Title
Indigenous Algorithms, Organizations, and Rationality

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Introduction

Decision-making is central to our understanding of human adaptation; the role of conscious thought and the use of maximizing algorithms in behavior is a crucial topic for understanding decision-making. The conflict between viewing people as what Michael Moerman (1968) characterized as “cultural dopes” versus “cultural sharpies” has been central to social theory and policy debate since the 19th century. Experience has shown that it is probably unresolvable as long as the primary focus is on *how* people think as opposed to *what* they think, in the sense of their interior mental processes as opposed to the knowledge, rules, and algorithms that they apply. If we find algorithms for rigorous, instrumentally rational, efficient, and efficacious decision-making in a community, it makes sense to assume that they are used for such decision-making. If we do not find them, the case is less clear.

The analysis of decision-making is closely connected to the problem of the relationship between individual and collective action. Portraits of people as cultural dopes are associated with theories that postulate cultural or social determinism: that the societal pattern is what is “real” and it impresses itself on or is manifested through individual action. French and German positivistic sociology—Durkheim and Weber—provide the theoretical prototypes. Portraits of individuals as cultural sharpies are associated with theories that see societal patterns as emergent from individual decisions and actions. Adam Smith’s analysis of the way the societal division of labor emerges from individual considerations of the advantages of specialization and his related conception of the mutual adjustment of supply and demand through market pricing are the theoretical prototypes, and still set the standard for theories in which the transformation from individual decisions to emergent patterns of collective action are not only conceptually clear but mathematically calculable.

In the last thirty years, the view of people other than ourselves as cultural sharpies has been strongly supported by ethnological and policy-related interest in “folk models” (Holy and Stuchlik, 1981), “local knowledge” (Timberlake, 1985), “time and place” knowledge (Ostrom, Shroeder, and Wynne, 1993: 49), “indigenous knowledge systems” (Warren, Slikkerveer, and Brokensha, 1995), “cultural idea systems” (Leaf, 1972; 1998), and local organization and knowledge (Kottak, 1991; 1995), among many others. It now appears to be well documented that local knowledge is often as good as, or better than, supposedly “scientific” knowledge that is not reflective of local experience. Local knowledge often provides better management of crops, more appropriate methods for damming local streams, more efficient types of pest control, better ways to build secure housing, locally available remedies for illness and injury, more viable systems of financing, and more effective managerial relationships. Moreover, at least in ethnological and development circles, it is also now well-accepted that local people make decisions that can be represented as rational or strategic with optimizing algorithms we impute, of various sorts. Earlier analyses of this sort involved game theory (Barth, 1959), binary decision trees and basic microeconomics (Tax, 1953; Schultz, 1964; Barlett, 1980). More recently, very high levels of predictive accuracy have been attained by multi-agent models that assume that the people whose behavior is being modeled act to optimize crop production as a function of pest control and irrigation efficiency (Lansing and Kremer, 1993) or that they locate their living areas so as to optimize maximize access to resources (Axtell et al., 1999), among others. But there still seems to be an unquestioned assumption that we (the bearers of Western science) are the only people who actually have really rational, “scientific,” allocative or maximizing decision algorithms *per se*, like game theory, binary decision theory, the theory of the firm, or linear programming. This assumption is false. Such indigenous algorithms exist and are the empirical key to understanding how efficient collective action emerges from instrumentally rational individual calculation and decision-making.

I have previously described an indigenous South Asian farm planning model and shown that it is functionally comparable to Western linear programming models and far superior to the micro-economically justified budget model that has long been used by Indian economists to analyze Indian farm economics (Leaf, 1998; 2000). So far this has had no impact on economics—doubtless largely because few economists read beyond their disciplinary borders and many appear to be disinclined to question their methodological and metatheoretical assumptions in the ways that ethnological results suggest they should, but perhaps also because this particular body of economic literature is not where the theoretical action is. So I will try again, with further examples and from a different perspective. Rather than comparing the indigenous algorithm to Indian farm budget studies I will relate it to two bodies of economic studies that are widely considered to be at the heart of modern theoretical developments, and which are therefore much more directly in competition with ethnological theory as paradigms of social analysis generally. These are arguments for bounded rationality and a subset of arguments in experimental economics. In doing so, I will focus much more than I have previously on the way the indigenous models vary to meet particular situations while still retaining their general formal characteristics. This will provide a clearer sense of exactly what these formal characteristics are, and thereby more clearly make the case for the models’ conceptual power.

Economists commonly claim or imply that their theoretical models of economic decision-making capture the nature of rational decision-making generally. This claim of universality is closely tied to the further claim that basic assumptions of such models are necessary postulates or first principles that do not depend on empirical proof. Logically, there is no empirical way to reject such an argument. If someone says they will ignore evidence, there can in principle be no evidence that can compel them to do otherwise. Yet the models are not on that account regarded by their proponents as devoid of empirical implications. They are the evident basis of a wide range of attributions of sub-optimal or irrational decision-making to a wide array of “others” under various rubrics and with various justifications. Myrdal’s characterization of “traditional” thinking, which he equated with Hinduism, as against modern and scientific thinking in *Asian Drama* (1968) is a particularly well known case in point, but there are many others. The notion of surplus labor in agriculture that began with Ricardo and was reinvigorated in the context of arguments for economic dualism by W. A. Lewis rests on the same foundation, as does the closely allied series of arguments for an economic “take-off” to self-sustaining growth that began with W. W. Rostow (1960). Both were foundational for development theory through the

1980s and still linger. This is the entrenched view that has led to a long and costly series of international development failures (Chambers, 1983; Cernea, 1991) and that the arguments of Timberlake, Ostrom et al., Holy and Stuchlik, and Warren et al., among many others, have been intended to overturn. Cernea and Kassam's *Researching the Culture in Agriculture: Social Research for International Development* (2006), documents the way "modernization theory" was associated with the same view of recipients of agricultural assistance as passive, inefficient, and non-innovative in the early history of the Consultative Group on International Agricultural Research (CGIAR), and the long and only partly successful struggle of anthropologists and other social scientists in the organization to overcome it.

It might seem that proponents of bounded rationality and experimental economics would be on the side of those who urge the importance of indigenous knowledge systems. They, too, seem to reject the presumption that market rationality is rationality in general, rather than just one kind of rationality. They, too, argue for more empirical attention to what people actually do and know; and they, too, argue for more interdisciplinary cooperation. But it is not a simple argument and the points of agreement are largely verbal. Disagreements are more substantive and important, although they differ in each case.

Experimentalist and bounded rationality arguments have four important implications in common with each other but in contrast to the arguments for recognition of the power and efficacy of indigenous knowledge. First, they do not recognize culture as part of context or make provision for eliciting or describing it. They provide no conceptual foothold for ethnology, and a logical way to be responsive to it. Second, they both assume, although in different ways, that only their own decision algorithms define real rationality or optimization. Third, their models are wholly individualistic; they reject or obscure the possibility that rationality might in fact be socially situated and constituted. And fourth, they implicitly deny that there can be indigenous algorithms for decision-making that embody very different assumptions and yet are every bit as efficient and efficacious as individualistic economic algorithms. I demonstrate here that all four of these assumptions are empirically false. Culture does provide the conceptual context of these decisions, and can be clearly and unambiguously elicited. There are well-formed indigenous (cultural) algorithms for rational decision-making. They are not based on an idea of selfish individuals in isolation but on the recognition that individuals exist in culturally constituted organizations; and they are at least as efficient and efficacious as the economic algorithms.

Decision Models and Experimental Economics

In 1998, Avner Greif described two "lines" of experimentalist studies under the common head of "Historical and Comparative Institutional Analysis," or HCIA. The first line "considers the impact of the internalization of traits through evolutionary processes and learning on the set of relevant rules," and "utilizes evolutionary game theory and learning models to study the process through which decision makers with particular traits ... emerge and the constraints on behavior that their interactions entail" (1998: 81). This line is commonly called evolutionary economics. The most important representative for our purposes is a series of studies by Joseph Henrich and others (Henrich, Boyd, Bowles, Cameron, Fehr, Gintis, and McElrath, 2001), which attempt to apply the methods cross-culturally and, thereby, move directly into the concerns of ethnology. Moreover, Henrich and Boyd are anthropologists. The second line, which includes Greif's own work, "considers the impact of strategic interactions and exogenous and endogenous cultural features ... on the set of relevant rules. It ... concentrates on the origin and implications of (nontechnologically determined) 'organizations,' and the constraints implied by beliefs prominent in a society on and off the path of play" (1998: 81, parentheses and quotes in original). Greif's account seems to make the two approaches complementary. From an ethnological perspective, however, they lead to entirely different kinds of theory and method.

The experiments I am primarily concerned with are those that consider the most rational, or "canonical," solution to be defined with the idea of a Nash equilibrium. My objection, however, is not to game theory *per se* as much as to oversimplified and confused social theory that its proponents use to assign the Nash equilibrium this significance.

A solution is at the Nash equilibrium when neither player would alter their decision given their knowledge of the moves of the other players, provided that no player expects the other to cooperate. Most games of this sort are set up in such a way that if the players would cooperate, they could all do better. The researchers then measure the difference between the Nash solution and the solutions actually chosen. The general result is that most experimental subjects do not behave as the models of rationality predict, and theoretical discussion usually focuses on how to account for the differences. Greif's two "lines" represent the two different types of answers.

Henrich and his colleagues saw these "large, consistent deviations from the predictions of the textbook representation of Homo economicus" as suggesting a need for a major effort to see if the same results would be obtained cross-culturally (Henrich et al., 2001: 73). According to their account, their project design built on a 1996 experiment by Henrich, working among the Machiguenga. Henrich was then a graduate student in anthropology at UCLA. With the anthropologists in the group and others, the team conducted three well-established types of experiments in 15 diverse small-scale, non-Euro-American, societies and compared the responses. The experiments were the ultimatum game, the public good game, and the dictator game. In each case, the rules are stated so as to preclude cooperation so that the optimal solution ought to be the Nash solution. The ultimatum game, for example, has one player get a sum of money (or something). He must then offer a portion of this to the other on a take-it-or-leave-it basis, but if the offer is refused neither player gets anything. The Nash solution is that the giving player offers just one unit and the receiver takes it. If they could negotiate, the receiver would probably want more and the giver would have to give it. In the public good game each player is given a sum of something like money, which they can decide to keep or put into a common pool. The pool is added to by the investigator and divided among the contributors. The Nash equilibrium solution is for no one to contribute anything to the pool; the actually most beneficial solution would be for all to give everything.

They summarize their results:

> First, the canonical model is not supported in any society studied. Second, there is considerably more behavioral variability across groups than had been found in previous cross-cultural research, and the canonical model fails in a wider variety of ways than in previous experiments. Third, group level differences in economic organization and the degree of market integration explain a substantial portion of the behavioral variation across societies: the higher the degree of market integration and the higher the payoffs to cooperation, the greater the level of cooperation in the experimental games. Fourth, individual level economic and demographic variables do not explain behavior either within or across groups. Fifth, behavior in the experiments is generally consistent with economic patterns of everyday life in these societies. (Henrich et al. 2001: 73-74)

In short, none of the predictions was very good but the more the experimental subjects had economic institutions that were similar to those of the investigators, the better the investigators' models predicted their behavior. Since by definition these deviations from rationality (or utility-maximization) could not be explained by rationality, the authors argued that they had to be explained by evolution—inherited predilections. Under competitive pressure leaders were selected who had (heritable) attitudes such as altruism that overcame the selfishness and radical individualism that the Nash model of Homo economicus required, and thereby became the foci or creators of social groups that do in fact cooperate and arrive at more beneficial outcomes. Alternatively, some of these evolutionists argue that shared genetics, relatedness, leads people to spread their self-identification or self-interest to the related group. One example is Gintis's notion of "strong reciprocity," which he says "probably has a significant genetic component" (Gintis 2000: 2).

The Henrich et al. studies were subsequently described in more detail as *Foundations of Human Sociality: Economic Experiments and Ethnographic Evidence from Fifteen Small-Scale Societies* (Henrich et al. 2004). This was critically reviewed from an anthropological perspective by Michael Chibnick (2005). Chibnick focuses primarily the authors' leap to evolutionary psychology to explain their results and the inconsistencies and self-contradictions that this generates. Although he notes exceptions, his most general criticism is that this amounts to explaining variable effects (the different departures from the canonical norm in different communities) by constant causes (universal human genetics). I agree, but there is more.

