

**Reply to Jonathan Morduch's
"Does Microfinance Really Help the Poor?"
New Evidence from Flagship Programs in Bangladesh"**

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I benefitted from the comments of Shahidur R. Khandker, Andrew Foster, Nidhiya Menon, and Chris Udry.

I. Introduction

This paper addresses the concerns raised by Jonathan Morduch in his paper “Does Microfinance Really Help the Poor? New Evidence from Flagship Programs in Bangladesh” about the methods and results of Mark M. Pitt and Shahidur R. Khandker, “The Impact of Group-Based Credit Programs on Poor Households in Bangladesh: Does the Gender of Participants Matter?” in the *Journal of Political Economy*, 1988, Vol. 106, No. 5, hereafter referred to as PK. Morduch makes five major criticisms of PK:

- 1)The eligibility cut-off of one-half acre of land owned is often violated, and this violation results in substantial bias in the PK estimates;
- 2)The restrictive way in which PK introduce village fixed effects to control for important components of unobserved heterogeneity fails to distinguish between eligible and ineligible households and thus is likely to result to bias;
- 3)“New evidence” in the form of simple difference-in-differences estimates using both de facto and de jure eligibility rules fail to find positive program effects;
- 4)The higher positive impacts of female credit as compared to male credit on household consumption may simply reflect diminishing marginal returns;
- 5)The linear functional form used by PK is insufficiently flexible. Nonlinearities in the effects of explanatory variables may cause the PK estimates to be positively biased.

Section 2 of the paper addresses each of these criticisms and finds them lacking because Morduch has misunderstood and mischaracterized the methods of PK, and has applied incorrect methods to obtain his new evidence. In order to present the reader with a clearer demonstration of the methods adopted by PK, as compared to those of Morduch, an appendix provides a set of

simulation programs in the form of Stata™ do-files that simulate the processes by which these credit programs, by gender, are allocated across villages, how eligible household make participation decisions, and how these participation decisions affect household outcomes (such as consumption).

II. Response to Morduch's criticisms

A. "Mistargeting" of program eligibility biases the results

Morduch notes that a significant proportion of program households report owning more land than the *de jure* eligibility amount of ½ acre. The eligibility rule of these credit programs is in terms of cultivable land. Table 1 presents data on the size distribution of cultivable land owned by program participating households at the time they joined a credit program. In the aggregate, a bit over 17 percent of program households had more than ½ acre of land at the time they joined a credit program. The Grameen Bank, with 21.1 percent, has the greatest proportion of "mistargeted" households. The difference between total land ownership and cultivable land ownership is primarily homestead land. Many poor households, even those without any cultivable land, often have a very small plot of land containing an extremely small shelter and perhaps a small garden. Some also possess uncultivable waste land. Average total land owned is only 14 hundredths of an acre larger than cultivable land owned.

Although Morduch makes much of land purchases by program households, they are in fact quite small on average in our sample. Table 2 presents the average total land and cultivable land owned at the time of the survey. These average landholdings increased by only 6 one-hundredths of an acre since the time of joining. Morduch reports that only half of the land acquisitions were purchases, most of the remaining acquisition were inheritance. If the average size of inherited plots were equal to the average size of purchased plots, purchases of land would amount to only 3 one-hundredths of an acre, or 121 square meters.

What constitutes "cultivable" land for the purposes of program eligibility is ambiguous. Many household have uncultivable or nearly uncultivable land, such as *char* lands that lie within river dikes. These lands cannot be cultivated during the peak season as they form part of a river

bed during that season. In the dry season, these lands are poorly suited to agriculture as a consequence of erosion, and property rights to them are poorly established since any plot markers and water containment bunds are washed away in the seasonal floods.¹ These credit programs are likely aware of the differential cultivability of plots of land, and might be expected to make adjustments for land quality in judging the eligibility of a household.²

There is a simple way to test whether this might be so with our data. The data set contains information on the current market value of land. If the credit programs take the quality of land into consideration in determining eligibility, that is, use some quality adjusted notion of efficiency units of land, then the unit price of land in participating households with more than ½ acre should be less than the unit price of land of other households with similar quantities of land. Table 3 presents results from estimating the determinants of unit land prices with our data. The results of five regressions are presented, two estimated by ordinary least squares and three with thana-level fixed effects. All specifications demonstrate quite conclusively that “mistargeted” households, that is, participating households owning more than ½ acre of total land at the time of the survey, have dramatically lower land values even after conditioning on total land area and its square, participant status and thana fixed effects. The mean unit value of land for those owning land is Taka 1484 per decimal. Consequently, conditional on the other regressors, mistargeted households have unit land values of about one-half that of households which are not mistargeted. To put this into perspective, the average unit value of all landholding for households owning

¹The importance of eroded char lands in Bangladesh is unfortunately quite evident and growing. On a recent trip to Rangpur district, I had the opportunity to visit one of our survey areas in which recent movements of the Teesta and Jamuna rivers has turned well-off farmers into destitute farmers in a few years. These farmers still own this newly eroded land and may plant some of it during the *boro* season, but its cultivation quality is so poor that it bears little resemblance to its pre-eroded state. One household in particular has gone from being one of the wealthiest cultivating households in the village to one of destitution in which the household head spends most of his time working as a wage laborer.

²Hossein (1988, p. 25) provides further evidence that land quality matters in the determination of eligibility. He notes that after 1983 “a person from a household that owns less than 0.5 acres of cultivated land, or assets with a value equivalent to less than 1.0 acre of medium-quality land, is eligible to receive a loan.” (my underlining)

more than 50 decimals of land is Taka 791 per decimal while the estimated decline in unit land value associated with being a “mismatched “ household is Taka 721 per decimal (column 5). All of which suggests that these credit programs are doing a better job of targeting than a first look at the data would have us believe.

This apparent use of efficiency units of land in determining eligibility on the part of these credit programs does not completely resolve the econometric issue of using eligibility criteria as the basis for parameter identification. The problem is that land quality is unobserved and thus we cannot be certain that a household with, say, 0.75 acres, is not a program participant because it is ineligible, or is not because it is eligible (perhaps because it’s land is *char* land or barely cultivable), but chooses not to participate. The problem of appropriately classifying households does not disappear. Morduch’s approach is to apply the *de jure* eligibility rule in estimating program effects. He gives no justification for this, and indeed there is none. If there is a “soft” (*de facto*) eligibility rule that takes land quality or other considerations (including lying or corruption) into account, there is no reason why one should get consistent estimates of program effects by imposing the wrong (*de jure*) eligibility rule on the data. Indeed, it is easy to show that taking program households with more than ½ acre of land and treating them as ineligible is precisely the wrong direction to take. The much better procedure is to take nonprogram households with somewhat more than ½ acre of land and treat them as if they have choice, not to take program households and treat them as if they are not program participants. Dropping from the sample those households owning more than ½ acre of land that are program participants imparts classical sample selection bias to any estimates of program effects.

By raising the eligibility cutoff to land ownership greater than ½ acre, say one acre, all households, whether program participant or not, are treated as having the choice to participate. In this way, those who really do have choice but choose not to participate are now treated appropriately. On the other hand, some of these households that are not program participants with land below 1 acre are actually ineligible. However, this classification error does not alter the consistency of the estimates of program effects. Treating a behavior as endogenous when it is in fact exogenous still yields consistent estimates. It merely reduces the efficiency of those estimates. This is akin to having uncertainty about the endogeneity of independent variables in a

regression or the appropriateness of random versus fixed effects. Treating a possibly exogenous regressor as endogenous (with instrumental variables methods) results in consistent estimates whether that regressor is exogenous or endogenous. Similarly, treating the time-persistent component of the residual as a fixed effect results in consistent parameter estimates even if this component is in fact orthogonal to the regressors and could have been treated as a random effect. Bias results only when endogenous behavior is treated as exogenous, not when exogenous behavior is treated as endogenous. The simulation program `sim12.do` in the appendix to this paper demonstrates this result numerically in the case of two-stage least squares.³

