Microfinance on the Margin: Why Recent Impact Studies May Understate Average Treatment Effects

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Bruce Wydick

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Abstract: A series of recent randomized trials estimate the impact of microfinance on incomes, consumption, and other key measures of welfare. This comment demonstrates why impact estimates obtained from experimental designs focusing on marginal microfinance borrowers are likely to understate the impacts yet realized by inframarginal borrowers, those having taken microfinance loans prior to implementation of an experiment, when field experiments are implemented in areas broadly served by microfinance.

Professor of Economics, University of San Francisco, 2130 Fulton St. San Francisco CA 94117-1080, e-mail: *wydick@usfca.edu*. The author is grateful to Chris Ahlin, Matthew Gentzkow, and two referees for helpful comments.

1. Introduction

A series of recently published randomized trials have produced the most rigorously gathered evidence to date of the impacts of microloans. In particular, the symposium of six field experiments presented in the January 2015 issue of the *American Economic Journal: Applied Economics* comprises one of the strongest collections of impact studies focusing on a single policy topic of widespread interest. This set of papers includes Angelucci et al. (Mexico), Attanasio et al. (Mongolia), Augsburg et al. (Bosnia-Herzegovina), Banerjee et al. (India), Crépon et. al. (Morocco), and Tarozzi et al. (Ethiopia). The symposium of papers presents intention-to-treat (ITT) estimates indicating access to microfinance increases borrowing and business activity, and in most cases business investment, but fails to realize significant impacts on household income and consumption.

The purpose of this comment is in no sense to downplay the major contribution of these studies. It rather helps to clarify what we can infer about the *average impact* of microfinance from recent field experiments that estimate treatment effects on borrowers at the external margin. In general, for these impacts to be equivalent, potential outcomes must be orthogonal to the order of self-selection into a choice-based treatment. However, there is evidence that this assumption is likely to be violated and that efficacy drops as new programs expand (Allcott, 2015).[1] What I aim to demonstrate in this short comment is that in the case of microfinance, there is good reason to expect that potential outcomes of early adopters exceed those of the later marginal adopter. What this implies is that even as these recent studies may yield unbiased estimates of impacts on the external margin, collectively they are likely to understate the average impacts of microfinance more generally, although to what degree it is difficult to ascertain. The results here may have implications for the randomized evaluation of other interventions in which similar adoption dynamics apply, such as complementary inputs to infrastructure, schooling, and health.

While there have been other excellent impacts studies on microfinance, such as Kaboski and Townsend (2012) and Field et al. (2013), I focus on the results of the symposium because of their more recent publication and the designed comparability of their results. Karlan, and Zinmann (2015) are careful to articulate an important qualification to the findings from the set of experiments:

> Another key caveat is that these studies have nothing to say about impacts on *infra*marginal borrowers. It may well be the case that impacts are substantially different on the borrowers and/or communities already being served before the lenders in these studies began experimenting on the margin. (p.3)

Here I will attempt to present a simple model that illustrates why the impacts of microfinance on borrowers at the external margin are likely to be substantially different—specifically lower—than those on inframarginal borrowers. Results from the model predict that the difference in treatment

effects between extra-marginal and infra-marginal borrowers is increasing in the degree of microfinance saturation within a given borrowing pool of potential borrowers. I will illustrate this through a framework that integrates a parsimonious microfinance borrowing model (an extension of Rajbanshi, Huang, and Wydick, 2015) into the structure of the Rubin (1974) causal model. Moreover, because the six experiments were implemented at heterogeneous levels of microfinance saturation in their respective countries, I suggest that the empirical results of this group of papers themselves contain clues which point to greater microfinance impacts on inframarginal borrowers than borrowers taking loans on the external margin. I conclude with some suggestions for future research strategies that may be helpful for ascertaining the impacts of microfinance on these inframarginal borrowers.

2. A simple model of microfinance borrowing

Consider an economy of m household microenterprises indexed by i. All income in the model is from self-employment. Absent any borrowing, the income in period t from each microenterprise is given by

$$Y_{it} = \phi_{it} K_{it}, \tag{1}$$

where K_{it} is the existing capital stock in period t, and ϕ_{it} is an unobservable complement to capital. The price of output is equal to one, making it equal to revenue. Suppose existing capital has no opportunity cost, is fixed, and cannot be sold or rented. Output of the microenterprise is a product of its capital and the unobservable complement to capital, ϕ_{it}, which can represent economic opportunity, entrepreneurial ability, self-motivation, or all three, any of which will increase the productivity of capital within an enterprise.

