High Noon for Microfinance Impact Evaluations: Re-investigating the Evidence from Bangladesh

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Abstract

Recently, microfinance has come under increasing criticism raising questions of the validity of iconic studies which have justified the microfinance phenomenon. This paper applies propensity score matching (PSM), which has become widely used for the analysis of observational data, to the study by Pitt and Khandker (1998) which has been labelled the most rigorous evidence supporting claims that microfinance benefits the poorest especially when targeted on women. After carefully reconstructing the data we differentiate outcomes by gender of borrower, take account of borrowing from several formal and informal sources, and find that the mainly positive impacts of microfinance that we observe are shown by sensitivity analysis to be highly vulnerable to selection on unobservables, and we are therefore not convinced that the relationships between microfinance and outcomes are causal.

Introduction

The concept of microcredit was first introduced in Bangladesh by Nobel Peace Prize winner Muhammad Yunus. Professor Yunus started Grameen Bank more than 30 years ago aiming to reduce poverty by providing small loans to the countries' rural poor (Yunus, 1999). It is argued that microfinance can not only enable the poor to access credit, providing them access to remunerative activities and relieving them of onerous debts (Khandker, 1998; 2000). A key feature of the Grameen Bank and many other microfinance organisations has been the targeting of women on the grounds that, compared to men, they perform better as microfinance institution (MFIs) clients and that their participation has more desirable development outcomes, an argument that is most authoritatively supported by Pitt and Khandker (1998 – henceforth PnK). However, despite the apparent success and popularity of microfinance, it is widely argued that there is little convincing evidence yet that microfinance programmes have positive impacts (Armendáriz de Aghion and Morduch, 2005; 2010); for reviews of microfinance impact evaluations reiterating this point see also Sebstad and Chen, 1996; Gaile and Foster, 1996; Goldberg, 2005; Odell, 2010).

A number of putatively rigorous studies suggest social and economic benefits from microfinance (Hulme and Mosley, 1996; PnK; Khandker, 1998, 2005; Coleman, 1999; Rutherford, 2001; Morduch and Haley, 2002). However, Dichter and Harper (2007), Roodman and Morduch (2009 – henceforth RnM) and Bateman and Chang (2009) argue that microfinance is neither always beneficial nor rigorously demonstrated. The debate over microfinance impact intensified recently with the publication of the first two randomised control trials (RCTs) in the sector (Banerjee et al, 2009; Karlan and Zinman, 2009) which both raise doubts about the causal link between microfinance participation and poverty alleviation.

Many of the early microfinance impact evaluations fail to address the problem of selection bias (Sebstad and Chen, 1996; Gaile and Foster, 1996); selection bias occurs because participants self-select or are selected into a programme (in a non-random way), and therefore differ from those who are not selected; this undermines simple impact estimates based on non-random control groups (Heckman, 1979). A few studies of microfinance have addressed this problem more thoroughly (for example Hulme and Mosley, 1996; PnK); these studies, however, have not been uncontested[1].

This paper re-examines the evidence of what is commonly seen as the most authoritative microfinance impact evaluation (RnM) which was conducted by PnK on three microfinance programmes in Bangladesh. The challenges of microfinance impact evaluations which PnK (Morduch, 1998; RnM) address is to account for participant selection and program placement[2] biases (PnK; Coleman, 1999); PnK do this using a specific model (see below), and Khandker (2005 – henceforth Khandker) adds data on the same households to construct a panel, putatively overcoming at least the problems for evaluation posed by participant selection. A number of studies have attempted to replicate the findings of the original PnK study, and of Khandker. For example, Morduch (1998 – henceforth Morduch) contested PnK but was seemingly refuted by Pitt (1999 – henceforth Pitt)[3]. RnM with considerable effort and difficulty replicated PnK and Khandker, producing variables which in some cases differ significantly from their equivalent in PnK and Khandker, and, using different estimating software, find no convincing evidence for either impact claimed by PnK and Khandker.

Chemin (2008 – henceforth Chemin), applies propensity score matching (PSM) to his construction of the PnK data; PSM has become a very popular technique in the area of development economics in recent years; it has roots in the literature on experiments beginning with Neyman (1923). Rubin (1973a, b; 1974; 1977; 1978) expands on this literature and laid the conceptual foundations of matching. The technique has been further refined in particular by Rosenbaum and Rubin (1983; 1984). PSM is performed by matching participants to non-participants drawn from a suitable population using a predicted probability of programme participation or the 'propensity score' (Ravallion, 2001; Caliendo and Kopeinig, 2005; 2008). The treatment effect is then estimated by comparing the mean outcomes of the participants and their matches (Ravallion, 2001). This method can account for selection bias due to observable characteristics used in the matching process. Its drawback, however, is that bias due to selection on unobservables remains (Smith and Todd, 2005). Selection on unobservables, or 'hidden bias' as Rosenbaum (2002) calls it, are driven by unobserved variables that influence treatment allocation as well

[1] Hulme and Mosley (1996) were contested by Morduch (1999) and PnK by Morduch (1998) and RnM.

[2] The locations of programmes are also chosen in a non-random way and therefore differ from other places that could be used as controls.

[3] The complexity of the PnK and Pitt method, using unique and unrecoverable computer code (see footnote 21 for correspondence between Roodman and Pitt), seemingly meant this debate remained unresolved in the grey literature until RnM replicated PnK.

as potential outcomes (Becker and Caliendo, 2007). Sensitivity analysis of PSM results can identify the vulnerability of the estimated impact to unobservables (Rosenbaum, 2002).

With his construction of the relevant variables from the unit level PnK data, Chemin finds statistically significant but smaller effects than those of PnK on all outcome variables except male labour supply (Chemin: 478). However, he does not distinguish outcomes by the gender of borrowers and does not apply sensitivity analysis, which is good practice in PSM studies (Ichino, Mealli and Nannicini, 2006; Nannicini, 2007). In this paper we apply PSM to our construction of the relevant variables, examine the effects of the gender of the borrower, and subject the results to sensitivity analysis; first we reconstruct the data. We find differences from variables reported in Chemin (but only minor differences from RnM[4]), and draw attention to borrowing from sources other than microfinance by sample households. Borrowing from MFIs may substitute (Khandker, 2000), or complement (Fernando, 1997; Coleman, 1999) borrowing from other sources. Borrowing from more than one source may occur either because the borrower requires more finance than a single MFI will supply[5], or because further finance is needed to make repayments (Fernando, 1997; Coleman, 1999; Venkata and Yamini, 2010). We apply sensitivity analysis to assess the robustness of our results and reflect on the usefulness of PSM in the context of these data.

The paper proceeds as follows: we briefly discuss the challenges of replication and (re-) construction of appropriate variables with the PnK data, and briefly introduce PSM and sensitivity analysis. We then outline the particularities in PnK's research design, apply PSM to (our reconstruction of) the PnK data, investigate effects of the gender of the borrower and the role of borrowing from other sources (by microfinance members and others) on microfinance impact; we apply sensitivity analysis to the matching results to draw conclusions as to the robustness and limitations of PSM in this context and what seems reasonable to conclude with regard to the impacts of microfinance by applying PSM to these data.

While our PSM results suggest that microfinance participation has some significant impacts (negative as well as positive), they are in general not distinguishable from those of other sources of finance. Moreover, sensitivity analysis shows that all impacts are very sensitive to unobservables, which are therefore quite likely to have confounded the results. We conclude that, properly applied with sensitivity analysis, PSM resolves the particular problems in the PnK study by showing that it cannot

[4] Because our constructed variables are so similar to those of RnM we do not exhaustively compare our variables to either RnM or PnK; we note differences with RnM in the relevant places.

[5] Including cases where microfinance borrowers may not be able to borrow sufficient to repay all previous outstanding loans from other sources (Coleman, 1999).

generate robust conclusions of impact with these outcome variables[6]. However, PSM may not be an appropriate tool because the data set does not contain a suitable, large and relatively homogeneous control group. Hence, by extension, PSM may not be the miracle tool implied by the recent epidemic of applications, as we discuss below.

Replication challenges

The objective of replication is to allow other researchers to assess the robustness of the findings (Hamermesh, 2007), and is a characteristic of natural if not social science. To allow replication, good documentation of the study design and data are required, and there should be access to the data, and details of their variable construction and analysis[7].

In the case of PnK, most of the data, including questionnaires and variable codes are (at the time of writing this paper) available on the World Bank website[8] but replication remains a challenge. Firstly, the survey forms and variable descriptions are problematic; secondly certain data necessary for replication were (and others are) missing[9]. Some of these data[10] were obtained after contacting the authors (either by Roodman, or ourselves). The replication exercise reported here was greatly facilitated by RnM who have made all their data and codes available[11].

We have compared our data with RnM's data, variable by variable; remaining minor discrepancies reflect differences in our interpretation of some variables. Nonetheless, re-running RnM's Stata do-files using our data set very closely approximates their substantive results. RnM replicated the key PnK studies[12], using similar estimation

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[6] Other outcome variables have been addressed in other papers using the PnK dataset (Pitt et al, 1999; Pitt, 2000; McKernan, 2002; Pitt and Khandker, 2002; Pitt et al, 2003; Pitt, Khandker and Cartwright, 2006).