Understanding whether *de facto* land classification (“mistargeting”) matters to the results of PK is ultimately purely an empirical issue upon which Morduch’s approach sheds no light. What remains to be seen is whether raising the land ownership cutoff to values above ½ acre will reduce or increase our earlier estimates of program effects based on the *de jure* cut off for nonparticipating households and the actual (*de facto*) cut off for participating households. This is actually a simple task. One just needs to re-estimate the model at higher land cutoff’s, treating more and more households as having endogenous choice rather than no choice, and see how parameter estimates respond. In the results reported below, we reclassify households as having the choice to join a microcredit program at land ownership values of 0.66, 1.20, 1.60 and 2.00 acres. Based upon the discussion above, we can hypothesize how estimated program effects would change at higher eligibility cut offs. Since Pitt and Khandker (1998) treated participating households with more than ½ acre of land as households with choice (endogenous), the source of any bias arises from incorrectly treating nonparticipating households with more than ½ acre of land who actually had choice as not having choice. If our earlier finding that higher consumption households are less likely to be credit program participants, conditional on the regressors, is true, then putting these “high” consumption households incorrectly into the control group rather than the treatment (choice) group would tend to underestimate program effects. Consequently, PK

³When there are limited dependent variables things become a bit murkier. Treating households without program choice as choosing to not participate in the program alters the empirical distribution of the credit participation errors and consequently, since the distribution of the errors matter in the limited dependent variable model, the parameter estimates may also change.

underestimate program effects. Choosing successively higher eligibility cutoffs should actually increase the estimated program effects. In fact the problem of signing the change is a bit more complicated than this, but this intuition reflects the first-order effects.

Table 4 provides estimates of the determinants of the logarithm of per capita household consumption with five different eligibility rules. The estimates of column (1), exactly reproduced from Table 2 in Pitt and Khandker (1998), are based on the *de jure* rule for nonparticipants and the (unknown) *de facto* rule for participants. The other four columns have progressively higher land ownership eligibility rules.⁴ As Table 1 notes, there are 157 participating households with cultivable land in excess of 0.50 acres prior to joining the program. With the imposed eligibility rules there are 127, 62, 46 and 28 program households with land owned in excess of the 0.66, 1.20, 1.60 and 2.0 acre rules, respectively. Consequently, as we progressively raise the *de facto* program eligibility rule, over 82 percent of the previously “mismatched” participating household are moved to within the eligibility rule. In the case of the 1.6 acre eligibility rule, about 5 percent of sample observations in program villages are considered “non-choice” observations, as compared to 10 percent with the 0.5 acre rule.

The estimated program effects in Table 4 reveal that under the ½ acre rule, Pitt and Khandker (1998) did indeed slightly underestimate the consumption returns to female program credit, and slightly overestimate the consumption returns to male program credit. The qualitative conclusions of Pitt and Khandker (1998), that there are significant and large returns to female borrowing and smaller and insignificant returns to male borrowing, is only strengthened by the results of Table 4. If all households with land ownership of less than or equal to 2.0 acres, a level of land ownership four times the *de jure* rule, are treated as having the choice to join the program, the effect of female credit program participation on consumption rises about 20 percent, and the asymptotic t-statistics rise by about 80 percent. This pattern, along with the pattern of algebraically smaller (more negative) correlation coefficients (**D**) for women, is consistent with

⁴In Pitt and Khandker (1998), households with more than 5.0 acres of land were dropped from the analysis to aid comparability. In the case of a 2.0 acre eligibility rule (Table 4, column 5), we have added these households back into the analysis for two reasons. First, five acres of land is certainly more comparable to 2 acres than it is to ½ acre. Second, the sample size of “control” households gets small as more households are deemed eligible for “treatment”.

the view that low expenditure households, conditional on a large set of regressors that include land area unadjusted for land quality, are more likely to have women become credit program participants. That is, this result is consistent with having the relative cultivation quality of land taken into consideration in determining eligibility.⁵

B. Village fixed effects are incorrectly specified and unduly parsimonious

Morduch criticizes PK's approach of controlling for village fixed effects, suggesting that if programs locate on the basis of qualities specific only to target groups, that village fixed effects as implemented by PK may not be an improvement, but may exacerbate bias by setting up the wrong benchmark (p. 9). To make his point, he characterizes PK's framework with regression equation (1), reproduced here:

$$Y_{iv} = \alpha_1(e_{iv}b_v) + \alpha_2e_{iv} + \sum_{v=1}^V \gamma_v d_v + \sum_{k=3}^{K+3} \alpha_k X_{ivk} + \epsilon_{iv} \quad (\text{M1})$$

where e_{iv} indicates eligibility status of household i in village v irrespective of whether a program is in fact available in village v , b_v indicates program availability in village v , the vector of X 's control for household characteristics, the d_v are village-level dummy variables, and ϵ_{iv} is an idiosyncratic error term. The coefficient α_1 is the difference-in-difference, the impact over and above the village mean -- the effect of eligibility status on the outcome Y_{iv} . There is no disagreeing with Morduch's statement that bias could arise in this framework:

“The problem arises since the programs explicitly limit their attention to just

⁵If an eligibility rule is *fuzzy* in that actual eligibility rule applied is a fixed value plus a random variable, the use of the PK procedure results in consistent estimates even if the value of the random variable is unknown, that is, even if the incorrect de jure eligibility rule is used in the econometrics. This result does not require that the random variable have zero mean. Application of this consistency result to these data does not seem reasonable in this case, nor is it necessary. This special case is illustrated in the simulation programs `sim9.do` and `sim10.do` presented in the appendix.

functionally landless households. Thus, the critical unobservables will be those specific to target groups within villages, not just unobservables that affect all villagers equally. “(p. 9)

The problem is that Morduch mischaracterizes the approach of PK. In fact, PK do exactly what Morduch claims should be done in the above quoted paragraph. That this, they explicitly allow for program availability to be responsive to the unobservables of only the target groups within villages. In PK, the “first-stage” credit reduced form demand equations are estimated over the subsample of (*de facto*) eligible households in villages with a program since they are the only ones who can join and borrow. Credit is deterministically zero for all villages without a program and for all ineligible households in program villages. There is no credit program behavior to be estimated at all as there is no element of choice. In PK’s paper, these first-stage equations (equation 1 for the case that does not consider gender, and equations 6 and 7 for the case that does) all have village fixed effects which are clearly separate and distinct from another set of fixed effects affecting the outcome Y (equations 2 and 8). Yet Morduch’s characterization of PK given by his equation (1) and the ensuing discussion, incorrectly suggests that there is but one village fixed effect per village in the PK estimation, and none specific to target households. In actuality, the first-stage equations of PK have two fixed effects (one per gender) specific only to the target households (of each gender) of each village with a credit program, and a village fixed effect for the outcome equation of villages.⁶

That this is the case in PK is quite clearly stated in their paper. Reproduced below are equations (6) and (7) from PK:

$$C_{ijf} = X_{ij}\beta_{cf} + \mu_{jf}^c + \epsilon_{ijf}^c \quad (6)$$

$$C_{ijm} = X_{ij}\beta_{cm} + \mu_{jm}^c + \epsilon_{ijm}^c \quad (7)$$

⁶The simulation programs beginning with sim6.do illustrate how fixed effects are actually specified in PK.

where C_{ijf} and C_{ijm} is the program credit obtained by females and males, respectively, in household i of village j , μ_{jf}^c and μ_{jm}^c are unmeasured determinants of C_{ij} that are fixed within a village, the X are observed household characteristics and the β_{cm} are parameters to be estimated. The “second-stage” outcome equation in PK (equation 8) is:

$$y_{ij} = X_{ij}\beta_y + C_{ij}\delta + \mu_j^y + \epsilon_{ij}^y \quad (8)$$

where β_y and δ are unknown parameters, μ_j^y is an unmeasured determinant of y_{ij} that is fixed within a village, and distinct from the fixed effects μ_{jf}^c and μ_{jm}^c . The log-likelihood presented as equation (5) in PK for the case without gender distinction, and in the Appendix to PK for the case with gender distinction, also clearly distinguishes separate fixed effects to control for the placement of credit programs from fixed effects that control for the effect of village unobservables on the behavior Y . The log-likelihood presented as equation (5) in PK (pages 968-969) and relevant parts of its discussion is reproduced verbatim below:

