The Uses of Economic Theory: Against a Purely Positive Interpretation of Theoretical Results

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Economists are excessively influenced by the so-called positive economics view, which says that economists should only describe and not prescribe. Here I argue that this view is flawed because it makes unreasonably strong assumptions about what players (the agents taking economic decisions) know and understand. I then use the example of micro-credit to show how this bias towards positive economics has distorted the policy debate.

1. Introduction: The question.

*"The philosophers have only interpreted the world in various ways;
the point is to change it."*

This oft-abused quote from Karl Marx[2] captures well the dilemma of economic theory: A theorist sees economic reasoning everywhere—from the trader deciding when to buy and when to sell, to the farmer setting the terms for his tenant, to the village money-lender figuring out how to lend most effectively, to the government department deciding how to auction off logging concessions, to the businessman figuring out how to deal with his regulators. What should she make of what she sees? Should her presumption be that the players have chosen their strategies as economists would have chosen them? Or should she try to influence their choices, on the presumption that she knows better and they would be better off if they took her advice. To put it differently, as an analyst, should she see her problem as being to find the economic environment in which the observed choices are the best response, or is it to try to influence the choices, presuming that she more or less understands the game that is being played? Should she merely interpret the world or try to change it?

This is of course the old question of whether economics is a positive or normative discipline. It is question that every practicing economist has had to come to terms with at some level, but for the most part the way it has worked is that each individual sub-discipline within economics makes its own choice. For example, the related sub-fields of positive political economy and institutional analysis are explicitly focused towards interpreting the world;[3] on the other hand, the sub-fields of market design and social choice, are, by the nature of their project, focused toward developing better trading institutions and better governance structures.

Other sub-fields, such as development economics, are less clear-cut. While there is a long tradition of institutional analysis (why do we observe sharecropping or bonded

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[2] Marx (1888).
[3] Canonical examples of this style of research include Stigler (1986), Olson (1965), and North (1981).

labor?) there is also a disquiet with this methodology, as reflected, for example, in the work of Robert Townsend. Townsend has done important work in the positive economics tradition,[4] but in his 1995 paper on "Financial Institutions in Northern Thai Villages", after observing that institutional performance varies widely across villages, he deliberately stops short of explaining the difference in performance. He suggests that perhaps no one has taken the initiative to develop the institutions in the worst-performing village. While he does not say so, this clearly opens up a space for an economist interested in promoting better institutions.

However, even within development economics, methodological discussions, or even explicit statements of methodological stances, are rare. This has the advantage of avoiding the endless methodological disputes that afflict the other social sciences, but, as I will argue in the coming pages, it also contributes toward muddying a number of important policy issues.

My strategy in this paper is as follows: I begin with an extended discussion of pure positive economics, as the one clear methodological stance that one finds among economists. I will argue that, at best it applies only to a quite limited domain within economics. The problem then is where to draw the boundary between description and prescription. I have, regrettably, no formula to offer on this point—what I do instead is to go through a case-study of the current debate on micro-credit, which, I will argue, is one particular instance of how an ill-drawn boundary can muddy the waters. I conclude with some admittedly loose thoughts on how better to draw the boundary.

2. Positive Economics?

The classic answer to the question posed at the end of the opening paragraph of the paper is, of course, Milton Friedman's plea for a positivist economics: Stick to interpreting the world---the world does not need your help. His argument relied on his famous analogy between economic actors and an expert billiard player. The billiard player does not need

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[4] See Townsend (1993) on the medieval economy.

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or even want to study physics in order to be able to clear the table—his intuition, built up from his experience on the table, serves. The economic agent, likewise, has strategy at his fingertips, though he may find it hard to articulate the theory behind his choices.

There are a number of reasons why I do not find this analogy particularly persuasive. First, because a billiard player acquires his expertise at the game by many hours of practice, where he tries out many different ways of making the same shot and finally discovers the one he wants. The analogous activity for a moneylender would be to try out different ways of organizing his lending operations. The problem is that he would need other people to participate in these trials and he would have to pay them for their time. Moreover, the stakes for the people participating in these trials have to be large enough to make it worthwhile for them want to play seriously (in this case, this involves thinking of ways of making it hard for the lender to collect). Running trials is therefore costly and it is not obvious that the moneylender can afford to run enough of them to know exactly what he needs to do.

Second, championship billiards, like all other sports, affords a very small margin of error. The ball has to go into the pocket and not merely in the vicinity of the pocket. This is what makes it hard to base one's strategy in billiards on an analytical model based on the laws of physics: To achieve this kind of precision the model would need to take account of a lot of very particular facts about the player and the setting, and at least some of those facts are nearly impossible to quantify---the feel of the surface, the way the player's arm moves, his posture, his stance, etc. If one misses or misjudges any one of them, the ball will miss the pocket. This is why good billiards players have to practice for endless hours and this is why most of us will never be good at billiards. Most economic judgments, by contrast, do not need to be exactly right—one just needs to get close enough. Therefore even analytical methods that offer only qualitative answers ("make sure that the collateral has value in those states of the world where the borrower will want to default"; "first price auctions tend to dominate second price auctions when the buyers are risk-averse") or loose quantitative answers ("the inflation rate will go up by 5 to 6%") may be good enough.