[7] The American Economic Review (AER), for example, requires its authors to make their data sets and code available which are then uploaded onto a website maintained by the AER especially for this purpose. Authors have been compliant with this policy so far but can opt out in case their data are proprietary and/or confidential (Hamermesh, 2007: 717).

[8] http://econ.worldbank.org/WBSITE/EXTERNAL/EXTDEC/EXTRESEARCH/0,,contentMDK:21470820~pagePK:64214825~piPK:64214943~theSitePK:469382,00.html. All data used for this paper come from this source.

[9] It is not possible to be sure that the data posted are indeed exactly the same as those analysed by PnK, but the main problems probably lie not in variations in the raw data but in subsequent manipulations, variable constructions, and analytical procedures.

[10] Such as data on consumer price indices, sampling weights and landholding details.

[11] http://www.cgdev.org/content/publications/detail/1422302. The data and variable construction are mainly in SQL, although statistical analysis is in Stata; our data manipulation and analysis is all in Stata.

[12] RnM do not replicate Chemin or a few other studies that used the PnK data (Khandker, 1996, 2000; Pitt et al, 1999; Pitt, 2000; McKernan, 2002; Pitt and Khandker, 2002; Pitt et al, 2003; Menon, 2006; Pitt, Khandker and Cartwright, 2006).

strategies but different software[13]. They find that 'decisive statistical evidence in favor of [*the idea that microcredit alleviates poverty, smoothes household expenditure and lessens the pinch of hunger especially when women are involved in borrowing*] is absent from these studies' (RnM: 40). We apply PSM which, as used by Chemin, currently provides the only remaining credible evaluation of microfinance using these data.

The Impact of Microfinance in Bangladesh

PnK use data from a World Bank funded study which conducted a survey in three waves in 1991-1992[14] on three leading microfinance group-lending programmes in Bangladesh, namely Grameen Bank (GB), the Bangladesh Rural Advancement Committee (BRAC) and the Bangladesh Rural Development Board (BRDB) (PnK: 959). A quasi-experimental design was used which sampled target (having a choice to participate/being eligible) and non-target households (having no choice to participate/not being eligible) from villages with microfinance programme (treatment villages) and non-programme villages (control villages).

The survey was conducted in 87 villages from 29 thanas[15]; the treatment villages were randomly selected from a list of villages provided by the MFIs' local offices and the control villages were randomly selected from the governments' village census; 1,798 households were selected out of which 1,538 were target households and 260 were non-target households (PnK: 974). According to PnK (974), out of those 1,538 households, 905 effectively participated in microfinance (59%). The three survey waves (henceforth R1-3) were timed to account for seasonal variations, (Pitt, 2000:28-29). The study focuses on measuring the impact of microfinance participation by gender on indicators such as labour supply, school enrolment, expenditure per capita and non-land asset ownership. PnK find that microcredit has significant positive impacts on many of these indicators and find larger positive impacts when women are involved in borrowing. For example, 'annual household consumption expenditure, […], increased 18 taka for every 100 additional taka borrowed by women from these credit programs [*GB, BRAC, BRDB*], compared with 11 taka for men' (PnK: 988)[16].

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[13] Our replication of PnK confirms RnM notwithstanding minor differences in variable construction (our differences with RnM arise, mainly, from different interpretation of variables, for example we included savings-in-kind when calculating non-landed asset variables and worked with slightly different assumptions when calculating landed asset variables. More details are available from the authors).

[14] In areas not affected by the cyclone of April 1991.

[15] A thana (literally police station, also known as upazila) is a unit of administration in Bangladesh; in 1985 there were 495 upazilas (Bangladesh Bureau of Statistics, 1985) and 507 upazilas in 2001 (Bangladesh Bureau of Statistics, 2004).

[16] A follow-up data set (henceforth R4) was collected in 1998-1999 re-surveying the same households that were already interviewed in R1-3 and some new households increasing the overall sample size to 2,599 households (Khandker: 271). Khandker uses standard panel analysis to conclude that

PnK adopt an estimation strategy for assessing the impact of microfinance participation involving comparisons of 'treated' and 'non-treated' households in 'treated' villages and 'non-treated' households in 'non-treated' (control) villages. Treatment refers to participating in the loan programme one of the selected MFIs; at the household level this varies according to the gender of the borrower, and at the village level to the presence of the MFI in the village. However, comparing households in treatment and control villages is not sufficient for obtaining impact estimates for microfinance programme participation because the villages differ (there is programme placement bias[17]) and households commonly self-select into microfinance. In this type of group-based lending individuals select themselves, can be selected (or excluded) by their peers and/or by microfinance loan officers, giving rise to selection bias.

In principle all the MFIs operate an eligibility criterion that participating households should be cultivating[18] less than 0.5 acres of land at the time of recruitment into the MFI programme, so that only households meeting this criterion are eligible. In fact, the eligibility criterion is not strictly met by quite a few microfinance borrowers as pointed out by Morduch, so that there is a gap between participation and eligibility[19]. PnK use the (de facto) participation criterion as their identification strategy, assuming that it is exogenous. They sample treatment and control villages containing non-target/landed and target/landless households. PnK's (ideal) identification strategy can be understood graphically by looking at Figure 1.

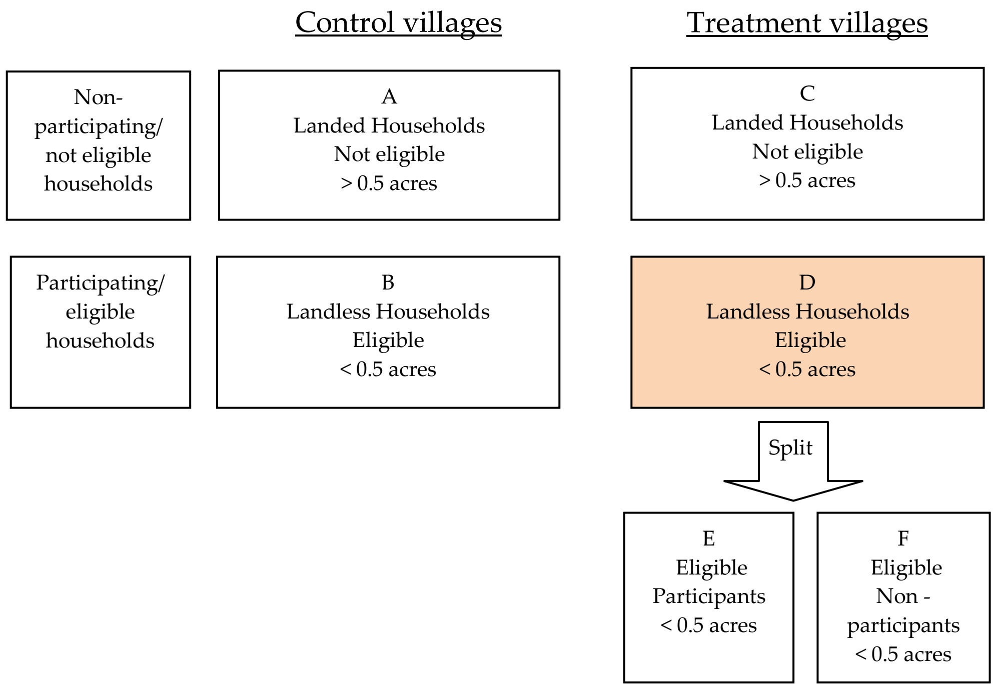
microcredit has positive impacts on the poorest and reduces poverty among programme participants, especially when women are involved in borrowing, and thus confirms PnK's headline results. However, RnM's replication of Khandker casts doubts about Khandker's approach and findings (RnM: 39). Using our, slightly different data we concur with RnM that panel estimation does not show clear evidence of microfinance impact. We do not further discuss this approach here.

[17] The assumption was that MFIs choose more remote and backward villages (PnK; Coleman, 1999). Hence, microfinance impact may vary according to village type.

[18] There is some confusion about whether the eligibility criterion is cultivated (operated) or owned land, and whether this includes homestead land.

[19] Thus there are de jure (cultivating less than 0.5 acres), and de facto (participating) eligibility categories; this is discussed further below.

Figure 1: Intended identification strategy



Source: Authors illustration based on Morduch and Chemin.
Notes: This diagram ignores that the eligibility criterion was not strictly (literally) enforced. Thus the actual strategy used (de facto) participation.

PnK suggest comparing the discontinuity between participant (eligible) and non-participant (not eligible households in treatment and control villages; that is, the discontinuity or cut-off point at the boundary between group B and A in control villages, and between group D to C in treatment villages (Figure 1). The difference between these two sets of comparisons is estimated by applying village-level fixed-effects to account for unobserved differences between treatment and control villages.