First, which Chibnick recognizes but does not state directly enough, is that "traits" as distinct genetically determined attitudes or conceptual predilections inherent in individuals or groups of related individuals have never been shown to exist, either in practice or in principle. Of course one can name qualities like altruism or selfishness just as one can name any of the other traditionally recognized virtues and vices, but they cannot be delineated in such a way that one can say that they are objectively either present to a measurable degree or absent, like blue eyes or wrinkled peas, and barring that there is no possible way to find a genetic basis for them. Contrariwise, to the extent that supposed traits can be identified concretely, like the ability to understand representative government, it is quite clear that they do not fall into Mendelian patterns. Beginning with Boas's demonstration in the 1880s that even measurable physical characteristics of supposed European racial stocks were subject to cultural modification, it is difficult to think of an idea that has been more thoroughly examined and more firmly rejected (Boas, 1940; Montagu 1946). Modern bio-chemical understandings of what genes are and how far what they control is from the content of conscious thought only reinforce the old conclusions. With the exception of a few disease or hormonal syndromes, we cannot find behaviors and attitudes that are inherited in a Mendelian way, or that can be associated with any specific gene or sequence of genes. Absent a genetic basis, talking about inheritance is talking nonsense.

The alternative to genes is culture, which is what the other line of experimentalist economics argues for. This is the "new institutional economics" (North, 1990; Greif, 1998). It agrees with the older institutional economics that, in Polanyi's phrase, "the economy is embedded in society" but differs in how that society and culture are to be characterized, and game theory is crucial to this difference. Essentially, the new institutional economists characterize institutions as games writ large, and society as either equivalent to an institution in this sense or as composed of institutions. As the players in a game maintain the rules and the game by pursuing their own self interest as defined within the game, without necessarily knowing how the game as a whole is constructed, so the members of society maintain their social equilibrium by following only their own self-interest:

> HCIA conceptualizes institutions as the non-technologically determined constraints that influence social interactions and provide incentives to maintain regularities of behavior. It considers institutions that are outcomes emerging endogenously and that are self-enforcing in the sense that they do not rely on external enforcement. HCIA thus considers the relevant rules of the game that actually constrain behavior in a society (as distinct from the technologically feasible rules) to be a self-enforcing outcome of forces, such as strategic interactions, evolutionary processes, and limits on cognition. These rules, in constituting part of a society's institutions, are complemented by self-enforcing constraints generated through interaction within these rules. An essence of HCIA is thus the examination of the factors determining the relevant rules of the game, the forces that make these rules self-enforcing, and the self-enforcing constraints on behavior that emerge within these rules. (1998: 80)

Douglass North, similarly, says that institutions are "the rules of the game in a society, or, more formally, are the humanly devised constraints that shape human interaction" (North, 1990: 3).

There are three main problems with this. The first is their idea of an institution itself. It does not correspond to any one ethnological reality. Rather, it incorporates parts of several which are quite different from one another, although interrelated. This applies to the old institutionalists (Lawson, 2005) as well as the new ones, with the major exception of Walter Neale (Leaf and Rosen, 2005). Second, these definitions are far too obscure to be operationalized without circularity. Is it anything that is self-enforcing? Is it any "outcome emerging endogenously?"

What is excluded from “the non-technologically determined constraints that influence social interactions” or “the rules of the game” or “factors determining the relevant rules”? And third, North and Greif envision a far-too-simple relationship between institutions as they think of them and “society” as a presumed singularity that encompasses them—as though there were just one game in any given society and as though its rules formed just one set or system. Positivistic and neo-positivistic social theorists from Weber and Durkheim to Geertz have tried to find such normative unities for over a century, and none has succeeded. The reason is that the wholes are not there—although a *sense* of such wholes does exist, which we can explain. Neither can anyone find any one system of rules. Of course one can always make some up and impute them, but for any one imputation it is always possible to find equal justification for others.

In fact, however, the focus of this line of analysis is not on anything whatever that would conform to this definition of an institution, but rather on organizations construed as such institutions. For example, Greif, Milgrom, and Weingast (1994) use a series of experiments to test different hypotheses regarding the emergence of merchant guilds in late medieval Genoa. They take the Nash equilibrium solution to the problem of trading as their conceptual baseline, under which guilds cannot form, and set up experiments to test alternative hypotheses about the ways that individuals might have been induced or constrained, mainly by the ruler, to move from it to the more cooperative and beneficial situation that the guilds actually embodied. How can the rules be modified to go from a game in which the stable solution is a Nash equilibrium to a game in which the stable solution is a coalition like the guilds? They phrase their conclusion in terms of “rules” rather than organizational forms, but because what they are calling an institution happens in fact to have also been an organization, their analysis applies only to that organization and its setting. Greif implicitly recognizes this in the definition of his “line” of research quoted at the beginning of this section, which speaks of “(non-technologically determined) ‘organizations’”—with ‘organizations’ in quotes. Why the quotes? He does not say, but the apparent intent is to suggest that while we see it as an organization, he sees that it is actually an institution, or that the difference is only verbal.

The three terms “institutions,” “organizations,” and “rules” all have wide ranges of accepted uses, but there is a core sense to each—and a core phenomenon associated with each—that is recognizably distinct. It is, for example, habitual to speak of society as containing or based upon just one or a few institutions as Greif and North do, but it is very difficult to think of it as containing or based upon just one or a few organizations. If we ask people to adduce examples of organizations they might offer families, shops, and law offices. An example of an institution might be kinship in general, but to say a specific person’s family is an institution is to say that there is something extraordinary about it. We can say “kinship is an institution of American society” and also, in the same description, “the family is an institution of American society” without being troubled by the relationship between kinship and family. But if we say “the family is an organization” and “kinship is an organization” we immediately recognize that the two statements are not equivalents and that the latter, at least, is troubling. We are not sure it is true, or we are not sure what it means. The economy and the legal system will be readily described as institutions, but not as organizations. We commonly speak of institutions as containing organizations, but not of organizations as containing institutions. Similarly, most people would say all organizations are in some sense "instituted," but no one would say that all things instituted—or all institutions—were organizations. We expect that in an organization every position will have one and only one relationship with every other position. We have no such expectations about positions we may deem to be in the same institution. And rules, of course, are different yet again. Organizations have rules, but rules are not organizations. "All things not permitted are prohibited" (I translate from the French), when on a sign near the Louvre is certainly a rule and might arguably be described as an institution, but is, again, still not an organization. Just plain "Keep Off the Grass," when on a sign on a patch of grass, is only a rule and neither an institution nor an organization.

These contrasting usages reflect contrasting social realities, but the difference is not what one might at first think. Particularly, while the idea of an institution seems to be an idea of something larger than an organization, that might encompass many organizations of related kinds, and perhaps even also related "rules of the game," there is no such observable encompassing entity. The idea is all there is, nothing more. This does not mean that as an idea it is unreal or unimportant. It is just important in a different way: not because what it seems to name actually exists, but because it does not, and not because it is operationalizable but because it is not. The idea of an institution in this sense is a social-psychological projection. It is part of the definition of the situation as a framework for social interaction, the sense originally articulated by W. I. Thomas. The way it arises appears to be a human cultural universal.

When any organization is created using cultural ideas like those that define the farm family as a managerial unit, this creative process necessarily involves creating a sense of *type* or *kind* of thing that the specific organization in question is one of, or represents. It is the background that the organization, as foreground, necessarily projects. As such, it is not and cannot be anything anyone is actually "in" or that actually does or produces anything. *Eo ipso*, it cannot be the kind of thing that can be represented by the interactions of a game. "Society," as a supposed totality, is another version of the same projection.

Organizations, by contrast, are enforced realities for their members. Members of an organization share what George Herbert Mead described as a "mutual adjustment of behavior," meaning that what one of them does is dependent on what others are expected to do (Mead, 1934: 78ff). "Expected" is a central concept. The membership is not merely brought together by events in the past; it is prospective. It has a future reference. Such a future reference is a purpose, in the general sense that the purpose of a family organization is to reproduce itself, the purpose of a military organization is to fight, the purpose of an educational organization is to educate, and so on.

Every society provides for many kinds of organizations, often with different and even opposed "rules of the game" associated with their different purposes. Individuals belong to several or many of these organizations and they are aware of doing so (Bailey, 1960; Leaf, 1972; 1998). Moreover, it is very doubtful that *any* rules are societal *per se*—in that they apply to all of those in the entire society in all organizational contexts or without regard to organizational context. Decision-making in organizations is shaped by specific conceptual algorithms that are integral to those organizations. These algorithms may be simple, informal, and vague or they may be complex, formal, and precise. The literature on folk-models generally describes the former. Those described here are complex, formal, and precise.

By confounding institutions, rules, and organizations while not providing a clear sense of any one of them, Greif and North create the illusion that their analysis of one organization within a society is the same as an analysis of the society—as though this one pattern of relations is the only pattern of relations—but in fact this is never the case.

This demonstration will have at least two fatal consequences for the experimentalist use of game theory, particularly as in evolutionary economics. First, if there are formal indigenous models for decision-making comparable to but different from the game theory models that the experiments are based on, then experimenters are not investigating inherent psychological processes but rather what happens when they try to impose alien decision algorithms over one or more indigenous algorithms that they are ignoring. Their results cannot mean what they think they mean, and the deviation of actual behavior from the Nash norm in any given experiment is a measurement with no clear meaning. Second, if individuals belong to multiple organizations and are aware of balancing their commitments to one against their commitments to another, then the experimental interpretations based on the assumption that the only relevant ideas and actions are those which are entirely endogenous to the experiment are fundamentally misconceived.

Bounded Rationality

The idea of "bounded rationality" began with Herbert Simon (1957) and now has a substantial body of proponents, apparently more in Europe. John Conlisk provided an important review of mainly economic and psychological studies in 1996. Gerd Gigerenzer with the Max Plank Institute and Reinhard Selten of the University of Bonn provided a coordinated set of essays attempting to bring together developments in a still wider range of disciplines in 2001. Briefly, bounded rationality is imperfect rationality. Simon assumes that classical microeconomic optimization, which is "unbounded", describes rationality, and argues that people who make actual economic decisions do not meet all the assumptions that the model requires. Mainly, they lack perfect information and exhibit various kinds of biases in processing the imperfect information that they have. They do not optimize; they merely "satisfice."

A few anthropologists have been concerned. Linda Garro, for example, discusses Simon's formulation among others in a review of descriptive decision theories that she tests against the problem of describing indigenous systems of medical diagnosis in a Mexican village. As she sees it, Simon defines bounded rationality by applying a "simplified model of the situation" but fails to recognize that such models are "culturally grounded" (1998:324). Proponents of bounded rationality have not been responsive to this type of criticism; they seem not to recognize what it means. Perhaps I can clarify.

In a 1986 article, Simon paid more attention than he had in 1957 to the idea of a "frame" around such bounded rationality. He recognized that it "must be comprehensive enough to encompass goals, the definition of the situation, and computational resources" (1986: 210). Moreover, he recognized that these components had to be determined empirically. Although it might seem logical to connect Simon's idea of a "frame" to cultural ideas or to organizations, this is not what he meant. Instead, he contrasted the empiricism of "cognitive psychology" with the postulational methods of "neo-classical theory" as represented largely Gary Becker, and further associated the more empirical methods with what he called a "process" conception of rationality and the neo-classical methods with a "substantive" conception. The distinction was evidently intended as a dichotomy. Rationality must be one or the other. He then simply equated "process" with bounded rationality and "substantive" with the usual economic stipulation that the reasoning had to maximize a utility function. In short, get the argument out in the open and what Simon is arguing is that rationality is either unbounded or bounded *by definition*.