Finally, even if it were possible to significantly improve one's economic judgment over some narrow domain by long practice, it is not at all clear that it would be in the economic agent's interest do so. While a great billiard player has no desire to help others play like him, the moneylender, auction designer or compensation consultant has a stake in knowledge that is easily taught. He wants a single formula that can be used in a range of situations, so he can delegate part of his job to others and extend the scope of his business: General principles are more useful to him than the exact solution in one specific case.

A more useful analogy, to my mind, is to think of economic decision-making as a craft, not unlike fishing or small-scale garment manufacturing. Like fishing and garment making, it is a craft that needs to be learned. A money-lender has to learn how to judge his potential customers—are they people who can be trusted, will they know how to make fruitful use of the credit he is offering them? A trader has to learn how to interpret market signals, and a businessman has to figure out how to negotiate with the regulators.[5] Moreover, like fishing and garment making, most people learn these things informally: There is no school for learning to be a money lender or a street vendor (or to take a more extreme example, for learning how to pay bribes) just as there is no school for would-be fishermen or petty garment producers. Finally, the craftsmen and the economic agent (read petty businessman) are often one and the same person, at least in developing countries: farmers lend money, fishermen also trade in fish, and the garment maker also runs a garment shop and deals with the regulators and the tax-men.

The statement that economists should not try to change the way people do business, if translated to the case of any of these other crafts, would amount to saying that the craftsman has nothing to learn from a technical specialist. Or at least that the technical expert has no autonomous role—if there were some know-how that the craftsman would have wanted from the technical expert, he would have already bought it from him.

I think it is reasonable to say that this is an excessively optimistic view of the market's

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[5] This is true even when the regulators are honest. There is usually a judgment call involved somewhere in the regulation process, and the businessman who does not know how to present his case effectively is always more likely to get into trouble.

role in promoting innovation in the crafts. As a point of fact, many of the new high-yielding varieties in agriculture were developed by agricultural scientists working for governments and international organizations, despite the fact that the basic techniques used in developing them were based on the idea, familiar to every farmer, of hybridization. The same is true of the development of chemical fertilizers: A large number of basic advances came from publicly funded research laboratories in mid-19th century Germany.[6]

This is, of course, exactly what we would expect. The standard reason why the market may under-supply innovations is the inadequacy of intellectual property right protection.[7] This is clearly an even bigger problem for craftsmen than it is for large corporate innovators with their armies of lawyers and other resources. In addition, at least some innovations rely on specialized scientific knowledge that the craftsman does not have,[8] and given his scale of operations, it does not pay to employ someone who has that knowledge.

Even when a better technology becomes available in one of the crafts, it takes a long time before it becomes widely implemented. This was the experience of hybrid corn in the U.S., which took many decades before it became universally accepted.[9] High-yielding varieties of wheat took over two decades before it covered most of the wheat-growing regions in India and HYV rice took even longer.[10] Conley and Udry (2000) argue that most pineapple producers in Ghana use less than the optimal amount of fertilizer, even though profits are sharply increasing from fertilizer use over much of the observed range of usage. Slow adoption of the latest technology has also been observed in many of the other common crafts, such as fishing[11] and leather-processing.[12]

In part, the slow diffusion reflects the limitations of the new technology—it might need

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[6] See, for example, Mokyr (1990).
[7] This is not the only reason. For a much more detailed discussion of the issues, see Aghion and Howitt (1998).
[8] Chemical fertilizers, for example.
[9] See Griliches (1957).
[10] See Munshi (2000).
[11] See Ahmed (1992).
[12] See Banerjee and Nihila (1995).

too much capital, or skills that the farmer does not have and cannot buy. Or it may be very risky. But it is likely that there is also a lot of (*ex post*) unjustified resistance to new ideas. For example, there is now a lot of circumstantial evidence[13] and some recent more direct evidence[14] that farmers base their decisions on what other farmers are doing. It is well known that this can lead to inefficient herding, with each farmer deciding against the new technology because no one else has gone for it.[15] Not surprisingly therefore, most countries have publicly funded extension programs which employ experts to persuade people to switch to the better technology, and NGO's have traditionally seen the dissemination of best practice technologies as part of their brief.

Note that the case I am making for the role of experts does not rely on the fact that technical experts are always right (I certainly would not want to be put in the position of claiming that economists are infallible).[16] As long as there is some mechanism for empirically evaluating and screening out their bad ideas, it is enough that they are right reasonably often and that the value of their good ideas outweighs the cost of trying them out.

For some or all of these reasons, most economists do accept the idea of an autonomous role for experts in the crafts, though they may question the efficacy of specific programs for promoting better technologies.[17] Why then should the craft of economic decision-making be any different?