“Distinguishing between households not having choice because they reside in a non-program village and households residing in a program village that do not have choice because of the application of an exogenous rule (landowning status), and suppressing the household and village subscripts i and j , the likelihood can be written as:

$$\begin{aligned} \log L(\beta, \delta, \mu, \rho) = & \sum_{\text{choice}} \log \Phi_2((\mu_p^c + X\beta_c)d_c, (\mu_p^y + X\beta_y + \delta I_c)d_y, \rho d_c d_y) \\ & + \sum_{\substack{\text{no choice} \\ \text{program village}}} \log \Phi((\mu_p^y + X\beta_y)d_y) + \sum_{\text{nonprogram village}} \log \Phi((\mu_n^y + X\beta_y)d_y) \end{aligned} \quad (5)$$

where M_2 is the bivariate standard normal distribution, M is the univariate standard normal distribution, μ_p^c are the village-specific effects influencing participation in the credit program in program villages, μ_p^y are village-specific effects influencing the binary outcome I_y in program villages, μ_n^y are the corresponding village-specific effects in nonprogram villages, and $d_c = 2*I_c - 1$ and $d_y = 2*I_y - 1$. The errors ϵ_{ij}^c and ϵ_{ij}^y are normalized to have unit variance and correlation coefficient D . Village-specific effects

(; δ) influencing the demand for program credit are not identifiable for villages that do not have programs.

The first part of the likelihood is the joint probability of program participation and the binary outcome I_y conditional on participation for those households that are *both* eligible to join the program (*choice*) and reside in a village with the program (*program village*). (Italics in original, boldface added for emphasis)

Thus, the fixed effects that determine the availability of a credit program is estimated over “ those households that are *both* eligible to join the program (*choice*) and reside in a village with the program (*program village*).” I am not sure that this could be said any clearer. That these fixed effects are specific to the eligible (target) households is also implied from the number of observations used in estimating the first-stage credit demand equation list in Table 2 (1105 for women and 895 for men, significantly less than the number used in the second-stage equations), and from footnote 4 which states that over 200 village fixed effects are estimated, but there are just 87 villages in the complete sample of which 72 have credit programs. How could Morduch’s characterization of one fixed effect per village be consistent with these numbers? Indeed, as the paper makes clear, there are three fixed effects in villages with male and female credit programs, two fixed effects with only a female or male credit program, and one fixed effect if there is no credit program. Furthermore, all of the credit demand fixed effects are explicitly limited to functionally landless (target) households. The large number of fixed effects seems inconsistent with a claim that PK were unduly parsimonious with them.

One might have the impression that I have cited more evidence than required to convince the average reader of my claim. In a number of communications with Morduch, I repeatedly stressed the point that there is no program choice for the ineligible, and that consequently the demand for credit and its village dummy variables pertain only to the eligible. All of which make his mischaracterization of PK all the more surprising. It appears that Morduch has some appreciation for these communications since with equations (4) and (5) of his pages 23 and 24 he once again offers a characterization of the instrumental variable method of PK. The top line of his equation (4) (reproduced below) has deterministic credit for the ineligible

$$C_{iv} = 0 \quad \text{if } e_{iv} = 0 \text{ or } b_v = 0.$$

That he understands this zero to be deterministic rather than stochastic, and thus unavailable to identify any credit demand parameters, is clear from the first line of text after he presents equations (4) and (5). There he states, “Apart from the first line of equation (4), this is a standard instrumental variables problem.” (24-35) This line ends with a footnote beginning “I thank Mark Pitt for stressing the importance of this distinction.”

C. Morduch’s “new evidence”

Morduch presents new estimates of credit program effects in his Tables 6 through 11. These tables present difference-in-differences estimates using both de jure and de facto eligibility rules. We have already noted that the use of the de jure eligibility rule is unjustified and will likely result in a biased estimate of program effects. However, failure to adequately deal with the mistargeting issue is not the only mistake to bedevil Morduch’s new evidence. He fails to set out a clear framework justifying his difference-in-the-differences estimate, so it might be hard to see what the issues are. It seems that his estimate is simply the application of the identification example of PK, reproduced from Pitt and Khandker (1998, p. 968) below:

“To illustrate the identification strategy, consider a sample drawn from two villages -- village 1 does not have the program and village 2 does; and, two types of households, landed ($X_{ij}=1$) and landless ($X_{ij}=0$). Innocuously, we assume that landed status is the only observed household-specific determinant of some behavior y_{ij} in addition to any treatment effect from the program. The conditional demand equation is:

$$y_{ij} = C_{ij}\delta + X_{ij}\beta_y + \mu_j^y + \epsilon_{ij}^y \quad (3)$$

The exogeneity of land ownership is the assumption that $E(X_{ij}, \epsilon_{ij}^y) = 0$, that is, that

land ownership is uncorrelated with the unobserved household-specific effect.

The expected value of y_{ij} for each household type in each village is:

$$E(y_{ij} | j=1, X_{ij}=0) = \alpha_1 \quad (4a)$$

$$E(y_{ij} | j=1, X_{ij}=1) = \beta_1 + \alpha_1 \quad (4b)$$

$$E(y_{ij} | j=2, X_{ij}=1) = \beta_2 + \alpha_2 \quad (4c)$$

$$E(y_{ij} | j=2, X_{ij}=0) = p^* + \alpha_2 \quad (4d)$$

where p is the proportion of landless households in village 2 who choose to participate in the program. It is clear that all the parameters, including the effect of the credit program α_2 , is identified from this design.”

The estimator of the program effect α_2 in equation (4d) is the differences-in-the-differences estimator widely applied in the general program evaluation literature. To see this, note that an estimate of α_2 is obtained from the following difference-in-the-differences, labeled as equation (a);

$$p^* = [E(y_{ij} | j=2, X_{ij}=0) - E(y_{ij} | j=2, X_{ij}=1)] - [E(y_{ij} | j=1, X_{ij}=0) - E(y_{ij} | j=1, X_{ij}=1)] \quad (a)$$

The estimates that Morduch present in his tables (6) through (11) are, as far as I can tell, based on (a). The problem is that while (a) represents that difference-in-the-differences estimator of the program effect α_2 in the illustrative example of PK above, it is likely to provide a biased estimate of program effects when applied to our data. This bias simply reflects the difference between an experiment, in which both observable attributes and unobservable attributes have the same expectations in both the treatment and control groups, and a quasi-experiment in which they do not. Our data clearly conform to a quasi-experiment. Morduch treats them as if they conform to an experiment. To see the problem this error causes, take the outcome equation (3), in which X_{ij} is a vector of observed household characteristics, and difference once:

$$\begin{array}{r}
E(y_{ij}|j=2, e_{ij}=0) = X_{02}\beta_y + \mu_{02} \\
- \quad E(y_{ij}|j=2, e_{ij}=1) = X_{12}\beta_y + \delta + \mu_{12} \\
\hline
(X_{02} - X_{12})\beta_y - \delta + (\mu_{02} - \mu_{12})
\end{array} \tag{b}$$

here $j=2$ indicates a credit program village, $e_{ij}=1$ indicates a target household, δ is “the impact over and above the village mean and the fact of being ‘eligible’ ” (Morduch p. 9), and the village fixed effect in this solved out equation varies between eligible (target) households and ineligible (nontarget) households, as in the actual application of PK. The negative of the remainder in (b) are the “differences” estimates of δ presented by Morduch in the last row of each of the tables 6 through 11. However, for this to represent the true differences requires that $(X_{02} - X_{01})\beta_y + (\mu_{02} - \mu_{01}) = 0$. Morduch himself strenuously argues that there is no reason to believe that $(\mu_{02} - \mu_{01}) = 0$.⁷ In addition, is it reasonable to expect that $(X_{02} - X_{01})\beta_y = 0$? This equality would hold if the means of the observed characteristics of poor target households are the same as the mean observed characteristics of the richer nontarget households, an equality that would be implied from experimental assignment to treatment and control groups. But households are not experimentally assigned to the Grameen Bank. Eligible household’s choose to join on the basis of observed and unobserved characteristics. Similarly, the Grameen Bank chooses to serve a village on the basis of observed and unobserved village characteristics that differ on average between eligible (target) households and ineligible (nontarget) households. These observed characteristics, which in PK include level of schooling, sex of household head, presence of males in the household, existence of landed non-coresident relatives, and land owned, are not the same, on average, between rich and poor. The only hope is that differencing the differences of the program and nonprogram village would alleviate this problem. That difference-in-the-differences is:

⁷It is odd indeed that after arguing that these credit programs are likely to limit their attention to the functionally landless in making program placement decision, and thus it is their unobservables that matters, Morduch ignores his own advice in calculating his new estimates.

$$\frac{(X_{01} - X_{11})\beta_y + (\mu_{01} - \mu_{11}) - (X_{02} - X_{12})\beta_y - \delta + (\mu_{02} - \mu_{12})}{\delta + [(X_{01} - X_{11}) - (X_{02} - X_{12})]\beta_y + [(\mu_{01} - \mu_{11}) - (\mu_{02} - \mu_{12})]} \quad (c)$$

Once again, since these data do not conform to an experiment, there is no reason to expect that the two expressions in square braces after the * sum to zero. In particular, if it is true that these credit programs are likely to be attentive to the unobserved attributes of eligible households in making program placement decisions and these unobserved attributes are not necessarily the same as the unobserved attributes of the village as a whole, as both Morduch and I agree, then these credit program should also be more attentive to the observed attributes of these eligible households than to observed attributes of the ineligible. If the unobserved attributes of the ineligible differ from the unobserved attributes of the eligible, then it is also likely that the observed attributes of the ineligible differ from the observed attributes of the eligible. To say that these credit programs are differentially attentive to these unobserved attributes means that they choose to place their programs in village on the basis of the differences between $(X_{01} - X_{11})$ and $(X_{02} - X_{12})$ and the differences between $(\mu_{01} - \mu_{11})$ and $(\mu_{02} - \mu_{12})$.⁸ For example, if programs target villages with the largest gap between rich and poor, and if low X's and low μ 's for the poor make them more needy (that is, $\beta_y > 0$), then a village with a large $(X_{02} - X_{12})$ will get programs as compared to a village with a small $(X_{01} - X_{11})$. Consequently, the quantity $[(X_{01} - X_{11}) - (X_{02} - X_{12})]\beta_y < 0$ and Morduch's estimates of * (the difference-in-the-differences in equation (c)) will be biased downwards. Likewise, a village with a large $(\mu_{02} - \mu_{12})$ will get programs as compared to a village with a large $(\mu_{01} - \mu_{11})$ for the same reason, further biasing Morduch's estimate downwards.⁹ In summary, the very nature of self-selection of credit

⁸In addition, the X's and the μ 's are correlated. See below.

⁹The pattern of credit effects on consumption reported in Pitt and Khandker (1998) Table 4 (p. 985) are consistent with a finding that Morduch's difference-in-the-difference's estimate are downward biased. The instrumental variable estimates that control for household self-selection into credit programs but do not include for village fixed effects (labeled WESML-LIML)

programs into villages argues that the “remainder” in equation (c) is nonzero and hence that Morduch’s estimates are biased. In particular, if programs target villages in which the poor are relatively worse off, as even Morduch suggests, than Morduch’s estimates will be downward biased. As we noted earlier, the incorrect imposition of a de jure classification of program eligibility will also bias measured program effects. It is no surprise therefore that Morduch’s “new evidence” reveals that credit programs actually harm the poor.¹⁰

There are other problems with Morduch’s approach, although perhaps not as crucial as the one described above. First, he ignores issues of gender. The choice problem of households is fundamentally different if only one gender can join a credit program than if both have the choice, even though only one can join. Moreover, the evidence in PK suggests that the determinants of credit program placement differ by gender of credit group, the determinants of household self-selection differ by gender of eligible participant, and the effects of credit program participation on the outcomes studied differ by gender of participant. Morduch does not state why he has chosen to ignore gender differences in calculating his new estimates. The inappropriate aggregation of the sexes likely would bias any result of program effects, even if the underlying econometric model were otherwise sound. Table 5 presents a re-estimation of the PK model ignoring gender. Not surprisingly, the estimated credit effects are downward biased for women and upward biased for men. Second, the program effect he claims to estimate, which is labeled α^* above, is actually α^*p where p is the proportion of participants among eligible households in program villages, as in my equation (4d), reproduced above. Since $p < 1$, Morduch’s program effect is necessarily smaller than the one estimated in PK.

Finally, Morduch also presents some regression estimates of program effects in table 13. Although he does not say so explicitly, Morduch apparently estimates his equation (1) (p. 9 of

estimate a positive and significant correlation coefficient for men’s credit and a negative and significant correlation for women’s credit. Both correlation coefficients become more negative algebraically when village fixed effects are added (WESML-LIML-FE) and the estimated program effects get larger for both men and women. That is, not controlling for selective program placement by introducing village fixed effects underestimates program effects.

¹⁰The bias in Morduch’s approach is well illustrated in the simulation program sim8.do in the appendix.

his paper), which is reproduced in section 2.B of this paper as equation (M1). But with one important difference. After making an issue of the importance of specifying village fixed effects for both target and non-target households in each village, two of the three sets of regression estimates do not include any fixed effects (the α_{vj}) at all! Is there any evidence that village fixed effects are empirically important? Pitt and Khandker (1998, p. 976) report: “The estimated village fixed effects associated with female credit program participation, the α_{jf} from equation (6), and the estimated village fixed effects associated with male credit program participation, the α_{jm} from equation (7), are significantly (at the 0.05 level) correlated with the regressors X_{ij} .” Biased regression parameters result from the correlation of the residual and the regressors. Furthermore, our previous discussion implies that the program effect estimated in this regression is biased downwards.

Only in the last set of regressions in Table 13 are village-level fixed effects included. Morduch and PK both agree that it is important to allow for separate village fixed effects specific to target and non-target groups. PK goes one step further and estimates separate village fixed effects for target groups by gender. After incorrectly claiming that PK do not estimate group-specific village-level at all, thus resulting in bias, Morduch proceeds to estimate a fixed effects that makes the very restriction he attacks. In the last panel of Table 13, he restricts village-specific heterogeneity to be the same for target and nontarget groups, not to mention failing to distinguish them by gender.

D. Implications for impacts by gender are likely misstated

On pages 26 through 28, Morduch suggest that PK’s result concerning the relative size of gender impacts have two faults. First, he claims that PK fail to control for village unobservables (fixed effects) specific to target groups. He says on page 28

:

“The fact that a man is in a village with no male groups may say something about the unobserved qualities of the men and the strength of their peer networks in that village. [...] The village fixed effects may pick up much of this unobserved

village heterogeneity, but as argued above, they will not control for features of peer networks that are specific just to target (functionally landless) households in program villages”

As we clearly demonstrated above, Morduch’s assertion that we do not control for peer networks and other unobserved heterogeneity specific to target households in just plain wrong. We include dummy variables for just these groups in every program village, by sex, in all of our estimations.

The second claim is that the higher marginal impact PK report for women as compared to men may simply reflect declining returns to capital. While declining returns may in fact exist, the difference in loan sizes that Morduch cites suggest that it highly unrealistic that this is an important explanation for the differential returns. In the case of the Grameen Bank, he claims that men who borrow have borrowed an average of 15,797 taka while women who borrow have borrowed an average of 14,128 taka. That is, men have borrowed 11.8 percent more than women. However, the estimated marginal impact of women’s borrowing on log household consumption per capita is 241 percent of the estimated marginal impact of men’s borrowing (Pitt and Khandker 1978, Table 2). Just by inspecting these numbers, one should conclude that the explanation offered by Morduch is not credible. The rate at which the marginal impact of capital falls must be phenomenally large for an 11.8 percent greater quantity of capital to account for a 2.4 fold difference in marginal impact. It is a straightforward exercise to compute the rate at which the marginal impact of capital must fall for the estimated male-female difference in Grameen Bank impact to represent diminishing returns. Morduch contends that the returns to program borrowing may be the same for men and women, thus taking the form:

$$\delta = f(C), \quad f' > 0, \quad f'' < 0$$

where δ is the return to borrowing, a function of the quantity borrowed C. The parameters PK estimate in Table 2 of their paper are the marginal effects $\delta = f'(C)$. Morduch contends that the marginal effect for women $\delta_f = f'(C_f)$ may be greater than the marginal effect for men $\delta_m = f'(C_m)$ because $C_f > C_m$. In PK (Table 2), $\delta_f = 0.0432$ and $\delta_m = 0.0179$. The arc elasticity of the change in the marginal return to borrowing as it falls from C_m to C_f is :

$$\frac{\Delta \beta}{\Delta C} \frac{C}{\beta} = \frac{(0.0432 - 0.0179)}{(14128 - 15797)} \times \frac{15797}{0.0179} = -13.4$$

That is, in order for the difference between the marginal effect of credit provided women on consumption (0.0432) and the marginal effect of credit provided men (0.0179) to be due to diminishing returns to capital would require that a 1 percent fall in the quantity of credit increases the marginal return to credit \$ by an incredible 13.4 percent.