So if "empiricism" does not define the "frame" of economic decision-making, where does it come into play? The answer is that it does so only for the very circumscribed problem of saying what the "bounds" of bounded rationality are on the assumption that the frame is non-problematic, and the sense that he gives to this only psychological:

> First, I would recommend that we stop debating whether a theory of substantive rationality and the assumption of utility maximization provide a sufficient base for explaining and predicting economic behavior. The evidence is overwhelming that they do not.
> We already have in psychology a substantial body of empirically tested theory about the processes people actually use to make boundedly rational, or "reasonable," decisions. This body of theory asserts that the processes are sensitive to the complexity of decision-making contexts and to learning processes as well.
> The application of this procedural theory of rationality to economics requires extensive empirical research, much of it at micro-micro levels, to determine specifically how process is molded to context in actual economic environments and the consequences of this interaction for the economic outcomes of these processes. Economics without psychological and sociological research... is a one-bladed scissors. (1986: S224)

Conlisk's review details the contextual studies Simon refers to. Some have been conducted by psychologists, some by economists. None are anthropological or sociological. The general pattern is to predict some "objectively" optimal pattern of choices or behaviors on the basis of an "unbounded" economic algorithm and then compare the prediction either to some range of economic data from standard sources or to a psychological experiment, usually involving students on a university campus. Deviations from the prediction are more common than not, and are usually attributed to psychological limitations or biases such as misunderstanding statistical data, mistaking random data for patterned data and vice versa, failure to appreciate the law of large numbers, mistaking statistical probabilities, or failures to recognize what is relevant and what is not (Conlisk, 1996:670).

All of these studies agree with Simon in assuming (not proving) that economic rationality is rationality in general. No one considers whether there might be other, non-economic, types of rationality or that economic rationality might be either culturally constituted or organizationally contextualized. They seek a culture-free or cross-culturally universal model of rationality and think they can get it. Their only question is the best way to characterize it—substantive or process, bounded or unbounded. In fact, however, the topic is not anywhere near this simple or this narrow. Simon's substantive rationality is just one instance of rationality as rule-following. His bounded rationality *might be* an instance of a rationality of purpose, but neither is the only example of each.

Gigerenzer and Selten and their contributors (2001) do not reject Simon's general argument explicitly, but they do shift away from it implicitly. Peter M. Todd, for example, asks "what is it that bounds our rationality?" and argues that "the human mind" "makes many decisions by drawing on an adaptive toolbox of simple heuristics, not because it is forced to by cognitive constraints, but rather because these fast and information-frugal heuristics are well matched to the challenges of the (past) environment" (2001: 51-52). But Todd still avoids saying that these heuristics actually optimize. He also still construes them to be a generalized aspect of human thought, as though when he and his colleagues get the list complete it will be universal. While they recognize culture as relevant to context and conceptualization in a general way, they do not set themselves the problem of finding and describing cultural ideas and algorithms and their uses *per se.*

Thus notwithstanding their differences from one another, experimental economics and bounded rationality share important assumptions and pose common problems from the point of view of ethnology. They insist that rationality is exemplified by conformance to models they invent and that such models are universally applicable. They firmly ignore indigenous cultural idea-systems and organizations, and they leap from analysis of one or a few types of decisions or decision processes to context-independent generalizations about the human mind or society without recognizing culture and with far-too-simple notions of what society as whole (or the human mind) actually has to involve. In the process, they also bypass the possibility that behaviors might be rational or optimal with respect to means-ends relationships of a less global sort, and based on algorithms that are culturally defined and organizationally situated.

Gujarat

I will describe three indigenous optimizing algorithms from one irrigation command area in Gujarat state, India. The underlying formal principles of the algorithms are universal in South Asian family farm planning, and I would not be surprised if they turn out to be coextensive with intensive peasant agriculture. But in detail each algorithm is specific only to the one farm where I elicited it.

My work in Gujarat was as the Senior Social Scientist on the Irrigation and Water Management project, from July of 1987 to August 1989. The project was sponsored by the USAID and the Government of India. Its purpose was to improve irrigation efficiency by improving irrigation management. This meant two things. First, it was to improve the skills and efficiency of the state irrigation departments, mainly by upgrading the curricula of their departmental training facilities. Second, it was also to improve the organization and efficiency of the irrigation "consumers," the farmers in the irrigation command areas. As part of this program, each training institute maintained an "action research" area in one of the irrigation projects of the state, in which we were to try innovations on an experimental basis.

In Gujarat, my assignment was to support action research and the social science aspects of the program of the state's Water and Land Management Institute (WALMI). The action research area for the WALMI consisted initially of Anklav minor in the Mahi Right Bank irrigation project. The Mahi Right Bank project covers seven Taluks (revenue subdivisions) of Kaira district on the north bank of the Mahi River just above where it flows into the head of the Gulf of Cambay. There have been several studies of this area, including a substantial resource analysis of the Mahi Right Bank Project itself by the Water Technology Centre of the Indian Agricultural Research Institute (Water Technology Centre, 1983).

This was one of the best developed irrigated areas in the state. It is all on flat, rich, alluvial land. Irrigation and ground water are available year round, and rainfall is good. The air is always hot or warm and moist, and the clay and clay-loam soils are good for a wide variety of crops. It is a major producing area for bananas, sugar cane, rice, cotton, and a kind of tobacco used for making *bidis*, which are small cigarettes made of rolled leaves tied with thread and sold very cheaply. There was considerable tropical fruit production, and along the main road there were many small areas growing flowers for urban markets. The command area is well provided with power and road infrastructure. Most of the farmers live in villages, which are large and orderly. Most village houses are substantial brick and concrete structures, commonly of two stories and including concrete floors. All the villages I saw were electrified and many have a minimal piped water system to public taps, although only for an hour or so a day. Some farmers live directly on their farms. Their houses are commonly simpler than the houses in the villages, but still are usually of brick with tiled roofs, although often with dirt floors. There were, in short, many signs of prosperity. Beneath the surface, however, things were not what they seemed.

The project had been a major World Bank investment, but was not operating anywhere close to the design specifications. In the action research area, water had never reached more than half-way to the tail of the minor, far fewer farmers than projected actually applied for the irrigation water, they did not follow the recommendations of the canal staff or agriculture department in their cropping and water use, and revenue collections were not adequate to pay for ongoing maintenance. This was typical for the command.

Land ownership is highly stratified. According to the Water Technology Centre study, about 4%

of the total number of holdings are larger than 6 hectares and occupy 22% of the total land; 56% of the holdings are less than one hectare and these occupy 19% of the land.

The action research program had been running for almost two years before I arrived. During that time the chief advisor for Gujarat (whom I replaced) was an Indian-trained civil engineer who had previously worked for the World Bank. He and the irrigation engineers in the WALMI were firmly of the opinion that the best way to improve irrigation efficiency was to support the "progressive" farmers who would be amenable to new, more productive, and water-saving, techniques. There was no point wasting time on the "traditional" farmers who would not. "Progressive farmers" in this case meant a small group of the larger landholders who grew primarily commercial crops, such as tobacco, cotton, and bananas. Traditional farmers meant the much larger group with smaller holdings, growing "subsistence" crops. The engineers' stereotypes had a long history in South Asia and reflected the widespread view of "traditional" or "subsistence" farmers as cultural dopes, which had been reinforced by over a century of studies of Indian farm economics. Like Indian farm economists, and also the modern experimentalists,. the engineers were imposing their own "economic" algorithms, finding they did not predict farmers' behavior, and concluding that the farmers' behavior made no rational sense at all.

On the basis of my previous experience and analysis, even before joining the project it seemed far more likely that what the economists and engineers thought of as rational behavior was not actually rational for the small farmers. There were several reasons. First, I knew that the economic studies on which the engineers relied had consistently misconceived the farmers' purposes. Farmers were not after profit as the economists had conceived it. Economists considered profit the difference between cost and income when all aspects of their operations were evaluated at their "economic" prices. Farmers were trying to expand the size and productivity of their holdings in material terms, and "holdings" in this sense included their households and household members. This did involve monetary calculations, but not on the bases that the economists imputed. The parameters of the economic studies made no sense in village terms. The items of account for the economists did not reflect the way goods and services were commoditized in the villages and the items of account in the villages could not be translated into those in the economic surveys (Leaf 1972: 122-128; 1984: 110-147). Even earlier, Walter Neale had shown the same thing in a different way by tracking down the survey forms the economic studies were supposedly based on (Neale, 1958). In actuality, most of the data had been added after the field interviews had been completed, at higher administrative levels. Theodore Schultz (1964) was much closer the truth in describing Indian farmers as maximizing several distinct "income streams"—rather than the single profit function. Indian agronomists commonly describe farm output as "four f's:" food, fodder, fuel, and funds, but this is not reflected in the Indian economic studies.

By the time the project activities brought me to Gujarat, my reasons were still stronger. I had by then discovered the farm budget planning algorithm and could see that it embodied precisely the kind of balancing problem that Schultz had recognized and that I had previously described in more general terms (Leaf 1972, 1984). Moreover, it did so with a level of precision beyond anything that I had imagined or that any conventional economic model embodied. After initially finding this algorithm in Tamil Nadu and then verifying that variants of it existed in Punjab, Maharashtra, and Rajasthan, I was confident that I would also find it in Gujarat. My aim, therefore, was to elicit some examples in the action research area, put them on the WALMI computers, and let the engineers see for themselves what the farmers goals and constraints were and whether what they were urging the farmers to do really was better in that context. This was not easy, however. I do not speak Gujarati and the WALMI staff were politely but firmly not interested in talking to farmers they thought of as backward, uninformed, and unimportant. In the first year, all I could get was a few interviews with “progressive” farmers in the project offices—not even on their farms where I could check what they were saying against what I could observe.

After several unsuccessful efforts, I finally got B. C. Barik, a young Marxist sociologist who had just joined the WALMI, to locate five farmers who could properly be described as economically “poor.” I interviewed them in November of 1988, with his help and that of K. M. Shah. Shah was a soil scientist with the WALMI who had considerable knowledge of agronomy. The first interview was conducted with the director and some other high WALMI officials present, and in consequence was rushed and has some unrectifiable gaps. The rest were conducted by Barik, Shah and me alone. Each was done at the farmer’s own house, in three different villages, and took up to four hours. I will describe two of them here. The third farm model I describe here is from one of the "progressive" farmers interviewed earlier. I include it to show the conceptual limits of the farm budget models and thereby to underline the crucial theoretical point that I am not offering this as yet another supposed universal. No real-world algorithm is. They must be situational and contextual because rational decision-making (optimizing) itself is situational and contextual.

The general algorithm

The farm budget algorithm is for planning land use in family farms. Indian households have multiple organizations. In one aspect, they are kinship organizations. In another they are economic organizations, and in another they are managerial. These are not all; they are only the most common (cf. Leaf 1984, 1998). Each of these organizations is built up with a distinct set of indigenous ideas. Each involves different conceptions of the members and their relations to each other, different terminologies for titles and duties, different goals, and different ways of meeting these goals. The algorithm that concerns us here is for planning as a management organization, and more specifically as a farm management organization. For reasons that we will see, this involves providing most of its material requirements directly out of its production rather than through market exchange. This does not imply an aversion to market exchange, as Marxists have often claimed. It simply recognizes that under many circumstances direct production and consumption is more efficient.

As noted, I first elicited the farm budget model in Tamil Nadu. Once I had done so, I could see that it also would explain the level of managerial precision I had previously encountered in Punjab. I checked back with some of the Punjab villagers and found that I could indeed elicit a version from them as well. Punjab and Tamil Nadu are at the north and south ends of the Indian sub-continent. I then began to look for it systematically in every state I was working with in between. It was always there.

The algorithm is readily represented by a computer spread-sheet. The rows represent the crops, grouped by the function they fulfill, which of the "four f's" they provide: food, fodder, and sometimes funds. (There are no separate fuel crops; anything not used for the other three is burned for cooking, and the ashes are used for cleaning and then recycled in compost). The columns are the household's constraints and resources: the food and fodder allowances, the people, the cattle, the yields, the costs, the areas planted and the amounts produced, and the surpluses or deficits at the end of the year in terms of the monetary value. Since most of the traditional crops produce both food and fodder they go in both the food and fodder rows, although of course with different yields for the different parts that are used for each purpose. A few are pure cash crops, producing neither food nor fodder. These have to go in a separate section. The matrix looks like a linear programming model and can be used like one for "what if" trials, but the mathematics required is only addition, subtraction, multiplication, and division.

