March 26, 2011

Response to Roodman and Morduch's "The Impact of Microcredit on the Poor in Bangladesh: Revisiting the Evidence"

Mark M. Pitt

Brown University

Abstract: This response to Roodman and Morduch seeks to correct the substantial damage that their claims have caused to the reputation of microfinance as a means of alleviating poverty by providing a detailed explanation of why their replication of Pitt and Khandker (1998) is incorrect. Using the dataset constructed by Pitt and Khandker, as well as the data set Roodman and Morduch constructed themselves, the Pitt and Khandker results standup extremely well, indeed are strengthened, when estimated with Roodman's *cmp* program, after correcting for the Roodman and Morduch errors.

Recently, David Roodman and Jonathan Morduch [2009] (henceforth RM) have written a paper that claims that the "headline result", as they call it, from the Pitt and Khandker (1998) (henceforth PK) paper published in a 1998 issue of *The Journal of Political Economy* ("The Impact of Group-Based Credit on Poor Households in Bangladesh: Does the Gender of Participants Matter?") cannot be replicated with the data. The headline result they refer to is that "annual household consumption expenditure increases 18 taka for every 100 additional taka borrowed by women…compared with 11 taka for men." (RM p. 980) It is this result that forms the basis for the claim made by Muhammad Yunus, the Nobel Laureate and founder of the Grameen Bank, that 5 percent of Grameen borrowers get out of poverty every year.

Briefly, PK find positive and statistically significant effects on household consumption of women's participation in all three microfinance programs studied (Grameen Bank, BRAC, and BRDB), but no statistically significant effects for men's participation. Not only do RM express doubt about these results, but their replication generates statistically significant results that are opposite in sign. They have widely disseminated their contrary results via Roodman's "Microfinance Open Book Blog" and other means[1], and these contrary results are now well-

[1] For example, at the recent "Evidence Summit on Broad-Based Growth" held on December 9-10, 2010. Roodman's Powerpoint slides used in this and presumably other presentations can be

known in influential academic and policymaking circles. In Roodman's blog where he discusses PK, he claims:

> "…academia has some explaining to do: first the most prestigious study says microcredit reduces poverty, then it is overturned even as ambitious, young researchers arrive on the scene with new studies, hardly mentioning the past.
>
> …I think my paper with Jonathan is the academic equivalent not of a citation but an indictment... It is a long document packed with logic and evidence that the flaws are not merely possible but provable in academic court and important enough to generate wrong results."[2]

and that

> "[the] message that a lot of research published in prestigious journals is wrong *does* carry over to economics in general and microfinance in particular. Cases in point are the papers that Jonathan and I replicated."[3]

Finally, in a blog entry title titled "Taking the Con Out of Econometrics", he writes:

> "… how could the economics profession have gone so wrong for so long? …the old research is fundamentally suspect and the new much better (though hardly perfect). The fancy math in what was once the leading study of microcredit's impacts is, though beautiful, typical of the old generation in its propensity to obscure rather than resolve the fundamental barriers to identifying cause and effect."

The RM paper seems to have had a broad impact, even contributing to the disparagement of the accomplishments of Professor Muhammad Yunus, founder of the Grameen Bank and Nobel Peace Prize Laureate.[4] The acceptance of the view that the results of PK are not to be believed is

---

found at http://www.usaid.gov/press/evidence/bbg/DavidRoodman.pptx. Roodman and Morduch have upcoming presentations at the Microfinance USA Conference on May 23-24, 2011 in New York City, where they will moderate a discussion of "Promise and Peril of Microfinance Impact Evaluations" along with Abhijit Banerjee of MIT/J-PAL.