The application of an eligibility criterion as an identification strategy is plausible provided it is strictly enforced. However, as Morduch points out, mistargeting occurred (see also Ravallion, 2008: 3818; Chemin: 465). Group D contains participants who own more than 0.5 acres of land. Pitt rationalises this by claiming that the value of land of treated households which cultivate/possess more than 0.5 acres is so low that the value of the land of these households is effectively less than the median value of 0.5 acres of average land; however, in control villages (groups A and B) households were categorised as eligible based on the less than 0.5 acres of cultivated land alone[20].

[20] This issue is addressed in appendix 5.

None of the authors who re-visited the original PnK study could replicate and confirm the original findings of PnK (Morduch; Chemin; RnM)[21].

PSM, PnK and the role of multiple sources of borrowing

Most microfinance impact evaluations are designed on the assumption that other formal and informal credit organisations are absent and would not have entered the financial markets in the absence of MFIs. However, as illustrated by Figure 2 (and Appendix 3), this is not what the data show. Households in the PnK data obtain loans not only from MFIs but also from other formal and informal sources, for example from formal sources such as government controlled banks like the Krishi Bank or from informal sources such as relatives, friends, landlords, traders, moneylenders, and so on. Khandker (2000) investigates the impact of microfinance on informal borrowing using a two-step approach. He finds that microcredit borrowing appears to reduce borrowing from informal sources, but does not explore the impact of other sources of borrowing on the outcomes explored in PnK.

While much of the literature seems to assume otherwise, there is evidence that the poor choose to borrow from multiple sources for various reasons, including for purposes not sanctioned by MFIs (Fernando, 1997; Coleman, 1999), and do not just access microfinance to access credit or reduce the burden of traditional sources of credit (as argued by Khandker, 2000). For example, poor borrowers use (fungible) credit for consumption; to augment microfinance loans which are rationed in order to invest in more remunerative activities which require larger amounts of credit; to make the regular payments required by MFIs when the income from the activities in which they have invested does not yield the regular returns required to meet the repayment schedule, to improve their portfolios, and, no doubt, other reasons. Those with different portfolios will have different observable and unobservable characteristics. Thus, a comparison of (eligible) participants with (eligible) non-participants will include among the participants those who also borrow from other sources, and similarly among the control group(s); these groups will be quite heterogeneous, as will any impacts of microfinance borrowing. While it might be desirable to compare more homogenous sub-groups separately so one could distinguish differences in impacts and probably obtain more precise and statistically significant results, this is constrained by sample sizes in existing data sets.

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[21] Apparently the data sets and code used for PnK were archived on CD-ROMs which are no longer readable (correspondence from Pitt to Roodman on February 28, 2008). Others who have used these data using similar procedures to PnK cannot supply their data or code (see personal communication with McKernan on April 16, 2009). Hence, it remains moot as to whether the differences between PnK and RnM are due to (1) differences in the raw data used; (2) differences in variable construction; or, (3) differences in the statistical estimations. (1) and (2) cannot be assessed, but those with the appropriate skills can assess RnM.

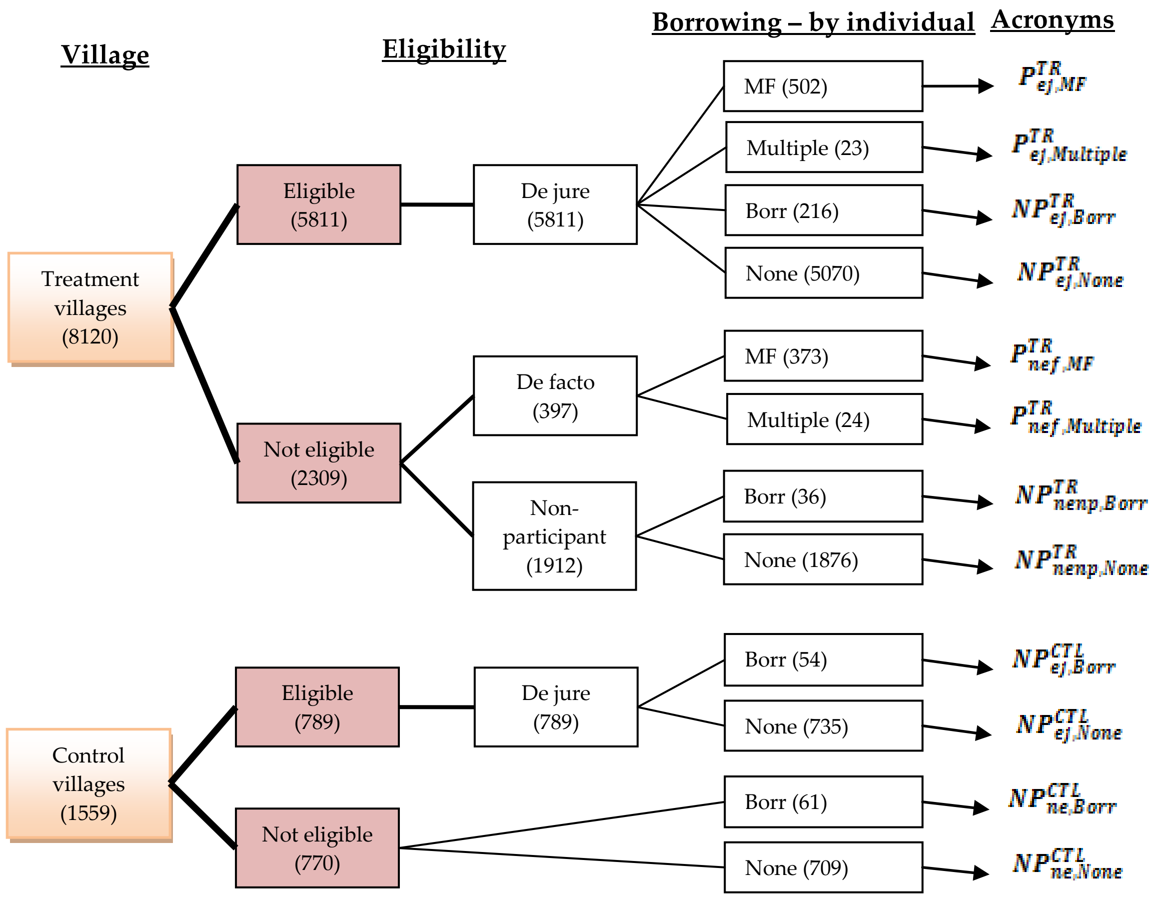
Figure 2 reports the distribution of individuals by reported borrowing characteristics[22]. Of 922 de facto (including de jure) microfinance participants[23] 47 had sources of borrowing other than microcredit. Among the eligible non-participating individuals in treatment villages, 216 had borrowing from other formal or informal sources, but 5,070 (87%) did not report borrowing. 397 (17%) not eligible individuals in treatment villages (out of 2,309 not eligible individuals) participated in microfinance – a significant proportion. In all the treatment villages 299 individuals had borrowings from other sources. In the control villages, there were a lower proportion of eligible individuals, but the borrowing from non-microfinance sources in R1-3 was much greater than among treatment villages (8% versus 3.5%, or 6.8 versus 4.1% among the de jure eligible)[24]. This suggests that microfinance may have partly crowded out other formal or informal sources of borrowing. Thus the empirical strategy envisaged by PnK may be misleading since a comparison between treatment and control group members is most probably confounded. Therefore, an alternative strategy using comparisons between different categories of borrowers and with non-borrowers may be more appropriate to identify heterogeneous impact estimates.

[22] Borrowing is reported in the data by individual. We assume for the purposes of this exposition that the reported borrower is acting autonomously and is not a proxy for another household member, as it is sometimes suggested (see Goetz and Sen Gupta, 1996).

[23] 502 + 23 + 373 + 24 = 922 borrowers (microfinance as well as non-microfinance sources); 23 + 24 = 47 microfinance participant which also use other non-microfinance sources. The sample of 47 is too small and cannot be used to identify further more homogeneous sub-groups within this sub-group.

[24] Appendix 3 provides a more detailed breakdown of individuals by borrower characteristics and by treatment and control villages across eligibility criteria to further illustrate that while it might be desirable to compare more homogenous sub-groups separately; this is likely to be constrained by small sample sizes.

Figure 2: Availability of treatment options in PnK study



Source: Authors illustration using PnK data, see footnote 8.

Notes:

1. MF=Participant in microfinance only; Multiple=Participant in microfinance and other non-microfinance (formal/informal) borrowing; Borr=Participant in other non-microfinance (formal/informal) borrowing; None=No borrowing at all
2. The number of individuals is given in brackets.
3. Eligibility is < 0.5 acres of land.
4. Explanation of acronyms:

$$Y^{a}_{b,c}$$

Where:

Y = treatment status

a = village type (TR=treatment village, CTL=control village); b = eligibility (ej=eligible de jure, nef=not eligible de facto, nenp=not eligible non-participant, ne=not eligible); c = treatment option (MF=MF, Multiple=Multiple, Borr=Borr, None=None)

While t-tests between different treatment and control groups, or simple analysis of variance can be applied with treatment and borrowing categories as factors, PSM matches participants and non-participants from within different groups on the basis of observable characteristics, reducing heterogeneity in the control group (Caliendo and Kopeinig, 2008). Firstly, as already noted, a significant proportion of microfinance borrowers are not formally eligible. Secondly, there is the question of whether microfinance participants who borrow from other sources should be considered similar to those who borrow from MFIs alone; for example, being unable to meet regular microfinance repayments, causing them to borrow from other sources, or having greater demand for credit because of observed (or unobserved) characteristics[25]. Borrowing from other sources cannot be included in the logit model (discussed below) because of potential endogeneity. It is possible that they have different characteristics.