E. Specification is too restrictive and inflexible

Morduch complains that PK's specification of the model of the impact of program credit on behavior is restrictive. The argument is made by Morduch on p.26 and extends beyond his faulty claims about the specifications of village fixed effects. His argument is that our instrumental variables:

“may pick up any systematic differences between the landless and landed in, say, the impact of age on income, even when the differences are not particular to the landless in program villages. If nonlinearities in the effects of explanatory variables are picked up by the instrument, the instruments can also pick up the effects of unobserved heterogeneity, providing a plausible explanation for their positive results on household consumption: “better” borrowers get bigger loan, yielding what appear to be positive and significant marginal impacts.” (p. 25-26)

There is of course no way to refute an allegation that one did not choose a flexible enough functional form in most types of empirical analysis without recourse to the data. Linearity is certainly a popular choice for functional form, but one can never rule out the possibility that allowing for interactions, quadratic terms or other nonlinearities might alter the results of any econometric analysis. On its face, estimating a model of the effects of credit on household consumption that jointly estimates 254 unrestricted parameters, as PK do, does not seem unduly

parsimonious. However, as PK are careful to point out (footnote 6, p. 969 in PK), the model they estimate is not nonparametrically identified. So worrying about how the addition of interactions to the PK specification affects results may be warranted, even though the specification used is already highly parameterized.

Morduch is particularly concerned with interactions of land with all of the other exogenous regressors because there may be “systematic differences between the landless and landed in, say, the impact of age on income.” The only way to ascertain the validity of Morduch’s concern is to re-estimate the model with these interactions.¹¹

Table 6 presents estimates of the effects of program credit, by program and gender, on the log of household per capita income, allowing for interactions between land ownership and all of the exogenous regressors and interactions between land ownership and all of the thana fixed effects. There are three villages in each thana in the sample design, and all three villages in each thana have the same credit program (BRDB, BRDB, or Grameen Bank). All 18 exogenous regressors and the 29 thana dummy variables are interacted with land and are included in the consumption equation. This is actually the most “crucial” interaction that could be included since it is the land based eligibility rule that is the source of identification in the model. Consequently, these interactions are the ones most likely to destroy identification if in fact it is the linearity of the consumption function that is driving the identification of credit effects. The “bottom line” from including these interactions is that the qualitative results of PK still hold – there are positive and statistically significant effects of female credit program participation on household consumption, and much smaller and generally statistically insignificant effects of male credit program participation on household consumption, as before.

The first column of Table 6 simply reproduces the WESML-LIML-FE result from table 2 of Pitt and Khandker (1998, p. 981). The following 5 columns add land interactions to the specifications of Table 4 in which the land cutoff for determining whether household have

¹¹ It strikes me that if one wishes to assert that a certain model may fit the data better than another, and one has the data, one should go out and test if that assertion is true. To claim “this could be” and “that could be”, and then make no effort to examine whether these speculations are true when they are easily tested with the data in one’s possession, strikes me as unfairly redistributing the burden of criticizing another’s work.

(endogenous) choice to participate in a credit program is progressively raised from 0.50 acres to 2.00 acres. The estimates with these interactions and the most conservative land eligibility cutoff, the 2.00 acre rule, are very comparable to those reported in Pitt and Khandker. Female credit effects are just slightly larger and more precisely estimated than in PK, and male credit effects are a bit smaller. However, they are still pretty much the same. It would seem that the results of PK are robust both to the land eligibility (“mistargeting”) problem and to even more richly parameterized specifications that might tend to destroy parameter identification if it were fragile.

Table 1

Distribution of Cultivable Land Owned by Participating Households at Prior to Joining Program
(in decimal = hundredth's of an acre)

	BRAC	BRDB	Grameen	Total
mean	34.18	32.92	35.21	34.21
median	0	0	0	0
no. with > 50 decimals	46	45	66	157
percentage with > 50 decimals	16.1	14.6	21.1	17.3
Total number of households	285	308	312	905

Distribution of All Land Owned by Participating Households Prior to Joining
(in decimal = hundredth's of an acre)

	BRAC	BRDB	Grameen	Total
mean	47.08	44.30	52.01	48.01
median	10	10	16	11
No. with > 50	59	56	88	203
percentage > 50 decimals	20.7	18.2	28.2	22.4
Total	285	308	312	905

Table 2

Distribution of Cultivable Land Owned by Participating Households at Time of Survey
(in decimal = hundredth's of an acre)

	BRAC	BRDB	Grameen	Total
mean	41.50	35.93	42.01	40.32
median	0	0	4	0
no. with > 50 decimals	50	51	74	179
percentage with > 50 decimals	17.5	16.6	23.7	19.8
Total number of households	285	308	312	905

Distribution of All Land Owned by Participating Households at Time of Survey
(in decimal = hundredth's of an acre)

	BRAC	BRDB	Grameen	Total
mean	54.93	48.18	59.48	54.78
median	10	13	21	15
No. with > 50	67	66	94	227
percentage > 50 decimals	23.5	21.4	30.1	25,1
Total	285	308	312	905

Table 3

Determinants of Unit Land Values
(Taka per decimal of land)

Variables	(1)	(2)	(3)	(4)	(5)
Total land (decimals)	-2.534 (-5.57)	-2.373 (-5.08)	-2.042 (-5.367)	-2.013 (-5.15)	-1.807 (-5.561)
Land squared (x 10,000)	4.399 (4.55)	4.119 (4.19)	3.710 (4.59)	3.662 (4.46)	3.293 (4.69)
Mistargeted=1	-730.9 (-3.04)	-868.9 (-3.37)	-680.48 (-3.32)	-704.1 (-3.23)	-721.5 (-3.58)
Participant=1		271.38 (1.48)		48.99 (0.32)	68.33 (0.48)
Intercept	1947.5 (20.80)	1863.2 (17.02)			
Method	OLS	OLS	Thana FE	Thana FE	Thana FE
observations	1446	1446	1446	1446	1743
Thana's	program	program	program	program	all

Table 4

Alternative Estimates of the Impact of Credit on Log Per Capita Expenditure

	0.5 acre rule	0.66 rule	1.20 rule	1.60 rule	2.00 rule
Amount borrowed by female from BRAC	.0394 (4.237)	.0413 (4.797)	.0432 (5.443)	.0452 (6.098)	.0500 (8.077)
Amount borrowed by male from BRAC	.0192 (1.593)	.0179 (1.338)	.0146 (1.003)	.0095 (0.556)	.0066 (0.265)
Amount borrowed by female from BRDB	.0402 (3.813)	.0421 (4.341)	.0439 (4.870)	.0463 (5.388)	.0512 (6.801)
Amount borrowed by male from BRDB	.0233 (1.936)	.0219 (1.617)	.0177 (1.157)	.0115 (0.654)	.0077 (0.303)
Amount borrowed by female from GB	.0432 (4.249)	.0451 (4.838)	.0470 (5.432)	.0490 (6.088)	.0527 (7.805)
Amount borrowed by male from GB	.0179 (1.431)	.0163 (1.164)	.0118 (0.739)	.0058 (0.321)	.0006 (0.023)
D (women)	-.4809 (-4.657)	-.5051 (-5.489)	-.5384 (-6.650)	-.5611 (-7.784)	-.6062 (4.302)
D (men)	-.2060 (-1.432)	-0.1855 (-1.134)	-.1568 (-0.862)	-.0811 (-0.391)	-.0389 (-0.129)
Observations with choice	3815	3845	3986	4079	4152
Observations in program villages without choice	531	501	460	267	306
Total no. of observations	5218	5218	5218	5218	5345