A word about units of measurement. Indian farmers readily move back and forth between English, metric, and several indigenous systems, but a spreadsheet cannot. I mainly use metric measurements, but some exceptions are necessary to maintain fidelity. In Gujarat, the farmers most often used *guntas* for measuring land area. A gunta is an administrative unit designed to facilitate conversion from acres to hectares. Forty guntas are held to be equal to one acre and 100 guntas equal to one hectare. But this is rough, since one acre is not 40% of a hectare but 40.47%. Moreover, odd numbers of guntas were commonly rounded to the nearest half acre. For simplicity and consistency, I state the areas here as they were most often stated to me, which was in acres.

Another important area unit is the *bigha*. This is purely indigenous, and more widespread. Its absolute size varies by region. In Gujarat it is 0.24 hectare. It continues to be used because for many operations it is the area that can be worked in a day – not by coincidence but as an important part of the system of evolved planning and management conventions and as a means of avoiding ambiguity in agreeing on work contracts.

The battery life of my laptop and consideration for the time of the interviewees did not allow me to fully elicit each model from nothing each time I conducted an interview. So I adopted the practice of using a template. Each time I entered a new area, I would strip out the household-specific details from a model previously elicited in another place as my initial frame. As the elicitation proceeded, if new categories were needed or if previous categories turned out to be useless, I would relabel or rearrange the columns and rows. This allowed me to explore directly the extent to which these models were uniform or varied across India and within regions. For the interviews in Gujarat my initial template was based on the elicitation from the village in Punjab, mainly because I had great confidence in the men who had supplied the information and also because the farming was similarly intensive. This initial template is Table 1.

Table 1. Initial Eliciting Template for Gujarat Spreadsheet

[The article homepage has a downloadable file in Excel with calculation formuli for this spreadsheet]

(Note: Mahi Right Bank Project; Kharif is the autumn and winter season, Rabi the winter season)

Mahi Template- Kharif 1988-Rabi 1989 BlankGuj.wk1

Projection of Land Needed for Subsistence By M. J. Leaf

Crop	Grams Meal/ Man	No. Men	No. Wom.	No. Child	Yield Q/Acre	Price /Kg.	Meal Fed	Acre Req.	Acre Sown	Pcnt MNSA	Rs. +/-
Maize	250	0	0	0	8.-	2.5-	210	0.00	0.-	0.00	0.00
Wheat	250	0	0	0	17.5	1.75	520	0.00	10.-	0.00	0.00
Rice	175	0	0	0	30.-	1.7-	50	0.00	11.-	0.00	0.00
Mah	50	0	0	0	5.-	8.--	200	0.00	0.4	0.00	0.00
Mungi 60	50	0	0	0	5.-	8.--	60	0.00	0.4	0.00	0.00
Arhar	50	0	0	0	2.5	6.5-	20	0.00	0	0.00	0.00
Gram	50	0	0	0	8.-	8.--	20	0.00	0	0.00	0.00
Cabbage	150	0	0	0	28.-	0.75	105	0.00	1.-	0.00	0.00
Sarson	10	0	0	0	1.-	27.--	125	0.00	2	0.00	0.00
Sugar	15	0	0	0	350.-	0.32	910	0.00	2.4	0.00	0.00
Chilis	1	0	0	0	10.-	24.--	730	0.00	0.01	0.00	0.00
Total								0.00	27.21	0.00	0.00
Crop	Kg/day Buff	Ox	Fodder Buf.	Jers	Yield Kg/a	Rs/ Kg.					
Jowar	28	0	0	0	7225	0.5-	60	0.00	0	0.00	0.00
Bajra	10	0	0	0	8000	0.12	45	0.00	0	0.00	0.00
Tur	5	0	0	0	1750	0.6-	290	0.00	0	0.00	0.00
Rice S.	7	0	0	0	2000	0	30	0.00	0	0.00	0.00
Wheat S.	10	0	0	0	25920	0.17	90	0.00	0	0.00	0.00
Barshin	20	0	0	0	25920	0.17	60	0.00	0	0.00	0.00
Cane leaf	3.4	0	0	0	800	0	120	0.00	0	0.00	0.00
Cab. L.	2.4	0	0	0	7000	0	60	0.00	0	0.00	0.00
Feed	3	0	0	0	0	2.-	185	0.00	0	0.00	0.00
O/cake, kg	0.5	1	2		250	3.2	180	0.90	0	0.03	0.00
Total								0.90	0	0.03	0.00
Max NSA	28		Act. SA		34.6					Rs.	0.00

COMMERCIAL CROPS	Yield	Area	Price	Costs	Net
Petha	492	0	13	0	0
Cotton	1	0	700	0	0
Sunflower	2.4	0	681	0	0

Total land farmed 15 Net gain 0

Two wells, 20 bighas canal. Subsistence +Commercial 0.00

4.63 bigha calculated at 1 acre.

Gain/acre 0.00

Reading across the columns, Grams/Meal/Man is the allowance in grams for one meal for a man. The amount per man is the common standard. Allowances for women and children are proportional and vary from region to region. “No. men,” “Wom.,” and “Child” are the numbers of men, women and children in the household, provided that they are taking their food there. (This is not my stricture, it is part of the model. There were cases of people in households who were explicitly not counted because they supplied their foodstuffs independently.) Yield in kilograms per acre was usually standard for the local community, although sometimes farmers had their own estimates. When they did, I used them. Rs/kg is the expected price for the foodstuff (or fodder) in rupees per kilogram. This was standard for the locality, but varied over time. Meal Fed is the number of meals at which this foodstuff (or fodder) is served in the stated quantity. This was elicited for each family, although there was a great deal of regional uniformity within any given income level. Acres required is calculated by the spread sheet as the allowance per meal times the number of meals divided by the yield per acre. This was almost always checked with the farmer for confirmation. Acre Sown is the actual area sown, in acres. “Pcnt MNSA” is the calculated percentage of the total subsistence land (mean net sown area) that the crop would occupy if it were sown in the amount required. I included it as a measure of the relative pressure exerted on the total array by the need for that particular commodity. And Rs +/- is the calculated value in rupees of the surplus or deficit of what is produced on the farm compared to what is consumed. This, too, was usually checked with the farmers.

For cattle, the traditional unit for calculating feeding is the “pair” or “plow.” A pair is two plow oxen (*bos indica*) or one buffalo. Since the Punjab village no longer has plow oxen, the “pair” was replaced by one buffalo as the planning unit. Because of the importance of dairying, it made sense to retain this for the Mahi Right Bank. “Jer.” is a Jersey-Bos Indica hybrid, which eats about the same as a buffalo but has a longer lactation period.

Reading down the rows and focusing on the names not likely to be familiar, *mah* is lentil, which is both a major protein source for people and a very good fodder supplement. This is a winter (*rabi*) crop, sown and harvested on nearly the same schedule as wheat. *Mungi 60* is a local variety of mung bean, similar to lentils, which has the advantage of being able to grow on residual moisture after a summer rice crop and maturing sixty days after sowing. *Arhar*, also called *tur*, is pigeon pea. This is good as a dried legume or in the green pods as a fresh vegetable. The leaves are good fodder, and the long, fibrous stems are used for making basketry and wickerwork. It fixes nitrogen and is often interplanted with cotton. *Gram* is chickpea, also a major staple. Dried beans and green pods are eaten by people; stems and husks are a high-protein fodder. Cabbage was a new crop in the village in Punjab because of improved access to urban markets. It is a useful vegetable and the leaves and trimmings have become an important green fodder. As it turned out, it was not grown in this part of Gujarat.

Sarson is mustard or its relative, rape. Mustard is grown in winter and rape in summer. Both are used as a green vegetable and as an oilseed. They are grown either in solid fields or in rows around plots of wheat, whose growing season is nearly the same.

Sugar is sugar cane, and is either a fourteen month crop or a ratoon crop, being planted, cut, and allowed to regrow for up to five years. It is encouraged by government agencies to earn foreign exchange, but because it spends so much time in the field the income from cane is often less than that from two or three seemingly lower valued "subsistence" crops. The leaves and tops are used for fodder but are not particularly good.

Among the fodders, *jowar* is sorghum, stalks and leaves. It is usually fed dry because it is toxic to cattle for the first 45 days of its approximately 120-135 day growing cycle. It is sown in the beginning of the rainy season. *Bajra* is pearl millet (*Pennesitum typhoides*), usually sown and harvested before jowar. Bajra is usually cut and fed green although it can also be dried and stored. It is generally considered inferior to jowar. "Wheat S" and "Rice S" are wheat and rice straw. To be used, they are chopped and usually mixed with dried legume residues and green matter. *Barshin* is Persian clover, a legume always cut fresh and chopped to be mixed with dry matter for immediate use. It is very productive per unit of land, high in protein, and fixes nitrogen.

"Feed" is commercial cattle feed, usually from dairy cooperatives. "O/cake" is oil cake, the residue from crushing mustard, cotton seed, and other oil-seeds. Both are important for good milk production. When farmers take mustard seed to pressers to sell, the arrangement usually includes a return of oil seed cake equivalent to the oil seeds brought.

Of the commercial crops, *petha* is white pumpkin or ash gourd. It is used to make a type of Indian sweet. The residues have no value as a fodder. Dried stems can are used as cooking fuel. Sunflower is an oil seed. Because it uses little water it is encouraged by the Gujarat irrigation department, but it produces no fodder and because of this is usually just barely profitable. The only reason farmers can make some money from it is that the stems are a good cooking fuel, so non-farmers will clear the harvested fields and take them as their pay.

I do not describe all these different crop products and their uses for the sheer love of detail, but to drive home an important theoretical point about the algorithm itself. The crop products are not simply physical objects and the algorithm is not simply a scheme of categories for naming or classifying them. The crop-products are the result of a complex cultural process of commoditization. This process includes on the one hand the actual biological domestication and evolution of the plants and animals themselves, and on the other the complex of technologies and process that make up their uses. The ways they are conceptualized for measurement and comparison is a relatively small part of this much larger process and cannot be separated from the larger processes. What this means is that the suggestion that we could somehow treat this all as "objective" facts and translate them into our own or "economic" categories simply by applying a currency exchange rate, imposing a single system of measurement like the metric system, and breaking the crop-products themselves down into their "objective" components like proteins and carbohydrates, or nitrogen and potassium, is nonsensical. Such translations would make no sense without also translating their contexts, and the contexts of those contexts, so that the proposition amounts to saying that we can translate one culture into another simply by verbal substitution, and this is not possible any more than one can translate all the literature of one language into another simply by producing a dictionary. There is more to context than content.

While all concepts in a community are cultural, it is not an overstatement to describe the farm budget algorithm as a kind of apex or top-level concept, a concept that mobilizes a large number of other conceptualizations of narrower and more specific scope.

Small farm with food strategy

At the time of the Gujarat interviews, I had no information on the statistical distribution of holdings by size. The smallest farm I visited was just over two hectares and the largest 3.14, but they covered a wide range of ratios of land per person. In the five families interviewed on their farms, the ratios were, in declining order: 31 people for 7.75 acres, 22 people for 5 acres, 9 people for 6 acres, 7 people for 7.2 acres, and 6 people for 7 acres. The two farms whose models are given here in Table 2 and Table 3 are those with 7 people and 31 people, in part because they are the extremes of the range and in part because the information was especially full and clear.

As seen by comparing the two charts below with the template above, the food allowances in Punjab and Gujarat are generally similar, but the crops differ and the yields in Gujarat are consistently lower.