The comparisons we propose are empirically derived and are guided by the number of observations available in each respective group. All comparisons are for individuals and include the spouses of microcredit participants as potential matches (noting that sex is a variable in the logit model which is discussed below). Thus, using the acronyms introduced in Figure 2 we compare persons who borrow only from an MFI to borrowers from MFI and other sources ($P^{TR}_{ej,MF} + P^{TR}_{nef,MF}$ versus $P^{TR}_{ej,Multiple} + P^{TR}_{nef,Multiple}$), since these groups may differ in observables and unobservables accounting for their different borrowing characteristics. However, this comparison will not yield useful results since the sample size of the latter group ($P^{TR}_{ej,Multiple} + P^{TR}_{nef,Multiple}$) contains few individuals for matching (see Appendix 3 for a further breakdown of this group by formal and informal sources of borrowing as well as by village type). Thirdly, since not eligible non-participants are observably different to eligible participants they are not a suitable control group (except perhaps for the non-eligible MFI borrowers). Fourthly, there is the question of whether the population of control villages can be considered appropriate counterfactuals at all since the village economies differ in ways which mean that the eligible participants (owning less than 0.5 acres of land) are significantly different from eligible non-participants in the control villages in observables, unobservables and due to living in a context which is different in complex ways from treatment villages.

Nevertheless, the eligible individuals in the control villages may be the most suitable control group (with or without those who borrow from non-microfinance sources), that is $NP^{CTL}_{ej,Borr} + NP^{CTL}_{ej,None}$. The next most appropriate control group may be the eligible non-participants in treatment villages (with or without those who borrow from non-microfinance sources - $NP^{TR}_{ej,Borr} + NP^{TR}_{ej,None}$), even though these people, presumably having the opportunity to borrow from MFIs, either self-selected out or were excluded possibly as the result of unobservables.

[25] Discussing the theoretical aspects of rural financial markets would extend an already long paper beyond its main purposes described above.

The specific comparisons are:

1. All MFI borrowers versus eligible non-borrowers[26]:

$$P^{TR}_{ej,MF} + P^{TR}_{nef,MF} \ versus \ NP^{TR}_{ej,None} + NP^{CTL}_{ej,None}$$

This comparison looks at all (de facto[27]) microfinance participants versus all other eligible individuals in both treatment and control villages that do not report any other borrowing. Since all individuals in this comparison fulfil the eligibility criterion, we assume a certain degree of homogeneity of members of these groups which makes them suitable for comparison.

2. All MFI borrowers versus all non-borrowers:

$$P^{TR}_{ej,MF} + P^{TR}_{nef,MF} \ versus \ NP^{TR}_{ej,None} + NP^{TR}_{nenp,None} + NP^{CTL}_{ej,None} + NP^{CTL}_{ne,None}$$

This comparison is analogous to comparison 1; it compares de jure and de facto microfinance participants versus all other individuals but irrespective of eligibility across treatment and control villages that do not have any other borrowing at all.

3. All borrowers (any source) versus all non-borrowers:

$$P^{TR}_{ej,MF} + P^{TR}_{ej,Multiple} + NP^{TR}_{ej,Borr} + P^{TR}_{nef,MF} + P^{TR}_{nef,Multiple} + NP^{TR}_{nenp,Borr} + NP^{CTL}_{ej,Borr} + NP^{CTL}_{ne,Borr} \ versus \ NP^{TR}_{ej,None} + NP^{TR}_{nenp,None} + NP^{CTL}_{ej,None} + NP^{CTL}_{ne,None}$$

In this comparison all individuals that participate in either microfinance or other non-microfinance borrowing across treatment and control villages and across eligibility criteria are pooled.

4. All MFI borrowers versus borrowers from other non-microfinance (formal and informal) sources:

$$P^{TR}_{ej,MF} + P^{TR}_{nef,MF} \ versus \ NP^{TR}_{ej,Borr} + NP^{TR}_{nenp,Borr} + NP^{CTL}_{ej,Borr} + NP^{CTL}_{ne,Borr}$$

This last comparison 4 examines de jure and de facto microfinance participants versus individuals that have other non-microfinance borrowing across treatment and control villages irrespective of eligibility. Descriptive statistics for individuals belonging to the respective treatment groups are in Appendix 2.

[26] We also compared eligible microfinance borrowers versus eligible non-borrowers and versus all non-borrowers, the results did not yield any meaningful differences to the results obtained from comparison 1 and 2.

[27] Using de jure microfinance borrowers does not alter the results.

As mentioned earlier, PnK find microcredit is more effective when women are involved. We also provide separate impact estimates for women and men separately[28].

Determinants of microfinance participation

Having identified relevant groups to compare, we now describe the matching process. We derive a model of observable variables that predicts their likelihood of microfinance participation (their propensity score), match treatment and controls using the propensity score, and then compute the treatment effects for the various comparison groups. Given the variables that the PnK data provide, the following propensity score model draws on Coleman (1999), Alexander (2001), Armendáriz de Aghion and Morduch (2005), Coleman (2006) and Chemin[29]:

$$\text{(1)} \qquad \text{Logit}\,(y_{ij}) = \alpha + \beta C_{ij} + \gamma G_{ij} + \delta Z_{ij}$$

Where:

y_{ij} = participating household

C_{ij} = vector of individual-specific variables

G_{ij} = vector of household-specific variables

Z_{ij} = village-level fixed-effects

The dependent variable (y_{ij}) in the model presented in equation (1) represents eligible participants (*i*) in village (*j*); a value of 1 is assumed when an individual participates and a value of 0 if not. C_{ij} is a vector of individual-specific variables such as age and marital status, and G_{ij} is a vector of household-specific variables representing variables such as education and wealth. Z_{ij} is a vector of village level variables. All estimations use village-level fixed-effects.

[28] We would ideally split other non-microcredit sources of borrowing (Y^{Borr}) into formal and informal sources but with the PnK data the comparison groups become too small to provide any meaningful results (see Appendix 3 for more details).

[29] We do not dwell in detail on the problems of replicating Chemin here or differences in our results. Suffice to say that the code available to us did not allow us to exactly replicate the descriptive statistics or the logit coefficients reported by Chemin. As mentioned earlier, our data set approximates that of RnM.

Table 1: Logistic regression model for all four treatment groups across treatment and control villages and across eligibility criteria

Independent variables	Y^{MF}	$Y^{Multiple}$	Y^{Borr}	Y^{None}
Sex HH head (male=1)	0.836*** *0.000*	0.792*** *0.000*	-0.472 *-0.333*	0.656*** *-0.001*
Age (years)	0.007** *-0.025*	0.008*** *-0.008*	0.044*** *0.000*	0.021*** *0.000*
Age household head (years)	-0.007* *-0.081*	-0.010** *-0.014*	-0.024*** *0.000*	-0.015*** *0.000*
Number adult male in household	-0.270*** *0.000*	-0.260*** *0.000*	-0.175** *-0.039*	-0.221*** *0.000*
Marital status (yes=1)	1.173*** *0.000*	1.179*** *0.000*	2.029*** *0.000*	1.472*** *0.000*
Highest education of any household member	0.007 *-0.615*	0.010 *-0.470*	0.070*** *-0.001*	0.035*** *-0.005*
Highest education any female household	-0.069*** *0.000*	-0.074*** *0.000*	0.022 *-0.352*	-0.043*** *-0.007*
Livestock value	-0.000** *-0.031*	-0.000* *-0.063*	0.000 *-0.417*	0.000 *-0.210*
Own non-farm enterprise (yes=1)	0.322*** *0.000*	0.324*** *0.000*	0.050 *-0.666*	0.263*** *0.000*
Household size	-0.041* *-0.081*	-0.046** *-0.042*	0.005 *-0.875*	-0.032* *-0.099*
Village dummies	Yes	Yes	Yes	Yes
Number of observations	5436	5436	5436	5436
Pseudo R-squared	0.125	0.129	0.135	0.103

Source: Authors calculations. For differences with Chemin, see footnote 29.
Notes: p-values in italics. * significant at 10%, ** significant at 5%, *** significant at 1%. Using PnK data, see footnote 8. Control variables such as savings, landholdings of household head's parents and landholdings of household head's brothers were included, all insignificant. Descriptive statistics for all four treatment groups can be found in Appendix 2.

Table 1 shows that the main variables that are statistically significant across all four treatment groups are age, age of household head, number of adult males in the household and marital status. Highest education of any female household member, ownership of a non-farm enterprise, sex of household head and household size are statistically significant across Y^{MF}, $Y^{Multiple}$ and Y^{None}. However, the sign of the coefficients and the level of significance vary from group to group. Further, note that the pseudo R-squared for the various models is rather low ranging from 0.103 to 0.135 (and lower than reported by Chemin).