[2] (http://blogs.cgdev.org/open_book/2010/03/can-we-talk-researchers-microfinance-leaders-meet-at-cgd.php)

[3] http://blogs.cgdev.org/open_book/2011/01/good-news-microfinance-research-not-as-bad-as-medical-research.php

[4] For example, one inflammatory article in the Bangladesh press that accuses Yunus of various misdeeds quotes Danish journalist Tom Heinemann: "In fact, renowned social scientists, such as

found in a new volume of the widely respected *Handbook of Development Economics*, where Chapter 2 ("Access to Finance") authored by Morduch[5] and Karlan summarizes the impact of RM on the work of not just PK, but all of my other papers on microfinance, those of my students, and on all of Khandker's other work, of which I am not a co-author:

> Roodman and Morduch (2009) attempt to find closure to the issue by returning to the data and rebuilding the analysis from scratch. They are unable to replicate results from Pitt and Khandker (1998) or Khandker (2005). In fact, their estimates carry the opposite sign. Rather than concluding that microcredit harms borrowers, however, they unearth a raft of identification issues which are not solved with panel data. *Their revised analysis casts doubt on all of the findings from the related set of papers*, including Morduch (1998)'s finding on consumption smoothing. [emphasis mine]

Roodman has even testified to the US Congress[6] about the supposed failings of PK, mentioning the PK and RM papers by name:

> "A couple of years ago I spent a good deal of time scrutinizing what was then the leading academic study of the impacts of microcredit. [He cites Pitt and Khandker by name in a footnote at this point.] To decide whether I believed the conclusion that microcredit in Bangladesh had helped families, especially when the loans were made to women, I decided to replicate the study, applying the original statistical methods to the original data. The math and computer programming were really complex. In time, with my coauthor Jonathan Morduch, I would conclude that the study does not stack up. We're not saying microcredit doesn't help people, just that you cannot judge the matter with the data in this study.

David Roodman, Jonathan Morduch, Thomas Dichter and Milford Bateman, agree on one thing: After 35 years of Microcredit there is no evidence that Microcredit lifts millions out of poverty." http://www.bdnewslive.com/2010/12/yunus-siphoned-tk-7bn-aid-for-poor/

[5] Morduch's paper ("Does Microfinance Really Help the Poor? New Evidence from Flagship Programs in Bangladesh") written in 1998 but never published also claims, as do RM 11 years later, that a second-look at the data could not replicate PK's headline results. That paper also gained much attention by being described at length in a 1999 survey paper of the microfinance literature titled "The Microfinance Promise." That survey paper, authored by Morduch (1999), appeared in the the *Journal of Economic Literature*.

[6] Written testimony for the House Financial Services Subcommittee on International Monetary Policy and Trade, April 28, 2010 available at http://www.house.gov/apps/list/hearing/financialsvcs_dem/roodman_testimony_4.28.10.pdf.

Note that in his actual testimony to Congress, Roodman skips the first half of written testimony so that he launches directly into his and Morduch's critique of PK. Available on Youtube at http://www.youtube.com/watch?v=5Y10XbCLys8. The response of Rep. Waters to the Roodman testimony is found at http://www.youtube.com/user/GlobalDevelopment#p/u/39/yBgozYleKtI

…we have little solid evidence that microcredit, the dominant form of microfinance, reduces poverty. "[italics in original]

This response to RM seeks to correct the substantial damage that RM's claims have caused to the reputation of microfinance as a means of alleviating poverty by providing a detailed explanation of why their replication of PK is incorrect.

In claiming that the headline (and other) PK results are not replicable, RM make two serious errors. First, RM make a fatal econometric error in their formulation and coding of the PK replication. That error, which is apparently logical rather than typographical, results in an econometric method that does not correspond to the PK model, nor for that matter, to any reasonable econometric model of the impact of credit. The Roodman and Morduch computer program used in their replication of PK's results generates estimates that are not interpretable much less comparable to PK's estimates. Second, RM inexplicably replace a key variable from PK with another seemingly unrelated variable. This mistaken substitution is itself sufficient to reverse the signs of the credit effects even if the econometric error were not present. Moreover, this error affects all of their estimates.