Table 2. Field Interview for Gujarat Small Farm 1

[The article homepage has a downloadable file in Excel with calculation formuli for this spreadsheet]

	Math- Kharif 1988-Rabi 1989							Ratora revision 25 Jul 93			
Projection of Land Needed for Subsistence								By M. J. Leaf 1 Nov 89			
FOOD								Revised to better fit notes.			
Crop	Gm/Meal Man	No. Men	No. Wom	No. Child	Yield Q/Ac	Rs. /Kg	Meal Fed	Acre Req.	Acre Sown	Pcnt MNSA	Rs. +/-
Bajra, HW	270	3	4	0	12	2	365	0.57	2	0.08	3420.30
Wheat	71	3	4	0	10	2.5	410	0.20	2	0.03	4490.58
Rice	190	3	4	0	8	4.5	365	0.61	0	0.09	-2184.53
Mung	71	3	4	0	5	10	52	0.05	0.05	0.01	-8.44
Tur	47	3	4	0	2.5	12	365	0.48	0.45	0.07	-91.02
Gram	27	3	4	0	8	8	0	0.00	0	0.00	0.00
Tomato	100	3	4	0	30	2	104	0.02	0	0.00	-145.60
Vegt	100	3	4	0	1	4	260	1.82	0	0.26	-728.00
Sugar	20	3	4	0	350	7	910	0.00	0	0.00	-891.80
Chilis	1	3	4	0	10	24	730	0.01	0	0.00	-122.64
Oils	23	3	4	0	10	25	730	0.12	0	0.02	-2938.25
Total								3.77	4.5	0.53	3738.85
FODDER			Buf.								
Crop	Kg/dy	Ox	Jers.	B.Calf	Yield	Rs/		Acres			
Number of	Buffalo	2	2	0	Kg/a	Kg		Req.			
Jowar dry	0	0	0	0	1400	0.50		0.00	0	0.00	0.00

Bajra	7.5	45	265	0	2125	0.12	2.03	2	0.29	-7.50
Wh straw	6	365	100	0	2000	0.60	1.70	2	0.24	366.00
Rice S.	0	0	0	0	1400	0.00	0.00	0	0.00	0.00
Gujraj	20	140	365	0	29000	0.17	0.60	0.6	0.08	0.00
Cane leaf	0	0	0	0	800	0.00	0.00	0	0.00	0.00
Sudan	10	0	0	0	20000	0.00	0.00	0	0.00	0.00
Conc.	6	0	185	0	0	1.65	0.00	0	515.92	-3663.00
M.O/cake,kg	1	0	185	0	0	5.50	0.00	0	0.00	-2035.00
Total							4.32	4.6	-515.31	-5339.50
Area Farmed	7.1		Total area required:			7.1	Net subsistence balance:			-1600.65

FUNDS		Yld	Area	Price	Costs	Net
Tobacco		8	2.5	1150	1600	21400
Cotton		1.6	1.5	1000	1100	1300
Sunflower		2.4	0	681	0	0
5 acres on canal. Other two reached by well water .	Total Commercial					22,700.00
Well privately owned by other farmer.	Subsistence + Commercial Crops					21,099.35
Rate for water is Rs. 15/hr. 20 hp. motor.	Milk					11,950.00
One acre = 3 hours	Total crops + Milk					43,799.35
	Gain per acre					6,168.92

The family of Table 2 lives in a single-storied house of about 5x10 meters floor space in the farmer’s own fields. The house had a tiled roof and whitewashed walls, but appeared to be made out of unbaked mud brick. During the interview, we sat on a few cots in a clearing about ten meters in front of it, under a tree. His crops were growing about us. Just in front of the house were a small kitchen garden and an intensive fodder plot, and just behind us was a small area fenced with a row of cactus stems and a small shed. The farmer was pleasant and helpful, and the answers direct and exact. His strategy of mixing consumption and cash crops was typical for the area. The consumption budget is fixed, the farmer does not choose whether to provide it but only how to do so: by what crops and, within fairly narrow limits, by what dietary choices. The family had three men and four adult women, no children. The farmer was in his upper fifties and his sons were apparently in their twenties. He has two oxen and two buffalo, and, like most of the other farmers in the area, was a member of the Anand dairy cooperative and sold milk to them.

The dairy cooperative provides full support in credit, advice, and infrastructure: it provides loans to get the buffalo (all milk animals are buffalo in this area), provides full technical advice, sells the needed diet supplement at fair prices and with reliable quality, and makes daily pickups of milk. While the animals are lactating, about seven months of the year, the farmers supplement the farm-grown ration with a fodder concentrate from the dairy that is fed at the rate of one kg for every liter of milk produced. In addition, this farmer fed one kilo of maize oil cake, also from the dairy, which cost Rs. 5.50 per kg In return, each animal produced 6 liters (kg) of milk, for which the dairy paid Rs. 5.00 per kg That is, the farmer spends Rs.15.4 on the material from the dairy, and gets from them Rs.30.00.

Since the fodder supplements are provided on a credit basis and the amounts owed are simply subtracted from the returns on the milk there is no problem with cash constraints, and the income is a welcome increment to farm operations. In many farms in the area, including this one, it makes the difference between barely breaking even and coming out more or less safely ahead. I will return to this after describing the food budget and the other aspects of the fodder situation.

Total land was 7.2 acres, of which 0.1 is evidently used for the house-site. The family consumes three major grains, bajra, wheat, and rice. The first is much the most important, and the stalks and leaves are a major fodder. In this case, the yield was given by the farmer as 1200 kg per acre, the food allowance as 800 kg for the family for the year, the stated area as 2 acres and he said he sold 1600 kg. At the quoted price or Rs. 2 per kg, however, the calculated surplus of Rs. 3,420.30 is the equivalent of 1710 kg. I asked the farmer if it the calculated figure was correct. He said the figures were fine.

For wheat, the farmer said the family consumed 300 kg a year, at 500 grams a meal for all, hence 71 grams per adult male. They grew 2 acres. I entered what is shown and asked if the calculated surplus was correct. He said it was and began describing expenses, as though by way of explanation. These came to a total of Rs. 2,600. Bajra costs, by contrast, were much less. The result, he said, was that even though the total value of the surplus wheat was Rs. 1000 more, he actually made more profit on the bajra. The point of the explanation was that the wheat surplus was not actually for sale; wheat stores much better than bajra, so the wheat would be held for consumption and as an emergency store while the bajra surplus bajra and an additional amount equivalent to the surplus wheat would be sold. Bajra is grown in the hot season, wheat in the following cold season. The usual practice is to hold enough bajra for a full year until the wheat is harvested, then to sell an amount of bajra equivalent to the wheat.

For rice, the farmer's statement was that the family ate 500 kg a year, or 1.5 kg a day. It is eaten every night, and all was from the market. Since 500 kg works out to 136 grams a day per person, while 1.5 kg a day works out to 214, I took 190 as a middle value at the time and the result was accepted.

The .05 acres (or 2 *guntas*) of *mung* is a catch-crop, planted in mid-February and harvested in the first week or two of June. I did not ask him precisely where he put it, but it could follow the tur or precede either tobacco or cotton. The only crop that actually covers the same period of growth is wheat.

For tur, the farmer’s statement was that the family ate 120 kg a year, 300 grams per meal, once a day every day. The plot was growing near where we were sitting.

For oil, there were actually two components. The first is vegetable oil from the market, which the farmer reported at 120 kg a year. Oil is a basic cooking ingredient, so I recorded this by dividing it among 730 meals for every person, hence 23 grams per person per meal. In addition, the family uses *ghi,* which they produce themselves: 2 kg a month, 25 kg a year.

Everything else, as the table indicates, is from the market. My notes suggest that the tomato and vegetable meals and days eaten came from the farmer, but the amounts per meal were accepted from the template. The amounts consumed are so variable from year to year and so opportunistic from day to day that rules of thumb cannot be as exact as those for products bought in bulk, stored, and used over a long period. My notes do not record the exact basis of 910 as the number of meals of sugar each year. Gujarati cooking is known for its use of sugar in vegetable dishes.

For fodder, farmers in this area had unusually exact ideas of what they were producing and consuming because of the influence of the Anand dairy scheme. Notice that the fodder section of the template is arranged differently than the fodder section here. This illustrates the way different formalizations are required for different situations. The row entries under each animal type were the number of animals that ate that type of fodder. The number of meals was a separate column, and the total consumption was obtained by multiplying the number of animals eating the fodder by the number of meals. This did not allow for the situation where different animals are fed the same fodder during the year, but not necessarily for the same times. In consequence, since I did not have time (or battery-power) to restructure the model in the field, I elicited only the days each fodder was eaten by the animal to which it was fed for the buffalos, since they are fed much more and for much longer. I did not collect separate figures on the oxen and have estimated them in retrospect from the other information and from their working periods. An ox is reckoned at one half a buffalo, and they are given fodder only when working, which is usually from 45 to 60 days a year. For buffalo, "working" is lactating and the period of lactation was stated was 180-185 days. Otherwise, in this area, they are given whatever farm residues are available.

For bajra, the farmer gave the allowance as 3 bundles per buffalo per day, his yield as 850 bundles per acre, and the weight of a bundle as about 2.5 kilograms. I have therefore used 7.5 kg per day as the feeding allowance. In the original elicitation, I recorded 240 days as the period of feeding, and this produces the lack of surplus or deficit the farmer reported.

For *gujraj* (napier-bajra hybrid), the farmer quoted the daily consumption at 20 kg per buffalo, the area as 24 *guntas* (hence 24/40ths of an acre) and the feeding period as all year. Actually, he said it is cut and ratooned for four years and then he replants. Since I had never seen the crop, he showed it to me. One patch of ground was dug up next to an area of equal size that had been recently cut over and was now regrowing. Since he cuts it every few days as needed and thinks more in terms of bundles than weight, he did not have a definite idea of the yield per year in kilograms, so the weights I report were based on discussion and adjustment to tally with the clear fact that he had neither surplus nor deficit.

All food crops together have positive market value of about Rs. 3,740. But his wheat production cost is Rs. 2,600, as noted, and his bajra production cost was about Rs. 800, so the balance is just a little over zero. This is his subsistence floor and absorbs 4.6 out of the total of 14.2 acres he has available over the two seasons (at 7.1 acres per season). The rest of the land is therefore used for providing funds, calculated as profit over costs. This is done with dairying and with two crops:

1.5 acres of cotton and 2.5 acres of *bidi* tobacco. Since both of these stay in the field for two seasons, being sown in early June and harvested between December and the end of February, they occupy the rest of his total holding. His tobacco yield was 8 quintals per acre on 2.5 acres, giving 20 quintals at Rs.1,150 per quintal. The costs were Rs.1,600, stated as a lump sum, so the gain was Rs. 21,400. This was a good year. Average yields for the state (which mainly reflect this area) are normally around 650 kg/acre. In 1988-89 they were 678 kg/acre[1]. For cotton, the total profit was Rs.1,300 over total costs of Rs. 1,100. The price was fixed. The yield, 1.6 quintals per acre, was calculated by the spread sheet itself, since the farmer did not know it. He was, however, sure that it was much lower than usual because of white fly. A more usual yield would be three to five quintals (lint and seed).

The total value of commercial production over costs thus rises to a positive balance of about Rs.22,000, but this says nothing about fodder. The family must maintain one pair of oxen for draft power. A milk animal is desirable for diet but not essential since they have no children. But because of the economics, they maintain two milk animals. Milk adds about 57% again to their total return, Rs.11,950. Each buffalo gives 6 kg of milk a day on average, over 180-185 days, as noted. Of this, 2 kg (liters) are kept for home use (from which the *ghi* is made among other things) and 10 are sold. They are paid, as noted, Rs.5.00 per liter. In addition, as a member of the cooperative he received a dividend for the year of Rs.1,450. He described this as "14%," presumably of his gross sale to the dairy. In the previous year it was more; it depended on total sales.

Thus his overall strategy is to allocate two acres for early bajra followed by a fallow in the peak of the rainy season and then wheat. During the rainy season, he sows 0.45 acres of tur as a vegetable and legume, 2.5 acres of tobacco, and 1.5 acres of cotton sown alone. Meanwhile, he has 0.12 acres of gujraj growing all year, replanted if needed in November. The .5 of *mung* goes in after the rainy-season crops are cleared, wherever he is planning on having a large enough gap of time and, probably, wherever he thinks the nitrogen will be most useful. The total land reported as cropped added up to 7.1 acres as against the total of 7.2 reported at the outset and, doubtless, based on official records. The difference is reasonably attributable to rounding with guntas.