An important aspect of ϕ_{it} is that it is observable and actionable by a borrower, but is unobservable to a microfinance lender or a researcher. A second assumption is that $\phi_{it} = \phi_i + \phi_t$, such that it is a composite of two additive terms: $\phi_i \in [0, \bar{\phi}_i]$, a productivity parameter that is fixed for each microenterprise i in every period, but exhibits a variance across borrowers of σ_i^2, and secondly, a dynamic productivity shock, $\phi_t \in [\underline{\phi}_t, \bar{\phi}_t]$, independently distributed with variance σ_t^2, affecting each borrower i heterogeneously across periods.

Each microenterprise is endowed with K_{it} units of capital, but may borrow to finance *one* more unit of capital at interest rate r. At the end of the period t, a borrowing household returns the one unit of principal with interest so that if a household borrows, $Y_{it} = \phi_{it}(K_{it} + 1) - r$ yields the expression for net income from self-employment.

Suppose that prior to the availability of microfinance loans, the credit market is serviced by moneylenders, and that moneylenders have information about ϕ_{it} and are fully exploitative first-degree price discriminators, charging an interest rate $r_{ml} = \phi_{it}$. The microfinance interest rate, however, is the same for all borrowers and across all time periods, lying (non-trivially) between the extreme realizations of ϕ_{it}, so that $r \in (0, \bar{\phi})$. Microenterprise owners realize ϕ_{it} at the beginning of any period before they make a borrowing decision, and will take loans when $\phi_{it} > r$. Let $\Phi_i(\cdot)$ be the cumulative distribution function for microenterprise i of realizations of ϕ_{it} over an infinite sequence of periods 1, 2…∞, so that $1 - \Phi_i(\phi_i, r)$ is the probability that microenterprise i takes a loan in any given period. Let the *first* period in which an microenterprise takes a microfinance loan, contingent on microfinance availability in period 1, be given by $n \in \{1, 2, \dots \infty\}$. Two relevant results follow from this framework:

(1) Because $\phi_{it} = \phi_i + \phi_t$, on average higher-productivity borrowers will take microfinance loans before lower-productivity borrowers, *i.e.* $E(n_v) < E(n_w) \mid \phi_v > \phi_w$, $i = v, w$. Using the mean of the binomial distribution, the expected period in which a microenterprise will take a microfinance loan is $E(n) = [1 - \Phi_i(\phi_i, r)]^{-1}$, so that $\Phi_i(\phi_i, r) = 1 - [E(n)]^{-1}$. Totally differentiating this expression yields $\frac{d\phi_i}{dE(n)} = \left[\frac{d\Phi_i(\phi_i, r)}{d\phi_i} E(n)^2 \right]^{-1} < 0$. This means that it is less productive borrowers who take their first loans in later periods.

(2) Expected net income for an early borrower in the first period in which she takes a loan is $E(\phi_{in} \mid (n = \tilde{n}))(K_{it} + 1) - r$ and for the later borrower, $E(\phi_{in} \mid (n > \tilde{n}))(K_{it} + 1) - r$. Subtracting the (non-borrowing) counterfactual in (1) from both shows that (in large samples) the estimated treatment effect on the treated (ATT) will be greater for early borrowers than for later borrowers:

$$E(\phi_{in} \mid (n = \tilde{n})) - r > E(\phi_{in} \mid (n > \tilde{n})) - r. \,^2 \tag{2}$$

Moreover, second differentiation of the above yields $\frac{d^2\phi_i}{dE(n)^2} = -2E(n)\frac{d\Phi_i(\phi_i, r)}{d\phi_i} \cdot \left[\frac{d\Phi_i(\phi_i, r)}{d\phi_i} E(n)^2 \right]^{-2} > 0$, implying convexity. This means that the ATT on net income for microfinance loans falls sharply after primary borrowers receive their loans in period 1, but that the declines in the ATT level off for borrowers who take their first microfinance loan in subsequent lending periods.