The econometric error

Using a new program written for Stata by Roodman, called "cmp" for "conditional mixed process" (Roodman 2009), RM claim to replicate all of the PK regressions, in the sense of applying the same methods to the same data. Column (1) of Table 1 presents the PK estimates as reported in our 1998 *JPE* paper. Column (2) presents the RM estimates from their paper. These latter estimates use variables that they themselves constructed from the raw data (henceforth the *RM estimation dataset*). They also report on, but do not present, estimates based upon the estimation data that I sent Roodman (henceforth the *PK estimation dataset*) in January 2008. As RM note, in "the regression that is meant to replicate the headline results, the coefficients on all three female credit variables are strongly negative. This is true too of …regressions with 14 village controls instead of village dummies. … The sharp contradiction of PK's headline result poses a mystery. To check our results, we run the same estimation program on the data set

provided by Mark Pitt [*PK estimation dataset*]. The coefficients on female credit remain strongly negative."

In PK, a censoring threshold for micro-credit of (log) 1000 taka is used. RM claim to impose this censoring threshold in their estimates, but also report that they "re-estimate using log 1 instead of log 1,000 for zero observations of credit variables; this reduces coefficient magnitudes but by and large does not affect signs and significance."[7]

The econometric error of RM arises because *cmp* does not correctly estimate models with a non-zero censoring threshold, and PK specify a censoring threshold of log(1000) in the credit demand equation. Roodman and Morduch deal with the censoring threshold by replacing zero levels of credit in the *PK estimation dataset* with log(1000). The RM approach works perfectly well if *cmp* is estimating a simple Tobit model. In that case, replacing the data for observations having a left-censored dependent variable with the censoring threshold generates correct estimates. But it does not work in the two (or more) equation LIML case – the PK model – and it is unclear why RM think that it would. It is also unclear why RM did not test their code with a nonzero censoring threshold as this is simple to do. RM devote three pages of their paper to an appendix outlining how they tested their code with synthetic data. However, the synthetic data that they used did not have a nonzero censoring threshold while the model they were trying to replicate does.

Let me start with the simplest synthetic model that is relevant to understanding the failings of *cmp*. This is a simple two equation simultaneous Tobit model[8]:

(1) $Y^*_{i1} = Z_i\pi + \varepsilon_{1i}$ where $Y_{i1} = Y^*_{i1}$ if $Y^*_{i1} > c$ and $Y_{i1} = 0$ if $Y^*_{i1} \leq c$

(2) $Y_{2i}=Y_{i1}\delta + \varepsilon_{2i}$

where the scalar c is the (left-) censoring threshold, and other independent variables have been omitted for simplicity. The model above is identified in the classical way via the exclusion

[7] The data and programs used by Roodman and Morduch can be found at http://www.cgdev.org/content/publications/detail/1422302 .
The data and program used in this response to RM can be found at http://www.pstc.brown.edu/%7Emp/RMdo/replication.zip

[8] The Tobit model given by equation (1) is nothing unusual. It is what Amemiya (1984) calls a Standard Tobit Model (Type 1 Tobit) in his classic survey paper of Tobit models.

restrictions on Z. If c=0, then *cmp* works perfectly. I generated 500 synthetic data sets having a zero censoring threshold (c=0), with each synthetic dataset having 5000 observations, and estimated the above model with *cmp*. The true value of δ is 1 and the mean of the estimated δ is 1.000 with a standard deviation of 0.0400. Then I changed the censoring threshold to c=8 (and scaled up the intercept for Y^*_{i1}), and again ran *cmp* on 500 synthetic data sets each with 5000 observations. The mean of the estimated δ is 0.112 with a standard deviation of 0.099. [9] This is a far cry from the true δ of 1. So why bother with this Tobit exercise if it does not correspond to the PK identification strategy? The peculiar attribute of the PK econometric model, having observations without "choice"[10] concerning the endogenous independent variable Y_{1i}, is not the specific cause of the failure of *cmp*. It is a much more general problem.

