

Appendix D:

Bounds on Complier Effects

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1 The role of take-up

One concern about the models presented in the main analysis is that they ignore the role of differential take-up in explaining the impact of microcredit. While the results of the analysis stand for themselves as group-level causal impacts, the economic interpretation of the results might differ if we knew, for example, that the zero impact along most of the outcome quantiles was entirely due to lack of take-up by most of the households in the sample. The main results contain suggestive evidence that the lack of impact at most quantiles is not solely due to lack of take-up: the 2 sites that randomized loan access rather than branch access and therefore had almost full take-up (Bosnia and the Philippines) displayed the same trend as all the other sites. Yet the observed pattern of zeroes could still be due to low differential in take-up between treatment and control group, which was recorded in most sites. It would be ideal to understand the effect of microcredit on those who are induced to take up loans by this random expansion of access (the "compliers" in the Neyman-Rubin causal framework).

The core challenge to an analysis of the impact on compliers is that the Stable Unit Treatment Value Assumption (SUTVA) is unlikely to hold for individual households within a village, such that there is no satisfactory way to identify the distributional treatment effect only on those households. Without SUTVA it may still be possible to infer certain average characteristics of the compliers as in Finkelstein and Notowidigdo (2018), but this exercise does not easily extend to quantiles and relies on zero effects for never-takers. Even if SUTVA did hold, the conventional LATE result is not available to us because there is two-sided non-compliance in these samples: I not only have treated households who do not take up loans, but

I also have control households who do manage to access loans from the MFI being studied. Finally, even if the above two complications were not present, we would still face the challenge of translating these results to a quantile effects framework which is nontrivial as quantile functions do not obey any law comparable to the law of iterated expectations.

As an alternative approach, I pursue a bounding exercise that provides suggestive evidence that loan take-up patterns are unlikely to be responsible for the precise zero results along most of the distribution. Ideally, the right comparison to make is between the group of households who took up microcredit only due to the random expansion of access, and the same group of households in the control group. This comparison estimates the distributional effect on the compliers. But we cannot identify those households in the control group, because they are indistinguishable from the "never taker" households. Nor can we separate the compliers from the "always takers" in the treatment group. However, under a set of broadly reasonable assumptions for the microcredit setting - that is, assuming SUTVA may be violated - it is possible to develop bounds on the changes in the compliers' distribution.

The bounds I propose can be intuitively described and justified using the following reasoning. First, in the style of an individual-rationality constraint, one assumes that the always-taker and complier groups who take up the microloans should see weakly positive effects from actually taking up these loans versus not taking them up, and that this should be the case even if other things are changing in the environment around these borrowers. Second one assumes that always-takers ought to do better than compliers from taking up, since they take up even if it is very costly to do so. Third, one assumes that the spillover effects or other consequences of any SUTVA violation ought generally to be smaller than the direct effects on the compliers or always-takers.

Under these assumptions, consider comparing the outcomes for individuals who took up in the treatment group (always takers and compliers) and subtracting the outcome of those who took up in the control group (always-takers). One would imagine that the difference in outcomes observed between these groups has to be smaller than the treatment effect on compliers, since it compares within a set of people who all took up loans, and if anything the compliers probably do worse than always takers in absolute terms in the take-up state. Hence, this comparison might form a sensible lower bound on the complier effect.

Now consider comparing the outcomes for individuals who took up in the treatment group (always takers, compliers) against individuals who did not take up in

the control group (compliers, never-takers). One expects the always takers to do better from microcredit than compliers do, and if one's treatment effect is somewhat positively correlated with one's raw outcomes (as seems to be the case in the cross-country evidence) then one expects never-takers to fare no better than compliers on average. Therefore, one might expect that the outcomes for those who take up are an overestimate of the complier outcomes in the treatment state, while the outcomes of those who don't take up in the control state might be an underestimate of the outcomes of compliers in the control state. Thus, subtracting the latter from the former ought to produce a larger gap than the difference in the outcomes that compliers see from taking up versus not.

In the following section I derive a set of sufficient conditions under which these intuitive bounds hold even when we encounter violations of SUTVA and moderate rank re-ordering of households (even such that they can cross ranks with households from other groups).

