its suggested outcomes—in fact, the predicted pattern of cooperation was very close to the independently verified pattern. The model also had a plausible mechanism for why this should be so,¹² and we concluded that location was an independent predictor of farmers’ attitudes toward cooperation. We also concluded, from modeling the farming system, that water’s “time of arrival, its delivery fre-

¹⁰ Developing and testing a model is a strange experience. When we looked at the village map of votes for and against the cooperative, we saw that this farmer—median-sized, upstream, commercially successful, and reporting no difficulty with his pre-cooperative water supply—had voted in favor of replacing the Irrigation Department with a watercourse-based cooperative. We were initially quite annoyed with him for having gone “against” our model.

¹¹ He meant before the cooperative rules were instituted, and when the watercourse was run by a very lax Irrigation Department. Conflicts about whose irrigation turn it was and who was encroaching were common.

¹² Programming models, with their key parameters calibrated to a particular context and the functional relationships between and among variables explicitly modeled, tell a clearer causal story than is sometimes possible with regression models. The causal mechanisms are built into the model and do not have to be inferred.

quency, and the seasonal nature of the crops' water requirements combine with the supply of labor, seepage down the channel, and the costs of stealing water to determine the potential for cooperation in complex, and sometimes surprising, ways" (Ray and Williams 2002, 151). This, too, still holds as a valid conclusion. But, at least in some of the prediction-compatible cases, the left-out social and political factors played a role in the success of the prediction. So, too, for the prediction errors—they were not always, or were not explained as being, on account of a location-driven or agriculture-driven calculus. We did not cover up these "deviations" in our paper, but we were silent about their implications when explaining the conclusions and significance of the model.

So the question is, do models such as this one—by abstracting from caste or influence or the desire for dignity—implicitly suggest that these factors are less important and that village-level cooperation can take place purely on the basis of a cost-benefit calculus, one driven by economic and geographic factors? This is what sociologists and anthropologists have been critical of, both from the social-scientific and political standpoints. I could defend our work by arguing that we made it clear in the paper that we wanted to isolate the effect of location so that we could see if location had an effect independent of caste, community, or wealth. We never claimed that wealth and influence were not players in village-based cooperation. But I suspect that an anthropological eye on the model would look askance at such a defense—not saying, they would claim, is also a form of saying.

Economists have to acknowledge the implications of what they are abstracting from—not to undermine the usefulness of their models and not to lose their tractability but because of the way that their models might be interpreted in policy circles. Modeling involves real gains and real losses, and these strategic losses should be brought back into the picture when interpreting outcomes and advising policy makers. Sometimes the losses could outweigh the gains when modeling complex systems (Krugman 1995, 79). Since simple and general policies are easier to recommend than nuanced and contextual policies, the simple and unqualified version of a model often carries the day in the policy advice process (Chambers 2001). There are many illuminating examples of (the better) economic models cutting through the clutter to isolate the effect of one variable from that of another. But policy has to be implemented, clutter and all. Thinking through the intellectual as well as political implications of what has not been said and not been asked would bring more caution, integrity, and precision into the interpretation of a model's outcomes and "answers."

C. Question 3: Can Anthropologists' Research Be Guided by Results or Correlations from Economic Models—Especially Counterintuitive or Unexpected Results—and Thereby Lead to New Analyses?

Economists' research on common property has already learned much from the hundreds of irrigation-related case studies conducted all over the world by anthropologists and other social scientists. It is normal to find economists justifying the assumptions of their models by reference to the field insights of anthropologists (Ostrom, Gardner, and Walker 1994; Sethi and Somanathan 1996). Anthropologists are less overtly influenced by economics, and critique of economic analysis is virtually a mini-industry in some anthropological circles. In this section I argue that, critique notwithstanding, economic models and their outcomes can (and, in the commons literature, already do) usefully inform anthropological inquiry. I also suggest that every now and then models surprise us with counterintuitive results or nonobvious correlations. Such results should be an invitation to anthropology to investigate new or changing social processes that were so far not thought of or which perhaps have started to emerge but have hitherto gone unnoticed.

One of the best-known pieces of research in the common pool literature in general and the irrigation literature in particular is Robert Wade's 1988 book *Village Republics*. Many of its central findings (though not the rich ethnographic detail in Wade's research) were first published in 1987 in the *World Bank Research Observer*. Wade's research is a study of canal-based cooperation in Andhra Pradesh, India—where he noticed that some villages had elaborate cooperative organizations while others did not. His question was, when will villagers cooperate to allocate their common water resources? His study is a part-anthropological and part-economic-institutional study, having no formal model and no regressions. The study's main results are the following:

1. There is a positive correlation between location on the canal system (implicitly the main independent variable) and the propensity of irrigators to cooperate (implicitly the dependent variable). Fewer head-end villages have cooperative institutions in place relative to tail-end villages.
2. A set of "conditions," especially high net collective benefits from cooperation coupled with high ecological and economic risks from not cooperating over scarce water, lead to irrigation-based cooperation. This explains the first finding: villages at the tail end of a canal system are more water short and have more to gain from formal water management rules.
3. Resource-based cooperation is rational and about "getting things done."