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[13] See Foster and Rosenzweig (1995) and Munshi (2000).

[14] See Conley and Udry (2000).

[15] See Banerjee (1992) and Bikhchandani et al. (1992).

[16] This is not to say that experts do not often get it wrong or that they have nothing to learn from practitioners (see World Bank, 1999, page 38, for a nice example of how women farmers in Rwanda and Colombia helped agronomists evolve better varieties of beans).

[17] To take an example from an area where there is a lot of research, Evenson and Westphal (1995) in their survey of "Technological Change and Technology Strategy" in the Handbook of Development Economics report that the mean rate of return to public investment in agricultural research is 80% in all developing countries if one includes the output of the international crop research centers, and around 50% if one does not include them. They also suggest that the attitude towards agricultural extension programs had shifted between the 1970's and 1980's. In the 1970's, the view was that extension programs were only valuable when there is a new technology, but the emerging consensus in the 80's and 90's was that there is scope for substantial improvement in farming practice even without new technologies, especially in sub-Saharan Africa. They then summarize the results from 23 studies of public sector agricultural extension programs and report that 13 of them report rates of return of over 50% and that mean rate of return reported in these studies is 50% for developing countries and 63% for developed countries. While many of these estimates of the rate of return suffer from potential endogeneity problems, these results clearly reflect the common sense of the discipline.

As I see it, there are three potential answers to this question. The first is an *a priori* answer that may go something like: All the decision-maker has to do is to solve an optimization problem or, at worst, a game, and if he does not know how to do it he can and will hire someone to solve it for him. The economist does not have to offer him a solution.

This argument misses the point that what the economist does best is not to solve problems, but to explain what the problem needs to be. Mirrlees' 1971 paper on optimal taxation was so influential because he showed how one would set up the problem in a range of situations, not because he solved a complicated maximization problem. The fact that now any graduate student knows how to set up problems like Mirrlees' problem does not mean that, *ex ante,* it was not a major insight. Indeed, figuring out the right way to set up a problem is not unlike inventing the internal combustion engine. The basic principles that went into the early engines were all well known from other applications (according to Mokyr, 1990, the Chinese might have known them for at least a millennium) but someone had figure out how to put them together.

The economic decision-maker who faces a new problem has to find the model that describes best his situation and then translate the model into a specific problem that can then be solved. We have, however, no reason to believe that he knows how to do so or even that he knows how to pose the right question to someone who could help him.

The second argument questions the economist's motives. Why should we trust him to give objective advice? My sense is that this is not very different from the case where it is the plumber who is offering the advice. We trust the plumber (when we do) because he has a reputation, because he knows that there will jobs in the future, because he has a conscience. Why would it be different for an economist? In other words, as O'Flaherty and Bhagwati (1997) have argued in a very similar context, it is not uncommon for the policymaker and his advisor to have interests that more or less coincide. Moreover as Basu (2000) has argued, if it were indeed true that economists never give useful advice because they always have an axe to grind, then people will stop asking them for advice---

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this cannot be a reason for the economist to not offer his advice, if asked.

The third argument for why the craft of economic decision-making is different from other crafts (and therefore why economics needs to be a positive science) is much harder to dismiss. It is simply the empirical claim that the average economic problem is simpler than the average technical problem. While there are obviously difficult economic problems (the problem of designing the optimal multi-good auction that has attracted so much attention in recent years, for example), I know of no way of refuting the general claim. I will therefore take refuge in one relatively elaborate example, inspired by the current policy debate about micro-credit. I will argue that this debate is less useful than it could be, because most of the participants do not recognize the very real complexity of the design problem involved in coming up with a really effective micro-credit scheme. In other words, complexity in economics problems is certainly important enough to be a practical problem.

3. An Example: Micro-credit.

Micro-credit is now famous as the revolutionary new tool for poverty alleviation. There are micro-credit organizations everywhere from Arkansas to Zimbabwe.[18] In Bangladesh, almost all anti-poverty programs are in the hands of micro-credit organizations. The Grameen Bank alone has more than 2 million members who receive $30-40 million in loans every year. In India, the government has decided that all the new initiatives for providing income-generating opportunities for women be based on micro-credit. The president of the World Bank, Mr. James Wolfensohn, has been talking about a half a billion people becoming beneficiaries of micro-credit by the year 2005.