Note: Figures in parentheses are asymptotic t-ratios

Table 5
 Estimates of the Impact of Credit Program Participation
 on Log Per Capita Expenditure Ignoring Gender

Total borrowed from BRAC	0.2648 (3.052)
Total borrowed from BRDB	0.2798 (3.125)
Total borrowed from GB	0.2732 (3.052)
D (total)	-0.3776 (-3.081)
Total no. of observations	5218

Note: Figures in parentheses are asymptotic t-ratios

Table 6

Alternative Estimates of the Impact of Credit on Per Capita Expenditure with Land Interactions

	No interactions	Full interactions with land ownership				
	0.5 acre rule	0.50 rule	0.66 rule	1.20 rule	1.60 rule	2.00 rule
Amount borrowed by female from BRAC	.0394 (4.237)	.0318 (3.286)	.0340 (3.876)	.0354 (4.305)	.0380 (5.110)	.0424 (6.647)
Amount borrowed by male from BRAC	.0192 (1.593)	.0165 (1.587)	.0150 (1.335)	.0151 (1.288)	.0114 (0.884)	.0089 (0.526)
Amount borrowed by female from BRDB	.0402 (3.813)	.0326 (2.996)	.0348 (3.524)	.0358 (3.845)	.03919 (4.505)	.0436 (5.734)
Amount borrowed by male from BRDB	.0233 (1.936)	.0209 (2.120)	.0191 (1.781)	.0190 (1.662)	.0140 (1.115)	.0109 (0.656)
Amount borrowed by female from GB	.0432 (4.249)	.0356 (3.354)	.0380 (3.939)	.0399 (4.401)	.0426 (5.215)	.0472 (6.866)
Amount borrowed by male from GB	.0179 (1.431)	.0141 (1.362)	.0125 (1.112)	.0123 (1.032)	.0073 (0.566)	.0048 (0.277)
D (women)	-.4809 (-4.657)	-.4200 (-3.601)	-.4484 (-4.379)	-.4765 (-5.079)	-.5066 (-6.261)	-.5501 (-8.759)
D (men)	-.2060 (-1.432)	-.2067 (-1.716)	-.1813 (-1.362)	-.1952 (-1.398)	-.1275 (-0.831)	-.0869 (-0.422)
Observations with choice	3815	3815	3845	3986	4079	4152
Observations in program villages without choice	531	531	501	460	267	306
Total observations	5218	5218	5218	5218	5218	5345

Note: Figures in parentheses are asymptotic t-ratios

Appendix

To help the reader understand some of the issues discussed in this paper, Stata™ do files that represent various data generating processes as well the estimation methods of PK and Morduch are presented below. The reader is encouraged to play with these files, altering assumptions and parameters as they see fit. To simplify this, these files can be downloaded from my home page on the World Wide Web located at <http://pstc3.pstc.brown.edu/~mp/>.

These do files start off with simple models that do not distinguish between genders or allow for different village effects for eligible and ineligible households, and proceed in stages to more complete models that capture fairly well the actual estimation problem. Many of the files contain comments that briefly describe the features of the empirical problems that are highlighted by the simulation. The naming/numbering of the files listed are not meaningful and only represent the progression of the authors thinking. There is a bug in Stata 5 that cause a parsing error for certain types of two-stage least squares commands. To avoid this error, be sure that there is at least one space before the closing parenthesis “)” of a two-stage least squares commands, for example:

```
xi: regress exp x1 x2 land credit I.vill (x1 x2 land h z* I.vill I.vill*t )
```

put a space after I.vill*t and the closing parenthesis.

```

*sim4.do
*written by Mark M. Pitt
* This is the simplest form of the Pitt-Khandker set-up.
* This special case does not distinguish
* between males and females borrowers. It also does not
* have different village FE's for credit and expenditure.
* These restrictions are relaxed in subsequent Stata do files.
*
* We allow for some eligible nonparticipants
drop _all
set matsize 400
set maxobs 10000 maxvar 300
log using sim4, replace
* set number of villages
set obs 90
* set village identifiers
gen vill=_n
* draw random village effect
gen mu = invnorm(uniform())
* only villages with mu's below some value get programs
* this is nonrandom program placement
count if mu > 1.0
* allow for 100 households in each village
expand 100
* two regressors
gen x1=uniform() - 0.5
gen x2=uniform() - 0.5
* generate a distribution of land
gen land = uniform()
* only those with less than 0.5 units of land have the choice to borrow
gen h=land<0.5
* only those in a program village have the choice to borrow
replace h=0 if mu > 1.0
* generate a household-specific error
gen e = invnorm(uniform())
* underlying credit equation which includes an additional iid error
gen credit = 1 + 2*x1 + 2*x2 + 2*land + invnorm(uniform()) + (2.0*e) + (2.0*mu)
* those without choice get zero program credit
replace credit = 0 if credit < 0
replace credit=0 if h==0
* underlying expenditure equation with an additional iid error
* note that the errors of the credit and expenditure equation have correlated
* village and household "unobserved" effects
gen exp = 2 + 2*x1 + 2*x2 + 2*land + 1*credit+invnorm(uniform()) + (2*e) +(2*mu)
* create interactions of choice and regressors
gen t=h==0
gen zx1=x1*t
gen zx2=t*x2
gen zland = land*t
* naive estimate: exogenous program placement and exogenous household participation
regress exp credit x1 x2 land
* naive estimate: endogenous program placement and exogenous household participation
* do village fixed effects
xi: regress exp credit x1 x2 land I.vill
* naive estimate: exogenous program placement and endogenous household participation
* do 2sls
regress exp credit x1 x2 land (x1 x2 land zx1 zx2 zland t )
* now do it right
* credit equation estimated only over those with choice
quietly xi: reg credit x1 x2 land I.vill if h==1

```

predict pc

* those without choice have deterministically zero credit

replace pc=0 if h==0

* basic Pitt-Khandker estimates

* two-step with deterministic zero credit for the ineligible

xi: regress exp pc x1 x2 land I.vill

* 2sls equivalent

xi: regress exp x1 x2 land credit I.vill (x1 x2 land h z* I.vill I.vill*t)

* demonstrate that dropping nonprogram villages does not matter. Program effect

* is identified without nonprogram villages

xi: regress exp x1 x2 land credit I.vill (x1 x2 land h z* I.vill I.vill*t) if mu <=1.0

log close

```

*sim5.do
*written by Mark M. Pitt
* This is the case in which the village-level
* attributes (FEs) of only eligible households affect placement
* of the program in villages. This FE is the
* "mu" of the poor (mupoor) and affects both program placement
* and the participation decision of poor households.
* Another village FE (mu), possibly correlated with mupoor, affects consumption.
* Contrary to Morduch's (1998) claim, the Pitt-Khandker(1998) model
* effectively handles this situation as it estimates separate village
* fixed effects for credit and for consumption.
*
* Allows for some eligible nonparticipants
drop _all
set matsize 400
set maxobs 10000 maxvar 300
log using sim5, replace
set obs 90
gen vill=_n
gen common = invnorm(uniform())
gen mu = sqrt(0.8)*invnorm(uniform()) + sqrt(0.2)*common
gen mupoor = sqrt(0.7)*invnorm(uniform()) + sqrt(0.3)*common
corr mu*
* programs are allocated according to the mu of the poor (mupoor)
count if mupoor > 1.0
expand 100
gen x1=uniform() - 0.5
gen x2=uniform() - 0.5
gen land = uniform()
gen h=land<0.5
replace h=0 if mupoor > 1.0
gen e = invnorm(uniform())
gen credit = 1 + 2*x1 + 2*x2 + 2*land + invnorm(uniform()) + (2.0*e) + (2.0*mupoor)
replace credit = 0 if credit < 0
replace credit=0 if h==0
gen exp = 2 + 2*x1 + 2*x2 + 2*land + 1*credit+invnorm(uniform()) + (2*e) +(2*mu)
*naive estimate
regress exp credit x1 x2 land
gen t=h==0
gen zx1=x1*t
gen zx2=t*x2
gen zland = land*t
quietly xi: reg credit x1 x2 land I.vill if h==1
predict pc
replace pc=0 if h==0
* two-step with deterministic zero credit for the ineligible
xi: regress exp pc x1 x2 land I.vill
* 2sls equivalent
xi: regress exp x1 x2 land credit I.vill (x1 x2 land h z* I.vill I.vill*t )
* show it does not matter if you throw out nonprogram villages
xi: regress exp x1 x2 land credit I.vill (x1 x2 land h z* I.vill I.vill*t ) if mupoor<= 1.0
log close