Moreover, cooperation is most often organized in accordance with traditional caste authority; otherwise, the social control needed to share the water could break down. Cooperation is not about opening up democratic spaces or about the moral economy of Geertz (1963) or Scott (1976).

David Mosse's 1997 historical-anthropological paper, published in *Development and Change*, started with the same question that Wade asked: when will villagers cooperate to allocate their common water resources? Mosse's ethnographic work in tank-irrigated districts of Tamil Nadu corroborated several of Wade's results, such as the finding that economic benefits and ecological risks made collective action over water more likely. But Mosse's critique of Wade, and by extension of other common property research in the economic-institutional vein, reflects three points of departure. The first concerns conclusions. Village-based cooperation, Mosse finds, is not merely about material gains and losses as conventionally measured but also about creating and asserting rights to symbolic resources. Following Bourdieu, Mosse argues that the symbolic value of the commons, such as temples, fish, and water, is inseparable from their material value. The second point of departure concerns modes of explanation. Deterministic relationships between ecological-economic conditions and cooperation, or the analytical outcomes of "what if" questions in models of cooperative management, ignore structures and processes of power.¹³ For example, the traditional dominance of upper castes in the "cooperative" villages was historically implemented through control over water and other common resources, and these groups retained the ability to bend the rules to their own purposes. Irrigation water is ruled rather than managed. Villages without collective action were those where irrigation water never had been part of the mode of caste control. The third point relates to the role of the state. Wade finds that the official water agency of the state had no idea of the extent of cooperative management at the village level. Mosse counters that, in fact, the state has always been tied to these "cooperative" ventures, from the colonial era to now. Moreover, models of harmonious cooperation under "rule regimes," where the state has little or no role, are used by donors to further currently fashionable policies of decentralized management.

What does Mosse versus Wade, and in particular Mosse's outcome-process-related critique, reveal about the role of economic analysis in furthering the work of anthropology? As always, we have to start with a clear understanding

¹³ This criticism of determinism directly reflects the outcome-process distinction. Of course, the longer the time frame of analysis and the larger the number and types of variables in the analysis, the more everything becomes endogenous, and the harder it is to disentangle what causes what.

of the purpose of models and of how their outcomes are to be understood. A theory or a model is not a representation of some social phenomenon—much less a comprehensive representation. It is a demonstration of the phenomenon and of a mechanism that might uphold the phenomenon under certain well-specified conditions. Theories (and by extension) theoretical inferences are best seen as “hints to suggest possible lines of research” (Crick 1988, 138) and as a source of ideas about “where to look.” Each model provides a starting point for a new step in a research program and suggests potentially important relationships that are worth further investigation. This is the role that models have played within the discipline of economics itself. Looking at Mosse versus Wade in that light, economic-institutional models seem to have served this purpose admirably for anthropological research on the commons and have been doing so since Hardin’s “tragedy” (Hardin 1968). Empirical studies by a range of scholars, including social anthropologists, have often confirmed the key findings of economic theory on different categories of common resources (Dasgupta 1993, 287). Even when economics provides a point of departure or a foil against which to make the case for alternative assumptions and alternative modes of explanation, this is in itself a useful contribution. Mosse himself accepts the starting point that Wade’s research affords him, and he then goes on to argue that his work goes beyond Wade’s to show that irrigation is a sociocultural, and not merely an economic, system (Mosse 1997, 474). But many critical anthropologists have—without quite acknowledging the debt, if we are to listen for the unspoken—decided where to look, and from where to take off, from models of collective action on the commons.

Finally, models are accused (sometimes with justification) of choosing their axioms cleverly so that they can yield the desired results. Anthropologists have also been critical of economic models for their alleged simplicity and excessive faith in quantifiable outcomes, many of which appear obvious to other social scientists. Of course, economic models and regression analyses can produce fairly obvious outcomes, but every now and then a model will yield an inference or a “what if” outcome that the modeler did not expect and that is counter-intuitive. Some of these surprises can alert researchers to potential unintended consequences of specific policy interventions that are under consideration or perhaps already in place. Such results provide avenues for anthropologists as well as economists to investigate new or changing social processes that were so far not anticipated or that may have started to emerge but have hitherto gone unnoticed.