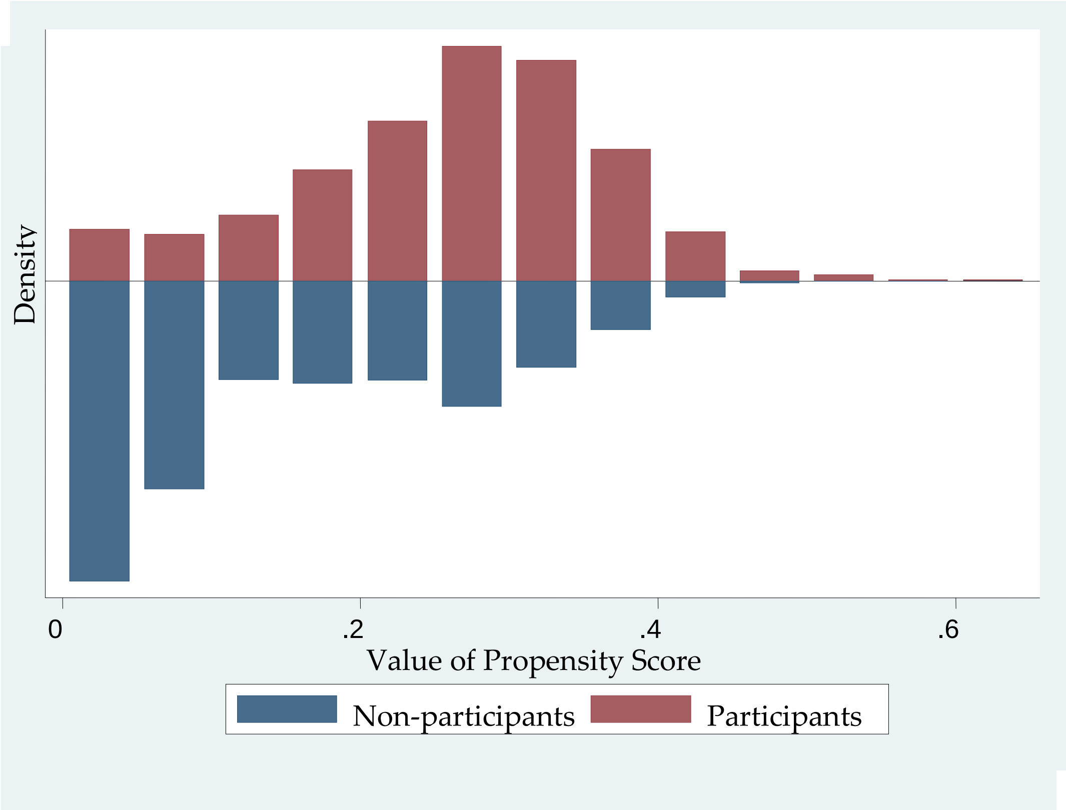
Treatment group results

As mentioned above, the basic idea of matching is to compare a participant with one or more non-participants who are similar in terms of a set of observed covariates *X* (Caliendo and Kopeinig, 2005; 2008). This requires predicting propensity scores for each individual, that is participants as well as non-participants using a logit or a probit model. We used the logit model presented in Table 1. Before implementing the actual matching process, we examine whether the common support assumption is satisfied.

Where the densities of the estimated propensity scores for participants and non-participants overlap reasonably well, the common support assumption is met (Abou-Ali et al, 2009). Figure 3 presents the density of propensity scores, indicating somewhat different densities for participants (Y^{MF}) and non-participants (Y^{None}) (pooling treatment and control villages). This implies that many households would not be good matches as the density of propensity scores of potential controls occurs at low propensity scores while than of treatment households are at high propensity scores[30]. Graphs (not shown) for other comparisons are similar even when using only de jure eligible persons.

[30] This is further explored in Appendix 4.

Figure 2: Distribution of propensity scores for participants (Y^{MF}) and non-participants (Y^{None}) (comparison 2), all villages



Source: Authors calculations.

Table 2 and Table 3 present the differences in the outcome variables for participants and their matched non-participants for all four comparisons described earlier. Table 2 illustrates the impact estimates for microcredit participation for all participants (male and female together) while Table 3 provides impact estimates for male and female participants separately. Two different matching algorithms were applied, nearest neighbour matching with replacement[31], and kernel matching using three different bandwidths (0.01, 0.02 and 0.05), to assess the degree of variability of the different matching results across algorithms[32].

The distributions of the covariates for the treatment and controls need to be similar, that is balanced (Abou-Ali et al, 2009). Our comparisons all pass balancing tests[33],

[31] This allows a control household to match to more than one treatment household.

[32] The literature on the choice of matching algorithms is not yet very developed. Morgan and Winship (2007: 109) argue that kernel matching, introduced by Heckman et al (1998) and Heckman, Ichimura and Todd (1998) appears to be the most efficient and preferred algorithm. Nearest neighbour matching was chosen for its popularity, which is probably due to it being easy to understand and easy to implement. We present only the kernel matching estimates with a bandwidth of 0.05. All other results can be obtained from the authors.

[33] The Stata command pstest was used.

although the differences between treatment and control group means were reduced considerably by matching in most cases.

Table 2: Simple matching estimates across gender using kernel matching bandwidth 0.05 for all four comparison groups

Outcome variables	Y^{MF} vs eligible Y^{None}	Y^{MF} vs Y^{None}	$Y^{MF} + Y^{Multiple} + Y^{Borr}$ vs Y^{None}	Y^{MF} vs Y^{Borr}
Comparison	**1**	**2**	**3**	**4**
	Kernel matching, 0.05[34]			
Variation of log per capita expenditure (Taka)	-0.014**	-0.014**	-0.001	-0.034*
Log per capita expenditure (Taka)	-0.019	-0.011	0.019	-0.089**
Log women non-landed assets (Taka)	1.036***	0.498***	0.349**	-0.022
Female labour supply, aged 16-59, hours per month	52.63***	57.81***	31.86***	78.43***
Male labour supply, aged 16-59, hours per month	-30.33**	-47.06***	40.00***	-276.22***
Girl school enrolment, aged 5-17 years	0.053*	0.060*	0.061**	0.077
Boy school enrolment, aged 5-17 years	0.027	0.035	0.060**	-0.011

Source: Authors calculations.
Notes: *statistically significant at 10%, **statistically significant at 5%, ***statistically significant at 1%. Using PnK data, see footnote 8. Stata routine psmatch2[35] using the logit model outlined in Table 1 is used. Standard errors (not reported) are bootstrapped.

The results in Table 2 are rather mixed, with different comparisons showing different levels of significance for different outcome variables. When comparing Y^{MF} versus all eligible and not eligible Y^{None} (comparison 2), microcredit participation appears to significantly improve women's non-landed assets, female labour supply and girls' school enrolment, for example female microfinance participants appear to work 57 hours more per month (presumed benefit) than non-participants. However, when

[34] 1-nearest neighbour matching as well as kernel matching with bandwidth 0.01 and 0.02 were applied in addition to 0.05 but the various algorithms and bandwidths results did not differ significantly and thus only the results using a bandwidth of 0.05 are shown here.

[35] Robustness checks were conducted using different Stata routines including psmatch2 (Leuven and Sianesi, 2003), and pscore (Becker and Ichino, 2002). The results obtained did not vary significantly.

directly comparing microfinance participants with participants in other non-microfinance financing schemes (comparison 4), microfinance participants do worse than non-microfinance borrowers in terms of log of per capita expenditure and the variation thereof. Comparison 1, Y^{MF} versus eligible Y^{None}, suggests that microcredit participation has significant negative impacts on the variation of the log of per capita expenditure, but that it significantly improves women's non-landed assets, female labour supply and girls' school enrolment. However, most other outcome variables remain insignificant within this comparison. Comparison 3 indicates significant positive impacts on all outcome variables except the log of per capita expenditure and its variation which are insignificant, implying that microfinance in combination with other forms of finance makes a bigger difference to the lives of the poor.

The results above recur for women's borrowing (see Table 3). It seems that microfinance participation has an apparently significant positive impact on female related outcome variables such as women's non-landed assets, female labour supply and partially on girls' school enrolment (see comparisons 1, 2 and 3). However, there are little significant effects on the remaining variables. Noteworthy are the significantly negative impacts of microfinance participation on the log of per capita expenditure and the variation thereof as indicated by comparisons 1, 2 and 4 in Table 2 and Table 3; this is in contrast to PnK's headline findings.