Micro-credit involves making credit available to the poor at interest rates that, though still high, are much lower than those charged by money-lenders. The excitement about micro-credit comes from the very high repayment rates (90-95%, or even more) that

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[18] The most prominent examples include Grameen Bank and BRAC in Bangladesh, BRI and BKD in Indonesia, BancoSol in Bolivia, and FINCA in Latin America.

many of these organizations have been able to secure,[19] which make it possible for them to lend money at these relatively low rates and still remain economically viable.[20] There are three main features that distinguish this breed of micro-credit organizations from the long line of failed attempts to lend to the poor, usually under the aegis of the government. The first is that each loan comes with a promise of repeat lending, if repaid on time. The second is regular repayment schedules—repayment starts immediately after the loan is disbursed and the borrower has to repay a fixed proportion of the loan every week (or month). The last, which is somewhat less universal, is some form of joint liability: The loan is made to a group, which is supposed to make sure that every loan to members of the group gets repaid. If some loans are not repaid, the entire group pays a cost (for example, they may be temporarily or permanently suspended from getting more loans).

As one might expect given the hype, there is a lot of controversy about exactly what micro-credit does and does not do. On the one side, there are a number of journalistic accounts that claim that micro-credit is nothing short of a miracle. It is credited not only with raising incomes among the very poor (and especially among poor women, the most vulnerable group in most societies), but also with diverse forms of consciousness-raising.[21]

Some of these optimistic claims are supported by results from an econometric study of Grameen Bank by Pitt and Khandker (1998). They use an identification strategy based on a discontinuity in the Grameen Bank's rules for being eligible to participate in their program, and conclude that participation in the program raised household consumption by 18 cents for every $1 lent to women. They do not, however, find any effect on contraception use (a measure of "consciousness") and a slightly *positive* effect on fertility.

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[19] There is some dispute about the basis for calculating the repayment rates. Morduch (1998) convincingly argues that the Grameen Bank's claims of repayment rates of 98% or more are exaggerated. When he recalculates the repayment rates, they fall in the 92-95% range. As he himself acknowledges, these rates are still very high compared to the rates of 41% that Pulley (1989) reports for IRDP loans in India (to take a prominent example of an earlier attempt to lend to the poor).

[20] As we will see, what economic viability means is not always obvious.

On the other side are a number of sharply critical assessments, mostly of the Grameen Bank, which is seen by many to be the standard bearer of the micro-credit movement.[22] The most important work along these lines is that of Jonathan Morduch, who has written extensively both about the Grameen Bank[23] and, in two review articles in the *Journal of Economic Literature*[24] and in *World Development*,[25] about micro-credit more generally. Morduch makes two basic points. First, he points out that the discontinuity that is the basis of Pitt and Khandker's optimistic evaluation of Grameen does not show up in the data, invalidating their strategy.[26] Instead Morduch compares the difference between those who meet the Grameen Bank eligibility criterion and those who do not in a village where Grameen has an operation, with the corresponding difference in a village where it does not have an operation. He comes to the conclusion that participation in Grameen Bank actually had an insignificant *negative* effect on household consumption. However, his difference-in-difference approach does not deal with the key issues about program placement. If Grameen Bank focuses on areas where the inequality between the rich and the poor is the greatest, Morduch's estimate will be biased downwards. It is worth noting however that Coleman (1999), using data from North-east Thailand, and an empirical strategy similar to the one Morduch uses, also finds no impact. He argues that he does not need to worry about program placement issues because the villages he chooses as controls were also chosen to be included in the program, albeit in the future. Even if we were to accept this, there remains the concern that the sorting into bank members and non-members in a village where the program has been running for several years (his treatment group) is very different from the sorting in a village where the program has not even started (his control group). My sense from all this is that we have no reliable estimates of the effect of the program on income and consumption.

The more damaging point that Morduch makes about the Grameen Bank is that it is not actually economically viable. Morduch (1999) reports calculations showing that the Grameen Bank loses money at the rate of about 10 cents for every dollar of credit

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21 For examples of this genre, see *New York Times* (1997), Bornstein (1996), and Counts (1996).
[22] Though many within the micro-credit community are highly critical of Grameen Bank.
[23] See Morduch (1998).
[24] See Morduch (1999).
[25] See Morduch (2000).
[26] See Morduch (1999).

outstanding.[27] This reflects a pattern among micro-credit organizations—very few, even among the most prominent and successful organizations, break even or make money.[28] For many (for example, CGAP, a donor consortium housed within the World Bank;[29] Otero and Rhyne, 1994, who are micro-credit practitioners opposed to the Grameen Bank model; or Remenyi, who has written a recent survey article called "Is there a 'State of the Art' in Microfinance"[30]),[31] this damns them. Their credo is that no program is worthwhile if it does not have financial sustainability without subsidies. Morduch criticizes this view, arguing that what matters for efficiency is a hard budget constraint and not the absence of subsidies.[32] But at the end of his review article,[33] he too sounds pessimistic: He fears that in the absence of a "second major wave of innovation", the micro-credit movement will end up limited to serving the richest among the poor.

In one sense, I agree with Morduch's assessment. I too believe that there has to be more innovation if the movement is to survive. My sense is, however, that what Morduch means by a "major wave of innovation" is more demanding than what I have in mind. One of things I will argue in the rest of this section is that the micro-credit movement has been hampered by the fact that economists have tended to take the institutions at face value. If economists can be persuaded to be more involved in suggesting other ways of doing things, perhaps the next wave of innovations is not far away.