Both Bardhan (2000) and Ray and Williams (2002), as well as Sethi and Somanathan (in this issue), have nonobvious or surprising results. Bardhan (2000) indicates that the relationship between intravillage inequality and co-

operation over water is not a monolithic one. Instead, there appears to be a U-shaped relationship between inequality of asset holdings and degree of cooperation over water allocation. The hypothesis that lower levels of inequality would be conducive to cooperation would have been a priori a plausible one. It is not obvious what causal processes are behind the U-shaped relationship suggested in the paper (or if indeed it holds under many conditions), and the finding itself is not obvious from the model specification. Ray and Williams (2002) show that reducing water theft and encouraging cooperation and equity in water allocation among farmers may cause the landless on the watercourse to suffer. This is because equity in water sharing alters the cropping patterns and spreads out the demand for labor over the entire year, thus leading to a greater use of family labor and reduced employment opportunities for the landless. Here the causal chain is clear, but it would not have been predictable simply from the model's assumptions and predictions. The Sethi and Somanathan paper models the evolution of cooperation on the commons with the assumption that (at least some) people are reciprocal rather than maximizing. Their model predicts, among other things, that cooperation is more likely when group size is sufficiently large. The literature on group size and cooperation is mixed, but ever since Olson (1971) the conventional wisdom has held that smaller is better. While these outcomes were not the central argument of any of the three papers, they do have consequences for governance arrangements and sometimes indicate potential trade-offs between desirable goals.

Unexpected model results allow researchers to see phenomena that were not seen before, as well those that may be just emerging. They provide an opening for further investigation for economists and anthropologists alike. For example, are these predicted phenomena actually being observed? If yes, what processes and structures explain their particular manifestations? What norms or power relations are changing or being renegotiated as a response to these emergent phenomena? If the phenomena are not observed, what did the model overlook, or what variable was simplified to the extent that it has no meaning? And, of course, how should these changes be interpreted, and from whose vantage point? These are questions about process and structure that anthropologists are theoretically and empirically better equipped to explain than are economists. As such, the surprises from otherwise plausible economics models could act as an "invitation to anthropology to widen its conceptions of how human beings engage their own futures" (Appadurai 2004, 63).

III. Conclusions

The premise of this article is that the outcomes of economic models and process analyses of anthropology are both essential for understanding social phenomena,

including those surrounding the commons. The main argument has been that an explanation of any model outcome is invariably about process and structure—the outcomes of several models are compatible with many different causal processes. Therefore, without a processual understanding of cooperative outcomes, new institutions of community-based cooperation cannot be fostered in specific places. In addition, anthropologists pay equal attention to silences and to what has been said. In that spirit, one must ask if models of resource management that abstract from, for example, influence or the desire for dignity implicitly suggest that these factors are less important than economic and ecological factors. This article makes the case that policy advice has to take into account the explicit findings of a model as well as its silences.

Anthropologists are also critical of economic models for their simplicity and their often obvious outcomes. But I have argued that influential models can and do provide anthropologists with useful starting points and constructs for their own research. In addition, every now and then models surprise with counterintuitive results, especially with respect to emergent social phenomena. Such results challenge both anthropologists and economists to investigate new or changing social processes that were so far not anticipated or that had gone unnoticed. For economists and anthropologists to learn from process and outcome, respectively, however, it is incumbent upon economists to interpret the outcomes of their models more modestly and more accurately than many are inclined to do and upon anthropologists to go beyond critique to judge the potential usefulness of models for their own work.

References

- Appadurai, Arjun. 2004. "The Capacity to Aspire: Culture and the Terms of Recognition." In *Culture and Public Action*, ed. Vijayendra Rao and Michael Walton, 59–84. Stanford, CA: Stanford Social Sciences.
- Bardhan, Pranab. 1989. *Conversations between Economists and Anthropologists: Methodological Issues in Measuring Economic Change in Rural India*. New Delhi: Oxford University Press.
- . 2000. "Irrigation and Cooperation: An Empirical Analysis of 48 Irrigation Communities in South India." *Economic Development and Cultural Change* 48: 847–66.
- Basu, Kaushik. 2000. *Prelude to Political Economy: A Study of the Social and Political Foundations of Economics*. New York: Oxford University Press.
- Baviskar, Amita. 1996. *In the Belly of the River: Tribal Conflicts over Development in the Narmada Valley*. New York: Oxford University Press.
- Bourdieu, Pierre. 1977. *Outline of a Theory of Practice*. Cambridge: Cambridge University Press.
- Chambers, Robert. 2001. "Qualitative Approaches: Self Criticism and What Can Be Gained from Quantitative Approaches." In *Qual-Quant: Qualitative and Quantitative*