Because the primary impact of microfinance on self-employment income is directly related to the difference between a borrower's productivity and cost of capital, the model is not arbitrarily specified. These primary impacts are likely to supersede secondary impacts from general equilibrium effects, other types of spillovers, risk sharing, and so forth. Still one potential objection could be that borrowers take microloans not to increase self-employment income in microenterprises, but to smooth consumption. But while some studies suggest that an important

impact of microfinance is consumption smoothing (e.g. Gertler et al., 2009), financing microenterprise expansion has always been the primary *raison d'etre* for both making and taking microloans, corroborated by large and significant impacts in business investment and expansion in the recent impact studies. Secondly one might be concerned that there is *double*-selection in a lending transaction. Borrowers not only self-select, but the lender also selects borrowers after reviewing applications. Indeed lenders generally select borrowers based not on productivity *per se*, but on ability to repay, although there clearly is a correlation between the two. The important point is that borrowers self-select *before* lenders select among the self-selected and that, all else equal, the portfolio of new applicants facing microfinance lenders declines over time in terms of potential outcomes. More productive, more highly motivated borrowers apply first.

Figure 1 presents results from a simulation of the model of 10,000 borrowers where $\phi_i \sim U[0,0.5]$, $\phi_t \sim U[0.2,0.8]$, $r = 0.25$, and $K = 0.20$. The simulation illustrates how the microfinance impact on self-employment net income declines sharply after primary borrowers. In a model where microfinance replaces a fully exploitative moneylender, impact on later borrowers falls to less than one-third of the impact that microfinance exhibits on the primary borrowers.

This makes intuitive sense and clearly applies to many other domains. For example, consider the difference in consumer surplus realized between those who wait in line to purchase a new-model iPhone on its release date (primary buyers) and those who purchase the same iPhone one or two years later through a promotion. Consumer surplus is likely to be much lower for the latter adopter. Like the timing of the purchase of an iPhone, the timing of microfinance borrowing involves substantial self-selection and, as shown in the model, it is highly endogenous. Moreover, the convexity of the relationship implies that experimental studies may fail to capture the most substantial microfinance impacts irrespective of whether an experiment is carried out only a few periods or many periods after the initial introduction of microfinance.[3]

When was this set of microfinance field experiments carried out relative to the year microfinance lending began in those countries? Figure 2 shows six plots from MIX Market data showing the growth in microfinance borrowers relative to the population in each of the six country sites from 1996 to 2013 along with markers that identify the year in which microenterprises were offered microfinance loans in the context of each field experiment. Interestingly, there is a high degree of variation across the studies. Relative to the quantity of microfinance borrowing that eventually took place in these countries, the Tarozzi et al. experiment began—in 2003—well before microfinance borrowing became widespread in Ethiopia, and similarly the experiments of Banerjee et al. (2005 in India) and Crépon et al. (2006 in Morocco). As seen in Figure 2, however, the other three studies (Bosnia-Herzegovina, Mexico, and Mongolia) were implemented at or very close to

microfinance saturation rates in their respective countries. Even though there may be scope for added microfinance lending in these contexts, for short-hand I will refer to these three as the "saturated countries."

Clearly the number of microfinance borrowers in a country may not correspond to microfinance availability in the particular *region* in which the experiment was carried out. Yet in the six studies in the symposium, it corresponds quite closely. Data from Table 1 in each study shows that in Ethiopia, India, and Morocco, the fraction of borrowers at baseline in the respective treatment regions who had MFI and formal bank loans was quite low, respectively (0.026, 0.026), (0.011, 0.036), and (0.01, 0.06). In contrast, in the saturated countries, Bosnia-Herzegovina, Mexico, and Mongolia, the figures were (0.61, 0.51), (0.10, 0.29), and (0, 0.47), respectively.[4] Thus the estimated impacts from microfinance borrowing in the saturated countries measure the impacts of expanding microfinance to marginal borrowers—an important policy question—but they do not capture treatment effects on primary and early borrowers.