Table 3: Matching estimates of impact by gender (kernel matching bandwidth 0.05 for all four comparison groups)

Outcome variables		Y^{MF} vs eligible Y^{None}	Y^{MF} vs Y^{None}	$Y^{MF} + Y^{Multiple} + Y^{Borr}$ vs Y^{None}	Y^{MF} vs Y^{Borr}
Comparison		**1**	**2**	**3**	**4**
		Kernel matching, 0.05[36]			
Variation of log per capita expenditure (Taka)	Women	-0.017**	-0.016*	-0.009	-0.047
	Men	-0.017**	-0.030***	-0.001	-0.039*
Log per capita expenditure (Taka)	Women	-0.019	-0.013	0.012	-0.126*
	Men	-0.021	-0.046**	0.015	-0.079**
Log women non-landed assets (Taka)	Women	1.009***	0.754***	0.561***	-0.848
	Men	1.297***	-0.000	0.244	0.249
Female labour supply, aged 16-59, hours per month	Women	54.42***	101.64***	54.71***	32.77
	Men	-43.74***	-42.02***	30.85***	93.03***
Male labour supply, aged 16-59, hours per month	Women	-51.80***	-257.41***	-49.52***	-83.27
	Men	18.42	401.30***	49.83***	-329.40***
Girl school enrolment, aged 5-17 years	Women	0.040	0.067*	0.061**	-0.216
	Men	0.097***	0.032	0.060**	0.133
Boy school enrolment, aged 5-17 years	Women	0.029	0.045	0.050	-0.128
	Men	0.039	-0.001	0.054**	-0.017

Source: Authors calculations.
Notes: Notes: *statistically significant at 10%, **statistically significant at 5%, ***statistically significant at 1%. Using PnK data, see footnote 8. Stata routine psmatch2[37] using the logit model outlined in Table 1 is used. Standard errors (not reported) are bootstrapped.

[36] As in the case of the results presented in Table 2, 1-nearest neighbour matching as well as kernel matching with bandwidth 0.01 and 0.02 were applied in addition to 0.05 but the various algorithms and bandwidths results did not differ significantly and thus only the results using a bandwidth of 0.05 are shown here.
[37] As before, robustness checks were conducted using different Stata routines. The results obtained did not vary significantly.

To conclude, the findings presented in Table 2 and Table 3 are mixed and it is not obvious that microcredit participation is associated with more significant impacts than participation in other non-microcredit sources of borrowing. Comparison 3 which looks at $Y^{MF} + Y^{Multiple} + Y^{Borr}$ versus all eligible and not eligible Y^{None} suggests that microfinance in combination with other forms of finance makes a real difference, while microfinance alone compared to other sources of finance (Y^{MF} versus Y^{Borr}) has mixed or even significantly negative impacts (comparison 4).

The results in Table 2 provide evidence that participation in both microcredit and other sources of borrowing is associated with significant positive effects for some outcome variables. It appears that any form of finance – microcredit, formal or informal borrowing - can be associated with higher well-being of participating households.

However, when examining the results by gender (see Table 3), we find that impacts for male labour supply is greater in the case of male borrowing (and female labour supply falls). Similarly the impact of female labour supply is greater for women in the case of female borrowing (and male labour supply falls).

To summarise our arguments so far; relatively few households served as matches as illustrated by Figure 3 and further explored in Appendix 4. This raises the question of the suitability of PSM in the context of PnK. The PnK data set has very few households in control villages (n=260), many of which are not likely matches not least because of because large differences in landholdings makes them 'not eligible'. There are relatively few non-borrowing eligible households in treatment villages, and anyway these are likely different in significant ways to microfinance borrowers by the very fact that they are not microfinance borrowers although they could have been. This is a limitation of the sampling strategy described above, and is a major drawback since PSM works best when there are more control than treatment households (Smith and Todd, 2005). Moreover, a rich and high quality data set is required to optimise results (Smith and Todd, 2005), which appears not to be the case here.

Sensitivity analysis on treatment group comparisons

Although significant effects are found using PSM it is questionable whether these are robust to unobservables because PSM cannot control for unobservable characteristics. Rosenbaum (2002) developed sensitivity analysis to explore the robustness of matching estimates to selection on unobservables (Rosenbaum, 2002). Ichino, Mealli and Nannicini (2006) argue that 'sensitivity analysis should always accompany the presentation of matching estimates' (19).

Rosenbaum (2002) invites us to imagine a number Γ (gamma) (≥ 1) which captures the degree of association, of an unobserved characteristic with the treatment and outcome, required for it (the unobserved characteristic) to explain the observed impact. Γ is the ratio of the odds[38] that the treated have this unobserved characteristic to the odds that the controls have it; a low odds ratio (near to one) indicates that it is not unlikely that such an unobserved variable exists. Cornfield et al (1959) use the example of the effect of smoking on lung cancer. In this case, which is now surely without doubt, data from the late 1950s gives a gamma > 5 for such an unobserved variable, which is, it is suggested, highly unlikely to have been unobserved because of its strong association between smoking and death.

This approach can be implemented using the **rbounds** procedure in Stata (Becker and Caliendo, 2007); this procedure uses the matching estimates to calculate the confidence intervals (for a given level of confidence – for example 95%) of the outcome variable for different values of Γ. A value of Γ that produces a confidence interval that encompasses zero is one that would make the estimated impact not statistically significant at the relevant level of confidence. If the lowest Γ (which encompasses zero) is relatively small (say < 2) then one may assert that the likelihood of such an unobserved characteristic is relatively high and therefore that the estimated impact is rather sensitive to the existence of unobservables (DiPrete and Gangl, 2004). Conversely, if the value of Γ that produces a confidence interval encompassing zero is large (say > 5) then it is rather unlikely that such a variable would not have been discovered, since its association with the outcome is so high. In this case one can say that the effect is rather robust to unobservables, and it appears unlikely that such a confounding variable would not have been observed.

Sensitivity analysis can be illustrated by calculating the Γ at which the estimated impact of microfinance participation on the log of women's non-landed assets for comparison 2 is no longer statistically significant. Table 2 shows that the kernel matching impact estimate with a bandwidth of 0.05 for the log of women's non-landed assets is 0.498 which is statistically significant at 1%. However, this may not be due to membership *per se* but to unobserved characteristics that account for membership (and or its impact). Sensitivity analysis explores the vulnerability of this impact estimate to selection on unobservables.

Table 4 reports the **rbounds** results, showing that when Γ = 1.2, a relatively small difference in the odds of exposure, or more, the 95% confidence interval of the point impact estimates encompasses zero; at gamma = 1.5 the Hodges-Lehmann point estimates encompass zero. This implies that a relatively small increase in the likelihood of being a participant due to an unobservable characteristic which also

[38] Odds, which are widely used in assessing probabilistic outcomes, are derived from probabilities ($0 \leq \pi_i \leq 1$) by the following formula: $\pi_i/(1-\pi_i)$.

increases the benefits from borrowing, is required to explain the observed impact. It is not unlikely that such an unobserved confounding variable exists. Consequently, we suggest, the observed impact of microfinance membership on the log of women's non-landed assets may well be confounded by one or more unobserved variables associated with both MFI borrowing and this impact – for example, unobserved entrepreneurial abilities.

Table 4: Sensitivity analysis for log of women's non-landed assets for microfinance participants across R1-3

	Significance levels		**Hodges-Lehmann point estimates**		**95% Confidence intervals**	
Gamma (Γ)	Minimum	Maximum	Minimum	Maximum	Minimum	Maximum
1	< 0.0001	< 0.0001	0.886	0.886	0.315	1.317
1.2	< 0.0001	< 0.0867	0.465	1.218	-0.245	1.570
1.3	< 0.0001	< 0.2329	0.274	1.341	-0.532	1.694
1.4	< 0.0001	< 0.4422	0.065	1.439	-0.710	1.796
1.5	< 0.0001	< 0.6547	-0.159	1.533	-0.886	1.891

Source: Authors calculations from data source given in footnote 8.

Sensitivity analysis was carried out on all outcome variables for all four treatment group comparisons. The evidence provided by those tests does not contradict this conclusion, namely that all the impact estimates presented in Table 2 and Table 3 are highly sensitive to selection on unobservables[39][40].

[39] The detailed results from those sensitivity tests are not presented here but the results and relevant Stata do-files can be made available upon request.

[40] As a brief note, we re-analysed the PnK panel by a combination of PSM and differences-in-differences. The PSM matches of R1-3 were retained and merged with R4. Our panel analysis confirms most of the cross-section findings described earlier and it can be concluded that neither cross-section nor panel data analysis support PnK's and Khandker's original claims, which provide an overly positive picture of the impact of microcredit. RnM's replication of Khandker also casts doubts about Khandker's findings (RnM: 39).

Conclusion

The replication of PnK and associated studies poses a challenge due to the complex research design and poor documentation of the data. All studies that deal with the PnK data, that is Morduch, Chemin, RnM and this study, agree that PnK overstate the impacts of microcredit. PnK estimated positive and significant impacts for literally all of the six outcome variables with stronger impacts when women were involved in microcredit (PnK: 987-988). Morduch argued that PnK overestimated the impact of microcredit in part because the eligibility criterion was not strictly enforced, and he cannot support PnK's claims that microcredit increases per capita expenditure, school enrolment for children (Morduch: 30) or labour supply. Pitt challenged Morduch's conclusions with simulated data, and confirms the results of PnK's undocumented and undocumentable estimation procedure. RnM confute Pitt's claims with their (available) data and documented computer code.