Now consider a simple variant of the PK model – simple in that it has only one credit variable. Consistent with the Appendix of RM, *cmp* applied to synthetic data sets having a censoring threshold of c=0 seems to work fine. The mean of 500 estimates of δ is 0.998 with a standard deviation of 0.0476. But if one changes the censoring threshold to c=8, the mean of 500 estimates of δ becomes 2.72 with a standard deviation of .0870. Once again, *cmp* produces a serious mis-estimate.

The RM estimates do, in fact, seem to correspond to a model but not one they could have intended. In particular, the estimated model by RM is a variant of the simultaneous equation Tobit model given by equations (1) and (2), but in Roodman and Morduch's "replication" of PK there is one crucial change:

(3) $Y^*_{i1} = Z_i\pi + \varepsilon_{1i}$ where $Y_{i1} = Y^*_{i1}$ if $Y^*_{i1} > c$ and $Y_{i1} = c$ if $Y^*_{i1} \leq c$

(4) $Y_{2i}=Y_{i1}\delta + \varepsilon_{2i}$

The difference in equations (3) and (4) as compared to (1) and (2) is that if Y^*_{i1} ("credit") is left-censored, it takes the value of the censoring threshold c and not the value zero as in equation (1). What this means in the context of the replication of PK is that Roodman and Morduch are assigning 1000 taka of micro-credit borrowing to those who were not participators

[9] It is easy to alter the data generating process so as to get a sign reversal on δ.

[10] These non-choice households are either not eligible for participation or live in a village without a credit program, and are a primary source of parameter identification in PK.

and who did not in fact borrow from a microfinance program at all. In addition, they assign 1000 taka of microfinance borrowing to those who have a deterministically zero value of credit either because they are not eligible for participation or because they live in a village without a credit program. **Quite simply, Roodman and Morduch arbitrarily assign 1000 units of treatment to the control group who were untreated.**[11]

Incorrectly omitted variable and an incorrectly included variable

Aside from credit, the only independent variables for the outcome equations, such as household consumption, that receive significant attention in the PK paper are landownership and target/nontarget status (eligibility status). In the illustrative example provided in PK, the only independent variable is target/nontarget status – a binary indicator of whether a household is landed or landless. The PK paper makes it clear how important this target/nontarget variable is, and clearly states that it is included as an independent variable:

> Even if land ownership is exogenous for the purposes of this analysis, it is necessary that the "landless" and the "landed" can be pooled in the estimation. In order to enhance the validity of this assumption, we restrict the set of nontarget households used in the estimation to those with less than five acres of owned land. In addition, we include the quantity of land owned as one of the regressors in the vector X_{ij} *and include a dummy variable indicating the target/nontarget status of the household*. As the illustrative example of the identification strategy (eqs. [3] and [4]) makes clear, identifying the effect of target (landless)/nontarget (landed) status on behavior requires a sample of households from villages without a credit program. [italics added for emphasis] (PK, p. 971)

[11]It is uncertain what RM intended. They specifically state that the six equations that make up RM equation (1) in RM (p. 6) is "the PK model." However, RM equation (1) cannot be a complete model as it is undefined when there is no credit choice (c_f=0 or c_m=0, in the terminology of RM). Equation (2) does define latent credit for both the credit choice (c_f=1 or c_m=1) and no credit choice cases but (i) it contradicts RM equation (1) for the credit choice case, (ii) is stochastic, rather than deterministic (as in PK) for the no credit choice case, (iii) assigns the censoring threshold (plus a stochastic term) to observations without choice, rather than zero as in PK, and (iv) makes the censoring "dummy" function in RM equation (1) true for all non-negative latent credit demands. The latter means that even those having zero latent demand for credit are assigned the (positive) credit censoring threshold. Whatever the logic might be, adding the censoring threshold to the latent index in RM's equation (2) strongly suggests that the econometric methods that they used were intended to estimate this incorrect model rather than being the result of an error in computer coding.