1.1 Analytical Bounds Derivation Set-Up

Denote the three possible groups of households: always takers, compliers and never takers, $G \in \{AT, C, NT\}$, adopting the no defiers assumption standard from the LATE literature. Denote the quantiles of a group's outcome distribution $Q_G(TU, T)$ where TU is a binary indicator of taking up the loans, and T is any vector of assigned treatment status for the households in the given village or local area. I now derive a set of sufficient, though strong, conditions that justify the empirical bounds I propose above.

Assumption 1. *Quantile treatment effects on compliers and always-takers are weakly positive regardless of any effects of changes in the treatment assignment allocation (that is, regardless of the potential for SUTVA violations). Thus, pointwise for any u and $\forall T, T'$:*

$$\begin{aligned} Q_{AT}(1, T)(u) - Q_{AT}(0, T')(u) &\geq 0 \\ Q_C(1, T)(u) - Q_C(0, T')(u) &\geq 0 \end{aligned} \tag{1.1}$$

Assumption 1 would be implied by a similar ordering on the individual treatment effects, but as this present ordering does not imply a full ordering on the individual effects, it is more general. Some individuals in these groups can experience negative effects, but not so many nor so punitively that they outweigh the countervailing set of individuals who experience positive effects. In spirit, this is a quantiles version of the assumption of a positive expected return from taking up a loan.

If SUTVA holds, then the secondary argument is irrelevant and this collapses to an ordering of quantile treatment effects within any given treatment assignment. Once we accept that SUTVA is unlikely to hold, requiring stability of effects across treatment assignment regimes is natural, since by definition the compliers only take up when assigned to do so and therefore their "treatment effect" comparison always involves a counterfactual assignment status.

In addition, I use the following first order stochastic dominance assumption to generate a partial ordering of the quantiles of the three groups.

Assumption 2. *Pointwise for any u , $\forall T, T'$:*

$$\begin{aligned} F_{AT}(1, T) \text{ FOSD } F_C(1, T) \text{ and} \\ F_C(0, T') \text{ FOSD } F_{NT}(0, T') \end{aligned} \tag{1.2}$$

When a particular CDF F FOSD another, let us say F' , then F always lies to the right of F' . Hence, when they are transposed to quantile functions, the quantiles of F always lie above the quantiles of F' . Hence, Assumption 2 implies that for example $Q_{AT}(1, T)(u) \geq Q_C(1, T)(u)$ for any u . This is a strong assumption and makes the derivation of the quantile effect bounds quite simple; the bounds will still hold under moderate violations of this assumption.

Assumption 2 is also a little unusual in that it involves an ordering on absolute outcomes rather than the conventional ordering on treatment effects that one uses in monotonic selection models. Yet the specific assumption 2 here will be implied by the monotonic selection assumption if in addition the levels of the outcomes are somewhat positively correlated with this treatment effect. Assumption 2 will be unlikely to hold, even approximately, if either of these two patterns does not hold in the data. Fortunately, it seems likely that this is indeed the case for microcredit.

To see why, consider that as it generally takes a lot of time and effort to access microcredit, households are more likely to do so if their own treatment effects are larger. Further, while we cannot be sure this reflects the ordering within sites, the cross-site correlation between the average treatment effects and the control groups' levels of consumption, profit, etc. is generally positive (Meager, 2018). This at least provides suggestive evidence that a positive correlation between levels and effects may be present within sites as well, and that as a result, these bounds are reasonable here. Thus while this assumption and thus the bounds I derive may not be applicable to every situation, they do seem applicable to the microcredit data.

Finally I employ the following assumption on the SUTVA violation adjustments – also known as spillover effects across treatment assignment statuses – requiring

them to be weakly smaller than the direct effects on compliers and always takers of actually *taking up* loans. This seems sensible since it is hard to imagine how any spillover could be larger than the direct effect occurring; if giving me a loan increased my neighbour's consumption more than my own, economic theory and practice would need to be quite different than it is now. Of course, this is likely not the case for the never takers who may not experience any effects – or even negative group effects – but fortunately my bounds do not require any assumption on the size of the spillovers on the nevertakers. This requirement for moderation in the effect of changing treatment regimes from T' to T on our complier and always-taker groups can be expressed in assumption 3:

Assumption 3. *For always-takers and compliers, spillover effects are always smaller than direct effects. Pointwise for any u , $\forall T, T'$, and $\forall G, G' \in AT, C$,*

$$Q_G(1, T)(u) - Q_G(1, T')(u) \leq Q_{G'}(1, T)(u) - Q_{G'}(0, T')(u). \quad (1.3)$$

Armed with these conditions, I can derive bounds without assuming that there is no effect on the never-takers (in contrast to Imbens and Rubin 1997, Abadie Angrist and Imbens 2002, and Finkelstein and Notowidigdo 2018) which is fortunate because this is unlikely when SUTVA is violated in general, and particularly when other households in one's village are taking up new sources of credit.