In this sense I am perhaps more optimistic than Morduch. In another sense however, I am more pessimistic. Morduch argues for slow decline, as donors eventually give up. I, on the other hand, put a significant probability on the possibility of a quick collapse, driven by the internal logic of repeat lending. Once again, in a way that will become clear, my view is different because I take the theory seriously.

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[27] In a related vein, see Matin (1997), and Jain (1996).
[28] See Morduch (1999).
[29] See CGAP (1996).
[30] See Remenyi (2000).
[31] Yaron et al (1998), who are from the World Bank, take a similar view.
[32] See Morduch (2000).
[33] See Morduch (1999).

3.2 A Simple Model of the Credit Market.

Before we enter a discussion of these views of micro-credit, it is useful to set out a simple model (based on Banerjee, Besley and Guinnane, 1994) that outlines the key issues.

3.2.1 The setup: Consider a world where there is an investment opportunity which requires an investment of 1 whose gross returns are $R(p)$ with probability p and 0 otherwise. There is an investor who has wealth $W < 1$ and needs to borrow the rest.

There is a competitive capital market in this economy and the (gross) cost of capital in that market is R. The source of distortion in this economy comes from the fact that p is a choice for the investor but is unobserved by the lender. $E(p)=pR(p)$ is a concave function of p with maximum at p^*.

The only possible contract is a loan contract. The contract specifies an interest rate r that the borrower pays the lender when his project is successful. When it is not successful he pays nothing, since he has nothing and there is limited liability in this economy. We justify the restriction to just loan contracts by the argument, originally due to Diamond (1989), that verifying the realized return is so costly that it uses up almost the entire return.

3.2.2 The incentive problem: In this world the borrower, who is assumed to be risk-neutral, has to choose p to maximize $E(p)-pr(1-W)$. It is clear that the borrower will choose p such that:

(B) $$E'(p) = r(1-W)$$

For $W < 1$, this is quite obviously inconsistent with the social optimum: The borrower clearly wants to choose $p(r,W) < p^*$. In other words, there is a tendency towards too much risk-taking.

This is the standard incentive problem in credit markets. It arises because society cares

about net output but the borrower only cares about what remains after paying interest.

Using the fact that the *E(p)* function is concave, it is easily shown that *p* is increasing in *W* and decreasing in *r*. This tells us that people who are more leveraged and people facing higher interest rates will tend toward less efficient choices. This ought to be intuitive—both higher interest rates and more leveraging tend to increase the degree of misalignment of incentives between the borrower and the lender.

3.2.3 The role of monitoring: The lender may want to limit the distortions that arise from this misalignment by monitoring the borrower.

Suppose the lender can force the borrower to choose a project no lower than *p* by monitoring him enough. A natural assumption here is that the cost of monitoring, *M,* is increasing in the amount borrowed and in the distance between what the borrower want to choose, *p(r,W),* and what the lender wants him to choose, *p*:

$$M = cM(1-W, p - p(W,r)), M_1(.) > 0, M_2(.) > 0.$$

where *c* is a parameter that measures the efficiency of the monitoring technology.[34]

3.2.4 Properties of the credit market equilibrium: Using this monitoring cost function and the fact that the credit market is competitive, which implies that lenders should not make pure profits, we can write the lender's participation constraint as:

(PC) $$r = \frac{R}{p} + \frac{cM(1-W, p - p(W,r))}{p(1-W)}$$

For any given *p* that the lender chooses, this equation determines *r*.

The key fact that I want to emphasize about this equation is that *r* enters positively on

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[34] This formulation of the monitoring technology is consistent both with monitoring being *ex ante* screening of projects and it being an *ex post* check on the borrower. Aleem (1990) describes these as the two most

both sides of this equation—an increase in the rate of interest that he has to pay inclines the borrower towards greater risk-taking, which increases the need for monitoring. Monitoring being costly, this pushes up the interest rate even further.

An implication of this observation is that the interest rate can be highly sensitive to changes in the efficiency of monitoring. Formally, using the equations (B) and (PC), we get:

$$\frac{d \ln r}{d \ln c} = \frac{1}{1+X} \frac{1}{1+Y},$$

where X and Y are defined as

$$X = \frac{R(1-W)}{cM}$$
$$Y = \frac{cM_2}{pE''}$$

Y is negative because E'' is negative. When it is close to -1, the elasticity of the interest rate with respect to the cost of monitoring will be very large, as claimed above.[35] More generally, a change in the cost of monitoring has a multiplier effect and the ultimate change will be larger than the change that sets it off.