The quote also clarifies that PK allows for target/nontarget status and landownership to directly affect outcomes (such as consumption) separately from their indirect affect by influencing microfinance.[12]

In addition, the variable "Nontarget household" is in Table A1 of PK (p.993), the heading of which reads "Weighted Means and Standard Deviations of Independent Variables." I provided Roodman with the *PK estimation dataset* that contained all of the variables required to replicate the consumption results of PK.[13] The nontarget variable was in that *PK estimation dataset*. RM also include the variable "Nontarget household" in their Table 1, which is titled "Weighted means and standard deviations of PK right-side variables, first survey round," which suggests that it is an independent variable (right-side variable) in the RM replication of PK. Consequently, it is inexplicable to find that this variable was not, in fact, included as a "right-side" variable by RM when I examined the Stata do files that attempt to replicate PK, nor is it in any of their other statistical estimations or procedures that attempt to assess the validity of the PK estimation strategy.

In addition, RM include another variable, labeled *crcensored* in Stata, that does not even appear in RM's Table 1 as a right-hand variable. Leaving aside their econometric error, substituting the nontarget variable with the *crcensored* variable is sufficient to reverse the signs

---

[12] It seems that this latter point is not fully understood by Roodman and Morduch in spite of the fact that they include in their replication, as do PK, a landholding variable on the right-hand side of the equation determining household consumption, as well as in all other outcome equations in PK and RM. In Roodman's blog and forthcoming book (*Due Diligence A Guide to Microfinance*, forthcoming), he claims that the PK model assumes that "*landholdings affect welfare only through borrowing.*" His incorrect characterization of the methods of PK continues in a footnote that says, " *More correctly—and fairer to Pitt and Khandker—they assume landholdings only affect welfare through microcredit or through other variables controlled for, such as the education of the head of household*."
http://www.cgdev.org/doc/blog/Roodman%20open%20book/Chap%206%204dr.pdf, p12

[13]In addition, in my email to David Roodman of January 16, 2008, I provided the exact Stata code used to create the nontarget dummy variable, writing:

"Nontarget households are identified as follows:
gen byte nontrgth = mod(nh,10)==5
label var nontrgth "Non-target household"

Until very recently, I presumed that the variable *crcensored* was Roodman's name for this variable.

of the PK parameters for the effect of credit on household consumption.[14] What is the *crcensored* variable? It takes the value of one if an eligible household participates in a microfinance program but has not borrowed, and zero otherwise. This variable (i) is used in all of the models that RM estimate but is never mentioned in the RM paper even in the tables that list variables, (ii) is best considered as endogenous, and, crucially from the perspective of "replication," (iii) is not used in PK.[15]

To be clear: (i) PK do not include this *crcensored* variable in any of the tables listing independent variables, (ii) PK never mention such a variable in the text of the paper, and (iii) I did not include this variable in the *PK estimation dataset* sent to Roodman.

Is it possible to replicate Pitt and Khandker's results with Roodman's *cmp* Stata program?

Although RM's use of Roodman's *cmp* command in Stata is fatally flawed, it may still be used to attempt a replication of PK. There is every reason to believe that *cmp* itself is not faulty in the special case of a censoring threshold that is zero; but it fails, as noted above, for non-zero values of the censoring threshold. As it turns out, using a censoring threshold of zero in *cmp* provides estimates for the second-stage endogenous variables ("credit") that are very close (only slight over-estimates) to known true values even when the data generating process has a nonzero censoring threshold in the first-stage.[16] That is because the first-stage equation has the same goodness of fit even though it generates first-stage parameters that differ from the true first stage.

[14] To be clear, RM attempt to replicate PK by introducing a credit censoring threshold of log(1000) but in an econometrically incorrect way, as described above. Because this misspecified model fits the data poorly, they also try the model with a credit censoring threshold of zero. In this latter case, they still get the reverse signs of PK but in this case it is only because of the incorrect set of independent variables, as described below.