There is a reason for this particular strategy beyond simply optimizing a balance between production for consumption and market. Discussing the overall results, I pointed out that the return from a rotation of wheat and bajra compared very favorably with the returns from cotton and tobacco, and that they were less risky. Would he not be ahead economically if he would grow more of them and less cotton and tobacco? He agreed with the economics but said that if you do that year after year the land deteriorates. Cotton and tobacco do not hurt the quality of the land. Therefore he has designed this strategy so that he can switch the bajra-wheat rotation to a different third of his holding each year and will not return them to any one field more frequently than every second or third year. Shah was inclined to argue, saying the land would not lose

[1] Area and Production of Principal Crops in India, 1989-90:195.

fertility if the recommended practices were followed. The farmer stuck to his position. Since I had been told similar things about the effect of the wheat-bajra rotation in comparison to other crops in Punjab, I am inclined to think there is probably something left out of the official recommendations. While land under cotton and tobacco surely benefits from the three month fallow period in this system, all crops probably benefit from the general practice of moving them, which both spreads the demand for any specific soil nutrient and minimizes the buildups of pest and diseases to which any one crop might be susceptible.

Is this farmer's strategy aimed at providing his family’s subsistence requirements? Yes. Is it optimizing in terms of market prices? Also, yes. Has he considered growing more commercial crops? Absolutely. Could he do better if grew different ones? He says no, and there is no evident flaw in his reasoning. The only major commercial crop in this area apart from cotton and tobacco is banana. This takes a very substantial investment that he probably does not have, and is usually grown by people with very good water supplies. As noted, he has to buy well water. It also is a five year crop, so it would clearly destroy his one-year-in-three system for shifting wheat and bajra. Rice is grown in the area, but he could not shift from bajra to rice for several reasons. First, it is planted later and would crowd his sowing of wheat. Second, it would leave him short of fodder. And third, although I did not ask, the costs and returns are comparable to wheat, not bajra, so total profit would be less.

What about planting different proportions of the same crops? To examine the possibilities, the spread sheet was set for three alternatives: a) one acre more of the bajra-wheat combination and one acre less of tobacco, b) one acre less of the bajra-wheat combination and one more of tobacco, and c) one acre less of the bajra-wheat rotation and one more of cotton.

With combination a), increasing bajra-wheat and reducing tobacco, the total return to crops drops to Rs. 20,004. To this loss, the farmer would have to add the additional expenses on the added acre of wheat, about Rs.1,300. On the other hand, milk sales could be increased by about 60% (assuming that all the new production would go for sale), for about Rs.7,170 a year. This could be done because the additional area would provide just about exactly the fodder needed for one more animal at the proven rates of production. Hence with this year’s figures, the net result would be an increase of about Rs.5,000 for the year, or 10%. But there is the long-term fertility problem noted, the risk of assuming the additional loan, and the need for additional dairying space—which the farmer could only get by cutting back on the cropped area. This is not clearly superior.

With combination b), reducing bajra-wheat by an acre and adding an acre of tobacco, the total return from crops goes up from Rs.20,812 to Rs.21,419. To this, the farmer would also add the savings on the acre of wheat not grown, or Rs.1,300, for a total gain of about Rs.2,000. It does not go up more because at this level there are substantial deficits in fodder from the bajra and wheat, totaling about Rs. 1,400. It is logical that the farmer would rather buy the fodder than cut the milk production. If he did the latter, since he only has two animals, it would have to be cut in half, for a loss of nearly Rs.6,000. All in all a gain of Rs.2,000 coupled with greater dependence on the volatile price of tobacco would surely not be worth the additional trouble of having to buy and bring the fodder every day, and the risk that a small increase in fodder price would wipe it out. In addition, tobacco is the most labor-intensive crop the farmer grows, taking about twice the man-hours of paddy and more than most garden vegetables (Water Technology Centre: 308).

With combination c), increasing cotton instead of tobacco, on the basis of the current yields, the return from subsistence and commercial crops makes a net drop from Rs.20,812 to Rs.15,227. At more normal yields, the results would be the same as increasing tobacco. I did not ask the farmer why he grew both cotton and tobacco, but usually when two cash crops are grown side by side the reason is that a pest that would damage one would not hurt the other, or that one will benefit from climatic conditions that will adversely affect the other.

Basic concepts of economic theory are notoriously difficult to apply to real-world cases, but this seems to be a clear instance of the general theoretical notion that the optimal (greatest) overall return to capital is realized when the returns to the possible alternatives at the margin are equal. Of course this is only a local optimum, but in agriculture all optima are local. The idea of a choice among all possible technologies and crops from all possible times and places is practically nonsensical.

Small farm with labor-freeing and cattle strategy

The second farm, Table 3, is rather unusual and presents many cautions in regard to some widely accepted generalities. One of these is that where labor is plentiful and land is scarce, labor intensity increases to obtain greater gain from the scarce resource – a kind of automatic balancing mechanism. In this case, population is so great that such intensification would not be sufficient. A second is that dairying is a second level of intensification, taking advantage of crop byproducts to produce additional marginal gains. In this case, dairying is displacing crop intensification. And finally, in many ways, this case indicates the vital importance of regional organization and infrastructure, against the idea that all one needs is better inputs and better information at the farm level.

Table 3. Field Interview for Gujarat Small Farm 2

[The article homepage has a downloadable file in Excel with calculation formuli for this spreadsheet]

	Math- Kharif 1988-Rabi 1989							Mohan Bhai			
Projection of Land Needed for Subsistence								M J Leaf			
FOOD	Grams										
Crop	Meal/ Man	No. Men	No. Wom.	No. Child	Yield Q/Acre	Price /Kg.	Meals Fed	Acre Req.	Acre Sown	Pcnt MNSA	Rs. +/-
Bajra Kh.	160	10	13	8	6	2.25	410	3.04	3	0.17	-53.28
Bajra HW	160	10	13	8	4	2.25	320	3.56	1	0.20	-2302.56
Rice	185	10	13	8	8	4.5-	365	2.35	0	0.13	-8447.38
Mung	92	10	13	8	5	10.--	24	0.12	0	0.01	-613.82
Tur	20	10	13	8	2.5	11.--	365	0.81	0	0.05	-2232.34

Gram etc.	50	10	13	8	7	7.--	48	0.10	0	0.01	-467.04
Veg farm	90	10	13	8	30	4.--	120	0.10	0.1	0.01	-0.96
Veg mkt	90	10	13	8	30	4.--	245	0.20	0	0.01	-2451.96
Sugar	15	10	13	8	350	7.--	910	0.01	0	0.00	-2656.29
Gur	12	10	13	8	350	5.--	365	0.00	0	0.00	-608.82
Chilis	1.5	10	13	8	10	24.--	730	0.03	0	0.00	-730.58
Oils	6	10	13	8	10	22.--	730	0.12	0	0.01	-2678.81
Total								10.32	4.1	0.59	-20565.04

FODDER	Kg	Ox	Buf.	B. Calf						
Crop	/Day	2	5	4	Yield	Rs/	Acre	Acre	Pcnt.	Rs.
	/Buff	Days:			Kg/a	Kg.	Req.	Sown	MNSA	+/-
Jowar dry	10	365	0	0	1400	1.00	2.61	1.--	0.15	-2250.00
Bajra dry	9	0	165	0	1855	2.50	4.00	4.--	0.23	-12.50
Rice st.	7	0	65	65	2000	1.00	2.05	0.00	0.12	-4095.00
Guar	2	100	0	0	800	10.00	0.25	0.00	0.01	-2000.00
Lucerne	9	88	89	0	32000	2.50	0.15	0.15	0.01	7.50
Maize wet	12	60	60	60	5700	0.35	1.26	0.75	0.07	-1023.75
Gujraj	20	36	36	36	29000	0.00	0.25	0.15	0.01	0.00
Gr. Grass	13.3	36	120	120	28800	0.00	0.52	0.00	0.03	0.00
Conc.	6	0	180	0	0	1.65	0.00	0.00	0	-8910.00
M.O/cake,kg	1	0	180	0	0	5.25	0.00	0.00	0	-4725.00
Total							11.08	6.05	0.636	-23008.75
Area Farmed		7.75	Subs. area		17.40		Total Food + Fodder			-43573.79

COMMERCIAL CROPS	Area	Yield	Price	Cost	Gain
Tobacco	0	0	1100	0	0.00
Cotton	2	6	600	2200	5000.00
Potato	0	0	0	0	0.00

Total land 7 acres 30 guntas (7.75 acres).	Net gain commercial	5000.00
All under canal and nearby private well.	Commercial + Subs.	-38573.79
Water charge is Rs. 12/hr. 20 hp. motor. 1 acre/3hr.	Milk plus bonus	37200.00
Because of net loss from agriculture, family does hired labor.	Cropping + Milk:	-1373.79
Income from labor is about Rs. 14,000/year.	Net gain/acre	-78.93

With thirty-one people and 7.75 acres, this family cannot make a living from farming alone. For this reason (and also because of the combination of the educational opportunities its members have had and the employment opportunities the region affords), its male members engage in custom agricultural work as a team—although I did not find this out until the interview was almost complete. Note that almost all the balances in the budget are negative, and the mathematical problem for the family is not maximizing their surplus but minimizing their deficit.

To free the men for work in the normal periods of intense labor demand, the family cannot grow the same crops as those they work for. Instead, they concentrate on raising a relatively large number of cattle, for which they grow fodder crops that can be cut by the women of the family when the men are away. Because of the many fodder crops and the difficulty of estimating fodder yields generally, there were many uncertainties about yields that were difficult to resolve. I will not go into all the details of why the particular solution presented was chosen. The difficulties are not sufficient to give reason to doubt that the basic outline of their situation was honestly presented – and cannot in any way be attributed to irrationality or cured by following the usual kind of development advice.

The farm was near the head of a second minor that the action research had been expanded to include while I was there. The minor bed was grassy, indicating that it had not been receiving water. The farmer, Mohan Bhai, was in his late 50's. His skin was dark and wrinkled, as one would expect from a man who worked in the sun, but he was cheerful and appeared to be in good health. The core of the family was Mohan and three brothers, with their mother, wives, and children. About five other sons, brothers, and nephews kept us company and joined in the discussion.
The first crop, bajra, was straightforward. It is grown in two seasons: one acre in the "hot" season (sown in mid-February, harvested in the first half of May), and three acres in the summer rainy season. An area this large would normally be planted in several separate fields over several weeks and harvested the same way. The bajra provides the main grain for the family, and all of it is eaten. It also provides the main fodder for the buffalos. None is sold. Instead, there is a deficit. It is eaten twice a day, five kg at each meal. This comes to about 161 grams per person, but somewhat more per adult male. The yields are as stated by the family: 6 quintals an acre for the hot weather, and 4 quintals for summer. Since both yields are low, Shah and I asked several times. The reasons were that the hot weather bajra in particular was low because it was planted late, in the fields of a preceding cotton crop as the latter was being cleared, and the rainy season bajra was planted very densely, to maximize fodder production.

The daily dietary allowance also seemed low, and I said so. They said they ate a lot of rice along with it. So the number was accepted.

The rice allowance stated was also 5 kg per meal, but just once a day, in the evening. The 185 grams per adult male was accepted, along with the annual cost of Rs.8447.

The consumption of *mung* was described as 60 kg a year, and it was eaten two times a month. All is from the market.

Tur consumption was put at 2 quintals a year, eaten every day, as indicated in the spreadsheet. I questioned the small daily allowance that resulted from entering this, 20 grams per adult male. They insisted that it was correct. They ate only a little, prepared as a very thin *dal* and mixed with the rice, as *kitchery*. Again the annual expense was approved as computed.

The statement on gram was that they ate it four times a month and bought 60 kg a year. The daily allowance and annual cost was computed by the spreadsheet and accepted.

Vegetables were described as eaten once a day for about 360 days, about a third (120 meals) from the farm and two-thirds from the market. The annual expense was stated as Rs.2400, but consumption per meal was uncertain and so was area. The family does not set aside a specific plot for vegetables but grows them tucked in here and there as land becomes available. The total yield of 30 quintals/acre was a collective guess, in part because of the uncertainty about the area and in part because we did not know if we were trying to estimate total production or vegetables as consumed.

Sugar consumption was given as 3.65 quintals a year, or 1 kg a day. The daily gram allowance and annual costs were accepted as calculated. None is grown. *Gur* (raw brown sugar) was stated as 10 kg a month, 120 kg a year, costing Rs.600. The daily allowance per person was acceptable as calculated.

Oil was again from the market, and the details of the estimate need not concern us; as with the others there was a mixture of hard and soft points from which we collectively estimated what is in the spreadsheet.