Moreover, since we have kept p fixed, the fall in the interest rate resulting from a fall in c is entirely due to the fact that we are using less resources in monitoring, which makes it a pure social gain. *In other words, if 1 + Y is close to 0, there is the prospect of large social gains from small improvements in the technology of monitoring.*

Is there any reason to think that the multiplier is large (i.e. that $1 + Y$ is small), at least in some places? I am not aware of any direct evidence on this point: To the best of my knowledge, there is no study that tells us how much each individual loan costs to monitor

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important and costly things that a lender has to do.
[35] The argument here assumes that p does not change when c changes. However in Banerjee (2002) I show that the same argument goes through even when p is endogenized. Intuitively, a cut in p does reduce the need for monitoring but this is counteracted by the direct effect of p going down, which is to raise p.

(which is what we need to estimate cM_2).

The closest thing to such a study of which I am aware is Aleem (1990), which gives the average amount spent monitoring loans per money lender, based on a sample from Chambhar in Pakistan. While this is quite far from what we want, Aleem's study does give us some circumstantial evidence that is suggestive. First, real interest rates in his sample are very high; and high monitoring costs are the reason. The average annual nominal interest rate he reports is 78.65% and the average annualized cost of lending for his sample of money lenders is between 68% and 79%, out of which 32% is average cost of capital. The rest, i.e., between 36 cents and 47 cents per dollar lent, was the cost of monitoring. This is clearly good news from the point of view of arguing that there can be large variations in the amount of resources devoted to monitoring.

Second, and more importantly, interest rates in his sample vary enormously. The standard deviation of the observed interest rate is 38.14%, which given the mean of 78.65%, says that interest rates between 0% and 160% are within two standard deviations of the mean. By contrast, interest rates for real estate lending in the U.S. rarely go outside a band of 4%. One obvious candidate explanation of the variation in Aleem's sample is that it comes from variations in the technology of monitoring (some people are old customers who are easier to monitor, etc.) combined with a large multiplier.[36]

None of this, of course, comes close to proving that our conjecture about $1 + Y$ is correct. One alternative possibility, for example, is that the interest rate varies so much because people borrow very different amounts. Note, however, that from equation (PC), increasing the amount borrowed, $1 - W$, has an unambiguous effect on the interest rate—which perhaps makes this explanation less compelling.

There is no reason, however, to push this point any further: Our goal here was simply to establish the *prima facie* possibility that reducing the cost of monitoring borrowers has

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[36] Aleem's study is unique in actually measuring the different elements of monitoring costs. The two basic facts, that there appears to be large gap between the interest rate charged and the cost of capital (a large part of which is likely to be monitoring cost) and the fact that the interest rate varies a lot, show up in many other studies as well. Banerjee (2002) summarizes the evidence from a large number of such studies.

large welfare benefits. We now return to the discussion of micro-credit.

3.3 Implications for Micro-credit.

3.3.1 Group Lending?

An on-going debate within the micro-credit movement is between those who believe in group loans à la Grameen Bank and those who believe in individual loans à la BRI. Policy-oriented economists thinking about micro-credit, such as Morduch in his review article and Remenyi in his report on the "State of the Art", discuss the issue at some length, though both avoid taking a stand. The typical argument in favor of group lending, which is made both by practitioners and evaluators, is that the group is an efficient mechanism for monitoring.[37] Two reasons are brought up to explain why this ought to be the case. One is that there is social and physical contiguity among members of most groups, which makes it easier for them to collect information about each other. The second is that there is social capital within the group—members of the group can shame or bully each other into behaving well, while a banker can only use legal threats.

The case against group loans is in part doubts about whether group members do monitor each other, and in part worries about the cost of running a group. In addition, there is the suggestion by Madajewicz (1997) that group loans may constrain the dynamism of individual members.

It is unfortunate that the debate has gotten framed in terms of the relative advantages of group and individual loans. As I see it, the facts are:

1. Even small savings in monitoring costs may be valuable in this context because of the multiplier.
2. Most people agree that group members have some advantage relative to outsiders (bank officials, for example) in monitoring loans.[38] While most of the evidence on

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[37] Here monitoring is assumed to include both *ex ante* screening and *ex post* monitoring of the project.
[38] See Guinnane (1994) for a discussion of the contrary view that sometimes the closeness of the group may actually make monitoring harder.

this point is anecdotal, [39] a recent paper by Karlan (2002) provides strong support for this view. He observes that the FINCA village banks in Peru are formed on a first come first serve basis and therefore their composition is more or less random, at least in the first few months after they have been formed. Some of these groups end up with a greater concentration of people who live near each other and/or are a part of the same socio-economic stratum. He finds that these village banks have a significantly better repayment record than the more diverse groups.

3. Anyone who monitors, whether a member of the group or someone who is hired for the task, has to have the right incentives. In fact, giving incentives to a hired monitor can be quite costly, as most bankers will acknowledge. Indeed this is what, presumably, a member of Grameen Bank was implying when, on being asked why the repayment rates were so high, she said:

> "….we go to the borrowers. Then we check up how the loan is being used. When we give a house loan, we watch the house go up, and we keep checking until it's finished. Only then do we say, 'Yes they built a house.' When Janata Bank and Krishi Bank[40] give a loan for a fish pond, they never watch whether anyone throws fingerlings into the pond", (quoted in Bornstein (1996), page 105).