Using PSM, Chemin finds lower impact estimates than PnK for all outcome variables except male labour supply (Chemin: 478). Doubts about both Morduch and Chemin arise because of problems in replicating their data constructions, and in the latter case the failure to conduct sensitivity analysis. RnM's findings of MFI impact are mixed and mostly insignificant.

The studies by PnK, Morduch, Chemin and RnM do not address the role of multiple sources of borrowing[41] which has implications for the nature and constitution of the treatment and control groups. As a result, this study, using PSM with sensitivity analysis, made different comparisons to examine impacts using putatively more appropriate, and homogeneous, treatment and control groups. This strategy found generally positive but mixed results when comparing microcredit participation with non-participation, but there is no clear evidence that microcredit as such is more beneficial than other sources of finance; moreover, sensitivity analysis shows that all these estimates of impact are highly vulnerable to unobservables, in part, perhaps, because of the poor quality of the matches.

Many microfinance adepts agree that individuals essentially need to borrow from multiple sources to obtain sufficient funds that would allow them to engage in more productive activities (see Fernando, 1997); microcredit loans are often too small to meet the needs of microentrepreneurs (Venkata and Yamini, 2010). In addition, multiple sources of borrowing are often required to smooth income and consumption patterns as well as to cope with emergencies (Venkata and Yamini, 2010). Fernando (1997), Coleman (1999) and Venkata and Yamini (2010) find that it is common for individuals to use borrowing from one source to pay off the loans of another, including microfinance, on time.

[41] Khandker (2000) only explores the effects of other borrowing sources on a limited set of variables.

Criticisms of the more strident and unqualified claims about microfinance (using RCTs) are becoming more common (Banerjee et al, 2009; RnM; Karlan and Zinman, 2009) and further investigations as to the impact of microcredit versus other financial tools should be encouraged, whether RCTs or carefully designed observational studies that collect a rich and high quality data set. It is arguable that carefully conducted observational studies using quasi-experimental designs can and perhaps should have come to the appropriate conclusions, and could have done so with even these data had the data manipulation and analysis been appropriate, without the need to engage in RCTs (for a critique on RCTs see Deaton, 2009; Imbens, 2009; Pritchett, 2009).

The analysis in this paper has raised doubts about the capabilities of PSM, to rescue robust estimates of impact, at least with the sorts of data available. A critique of econometric techniques is not new; in a landmark paper Leamer (1983) criticises the key assumptions many econometric methods are built on and complains about 'the whimsical character of econometric inference' (38). Despite his pessimistic view on the usefulness of econometric methods, there has been a trend towards ever more sophisticated techniques which, however, do not necessarily provide convincing solutions to the challenges of impact evaluation. A similar conclusion would seem to apply to PSM.

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Appendix

Appendix 1: Weighted means and standard deviations, PnK and RnM

Variables	PnK 1998[1]		RnM 2009[2]	
	Mean	Standard deviation	Mean	Standard deviation
Age of all individuals	23	18	23	18
Schooling of individual aged 5 or above (years)	1.377	2.773	2.066	3.136
Parents of HH head own land?	0.256	0.564	0.254	0.563
Brothers of HH head own land?	0.815	1.308	0.810	1.305
Sisters of HH head own land?	0.755	1.208	0.750	1.206
Parents of HH head's spouse own land?	0.529	0.784	0.529	0.783
Brothers of HH head's spouse own land?	0.919	1.427	0.919	1.427
Sisters of HH head's spouse own land?	0.753	1.202	0.753	1.202
Household land (decimals)	76.142	108.540	76.145	108.052
Highest grade completed by HH head	2.486	3.501	2.523	3.525
Sex of household head (male=1)	0.948	0.223	0.948	0.223
Age of household head (years)	40.821	12.795	40.874	12.789
Highest grade completed by any female HH member	1.606	2.853	1.664	2.999
Highest grade completed by any male HH member	3.082	3.081	3.277	4.016
Adult female not present in HH?	0.017	0.129	0.017	0.129
Adult male not present in HH?	0.035	0.185	0.035	0.185
Spouse not present in HH?	0.126	0.332	0.123	0.329
Amount borrowed by female from BRAC (Taka)	350	1,574	349	1,564
Amount borrowed by male from BRAC (Taka)	172	1,565	173	1,575
Amount borrowed by female from BRDB (Taka)	114	747	114	746
Amount borrowed by male from BRDB (Taka)	203	1,573	204	1,576
Amount borrowed by female from GB (Taka)	956	4,293	972	4,324
Amount borrowed by male from GB (Taka)	374	2.923	360	2,895

Notes:

1. Source: PnK, table A1, p. 993, based on R1.
2. Source: RnM, table 1, p. 15, based on R1.

Morduch and Pitt do not provide any descriptive statistics.

Appendix 2: Descriptive statistics of individuals belonging to any of the four treatment groups across treatment and control villages and across eligibility criteria[42]

Independent variables	Y^{MF}	$Y^{Multiple}$	Y^{Borr}	Y^{None}
Sex HH head (male=1)	1.055 *0.228*	1.053 *0.224*	1.016 *0.126*	1.036 *0.187*
Age (years)	34.616 *10.551*	34.672 *10.529*	40.687 *12.752*	32.618 *15.244*
Age household head (years)	41.044 *11.944*	40.867 *11.881*	43.589 *13.334*	43.178 *12.407*
Number adult male in household	1.344 *0.794*	1.347 *0.796*	1.639 *1.073*	1.612 *1.085*
Marital status (yes=1)	0.873 *0.333*	0.873 *0.333*	0.938 *0.241*	0.337 *0.473*
Landholdings HH head parents	0.209a *0.533*	0.222b *0.549*	0.261c *0.565*	0.248d *0.561*
Landholdings HH head brothers	0.557a *1.071*	0.567b *1.083*	0.766c *1.414*	0.720d *1.223*
Highest education any HH member	3.619 *3.429*	3.649 *3.424*	5.350 *3.944*	4.455 *4.022*
Highest education female HH member	1.178 *2.341*	1.183 *2.346*	2.248 *3.378*	1.788 *3.118*
Savings	3543.534 *5168.575*	3651.482 *5533.265*	4418.86 *20083.07*	4091.61 *17911.66*
Livestock value	2603.311 *3843.594*	2654.342 *3908.822*	3935.737 *5926.48*	3678.958 *6014.571*
Own non-farm enterprise (yes=1)	0.555 *0.4972*	0.556 *0.497*	0.442 *0.497*	0.467 *0.499*
Household size	5.456 *2.063*	5.454 *2.081*	6.191 *2.633*	6.514 *2.735*
Outcome variables				
Total HH expenditure per capita per week (Taka)	76.872 *33.196*	77.805 *34.639*	97.231 *62.918*	81.035 *48.065*
Women non-landed assets (Taka)	2476.51 *6736.685*	2434.943 *6634.52*	2968.477 *13068.11*	2741.315 *9006.549*
Female labour supply, hours per month, aged 16-59 years	101.409 *166.251*	98.449 *165.597*	13.350 *62.106*	18.481 *74.266*
Male labour supply, hours per month, aged 16-59 years	225.607 *332.272*	237.157 *334.151*	456.793 *303.905*	121.542 *257.661*
Girl school enrolment, aged 5-17 years (yes=1)	0.638e *0.481*	0.644f *0.479*	0.681g *0.467*	0.616h *0.487*
Boy school enrolment, aged 5-17 years (yes=1)	0.652i *0.477*	0.656j *0.475*	0.758k *0.429*	0.665l *0.472*
Number of observations	875	922	371	8387

[42] The interested reader can compare our descriptive statistics with those in RnM who also provide comparisons with PnK.

Source: Authors calculations.

Notes: Standard deviation in italics. Using PnK data, see footnote 8. MF=Participant in microfinance only; Multiple=Participant in microfinance and other non-microfinance (formal/informal) borrowing; Borr=Participant in other non-microfinance (formal/informal) borrowing; None=No borrowing at all.

- a: n = 861; b: n = 908; c: n = 368; d: n = 8278
- e: n = 516; f: n = 542; g: n = 232; h: n = 5621
- i: n = 554; j: n = 582; k: n = 248; l: n = 5769

The mean values in Appendix 2 differ from the mean values presented by PnK and RnM as illustrated in Appendix 1. ANOVA has been applied examining all possible pairwise comparisons to assess whether the differences in the mean values between the various comparison groups are statistically significant. The ANOVA results show that for most variables differences are not significant at conventional levels of significance, with few exceptions. Mean values of Y^{MF} versus Y^{None} significantly differ for age of household head, landholdings of household head's parents, total household expenditure per capita per week, log of female non-landed assets, female and male labour supply.

Appendix 3: Number of individuals by treatment option by treatment and control villages across eligibility criteria

	Treatment villages			**Control villages**	
	BRDB	**BRAC**	**GB**	**Control**	**Total**
Microfinance only	298	279	298	0	875
Microfinance & informal borrowing only	15	8	7	0	30
Microfinance & formal borrowing only	9	1	1	0	11
Microfinance & both formal & informal borrowing	6	0	0	0	6
Non-microfinance borrowing - informal borrowing only	73	54	36	91	254
Non-microfinance borrowing - formal borrowing only	30	26	22	18	96
Non-microfinance borrowing - both formal & informal borrowing	6	3	5	6	20
No borrowing	2,287	2,329	2,327	1,444	8,387
Total	**2,724**	**2,700**	**2,696**	**1,559**	**9,679**

Source: Authors calculations.