In the following sections, I provide the upper and lower bounds for which the above assumptions are sufficient but not necessary. The value of the sufficiency conditions is that they provide some intuition for the situations in which the bounds are likely to hold. The bounds themselves are their own necessary conditions. The sufficiency conditions are nevertheless important because they allow us to develop an understanding of why and how we might expect these bounds to be relevant in any given study.

1.2 The Upper Bound

First, consider comparing the outcome of the households who take up in treatment versus those households who do not take up in control, as a potential upper bound on the distributional effects on compliers. These groups are composed of combinations of the three groups denoted above, so denote the quantiles of a combination of two groups by Q_{ATC} for the pooled set of always-takers and compliers, and Q_{NTC} for the pooled set of never-takers and compliers.

Theorem 1.1. *Under assumptions 1 and 2, the following **upper boundary condition on the complier quantile effects** holds pointwise for any quantile u .*

$$Q_{ATC}(1, T)(u) - Q_{NTC}(0, T')(u) \geq Q_C(1, T)(u) - Q_C(0, T')(u) \forall T, T'. \quad (1.4)$$

Proof. From assumption 2, we know $Q_{AT}(1, T)(u) \geq Q_C(1, T)(u) \forall u$. A random pooling of the two groups - such as occurs during a randomized trial - will generate an intermediate set of quantiles such that

$$Q_{AT}(1, T)(u) \geq Q_{ATC}(1, T)(u) \geq Q_C(1, T)$$

By a reverse argument, assumption 2 also implies that

$$Q_C(0, T)(u) \geq Q_{NTC}(0, T)(u) \geq Q_{NT}(0, T)$$

Hence,

$$Q_{ATC}(1, T)(u) - Q_{NTC}(0, T')(u) \geq Q_C(1, T)(u) - Q_C(0, T')(u).$$

□

1.3 The Lower Bound

Continuing with the same set-up, now consider comparing the outcome of the households who take up in treatment versus those households who take up loans in control. For this comparison to form a lower bound on the distributional effects for compliers, it must be that the following result holds.

Theorem 1.2. *Under assumptions 1, 2 and 3, the following **lower boundary condition on the complier quantile effects** holds pointwise for any quantile u .*

$$Q_{ATC}(1, T)(u) - Q_{AT}(1, T')(u) \leq Q_C(1, T)(u) - Q_C(0, T')(u) \forall T, T' \quad (1.5)$$

Proof. From assumption 2, we know $Q_{ATC}(1, T)(u) < Q_{AT}(1, T)(u)$, so

$$Q_{ATC}(1, T)(u) - Q_{AT}(1, T')(u) \leq Q_{AT}(1, T)(u) - Q_{AT}(1, T')(u).$$

The only difference in $Q_{AT}(1, T)(u)$ and $Q_{AT}(1, T')(u)$ is a spillover effect from the change in the treatment allocation regime. But for always takers and compliers, assumption 3 guarantees that this spillover effect must be smaller than the treatment

effect at any quantile. Hence,

$$Q_{AT}(1, T)(u) - Q_{AT}(1, T')(u) \leq Q_C(1, T)(u) - Q_C(0, T')(u).$$

Combining these two statements produces the required bound. \square

2 Empirical Bounds Results

I compute these bounds and I find that the posited lower bound does lie weakly below the posited upper bound in all cases (and strictly below in the case of consumption). However, the bounds are very close together and overall similar to the main distributional effect estimated by comparing treatment status itself (the "ITT" comparison, or the "access as treatment" comparison). Comparing the households who took up the loans in the treatment group to households in the control group who did not take up loans produces largely similar results - although they are weakly more positive - as comparing all treated and control households, as shown in figure 1. The results of comparing the households who took up the loans in the treatment group to households who took up in the control group for all outcomes is shown in figure 2. These effects tend to be broadly similar to the impact of mere access, in that they are zero almost everywhere, although on average the effects are estimated to lie weakly below the ITT effect. Taken together these results suggest that the bounds are themselves applicable to the microcredit studies and that the broad pattern of zero effects along most of the distribution occurs within the complier group as well as in the general population of households.