[15] This variable is in a predecessor working paper that I presented at a conference in Dhaka, Bangladesh in March 1995 and subsequently distributed as a World Bank Discussion paper (Pitt and Khandker, 1996). That paper had a different title, presented different estimates, and used a different sub-sample of the data than PK.

[16] This was established using a synthetic data generating process similar to that used above to show that Roodman's use of his *cmp* command generates wrong estimates with a nonzero censoring threshold. Imposing a zero censoring threshold in *cmp* when the true censoring threshold is 8 with 2500 replications (5000 observations in each replication) generates a mean estimate of δ of 1.039 (compared with a true $\delta=1$) with a standard deviation of 0.0185. Strik-

I have not checked the 1000+ lines of Stata codes that make up the *cmp* command (ado file), nor do I have the expertise to do so. My conclusions are based on running *cmp* with synthetic data. The true values of all parameters are known when synthetic data are used, and these known values can simply be compared to the parameter values produced by *cmp*. In short, it is possible to try to replicate PK using Roodman's *cmp* program even with a mispecified first-stage (credit) equation, although not in the way that Roodman and Morduch have done it when using a nonzero censoring threshold.

In addition to incorrectly specifying the credit censoring threshold, omitting the nontarget variable, and erroneously including a dummy variable for participating non-borrowers, at least two other less important issues affect the replication estimates in RM using the *PK estimation dataset* that I provided to Roodman. First, for a relatively small number of households, their separation of the sample into households with choice to borrow and households without choice to borrow, by gender, is wrong. They did not use the variable in *the PK estimation dataset* that I sent them. This error is, however, relatively inconsequential empirically. Second, RM use all three rounds of data in the estimation of the "first-round" demand for credit equation, while PK use only one round of data. Khandker and I did so because we think of credit as a stock variable rather than a flow variable, and there is not a great deal of variation in the lifetime stock of credit between rounds that are only a few months apart. Adding in extra rounds with little variation between rounds would simply make standard errors smaller and make it more likely that the instruments would be statistically significant.[17] Using all three rounds in the credit demand equation is fine as long as the errors are correctly clustered (they are). The most likely outcome

---

ingly, 98.6 percent (2465 out of 2500) of the estimated δ are greater than 1. Specifying different parameter values for the data generating process consistently generated over-estimates of similar magnitude. I can offer no guidance as to why *cmp* overestimates δ by about 4 percent, and consequently any results for δ (credit effects) generated by this method have unknown properties. The first-stage estimated parameter values are, of course, never those of the true data generating process.

[17] The *cmp* command does not allow there to be only one round of data for credit demand while at the same time having all three rounds for consumption. If one excluded two rounds from the credit demand equation by setting the *cmp* indicator 'ind' to zero for the credit demand equation, it would inappropriately treat the excluded two rounds of credit observations as if they were households who were excluded from the credit program and had no choice of whether to participate.

of adding in these two additional rounds is that standard errors will fall. As we will see shortly, it is exactly what the results using *cmp* show – asymptotic t-ratio are larger in absolute value, although not by much.

Finally, RM also create their own estimation sample in addition to using the *PK estimation dataset* that was sent to Roodman, and there are small differences between the variables RM create and those in the *PK estimation dataset*. These are second order in the sense that even with RM's version of the data, correct estimation with the *cmp* command returns the PK results, as reported below.