How do the microfinance impact estimates compare between the more and less-microfinance-saturated countries? Although the papers provide impacts on many dependent variables, including net income from self-employment, remittances, work hours, consumption, savings, school attendance, loan repayment, and stress levels, the model yields impact predictions on none of these except for what is arguably primary among them: net income from self-employment. Figure 3 plots point estimates of impacts on net income from self-employment divided by the control mean against the existing level of microfinance lending in each country study, overlaid by a quadratic fit.[5] Point estimates for impact on net income from self-employment in the three non-saturated countries, which capture impacts on many primary and early borrowers, are all higher than every point estimate obtained in the saturated countries. Moreover, the impact relationship between the six countries strongly resembles the convex relationship generated by the simulation in Figure 1. (Point estimates of impact are lowest in the Augsberg et al. study in Bosnia-Herzegovina, where not only was microfinance saturation across the sample so high (61%) that impact estimations would virtually preclude the inclusion of initial borrowers in the sample. In addition, the study was carried out on marginally credit-worthy borrowers.) If low credit-worthiness is also correlated with low productivity, then we would expect estimates of the ATT in the study to be much lower than the average impact of microfinance across all microfinance borrowers.

Once there is a clear rationale for the existence of greater impacts on primary borrowers, one becomes drawn to some of the evidence pointing to positive self-employment income effects in the Ethiopia, India, and Morocco studies. Point estimates tend to lack statistical significance, in part because five of the six studies use a cluster randomization, which is helpful for capturing

externalities. But low take-up rates create large standard errors that preclude making definitive statements about impacts even in the three non-saturated countries where early borrowers were among the treated. Furthermore, these new credit areas were not primary but rather marginal *regions*, where in the case of India as Banerjee et al. note, Spandana was relatively indifferent towards program expansion.

Still, results hint at positive income impact. Tarozzi et al.'s regression coefficient from an index of dependent variables related to income impact in Ethiopia yields a *t*-statistic of 1.73, but a Hochberg correction of the family-wise error rate knocks the corresponding *p*-value down to 0.35. However, when so stringently controlling the Type I error rate, one must be also wary of creating Type-II errors that would fail to reject a false null of non-impact, especially in the context of a symposium that places specific constraints on what dependent variables can be reported and where a primary outcome family is indexed. Crépon et al. show point estimates on self-employment income that are twice as large as the corresponding decreases in wage income. Banerjee et al. find positive point estimates on both end-line measures of self-enterprise income, though driven largely by the upper tail of businesses that pre-existed microfinance access.[6] But as the studies in Ethiopia, India, and Morocco suggest, even if microfinance impacts on early borrowers are significantly larger than on late borrowers, they are almost certainly more modest than many microfinance advocates claim.

3. Conclusion

The experimental studies of microfinance yield a clear answer to an important policy question related to the benefit of expanding microfinance in places in the developing world where it already substantially exists. The answer is that the benefits, if not negligible, are surprisingly small and few. Indeed, taken in conjunction with the notion that impacts decline substantially among later borrowers, another policy insight may be that "less microfinance is more."

Experiments that use cluster randomization at the community level in areas with existing access to microfinance are well-suited for capturing externalities and community-wide effects, but in the process they set a high bar for revealing statistically significant household-level microfinance impacts, especially if an experimental intervention does not induce high marginal take-up rates.[7] Randomization at the household level in a context where demand for credit is high and microfinance is initially introduced through the experiment—the opposite extreme—may yield higher statistical power and the greatest advantage for revealing positive microfinance impacts, but estimates using individual randomization must be carefully specified or they may fail to account for any general equilibrium effects of microlending.[8] But the latter, not the former, constitutes the low bar that microfinance must fail to clear in order to draw stronger conclusions about a general ineffectiveness of microfinance.

Nevertheless the evidence that has been presented in this set of studies creates a strong case that a deepening of microfinance penetration into already-served areas is highly unlikely to result in significant impacts on poverty reduction. As such the recent experimental studies represent the best empirical evidence to date on the effects of microfinance expansion. They constitute a key piece to the puzzle of microfinance impact, but as the authors themselves carefully articulate, it is not the final piece.

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[1] More generally, Vivalt (2015) presents evidence from a meta-study of randomized trials highlight the different issues that surround the question of external validity in experimental studies related to development economics.

[2] The six microfinance studies in the symposium focus on Intention-to-Treat (ITT) estimates which spread impacts over the targeted sample. Assuming that k out of m total microenterprises obtain their first microfinance credit in period n as a result of the experiment, the inequality in the ATT between field experiments carried out in early versus later periods amounts to dividing (2) by m/k in the absence of externalities.

[3] The results here shed light on how different inferences should be made across experiments over different types of interventions. For some treatments, such as vaccines, in which the human body is likely to react in ways that are orthogonal to treatment order, the estimated impacts on marginal subjects should closely reflect impacts on inframarginal subjects. But for other types of treatments, especially those where behavior is strongly complementary (substitutionary) to the productivity of the treatment, impacts measured from experiments operating at the external margin are likely to under- (over-) represent impacts on inframarginal subjects.