- Poverty Appraisal: Complementarities, Tensions and the Way Forward*, ed. Ravi Kanbur, available at <http://www.arts.cornell.edu/poverty/kanbur/QQZ.pdf>.
- Chow, V. T. 1959. *Open Channel Hydraulics*. New York: McGraw-Hill.
- Collier, David, James Mahoney, and Jason Seawright. 2004. "Claiming Too Much: Warnings about Selection Bias." In *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, ed. Henry Brady and David Collier, 85–102. Lanham, MD: Rowman & Littlefield.
- Collier, David, Jason Seawright, and Gerardo Munck. 2004. "The Quest for Standards." In *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, ed. Henry Brady and David Collier, 21–50. Lanham, MD: Rowman & Littlefield.
- Crick, Francis. 1988. *What Mad Pursuit: A Personal View of Scientific Discovery*. New York: Basic.
- Dasgupta, Partha. 1993. *An Inquiry into Well-Being and Destitution*. Oxford: Clarendon.
- Foucault, Michel. 1980. *Power/Knowledge: Selected Interviews and Other Writings, 1972–1977*, ed. and trans. Colin Gordon. Sussex: Harvester.
- Geertz, Clifford. 1963. *Agricultural Involution: The Process of Ecological Change in Indonesia*. Berkeley: University of California Press.
- Goldman, Michael. 1997. "Customs in Common: The Epistemic World of the Commons Scholars." *Theory and Society* 26:1–37.
- Hardin, Garrett. 1968. "The Tragedy of the Commons." *Science* 162:1243–48.
- Henrich, Joseph, Robert Boyd, Samuel Bowles, Colin Camerer, Ernst Fehr, and Herbert Gintis. 2004. *Foundations of Human Sociality*. Oxford: Oxford University Press.
- Klamer, Arjo. 1988. *Conversations with Economists*. Totowa, NJ: Rowman & Allanheld.
- Krugman, Paul. 1995. *Development, Geography and Economic Theory*. Cambridge, MA: MIT Press.
- Li, Tania M. 1996. "Images of Community: Discourse and Strategy in Property Relations." *Development and Change* 27:501–27.
- . 2002. "Engaging Simplifications: Community-Based Resource Management, Market Processes and State Agendas in Upland Southeast Asia." *World Development* 30:265–83.
- Lipton, Michael. 1992. "Economics and Anthropology: Grounding Models in Relationships." *World Development* 20:1541–46.
- McCloskey, Deirdre, and Stephen Ziliak. 1996. "The Standard Error of Regression." *Journal of Economic Literature* 34:97–114.
- Mohan, Giles, and Kristian Stokke. 2000. "Participatory Development and Empowerment: The Dangers of Localism." *Third World Quarterly* 21:247–68.
- Mosse, David. 1997. "The Symbolic Making of a Common Property Resource: History, Ecology and Locality in a Tank-Irrigated Landscape in South India." *Development and Change* 28:467–504.
- Olson, Mancur. 1971. *The Logic of Collective Action: Public Goods and the Theory of Groups*. New York: Schocken.
- Ostrom, Elinor. 1990. *Governing the Commons: The Evolution of Institutions for Collective Action*. Cambridge: Cambridge University Press.

- Ostrom, Elinor, R. Gardner, and J. Walker. 1994. *Rules, Games and Common-Pool Resources*. Ann Arbor: University of Michigan Press.
- Peluso, Nancy. 1992. *Rich Forests, Poor People: Resource Control and Resistance in Java*. Berkeley: University of California Press.
- . 2003. "Coercing Conservation: The Politics of State Resource Control." In *The State and Social Power in Global Environmental Politics*, ed. Ronnie Lipschutz and Ken Conca. New York: Columbia University Press.
- Ray, Isha, and Jeffrey Williams. 1999. "Evaluation of Price Policy in the Presence of Water Theft." *American Journal of Agricultural Economics* 81:928–41.
- . 2002. "Locational Asymmetry and the Potential for Cooperation on a Canal." *Journal of Development Economics* 67:129–55.
- Runge, Carlos F. 1984. "Institutions and the Free Rider: The Assurance Problem in Collective Action." *Journal of Politics* 46:154–75.
- Scott, James. 1976. *The Moral Economy of the Peasant*. New Haven, CT: Yale University Press.
- Seabright, Paul. 1993. "Managing Local Commons: Theoretical Issues in Incentive Design." *Journal of Economic Perspectives* 7:113–34.
- Sethi, Rajiv, and E. Somanathan. 1996. "The Evolution of Social Norms in Common Property Use." *American Economic Review* 86:766–88.
- . 2006. "A Simple Model of Collective Action." *Economic Development and Cultural Change*, in this issue.
- Wade, Robert. 1987. "The Management of Common Property Resources: Finding a Cooperative Solution." *World Bank Research Observer* 2:219–34.
- . 1988. *Village Republics*. Cambridge: Cambridge University Press.