Replication using *cmp*

Column (1) of Table1 presents the PK estimates as reported in our 1998 *JPE* paper. Column (2) presents the RM estimates from their paper. These latter estimates use variables that they constructed from the raw data. They also report on, but do not present, estimates based upon the *PK estimation dataset* that I sent Roodman in February 2008. As RM note "the regression that is meant to replicate the headline results, the coefficients on all three female credit variables are strongly negative. This is true too of …regressions with 14 village controls instead of village dummies. … The sharp contradiction of PK's headline result poses a mystery. To check our results, we run the same estimation program on the data set provided by Mark Pitt. The coefficients on female credit remain strongly negative." (RM, p. 22-23)

In PK, a censoring threshold for micro-credit of (log) 1000 taka is used. RM claim to impose this censoring threshold in their estimation, but also report that they "re-estimate using log 1 instead of log 1,000 for zero observations of credit variables; this reduces coefficient magnitudes but by and large does not affect signs and significance." They try the use of log(1) (=0) instead of log(1000), noting:

> "modeling the log of cumulative borrowing as censored forces a choice about what small value the assumed censoring level should take. *The difference between 1 and 10 taka, say, is minor in levels since most loans are thousands of taka, but major in logs. Although this issue is ultimately secondary to our conclusions, it may help explain large differences between the original regressions and our replications* in the magnitudes of coefficients of interest (though not in the signs or significance). The lowest observed non-zero value for a credit variable is 1,000, and PK use 1,000 in a simplified example without logarithms in their appendix. For these reasons, we censor with log 1,000 ≈ 6.9.

> *We have not ascertained what level the PK regressions use, but have reasons to think that it is log 1 = 0, the chief being that we get a better match in OLS using that value*."[18] (RM, p.12) [emphasis mine]

As I have demonstrated above, the poor "match" from using log(1000) is simply a result of Roodman and Morduch's econometric error.[19]

Columns (3) and (4) present estimates of PK using Roodman's *cmp* command, using the *PK estimation dataset* that I sent Roodman (labeled PK data) and their own reconstruction of the variables (labeled RM data). These estimates are very close to the PK published estimates – the women's parameter estimates are a little bit larger, as they are with synthetic data, as are the asymptotic t-ratios. Recall that this model is also not exactly the same as PK since there is a limitation in *cmp* that necessitates the use of all three rounds of credit data in the first round rather than one round as originally estimated.

The "sharp contradiction of PK's headline result" (RM, p.23) is no longer a mystery. Using the data I sent Roodman, and with the data set Roodman and Morduch constructed themselves, the PK results standup extremely well, indeed are strengthened, when estimated with Roodman's *cmp* Stata program.

Conclusion

This response demonstrates exactly why the attempt by RM to replicate the "headline" results of PK fails, and confirms that, when correctly done, the PK results hold. RM make two major errors neither of which arise from incomplete documentation or from any lack of cooperation on

[18] One might infer from the line in RM "*We have not ascertained what level the PK regressions use, but have reasons to think that it is log 1 = 0"* that RM were forced to just guess about what PK did. But in a January 4, 2008 email to David Roodman trying to assist in his replication, I noted a number of issues that he needed to take into account in his replication. The very first item in that emailed list is this line: "1. the censoring threshold for credit is 1000 (see PK p. 988)." Presumably, the strange estimates they obtained by using a censoring threshold of log(1000) as a consequence of their econometric error specific to nonzero censoring thresholds made them doubt the veracity of the information that I emailed Roodman.

[19] In a footnote to this sentence they claim that the data that I sent Roodman includes some credit variables censored at log(1000) and others at 0. This is incorrect. The credit variables that I sent are log(credit) – log(1000) if credit >0, and zero if credit=0. In no case did a credit variable have the value log(1000).

my part. One RM error is just a logical error in formulating and coding the censoring threshold in the econometric model that they set out in their paper. The other error is their substituting the non-target dummy variable, which is clearly listed in the text and the table of independent variables in PK (and was in the *PK estimation dataset* provided to Roodman), with a "credit censored" variable (*crcensored*), which is not mentioned in PK (or RM).