4. Group loans or joint liability is only one way to give incentives to monitor. Indeed, it is only after the recent work of Ghatak (1999) and Armendariz de Aghion and Gollier (2000), that we understand why joint liability generates better peer selection in adverse selection settings.[41]

5. There is no evidence that the current "State of the Art" in micro-credit was arrived at through extensive experimentation. My sense is that despite there being hundreds of micro-credit organizations, they all started out with one of two models---group loans and individual loans. [42] This probably reflects the very strong influence of early successes like the Grameen Bank and BRI---indeed many of the later micro-credit

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[39] Morduch (1999) concludes that the very limited "hard" evidence we have on this issue tends to support these arguments.

[40] Janata Bank and Krishi Bank are two banks in Bangladesh that give loans to individuals.

[41] More generally, theoretical work on these issues is less than twelve years old---the classics of the field are Varian (1990) and Stiglitz (1990).

[42] There is much more variation and experimentation within the class of group-lending organizations. For example, there is a lot of experimentation in group size: Grameen Bank believes in groups of five but at the early stages also tried groups of 10 or more (Hossain, 1988). FINCA, at the other extreme, works with groups of 30 women. There is also some theoretical work on this issue of optimal group size (Armendariz de Aghion, 1999; Ghatak and Guinnane, 1999).

organizations actually set out to be Grameen replicators.

My view is that the economist's task in this setting ought to be to try to design mechanisms that make the best use of the monitoring advantage that some members might have. By narrowing down the problem to be one of identifying the trade-off between group liability versus individual liability, we are being positive economists— that is, we are allowing the world to dictate to us what the options should be. It is possible, and indeed likely, that neither of these options is really optimal and everyone should switch to some other mechanism. For example, given that not everyone's time is equally valuable, people should have the option of self-selecting to the contract that suits them best. If there are those who, as Madajewicz (1997) has suggested, who would be better off with an individual loan, the program should perhaps offer both individual and group loans and allow people to self-select. If the group meeting is seen as taking up too much time, perhaps the meeting should be radically abbreviated by limiting the set of options that are decided by the group.[43] Perhaps joint liability is unnecessary: Perhaps all we need is a structure where members of the group get rewards if they manage to prove that some member is lying. Perhaps group liability should be combined with some outside monitoring, or individual liability supplemented by reports from neighbors.[44]

One could go even further: We know from the work of Maskin (1977) and others on Nash and Sub-game Perfect implementation theory, that if two or more people know the same fact, it is usually possible to get them to reveal it, by making them play a message game. Given that the efficient extraction of information from the members is an integral part of the group-lending strategy, one should consider bringing some of these message games into praxis of micro-credit. In general it is clear that there is potentially much good work to be done, once economist theorists recognize that they have something to offer here.

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[43] ASA, a new micro-credit organization in Bangladesh, has taken this approach (everyone gets the same size of loan, on the same day, etc.) and has been able to cut costs and grow fast (see Morduch, 1999; Rutherford, 1998).

[44] These doubts are reinforced by a series of new papers by Laffont and N'Guessan (1999), Laffont (2000) and Rai and Sjostrom (2000), showing that in a wide range of cases, group liability is dominated by other types of mechanisms. These papers do not however always make clear whether they see their task to be interpreting what Grameen Bank type institutions actually do or suggesting new ways of organizing micro-credit.

By opening up the discussion to include all of these different mechanisms, I hope to underscore the fact that there are a lot of potential ways to organize micro-credit, of which only a few have been tried. While there is a lot of discussion of the need for experimentation, as far as I know, micro-credit organizations have either pure individual liability or pure group liability. One could argue that this reflects the superior understanding of those who started micro-credit organizations, but given that even trained economists are only beginning to explore this class of mechanisms, this seems implausible. I therefore see this as evidence that there is considerable scope for innovative thinking by economists in this area.

3.3.2 Repeat Lending

Rather surprisingly, the feasibility of enforcing credit contracts through repetition is rarely questioned. Morduch (1999) does have a lengthy discussion of the issue but he focuses on the implications of competition and the possibility of people switching to other lenders after they default. This seems to me to be the less serious issue: As pointed out by Ghosh and Ray (1996), the fact that borrowers who switch have to acquire a reputation *vis á vis* their new lenders (otherwise they will get small loans), should discourage people from adopting this strategy.

I am more concerned about the possibility suggested by Bulow and Rogoff (1989) in the context of sovereign debt. Their argument is that there will typically be a limit to how much a borrower can ever get in loans, if only because the marginal product of capital goes down with more and more investment. Knowing this, the borrower will want to wait till he can borrow the maximum amount and then default and put the money in the bank—once he knows that he will only get the same amount next period, there is no incentive to repay.