Notes: Figures correspond to those presented in Figure 2 but a more detailed breakdown is provided within sub-groups and by village type.

Appendix 4: Simple matching estimates across gender using nearest neighbour matching for all four comparison groups with number of matches and pseudo R-squared

Outcome variables	Y^{MF} **vs eligible** Y^{None}	Y^{MF} **vs** Y^{None}	$Y^{MF} + Y^{Multiple} + Y^{Borr}$ **vs** Y^{None}	Y^{MF} **vs** Y^{Borr}
Comparison	**1**	**2**	**3**	**4**
1-nearest neighbour matching				
Variation of log per capita expenditure (Taka)	-0.017*	-0.004	0.000	-0.019
Log per capita expenditure (Taka)	-0.012	0.003	0.035**	-0.081
Log women non-landed assets (Taka)	1.230***	0.589**	0.115	0.333
Female labour supply, aged 16-59, hours per month	61.70***	51.92***	36.92***	71.62***
Male labour supply, aged 16-59, hours per month	-47.45***	-27.46	32.68**	-259.98***
Girl school enrolment, aged 5-17 years	-0.012	0.073*	0.034	0.043
Boy school enrolment, aged 5-17 years	0.006	0.082*	0.053	-0.012
No of treated observations used	861	861	1,275	861
No of untreated observations used	655	674	918	191
Total number of observations	4,123	5,068	5,436	1,229
Pseudo R-squared	0.077	0.130	0.104	0.263

5-nearest neighbour matching				
Variation of log per capita expenditure (Taka)	-0.016**	-0.004	-0.000	-0.045*
Log per capita expenditure (Taka)	-0.014	-0.006	0.024*	-0.072*
Log women non-landed assets (Taka)	1.100***	0.475**	0.389**	0.231
Female labour supply, aged 16-59, hours per month	54.28***	57.75***	33.33***	69.17***
Male labour supply, aged 16-59, hours per month	-27.87*	-43.64***	32.12***	-254.63***
Girl school enrolment, aged 5-17 years	0.061*	0.073*	0.048*	0.073
Boy school enrolment, aged 5-17 years	0.040	0.082*	0.068**	-0.022
No of treated observations used	861	861	1,275	861
No of untreated observations used[43]	655	674	918	191
Total number of observations	4,123	5,068	5,436	1,229
Pseudo R-squared	0.077	0.130	0.104	0.263

Source: Authors calculations.
Notes: *statistically significant at 10%, **statistically significant at 5%, ***statistically significant at 1%. Using PnK data, see footnote 8. Stata routine psmatch2 is applied. The logit model outlined in Table 1 is used. The results in this table refer to the differences in the mean values between matched samples. t-tests before and after matching were employed for all results presented in this table to investigate the differences in the mean values for each covariate X across matched samples; as before, the test provided conclusive results. Standard errors (not reported) are bootstrapped.

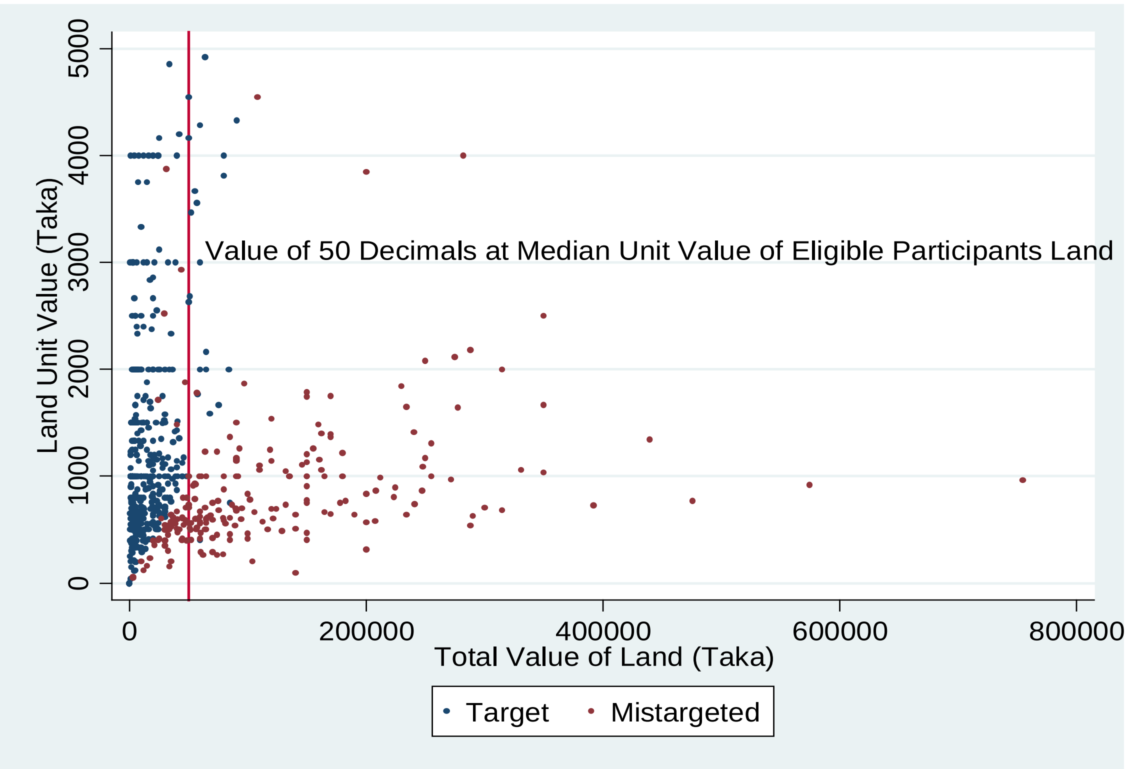
[43] The identical number of cases of matched untreated for 1- and 5- nearest neighbour matching is further evidence of the lack of plentiful comparison cases.

Appendix 5: Eligibility Criterion

Referring to Figure 1, Morduch points out that PnK label any participating households in the programme villages (group D) as eligible, even households that should have been excluded according to the less than 0.5 acres eligibility criterion. As a result, according to Morduch, mistargeting occurred, as Group D contains participants who own more than 0.5 acres of land. Ravallion (2008, p. 3818) and Chemin (p. 465) support Morduch's view that PnK do not strictly enforce the eligibility criterion.

Pitt rationalises this claiming that the value of land of treated households which cultivate/possess more than 0.5 acres is so low that the value of the land of these households is effectively less than the median value of 0.5 acres of average land. If Pitt's claim is indeed true and the three microfinance programmes do take land quality into account when establishing programme eligibility, then the mistargeted households that participate should have total land values of no more than the median unit value of land of the correctly identified households that participate (that is less than 0.5 acres). The data are depicted in the following Figure.

Figure 4: Land unit values by total land value and targeting



Source: Author's calculations based on PnK data R1 downloaded from the World Bank website.

The median unit value of land of eligible participating households (having less than 0.5 acres of land) equals 1000 Taka per decimal (50 decimals equal 0.5 acres). Thus, one might suggest the cut-off point for establishing programme eligibility is 50,000 Taka, that is mistargeted households that participate should have a total value of land less than 50,000 Taka. However, 50% of the mistargeted households that participate have total land values of greater or equal to 85,000 Taka, and 72% of those mistargeted households have total land values of greater or equal to 50,000 Taka (a reasonable cut-off for using a value of land criterion). Hence, Pitt's argument does not convince. Further information and a scatter plot showing the details can be obtained from the authors. PnK support the use of landownership as an eligibility criterion and argue that the virtual absence of an active land market justifies its application (PnK: 970). Morduch provides evidence to the contrary; he argues that there is substantial evidence on an active land market in South Asia (Morduch: 4). He argues that close to one eighth of participants in fact bought substantial amounts of land a few years before the survey was conducted.

Chemin and Morduch argue that simply comparing groups E to F or groups E to B (see Figure 1) is misleading due to selection bias. As a result, Morduch proposes comparing the outcomes of groups E + F to those in group B which would provide bias-free impact estimates. However, this comparison assumes that landholdings are exogenous, that is that membership in groups E, F or B is not influenced by self-selection (Morduch: 7). Furthermore, the comparison Morduch proposes does not '…reflect general differences across villages' (Morduch: 8). Therefore, assuming that there are minor spill-over effects from group E to C or A, he suggests employing a simple differences-in-differences (DID) estimation that compares the outcomes of groups E + F to C. Similarly, he recommends conducting a comparison for group A relative to group B (Morduch: 8). After employing these comparisons, Morduch finds no statistically significant impacts of exposure to microfinance. Pitt, and Khandker, 2000, address the potential contamination problem, and, find that it appears to make no substantive difference to the results, and this is supported by our results. To avoid confusion, we report whether we use a de jure or de facto classification.