References

Karlan, Dean and Jonathan Morduch. 2009. "Access to Finance," Chapter 2 in *Handbook of Development Economics, Volume 5*, Dani Rodrik and Mark Rosenzweig, editors. Manuscript available at http://www.nyu.edu/projects/morduch/documents/articles/2009-06-HDE_AccesstoFinance.pdf

Morduch, Jonathan. 1998. Does Microfinance Really Help the Poor? New Evidence from Flagship Programs in Bangladesh. New York University. Department of Economics. Available at http://nyu.edu/projects/morduch/documents/microfinance/Does_Microfinance_Really_Help.pdf

Morduch, Jonathan. 1999. The Microfinance Promise. *Journal of Economic Literature* 37(4): 1569–1614.

Pitt, Mark M. 1999. Reply to Jonathan Morduch's "Does Microfinance Really Help the Poor? New Evidence from Flagship Programs in Bangladesh." Department of Economics. Brown University. Available at www.pstc.brown.edu/~mp/reply.pdf.

Pitt, Mark M., and Shahidur R. Khandker. 1996. "Household and Intrahousehold Impact of the Grameen Bank and Similar Targeted Credit Programs in Bangladesh." World Bank Discussion Papers Number 320.

Pitt, Mark M., and Shahidur R. Khandker. 1998. The Impact of Group-Based Credit on Poor Households in Bangladesh: Does the Gender of Participants Matter? *Journal of Political Economy* 106(5): 958–96.

Pitt, Mark M., Shahidur R. Khandker, Omar Haider Chowdhury, and Daniel L. Millimet 2003. Credit Programs for the Poor and the Health Status of Children in Rural Bangladesh. *International Economic Review* 44(1): 87–118.

Roodman, David. 2009. Estimating fully observed recursive mixed-process models with cmp. Working Paper 168. Washington, DC: Center for Global Development

Roodman, David. 2011. *Due Diligence A Guide to Microfinance*. Center for Global Development. Forthcoming.

Roodman, David and Jonathan Morduch. 2009. "The Impact of Microcredit on the Poor in Bangladesh: Revisiting the Evidence." Center for Global Development Working Paper Number, August. Available at http://www.cgdev.org/files/1422302_file_Roodman_Morduch_Bangladesh.pdf

Table 1

Estimates of the Impact of Credit on Per Capita Expenditure

Explanatory Variables	Log of Weekly Total Expenditure per Capita			
	PK published	RM paper	PK data using Roodman's *cmp* program	RM data using Roodman's *cmp* program[1]
	(1)	(2)	(3)	(4)
Amount borrowed by female from BRAC	.0394 (4.237)	–0.103 (2.605)	.0443 (4.78)	.0429 (4.35)
Amount borrowed by male from BRAC	.0192 (1.593)	–0.001 (0.011)	.0093 (0.52)	.0209 (1.34)
Amount borrowed by female from BRDB	.0402 (3.813)	–0.146 (2.938	.0458 (4.30)	.0470 (4.01)
Amount borrowed by male from BRDB	.0233 (1.936)	0.005 (0.100)	.0128 (0.70)	.0235 (1.50)
Amount borrowed by female from GB	.0432 (4.249)	–0.087 (3.114)	.0420 (4.80)	.0408 (4.38)
Amount borrowed by male from GB	.0179 (1.431)	–0.012 (0.314)	.0072 (0.45)	.0153 (1.06)
No. of observations	5218	5218	5218	5218

Note: Figures in parentheses are asymptotic t-ratios clustered at the household level.

[1]The data used in this column of estimates are RM's (*RM estimation dataset*) except that the incorrectly excluded non-target variable is substituted in for the incorrectly included *crcensored* variable.

Sources:
Col (1): PK Table 2, page 981;
Col (2): RM Table 3;
Col (3): My new estimates using the data I sent to RM (*PK estimation dataset*) and Roodman's program cmp;
Col (4): My new estimates that correct for the errors of RM. These estimates use the Roodman-Morduch version of the data and Roodman's program cmp. The RM errors corrected for are: (i) censoring threshold error, (ii) inclusion of *crcensored* variable, and (iii) exclusion of the non-target dummy variable.