[4] Baseline borrowing in the control group is generally given in Table 1 in each study. Instead of Table 1, Angelucci et al. provide specific baseline borrowing data in Table 2A.

[5] Impact estimates on net self-employment income are given in Table 4 in each study. For Augsburg et al., Figure 3 presents microfinance borrowing only and because the control mean is negative, uses the baseline control mean.

[6] Other recent work that has found substantial impacts during initial microfinance introduction is Rajbanshi et al. (2015), who find large and significant ITT impacts on business investment in difference-in-difference estimations from data in an area in eastern Nepal where microfinance was introduced subsequent to Comprehensive Peace Accord ending the Maoist insurgent conflict. Here microfinance take-up was 51% among the target population, much higher than in the experimental studies. These estimates are, of course, subject to all of the caveats involved in the analysis of non-experimental data.

[7] Augsburg et al. (2015) randomize at the individual level among a group of borrowers marginally rejected for microfinance loans.

[8] It may be challenging to experimentally replicate impacts on the early adopters of microfinance. Areas chosen for initial program placement are not random. They may be areas where demand and aspirations among clients are higher; even a randomized expansion into new areas yet unserved by microfinance may not replicate early impacts realized in primary branch locations.

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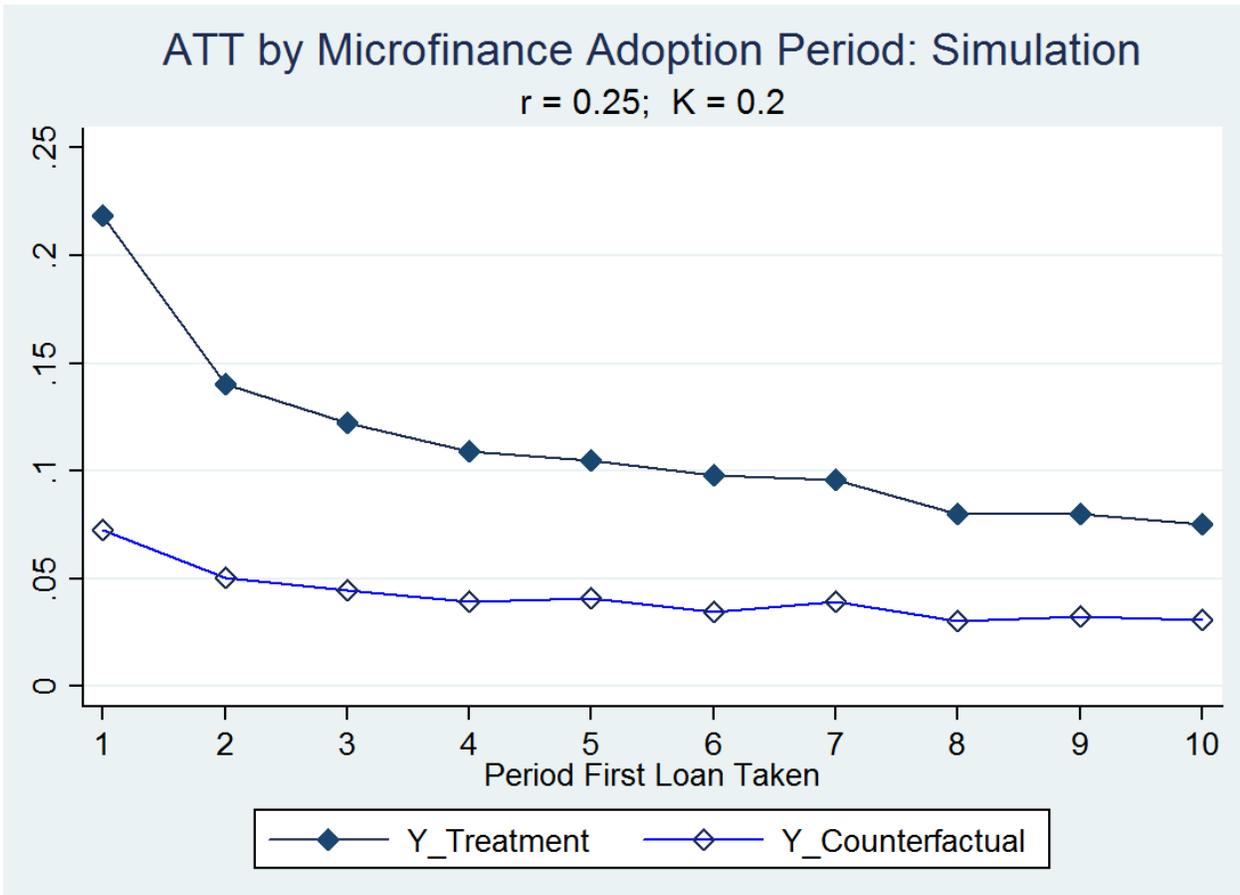
Figure 1: Simulation of Microfinance Impact on Enterprise Net Income by Initial Take-Up Period
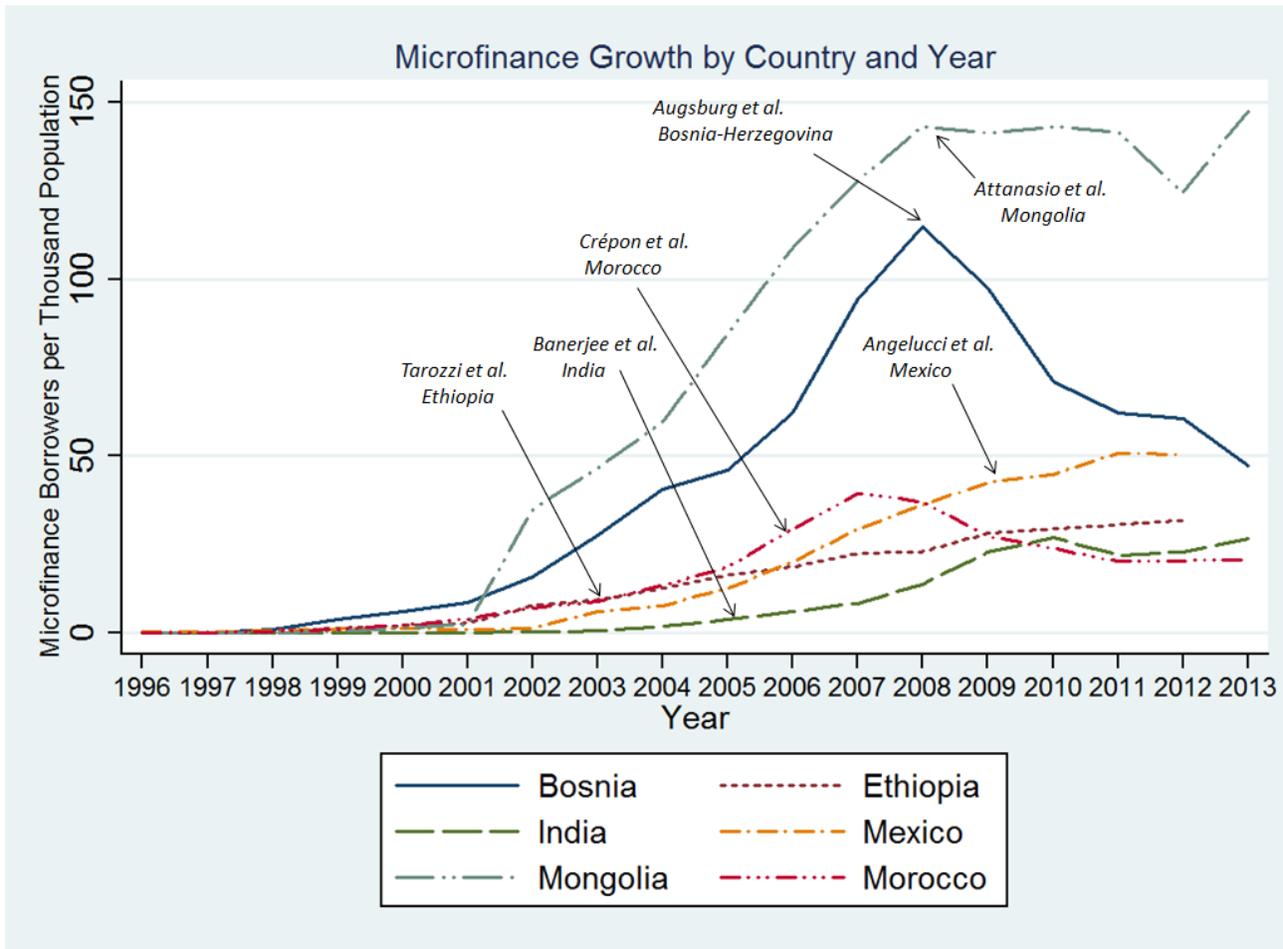(Source: Author Simulation of Model)