This is potentially a very serious problem for micro-credit organizations, which typically have no other way to punish defaulters. In fact, it affects equally the organizations that deal with very poor borrowers and those like BRI that deal with somewhat more affluent borrowers. Individual loans may be somewhat more affected by it than group loans,

since it is more likely that there is someone in the group who has not "maxed out" yet, but essentially it is bad news for everyone.

The fact that this is not taken seriously once again reflects the tendency to take the positive view: This does not yet seem to be a big problem in the world, therefore it is not a big problem. There may be many reasons why this has not yet been a big problem. The pessimistic view is that no one has figured the optimal default strategy yet. After all before Bulow and Rogoff (1989), economists routinely argued that repeat loans can sustain sovereign lending. The optimistic view is that it reflects some structure of the world, hitherto undiscovered. Perhaps people are afraid that the Grameen Bank will use its considerable political clout against them if they were to default. Perhaps they fear that after they default they will be flush with money and this will induce them to spend it too fast and then they would rue the fact that they cannot borrow any more. Perhaps there is some social sanction against people who deliberately default against an organization that is seen to be contributing to the public good.

Knowing which of these is the right story is, of course, central to what advice we offer to micro-credit organizations. Notice that this takes us back to descriptive economics, but perhaps with a list of questions that a purely positive economist may not have thought to ask. Indeed it is evident that normative economics can only function on a base of positive knowledge---before we design mechanisms, we clearly need to know what we can take as given. The distinction is in what questions are interesting and in what is considered to be the scope of positive knowledge---positive economics, as practiced, rules out positive knowledge of the form: "People have not yet figured out how to default profitably." Normative economics, like experimental economics, thrives on precisely this type of fact.[45]

3.3.3 Subsidies

Subsidy is clearly a dirty word in the world of micro-credit. Those who cannot live without subsidies, adopt elaborate ruses to minimize dependence on subsidies,[46] while

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[45] Although this example is not exactly ideal since most normative economists would baulk from advising people how to default.
[46] For example, the Grameen Bank claims that it made a profit of $1.5 million per year in the 1985-96

those who can avoid subsidies are often very critical of those who need them.[47] Morduch discusses the fact that Grameen Bank will probably need to raise the interest rate in order to stay afloat in the long run. He sees the cost of this being in terms of equity, since the poorest, he feels, will drop out. Remenyi in his discussion of the "State of the Art" is more categorical in asserting that micro-credit organizations must raise interest rates to achieve financial viability, which is also the position taken by CGAP.

Yet the one immediate implication of the theoretical discussion above is that subsidies here promote efficiency. A subsidy that will pay for a 1% reduction in the interest rate will actually generate a substantially larger actual fall in the interest rate through its multiplier effect. Conversely, when Morduch calculates that the Grameen Bank will have to raise its interest rate from 20% to 33% to break even, he takes the repayment rate and the cost of monitoring as given. Our theory, on the other hand, predicts that monitoring will go up and the repayment rate will go down, and therefore the interest rate will have to be raised by much more. This is perhaps why most financially viable micro-credit organizations have higher interest rates than 33% (BancoSol, for example, charges 48%, but since repayments have to begin immediately after the loan is given, the effective rate is almost twice as high).

Once again the disjuncture between the theory and the policy debate is striking. It is not that eliminating subsidies may not in the end be necessary,[48] but that there is no one who is talking about the efficiency cost that this will generate (CGAP's view, which Morduch criticizes, is that efficiency will go up once the interest rates are raised).

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period when, but when Morduch recalculates their profit using more conventional accounting norms, it turns out that they were losing $18 million every year.

[47] For example, see Otero and Rhyne (1994).

[48] Though this too is in some sense too quickly accepted. There is a lot of spending which is nominally for the poor in the world, most of it through inefficient subsidies which often do not go to the poor. The micro-credit programs are relatively successful in targeting the poor (see Amin et al., 1999, on targeting by the Grameen Bank), and they use relatively little subsidy for every dollar delivered to the poor (see Khandker, 1998). So why is it obvious that we should not try to eliminate all other subsidies and keep this one?

4. Conclusion

How then, does one practice normative economics? One cannot always be an expert, always know more than the people who are directly involved. There will always be situations—indeed perhaps this is the majority of situations—where positive economics is the right answer: We should just assume that people are right in what they are doing and try to understand what makes that the right answer. Where does one draw the line?

I have no recipe to offer, nor do I believe that there is such a recipe. I have only the following rules of thumb: Trust one's instincts (and the theory that goes into that instinct), if something looks wrong, follow it up. Ask whether there is any *prima facie* reason to assume that the actors know what they are doing—are there only a few options, are all the options more or less similar, has there been a lot of experimentation, does the discussion show that people are aware of the alternatives? If not, perhaps something is indeed wrong and people can benefit from the advice. Be brave: It is true that we can make costly mistakes and hurt people whom we want to help, but the alternative may be to let them remain in the mire forever because we were too squeamish to tell them what we knew.

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