The cattle were 2 oxen and 5 buffalo. In addition, there were 12 buffalo calves of various ages. The family will keep them either for sale or for milk. They start to give milk at three years. The price they would fetch depends on the milk yield. Five liters a day was expected, and for that the price for the animal would be Rs.2500. For three liters it would be Rs.1500. In effect, the price is Rs.500 per liter of milk production per day. When I remarked on the odds against getting so many female calves, Mohan Bhai laughed and said in that at least, God has given them good luck. The two and three year old animals require fodder along with the adults. The differing amounts they ate could not be recorded during the interview for lack of time, but we agreed that they were the equivalent of about four more adult buffalo. I have included them in retrospect by recording 4 in the column “B Calf” and applying the adult feeding allowances.

For fodder sorghum, everything was clear. One acre was stated, as was the allowance of 10 kg per day per pair, for oxen alone and not buffalo, all year. The yield was clearly given as 1400 kg an acre, and the resulting expense of Rs.2250 was pronounced correct.

Bajra, however, was confusing. It was, as usual, fed in bundles, not kg. The stated allowance was 5 bundles a day per buffalo for 180 days. One bundle was stated as 2.5 kg, and hence the yield would have been 1750 kg/acre. The family further said, however, that they had neither a deficit nor a surplus. The balance was even. The spreadsheet showed a substantial deficit. On further discussion, however, it turned out that the bajra was often fed mixed with the rice straw.

The rice straw itself was first stated as fed at the rate of 6-7 bundles a day per buffalo, mixed with bajra or alternating with it. The weight of a bundle was not clear. It was apparently 1 kg, but may have been 2 kg. Bundle is actually a unit of girth, not weight; people could readily indicate with their hands what the diameter should be, which was about ten centimeters. The expenditure was Rs.2200 for the year, and it was fed for 60 days. To reconcile all of this and come out with the same firm points, I have lowered the daily feeding allowance of bajra in kilograms from 12.5

kgs to 9, raised the yield marginally to 1855 kg/acre (still low in agronomic terms), and lowered the number of days fed to 165 to compensate for the indefinite but presumably significant number of days when the bajra is mixed with rice straw. At the same time, for rice straw I have taken the bundle weight as 1 kg and the daily feed allowance at 7 kg (or we can simply read it as seven bundles) and slightly increased the days fed, also to compensate for the mixing. This brings the rice straw expense to the stated amount. Together, they should be read as covering the period consisting of the last two months of pregnancy and the 180-185 days of lactation, or 240 days. For the other 120 days, in the *kharif* season, the family said the animals eat green grass in place of the bajra and rice straw. This is cut daily from wherever it can be found. It isn't an out-of-pocket expense, but it is a great deal of work. They stated their allowance as 80 kg a day for all.

The sorghum, bajra, and straw are fed dry. The green component comes from a sequence of crops: cut fodder maize (maize planted very densely, so as to produce only vegetative growth), lucerne, and gujraj. *Guar* is something different. It is a coarse legume, bought as a meal and fed as a supplement to the oxen in their main working periods. The number of days (100) and the daily consumption and cost were all stated without inconsistencies (the price was Rs.10 per kg and the total was Rs. 2000). But this is too long a working period for the crops they are growing and the amount of land they are farming. The explanation is that they are hiring the animals out with themselves in their laboring activities – thus not merely doing agricultural labor but custom plowing, harrowing, and the like, which gives about five times the return of ordinary farm labor.

Gujraj grows all year, as on the previous farm, but in this case there are too many animals to rely on it alone. The daily consumption of 20 kg per buffalo is consistent with the previous farmer's allowance.

Lucerne is planted in the beginning of August. Cutting starts in about 60 days and ends in the hot weather, by the beginning of April – when the maize cutting would begin. It is normally arranged so that it can be cut in small pieces each day and by the end of the month cutting will return to the first section. All the points of the table were read out and approved.

The maize oil cake and concentrate were straightforward and standard for all lactating animals under the Anand dairy scheme. Non-lactating animals (such as the calves) would not require any.

The only commercial crop was cotton. Area, input, yield, and cost were stated and the family accepted the calculated return.

Milk production was 15 liters twice a day for all, for 240 days. The dairy paid Rs.5 per liter, which came to Rs.36,000, plus a bonus of Rs.1,200.

It was only at this point in the elicitation, when the total balance could be computed, that it became clear that the family had an overall deficit. I said so and asked about it. They agreed that it was so, and that this was why they had to take work as laborers. The discussion was extended, but they explained that their combined earnings were Rs.14, 000, for the year, and they had to cover a deficit of Rs.8,000. (This would have included clothing, schooling, household maintenance, and all the other expenses in addition to the Rs.1,373 loss on agricultural operations alone that the table shows.) So I asked why they did not take more land on tenancy. They said that as laborers they earn more than they would as half-share tenants after costs. Also, they do not have the money for the inputs. Mohan Bhai made a point of saying that he was unable to pay the last year's water charges. This was probably also related to why they grew cotton and not tobacco, which takes more labor.

Thus this family's strategy was to adjust its cropping strategy to make their family labor available to others at the times when labor demand in the region is greatest and, therefore, the daily wage is the highest. These peak periods are for sowing and harvest, mainly around late May and early June, October-November, and January through early March. So in place of the crops everyone else is concentrating on, this family concentrates on fodders and vegetables. The grains it eats are a by-product of the fodder production and what it buys with the cash it earns. The dry fodders (bajra and sorghum) do not require very much labor to grow, and after harvest are simply chopped and fed on a daily basis. It is normally men's work, but women can do it. Green fodders and vegetables are labor intensive on an annual basis, but it is spread over many small daily increments. Thus while all the calculations are grounded in the traditional planning algorithm, there is no way to think that the algorithm is regarded as simply a model of a typical farm that one must imitate or manifest.

Although the progressive farmer who cannot be characterized by the subsistence budget model is at the other end of the scale of wealth and freedom from the need to produce directly for daily consumption, his strategy is very similar.

Commercial strategy

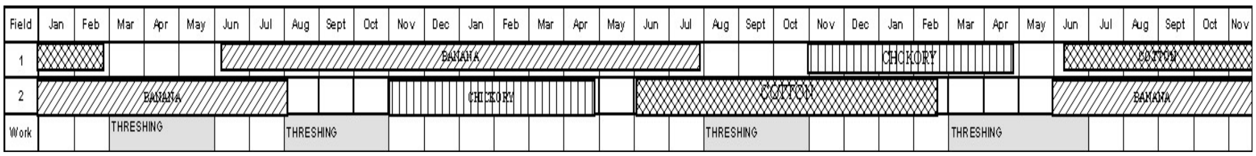
This last farmer, Indravadan Bhai, holds a B.Sc. in agriculture from Gujarat Agricultural University in Anand, and an L.L.B. His age appeared to be about thirty. After the interview was over, he showed me some material he had brought with him that he had wanted to discuss: a letter from an uncle to a growers' association in Indio, California, and a brochure apparently from a tour to the United States with this same uncle's name it. He also had some material on almonds from another growers’ association in central California. He said he was not interested in the crop so much as the way the association itself was organized. He grew only three crops: banana, chicory of a specific kind whose root was used in making chicory coffee (which is popular in India), and cotton. None are for home consumption. Although the farm is in the area of Anklav minor, it is too high to receive canal water. He has three hectares.

Indravadan Bhai said his house is brick, with electricity and all the furnishings he wants. He has a tractor and no animals, which is why he needs no fodder. He obtains all of his food in the market. He lives with his father and three brothers, who are in business. There is thus no manpower pool tied to the household that has to be maintained. Secondly, because he does not get canal water, he had to invest in a well. Given this, it made sense to him to invest in a big one.

A private market in water is well established in the area, and he knew a larger well could yield a return over and above that from his crops. His has a 20 hp. motor and the recharge rate of the well is high enough that he can supply water 24 hours a day.

The income from crop operations works out to about Rs. 40,000 per hectare, which is about average for the commercial crops in this area. In addition he makes Rs. 25,000 a year from selling water from the well and Rs. 50,000 from hiring out his tractor. When he said this, he stressed that it was for threshing only. The reason is clear from Table 4, which is *his* planning algorithm. He does not need a model to calculate the economics or food production aspects of his operation; his central concern is to plan his time.

Table 4. Commercial farm plan



He divides his land in two halves. He grows the same two-year rotation on each half: cotton in the kharif season followed by Banana for fourteen months followed by chicory in the next winter. But he staggers them as shown in Table 4. The continuous blocks indicate the months the crops are in the fields. The blanks indicate the months when the fields are bare. He thus has important slack periods between cotton and bananas in April/June, between bananas and chicory in September/October, and between chicory and cotton in May/June. The rotation gives very little fallow time, a good spread of income, low peaking of demand for labor and water, makes the best use of fertilizer residues, and minimizes pest accumulations by holding the amount of time banana is growing in both fields at once to every other summer.

Like the previous family, he has arranged his cropping with a view to freeing himself to provide custom services to other farmers in the normal peaks of seasonal demand – in this case for water and threshing. The major threshing season for paddy is early August, that for wheat is February-March. At these times his own crops are quietly growing and require little attention. So he can supply what his neighbors need just at the time they most need it, which also enables him to get the best use of his relatively great investments in non-landed and human capital.

This pattern would make no sense in isolation. At the end of the discussion, I asked how the income he made from farming compared with the incomes of his father and brothers. He indicated it was quite good. I asked if he could stay in farming. He said he would as long as he could make good money at it. He was also carrying a pamphlet on chicken-raising, and said he was quite interested in it.

Implications for Bounded Rationality

Precise and complex indigenous algorithms like the farm budget model, as opposed to "fast and frugal" rules of thumb, have three major implications for the arguments surrounding bounded rationality.

First, because they are clearly indigenous while also unlike anything proponents of bounded rationality have described, they falsify the claim or assumption that bounded rationality provides the only alternative to classical economics as a source of such algorithms, however they might be classified.

Second, since the algorithms are clearly optimizing, they falsify the claim or assumption that the only conceptual choice for analyzing optimization is between an account that portrays some type of sub-optimization or "satisficing" plus some type of psychological "bias" and an account that applies the algorithms of "classical" economics or Leonard Savage's purportedly more generalized calculus of subjective utility (1954).

Third, however, since these indigenous algorithms are clearly situated in a particular cultural-ecological setting, they reinforce the case of some of Gigerenzer and Selten's contributors, especially Todd, for paying attention to the way the use of optimizing algorithms is affected by the material (and not necessarily cognized) contexts in which they are deployed.

And finally, since the farm budget algorithm of the subsistence farms is clearly different from the economic business-like model of Indravadan Bhai's commercial farm, while in fact both sets of families know how to make both kinds of calculations, it follows that to be understood such algorithms must be located within very specific cultural ecologies rather than simply attributed to a "mind" or psychological makeup.

I have no quarrel with the models Gigerenzer and Selten's contributors adduce. Many use iterative processes that nicely illustrate the way decision makers can improve their positions by interacting with an environment in ways that classical economic models do not, but real decision processes almost always do. It is wrong, however, to assume that the only place to find such models is in their own imaginations, rather than in the many bodies of productive knowledge that come to us as the legacy of the long history of human evolution and adaptation. It denies us the ability to learn from others and to understand their problems of production and environmental sustainability that in the end affect us all

Implications for Experimental Economics

The implications for experimental economics are more complex because the theoretical implications of experimental economics are more complex, bringing up as they do not only the problem of culture versus biology, but also the way the institutionalists conflate and confuse the concepts of society, institutions, and organizations as already noted.

First, the farm budget algorithm is not individualistic and cannot be broken into individualistic components. It is an algorithm for a farm family as a managerial organization. It is used collectively in the same kind of interactive process that is evoked during the elicitations, and the fact that a version of it is built up and held in common memory by a group of individuals in this way is a large part of what defines them as participants in the organization. When so used, its users are at one and the same time deciding what to do as a collectivity and what each person's contribution to that collectivity will be. These decisions are not made in absolute detail at one moment in time, of course, but as an ongoing process and within broad and well understood parameters that can be elicited as still further algorithms which concern the ways various specific tasks are performed as well as the way individuals move from task to task on the basis of skill, sex, availability, and deference. Since the use of a model involves making commitments to cooperate, it follows by definition that such use cannot be represented by a Nash equilibrium game.