Figure 2: Microfinance Borrowers by Country and Year with Timing of the Six Field Experiments

[Source: MIX Market data; Angelucci et al. (2015); Attanasio et al. (2015); Augsburg et al. (2015); Banerjee et al. (2015); Crépon et. al. (2015); and Tarozzi et al. (2015).]
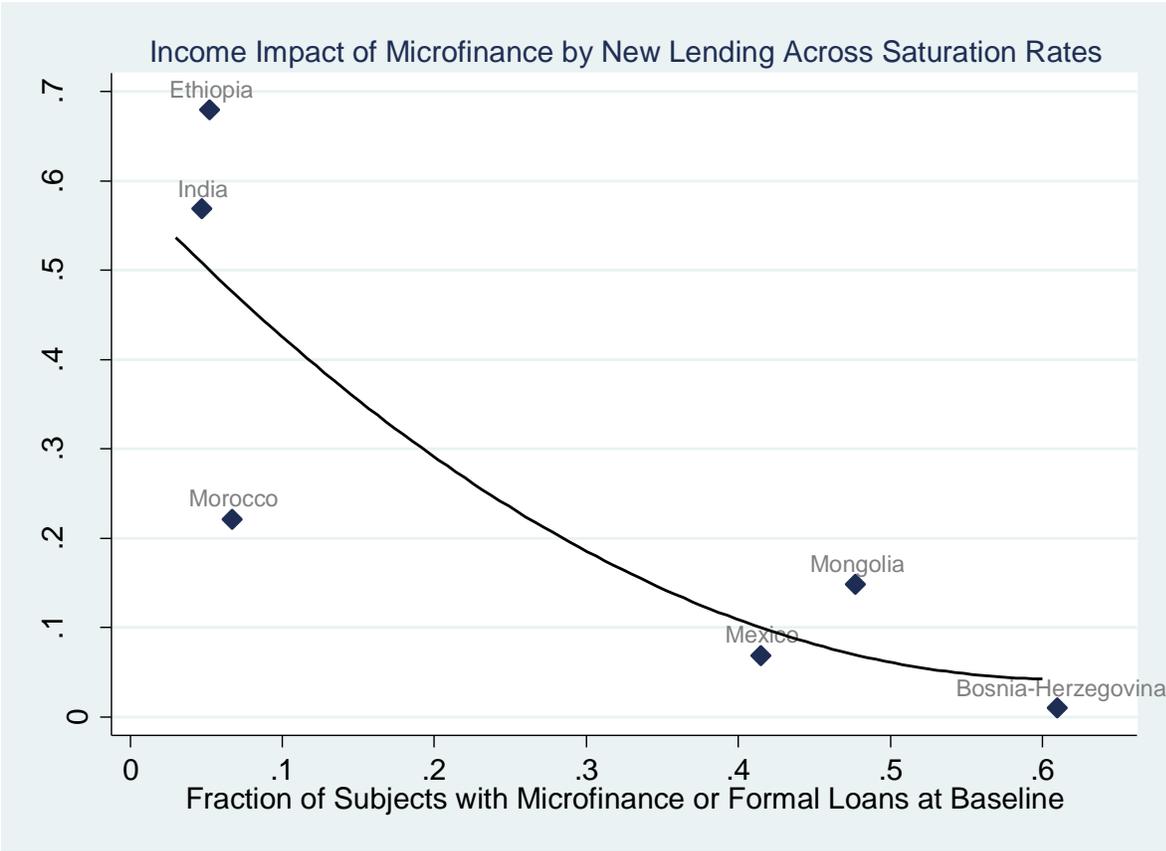
Figure 3: Marginal Impact of Microfinance at Different Levels of Formal Lending Saturation

[Source: Angelucci et al. (2015); Attanasio et al. (2015); Augsburg et al. (2015); Banerjee et al. (2015); Crépon et. al. (2015); and Tarozzi et al. (2015).]