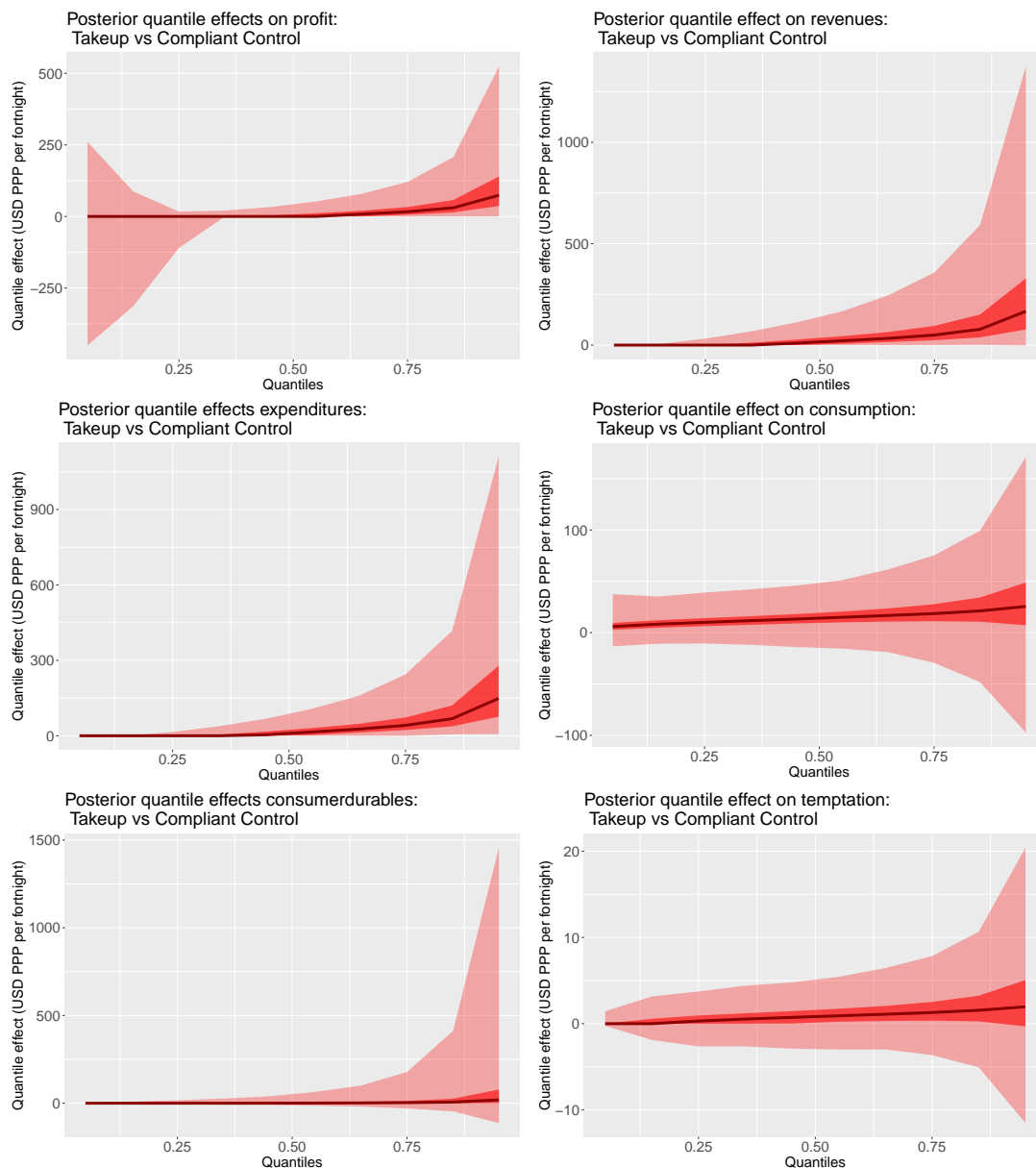


Figure 1: General Quantile Treatment Effect Curves for Business Outcomes: Treated households who took up vs Compliant control households who did not take up. This effect should overestimate the true impact of microcredit on those who take it up in a simple selection framework. The dark line is the median, the opaque bars are the central 50% interval, the translucent bands are the central 95% interval. [\[Back to main\]](#)