```

```

*sim6.do
*written by Mark M. Pitt
* This is the case in which the village-level
* attributes (FEs) of only eligible households affect placement
* of the program in villages. This FE is the
* "mu" of the poor (mupoor) and affects both program placement
* and the participation decision of poor households.
* Another village FE (mu), possibly correlated with mupoor, affects consumption.
* Contrary to Morduch's (1998) claim, the Pitt-Khandker(1998) model
* effectively handles this situation as it estimates separate village
* fixed effects for credit and for consumption.
*
* Allows for some eligible nonparticipants
drop _all
set matsize 400
set maxobs 10000 maxvar 300
log using sim6, replace
set obs 90
gen vill=_n
gen common = invnorm(uniform())
gen mu = sqrt(0.8)*invnorm(uniform()) + sqrt(0.2)*common
gen mupoor = sqrt(0.7)*invnorm(uniform()) + sqrt(0.3)*common
corr mu*
* programs are allocated according to the mu of the poor (mupoor)
count if mupoor > 0.75
expand 100
gen x1=uniform() - 0.5
gen x2=uniform() -0.5
gen land = uniform()
gen h=land<0.5
replace h=0 if mupoor > 0.75
gen nop = mupoor > 0.75
gen nvill = vill*10 if nop==1
gen e = invnorm(uniform())
gen credit = 5 + 2*x1 + 2*x2 + 2*land + invnorm(uniform()) + (2.0*e) + (2.0*mupoor)
replace credit = 0 if credit < 0
replace credit=0 if h==0
* make credit a binary outcome (to look like Morduch's setup)
replace credit = 1 if credit > 0
gen exp = 2 + 2*x1 + 2*x2 + 2*land + 1*credit+invnorm(uniform()) + (2*e) +(2*mu)
gen t=h==0
gen zx1=x1*t
gen zx2=t*x2
gen zland = land*t
* do it as in Pitt-Khandker (1998), allowing for a FE for the poor affecting credit
xi: regress exp h x1 x2 land I.vill (x1 x2 land z* h I.vill )
* now do it in the way Morduch's incorrectly claims Pitt-Khandkher do - use a single fixed effect
xi: regress exp h x1 x2 land credit I.vill
tab credit if h==1
log close

```

```

*sim7.do
*written by Mark M. Pitt
*This version has gender
* Single gender groups and gender specific effects
* This is the case in which the village-level
* attributes (FEs) of only eligible households affect placement
* of the program in villages, this FE is the
* mu of the poor (mupoor) and affects program placement
* and the participation decision.
* Another village FE (mu), possibly correlated with mupoor, affects consumption
* Pitt-khandker (1998) handles these possibilities
*
* allows for some eligible nonparticipants and
* tacks on average effect estimation
*
* requires more memory to run
*
drop _all
set matsize 800
set maxobs 10000 maxvar 550
log using sim7, replace
set obs 90
gen vill=_n
* whether a village has a program for a gender depends on a gender-specific
* component and a common component
gen common = invnorm(uniform())
gen mu = sqrt(0.8)*invnorm(uniform()) + sqrt(0.2)*common
gen mupoorf = sqrt(0.5)*invnorm(uniform()) + sqrt(0.5)*common
gen mupoorm = sqrt(0.5)*invnorm(uniform()) - sqrt(0.5)*common
corr mu*
* programs are allocated according to the mu of the poor by gender (mupoor)
count if mupoorf > 0.50
count if mupoorm > -0.20
count if mupoorf > 0.50 & mupoorm > -0.20
expand 100
gen x1=uniform() - 0.5
gen x2=uniform() -0.5
gen land = uniform()
gen h=land<0.5
gen hm=h
gen hf=h
* more women's programs and men's
replace hf=0 if mupoorf > 0.50
replace hm=0 if mupoorm > -0.20
gen e = invnorm(uniform())
gen em = (sqrt(2.0)*e) + sqrt(2.0)*invnorm(uniform())
gen ef = (sqrt(2.0)*e) + sqrt(2.0)*invnorm(uniform())
gen creditf = 2 + 2*x1 + 3*x2 + 1.0*land + invnorm(uniform()) + (2.0*ef) + (2.0*mupoorf)
gen creditm = 1 + 1*x1 + 2*x2 + 2*land + invnorm(uniform()) + (2.0*em) + (2.0*mupoorm)
* if both male and female eligible to borrow, the borrower is the one
* with largest latent credit
gen female = (creditf - creditm)
replace creditf = 0 if creditf < 0
replace creditf=0 if hf==0
replace creditf=0 if female==0
replace creditm = 0 if creditm < 0
replace creditm=0 if hm==0
replace creditm=0 if female==1
tab hm hf
gen exp = 2 + 2*x1 + 2*x2 + 2*land+ 1.5*creditf+0.5*creditm+invnorm(uniform()) + (2*e) +(2*mu)

```

```

*naive estimates
gen credit = creditm + creditf
* naive estimates
regress exp credit x1 x2 land
regress exp creditf creditm x1 x2 land
gen tf=hf==0
gen zfx1=x1*tf
gen zfx2=tf*x2
gen zfland = land*tf
gen tm=hm==0
gen zmx1=x1*tm
gen zmx2=tm*x2
gen zmland = land*tm
gen t = (tm==1) | (tf==1)
* ignore male female distinction
quietly xi: reg credit x1 x2 land I.vill if hm==1 | hf==1
predict pc
replace pc=0 if h==0
* two-step with deterministic zero credit for the ineligible
xi: regress exp pc x1 x2 land I.vill
* 2sls equivalent
xi: regress exp x1 x2 land credit I.vill (x1 x2 land h z* I.vill I.vill*t )
* pay attention to male female distinction
* note in doing so we allow for different village FEs for both male and female credit
* as in Pitt-Khandker (1998) which handles this exact case of differential
* village selection on the basis of the unobserved attributes of the poor (mupoor)
quietly xi: reg creditf x1 x2 land I.vill if hf==1
predict pcf
replace pcf=0 if hf==0
quietly xi: reg creditm x1 x2 land I.vill if hm==1
predict pcm
replace pcm=0 if hm==0
* two-step with deterministic zero credit for the ineligible
xi: regress exp pcf pcm x1 x2 land I.vill
* 2sls equivalent
xi: regress exp x1 x2 land creditf creditm I.vill (x1 x2 land h z* I.vill I.vill*tm I.vill*tf )
* does not matter if you throw out nonprogram villages
xi: regress exp x1 x2 land creditf creditm I.vill (x1 x2 land h z* I.vill I.vill*tm I.vill*tf ) if mupoorf <=0.50 |
mupoorm <= -0.20
log close