Of course some of the individuals in the organization may evaluate the possibility of leaving or staying with respect to their purely personal benefit or advantage, but that is a separate calculation that uses a different algorithm or set of algorithms. As long as they are in it, the participants do not on a day-to-day basis, when faced with one of the tasks that the model commits them to, ask the others "What will you give me if I do it?" It follows that two basic methodological assumptions of both lines of experimentalist analysis are unjustified: that optimizing decisions are necessarily one-off and individualistic, and that the constraints that lead to cooperative behavior cannot be represented within an optimizing algorithm but must be represented as exogenous to it, either in supposed inherited psychological proclivities or as institutional rules and constraints. In the farm budget algorithm, the rules for cooperation are intrinsic.

Second, it might be objected on behalf of the evolutionary economists that while a Nash equilibrium game cannot define the rules for interaction within an organization like a family farm because they are genetically related, it nevertheless can define the rules for interaction between such organizations. That is, it might be claimed that the farm families, not the people in them, act as Nash individualists. This claim would also be false. As has been shown, the process of optimizing internally is at one and the same time a process of anticipating cooperation externally, using the same farm budget algorithm but seeing it from a different perspective.

Henrich and his colleagues recognized that there is something seriously wrong with the game-theoretic models of rationality their study was based on, but misplaced what it was. Their conclusion was that while their "results do not imply that economists should abandon the rational-actor framework" they did suggest two major revisions. The first was that the "canonical model of the self-interested material payoff maximizing actor is systematically violated" (Henrich et al., 2001: 29). The second was that "preferences over economic choices are not exogenous as the canonical model would have it, but rather are shaped by the economic and social interactions of everyday life" (*ibid*). I agree and disagree. I agree that they should not abandon the rational actor framework, but I disagree that the "canonical model" describes what a rational actor is—in the strict sense of describing how to maximize "material payoff." Gintis has occasionally noted that John Nash was schizophrenic, but he has not quite drawn the conclusion that the model is actually a formalization of Nash's disorder rather than of rationality in any normal sense. The Homo economicus of the Nash equilibrium is not the Homo economicus that would be recognized by the author of the *Theory of Moral Sentiments*. There is an old joke that involved a motorist whose tire went flat next to an insane asylum. The motorist was surprised by very good advice from a person on the other side of the fence. So he asked the person if he was an inmate. The person said he was. The motorist expressed disbelief. The person explained: "Just because we're crazy doesn't mean we're stupid." Is rationality just being smart, or is finding how to solve problems? And if the latter, how can we expect real rational decision-making to ignore the practical organizational contexts that provide both motive and means for doing so?

I also agree that preferences are shaped by "the economic and social interactions of daily life" but disagree that this formulation is in any way adequate as an explanation of anything. Saying our preferences are influenced by our interactions is like saying our compass heading is influenced by the direction in which we are traveling. Interactions are constituted social processes, and preferences are part of what constitute them. These farm planning algorithms articulate such preferences. Eliciting them involves not only making the algorithm themselves available for observation, it also makes available instances of the "interactions of daily life" that they are used to constitute and the manner of that use.

Empirical Status of the Algorithm

Now if it is clear that these algorithms exist, are widely distributed, have a common formal structure, and yet are adaptable to highly specific and diverse farm situations, the next question has to be "*How does it happen*?" We can say that they exist in consensus; we can say they are distributed knowledge. But these are not explanations. What is consensus or distributed knowledge in this case? How does such an algorithm exist, and how is it maintained?

While I do not think that the farmers literally had spreadsheets in their imaginations, I can infer what they did have. First of all, it was a picture of their land divided into fields. These fields are demarcated by irrigation channels. In rainfed irrigation, they are demarcated by raised earthen bunds. Changing their boundaries is possible, but is a major undertaking that there is seldom time for. Given the fields, the farmers next think of a rotation of crops through each one. And given this, they think of this rotation in terms of the mathematical variables and relationships of the model: yield times area equals production; daily allowance times number of meals times number of people equals consumption; consumption minus production equals surplus or deficit.

It is these mathematical relationships that are most fundamental, in the sense that they are what allow the rest of the values to be regenerated in a consistent way and in the detail that the spreadsheet records. To regenerate the entire model, the only other things the family members need to remember are the identities of the crops, how well or poorly they did, and how well the rules worked out—whether they ate well or poorly, whether their predictions were close or not-close. There are also, as noted, some points in the data that are much firmer than others. For food crops, one of these is net surplus or loss. Another is how much had to be bought in the market. For commercial crops it is total weight or value. Fodder volume is relatively easy to remember, but fodder weight is unstable and not very important. With such information the picture that the spread sheet conveys can be reconstructed whenever the family comes together by pooling their experiences and using the underlying mathematics to bring these recollections into a consensus, exactly as in the interview process.

This is the indigenous idea system (and idea-process) that the spreadsheet captures, and while the idea-system is not literally a spreadsheet itself, the spreadsheet is conceptually isomorphic with it. Thus the spreadsheet does not merely "correspond to" or "denote" actual farms. The mathematical consistency and integration of the spreadsheet is based on the same conceptual principles as the actual farm operations, so that even if the model as I put it together in the computer is not fully formed in the thinking of any one person in the farm household, it will emerge coherently whenever the household members bring their several experiences and perspectives together to plan farm operations or, as in the present case, to respond to an interview about such operations.

Optimization in General

Game theory and bounded rationality apart, it might be objected that more widely used Western models for crop planning, most notably linear programming models, are genuinely optimizing while the indigenous farm budget model is only imperfectly so. The reason is that the Western models maximize their objective function by purely mathematical methods without adjustments that take into account practical circumstances, like maintaining fertility, available labor, crop timing, probabilities of crop loss, and the other practical constraints that I have cited in constructing these models and their "what if" iterations. There are at least three reasons to reject this objection. First, while the computational methods are different, the simplest underlying mathematical logic is actually the same. They are both hill-climbing algorithms. You continue to adjust the model to increase the return and you know you have reached a maximum when all further adjustments lead to decreases in the return. Second, if a linear programming model were to be used in such a way as to produce the same level of accuracy as this indigenous algorithm, it too would have to take into account the non-computable or much less than perfectly computable constraints like crop timing that this model more naturally points us to. And third, this algorithm can take into account qualitatively different objective functions and transfer functions, which actual linear programming computer programs cannot.

In the United States, linear programming is regularly used for farm planning, but the models assume just one objective function, which is maximizing profit. They also take all resources as purchasable, so that they can be stated in terms of costs. Moreover, on any given farm very few crops are grown, sometimes just one. In India, A. S. Kahlon and Karam Singh, of Punjab

Agricultural University, have been articulate proponents of applying similar modeling to farms comparable to those described here (Kahlon and Singh, 1981). But they do this by limiting the problem to just some crops and some cultivation choices by removing at the outset any requirements for production that are fixed for the farm, such as fodder requirements, as well as any fixed resources, like the family labor. The linear programming model is used to maximize only what is left, and only in relation to one objective function, which is maximizing profits. It is a knowledgeable and appropriate approach to the problem of adapting this technique to this type of farming, but in the nature of the case it cannot establish the superiority of linear programming over the indigenous budget algorithm (which they do not seem to be aware of) since the farmer would actually have to use the indigenous model to set up the context in which the linear programming model would be applied.

In short, there is no way to argue that linear programming is technically superior to the indigenous farm budget algorithm, and if this is clear for linear programming it ought to be far more clear for such less precise models as Edgeworth boxes, the theory of the firm as represented in Indian farm budget studies, and of course the algorithms of bounded rationality and the games-plus-deviations of experimental economics.

It is a very good bet that algorithms like the farm budget model are imbedded in virtually all major organizational strategies, but it is also a good bet that they are very easy to miss unless you ask exactly the right questions in order to evoke exactly the context in which they are employed. These are not ideas that people use to entertain one another or even to convey general values; they are ideas that they use to organize sustained productive effort. If we really want to understand what effective adaptive thinking is, we need to find them.

Conclusion

The new institutionalists, particularly Greif, were right to identify the problem of relating the analysis of decisions to collective outcomes as central to all of the social sciences. They are also right in seeing Smith's analysis of the division of labor and the establishment of market prices as paradigmatic solutions—my terminology, not theirs. They are wrong, however, to see the problem of extending Smith's analysis only as a problem of finding some more general kind of theory in which individual choices lead to a stable equilibrium outcome and in treating collective patterns only as emergent from individual self-interest rather than recognizing that individuals commonly anticipate the effects of their interactions and build for themselves safeguards against the kind of selfishness represented by Nash's assumptions.

The empirical question to ask is not what is the most general version of Smith's theory but rather what are the more encompassing forms of the social processes he described? What other social processes or phenomena are the division of labor and the pricing mechanism akin to or part of ? The answer is not "society," because society in the sense intended—as a single overarching, stable, system of relationships—cannot be shown to exist. Rather, the answer is that economic conventions, processes, and organizations are part of the system of adaptation.

I have shown elsewhere how the farm budget algorithm can also describe aggregates of farm families: villages and whole irrigation commands. So applied, it provides accurate analyses of current production and of the impacts of possible changes (Leaf 1998: 75-81). But I would not on that account claim that this is an analysis of a "society" in the way that Greif, Milgrom, and Weingast suggest for their analysis of Genoese merchant guilds. Although society-wide in jurisdiction (if Genoa can be taken as a society), the guilds were still one kind of organization among many. Genoa was also a republic; it was also (depending on the exact time) part of the emerging Holy Roman Empire, and it also had a religious organization. Counterparts in the Mahi Right Bank project would be the Anand Dairy Cooperative, the district revenue organization, and the irrigation command organization, among others. A complete analysis of the whole region, or society, would require recognizing the farmers' productive algorithms, the dairy production model, the irrigation department's model, and those of all the other organizations that contribute to the production and transfer of people and goods, and then, once all of this was done, there would need to be the third analytic level to show how they interact.

This third level is Smith's overall division of labor and the flow of goods it produces, the true wealth of nations, which he did not represent fully or in anything like a computable form. In his time such a representation was inconceivable. But not now. It would not be a game, but it would be either a system model or, better, a multi-agent model in which the agents would represent all of these organizations with all of their algorithms, and their decisions would model the flow of materials and people that results from using them. The equilibrium, if any, is therefore not inherent in the decision rules *per se* but rather in the way the rules lead the agents to interact with their environmental constraints. If these relationships are sustainable, the community persists. If not, it fails. Lansing and Kremer (1993) and Axtel et al. (1999) demonstrate the power of such models to post-dict the actual layout of physical resources in a landscape. Cathy Small's *TongaSim* shows how such models can represent the application of multiple algorithms in a single population. This is a multi-agent model of Tongan society intended to simulate the relationship between warfare and traditional rules for marriage. It is described in "Finding an Invisible History: A Computer Simulation Experiment (in Virtual Polynesia)" (1999). One algorithm describes marriage choice based on ideas of hypergamy and gift exchange called *fahu*. The other algorithm is for increasing territorial control by warfare. The warfare model is a switch, so the model can be run with only the marriage rules or with the two sets of rules running side by side, and the different outcomes can be compared. Obviously, we are a long way from being able to do this for something as large and complex as the Mahi Right Bank irrigation system, but it is not because of any problem in principle. It is mainly that we do not have a data-set as massive and fine-grained as this kind of analysis would require. But equally obviously, leaping from classroom experiments to hypothetical game-theoretic analysis supposed to represent entire societies while ignoring the different kinds of organizations at play within them is not a step on the way to getting there.

Anne Mayhew once characterized North's description of the origins of agriculture as "fiction"

(1982). There were two main reasons. First, it was not consistent with the ethnology and archaeology. Second, when one understood the ethnology and archaeology it was self-contradictory. North's account at that time had nothing to do with game theory, but did rest on the same underlying substantive slipperiness. The challenge still holds. Should economics be ethnologically responsible? Will it produce better economic analysis if it is? I agree with Mayhew, and go further. Economic systems are no less systems of culturally established ideas and their uses than kinship, government, or religion, and they need to be studied as such.

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