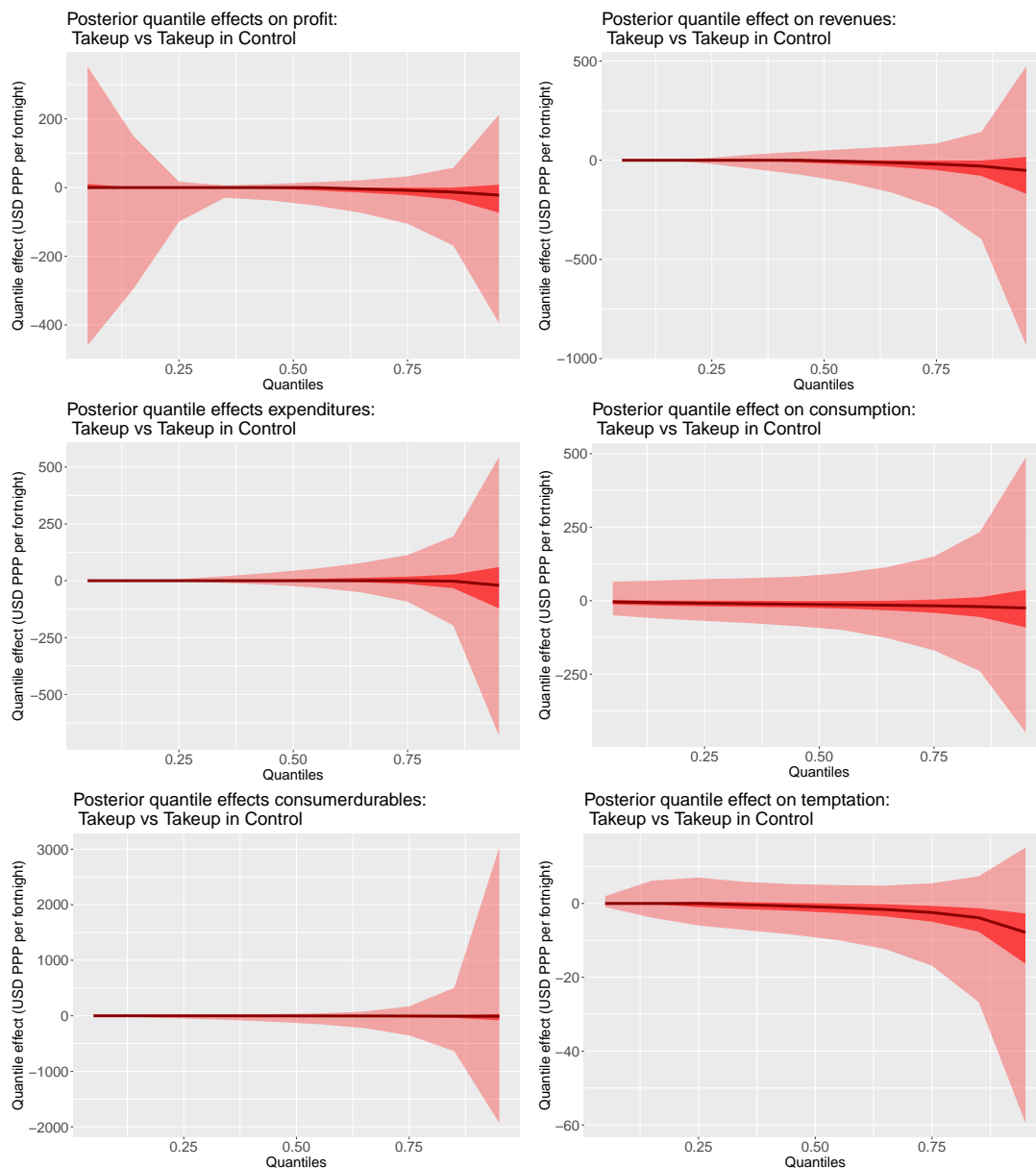


Figure 2: General Quantile Treatment Effect Curves for Business Outcomes: Treated households who took up vs Control households who took up. This effect should underestimate the true impact of microcredit on those who take it up in a simple selection framework. The dark line is the median, the opaque bars are the central 50% interval, the translucent bands are the central 95% interval. [\[Back to main\]](#)