```

```

*sim8.do
*written by Mark M. Pitt
* Does Morduch's difference-in-the-differences
* correctly estimate program effects in a
* quasi-experiment?
*
*This is the standard set-up with village FE's
* and some eligible nonparticipants
drop _all
set matsize 400
set maxobs 10000 maxvar 300
log using sim8, replace
set obs 90
gen vill=_n
gen mu = invnorm(uniform())
count if mu > 0.75
expand 100
gen x1=uniform() - 0.5
gen x2=uniform() - 0.5
gen land = uniform()
gen h=land<0.5
replace h=0 if mu > 0.75
* the rich (land > 0.5) in the program village have bigger x's (incl. land)
replace x1 = x1 + uniform() + 0.1 if (land > 0.5 & mu < 0.75)
replace x2 = x2 + uniform() + 0.1 if (land > 0.5 & mu < 0.75)
replace land = 1.25*land if (land > 0.5 & mu < 0.75)
gen e = invnorm(uniform())
gen credit = 1 + 2*x1 + 2*x2 + 2*land + invnorm(uniform()) + (2.0*e) + (2.0*mu)
replace credit = 0 if credit < 0
replace credit=0 if h==0
gen exp = 2 + 2*x1 + 2*x2 + 2*land + 1*credit+invnorm(uniform()) + (2*e) +(2*mu)
* naive estimate
reg exp credit x1 x2 land
* Pitt-Khandker estimate
gen t=h==0
gen zx1=x1*t
gen zx2=t*x2
gen zland = land*t
quietly xi: reg credit x1 x2 land I.vill if h==1
predict pc
replace pc=0 if h==0
* two-step with deterministic zero credit for the ineligible
xi: regress exp pc x1 x2 land I.vill
* 2sls equivalent
xi: regress exp x1 x2 land credit I.vill (x1 x2 land h z* I.vill I.vill*t )
* Morduch diffs-in-diffs estimate treating data as experimental
*diffs of nonprogram villages
quietly summ exp if mu > 0.75 & land > 0.5
scalar npland = _result(3)
quietly summ exp if mu > 0.75 & land <= 0.5
scalar npless = _result(3)
scalar npdiff=npless - npland
display npdiff
*diffs of program villages
quietly summ exp if mu < 0.75 & land > 0.5
scalar pland = _result(3)
quietly summ exp if mu < 0.75 & land <= 0.5
scalar pless = _result(3)
scalar pdiff = pless - pland
*program effects by differencing

```

```
display pdiff
*program effect by differencing the differences
scalar diffdiff = pdiff - npdiff
display diffdiff
* Is the above estimate near to the true estimate of +1.0?
log close
```

```

*sim9.do
*written by Mark M. Pitt
* Demonstrate that random mistargeting does not necessarily affect consistency
* Two-sided mistargeting. Special case of mistargeting "fuzz" orthogonal to the
* credit and exp errors.
*
*This is not the argument I rely on in the paper. It is presented simply
*to illustrate and interesting special case.
*
*This is the standard set-up with village FE's
* and some eligible nonparticipants
drop _all
set matsize 400
set maxobs 10000 maxvar 300
log using sim9, replace
set obs 90
gen vill=_n
gen mu = invnorm(uniform())
count if mu > 1.0
expand 100
gen x1=uniform() - 0.5
gen x2=uniform() -0.5
gen land = uniform()
** fuzzy rule, allows for random mistargeting up to 0.5 acres **
* note that landed hh's incorrectly made eligible are more likely to borrow *
gen fuzz = 0.5*(uniform() - 0.5)
gen h=(land + fuzz) < 0.5
replace h=0 if mu > 1.0
** count mistargeted households in program villages **
gen byte landed = land > 0.5
tab h landed if mu < 1.0, row
gen e = invnorm(uniform())
gen credit = 1 + 2*x1 + 2*x2 + 2*land + invnorm(uniform()) + (2.0*e) + (2.0*mu)
replace credit = 0 if credit < 0
replace credit=0 if h==0
gen exp = 2 + 2*x1 + 2*x2 + 2*land + 1*credit+invnorm(uniform()) + (2*e) +(2*mu)
*naive estimate
reg exp credit x1 x2 land
gen t=h==0
gen zx1=x1*t
gen zx2=t*x2
gen zland = land*t
quietly xi: reg credit x1 x2 land I.vill if h==1
predict pc
replace pc=0 if h==0
* two-step with deterministic zero credit for the ineligible
xi: regress exp pc x1 x2 land I.vill
* 2sls equivalent
xi: regress exp x1 x2 land credit I.vill (x1 x2 land h z* I.vill I.vill*t )
log close

```

```

*sim10.do
*written by Mark M. Pitt
* Demonstrates that random mistargeting does not necessarily affect consistency
* One-sided mistargeting. All the landless are eligible
* but some of the landed also get in. Special case of mistargeting "fuzz"
* orthogonal to credit and exp errors.
*
*This is not the argument I rely on in the paper. It is presented simply
*to illustrate and interesting special case.
*
*This is the standard set-up with village FE's
* and some eligible nonparticipants
drop _all
set matsize 400
set maxobs 10000 maxvar 300
log using sim10, replace
set obs 90
gen vill=_n
gen mu = invnorm(uniform())
count if mu > 1.0
expand 100
gen x1=uniform() - 0.5
gen x2=uniform() -0.5
gen land = uniform()
** fuzzy rule, allows for one-sided random mistargeting up to 0.5 acres **
* note that landed hh's incorrectly made eligible are also more likely to borrow *
gen fuzz = 0.5*(uniform() - 0.5)
gen h = land <= 0.5
replace h=1 if (land + fuzz) < 0.5
replace h=0 if mu > 1.0
** count mistargeted households in program villages **
gen byte landed = land > 0.5
tab h landed if mu < 1.0, row
gen e = invnorm(uniform())
gen credit = 1 + 2*x1 + 2*x2 + 2*land + invnorm(uniform()) + (2.0*e) + (2.0*mu)
replace credit = 0 if credit < 0
replace credit=0 if h==0
gen exp = 2 + 2*x1 + 2*x2 + 2*land + 1*credit+invnorm(uniform()) + (2*e) +(2*mu)
*naive estimate
reg exp credit x1 x2 land
gen t=h==0
gen zx1=x1*t
gen zx2=t*x2
gen zland = land*t
quietly xi: reg credit x1 x2 land I.vill if h==1
predict pc
replace pc=0 if h==0
* two-step with deterministic zero credit for the ineligible
xi: regress exp pc x1 x2 land I.vill
* 2sls equivalent
xi: regress exp x1 x2 land credit I.vill (x1 x2 land h z* I.vill I.vill*t )
log close

```

```

*sim13.do
*written by Mark M. Pitt
* Demonstrate that choice of a high enough break-point
* even with non-random mistargeting may still yield consistent
* estimates if breakpoint is set high enough
*
* Two-sided mistargeting
*
*This is the standard set-up with village FE's
* and some eligible nonparticipators
drop _all
set matsize 400
set maxobs 10000 maxvar 300
log using sim13, replace
set obs 90
gen vill=_n
gen mu = invnorm(uniform())
count if mu > 1.0
expand 100
gen x1=uniform() - 0.5
gen x2=uniform() -0.5
gen land = uniform()
** fuzzy rule, allows for non-random mistargeting up to 0.3 acres **
* note that landed hh's incorrectly made eligible are more likely to borrow *
gen fuzz = 0.3*(uniform() - 0.5)
gen factor=fuzz
replace factor = 0.0 if fuzz > 0.15
replace factor = 0.0 if fuzz < -0.15
gen h=(land + fuzz) < 0.5
replace h=0 if mu > 1.0
** count mistargeted households in program villages **
gen byte landed = land > 0.5
tab h landed if mu < 1.0, row
* fuzz factor affects demand for credit and exp directly through error e
gen e = 0.75*invnorm(uniform()) + 5*fuzz
gen credit = 1 + 2*x1 + 2*x2 + 2*land + invnorm(uniform()) + (2*e) + (2*mu)
replace credit = 0 if credit < 0
replace credit=0 if h==0
gen exp = 2 + 2*x1 + 2*x2 + 2*land + 1*credit+invnorm(uniform()) + (2*e) +(2*mu)
*naive estimate
reg exp credit x1 x2 land
*breakpoint is set to actual fuzzy brekpoint of 0.5
*correlated "fuzz" causes bias
*gen t=h==0
gen t=h==0
tab t h
gen zx1=x1*t
gen zx2=t*x2
gen zland = land*t
* with breakpoint of 0.5 get bias
* use t instead of h below
xi: regress exp x1 x2 land credit I.vill (x1 x2 land t z* I.vill I.vill*t )
*now use higher breakpoint to eliminate fuzz error (use 0.65 instead of 0.50)
drop t zx1 zx2 zland
*gen t=h==0
gen t=h==0
replace t = 0 if land < 0.65
tab t h
gen zx1=x1*t
gen zx2=t*x2

```

gen zland = land*t

* Pitt-Khandker estimates with high breakpoint

xi: regress exp x1 x2 land credit I.vill (x1 x2 land t z* I.vill I.vill*t)

log close