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Abstract

We use data from a randomized controlled trial conducted in 2003-2006 in rural Amhara and Oromiya (Ethiopia) to study the impacts of the introduction of microfinance in treated communities. We document that borrowing increased substantially in locations where the programs started their operations, but we find mixed evidence of improvements in a number of socio-economic outcomes, including income from agriculture, animal husbandry, non-farm self-employment, schooling and indicators of women's empowerment.

Key words: Microcredit; Cluster Randomized Controlled Trial; Ethiopia
JEL codes: O12, O16

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1 Introduction

This paper describes the results of a large-scale clustered randomized controlled trial (RCT) conducted in rural Amhara and Oromiya (Ethiopia) between 2003 and 2006. The study locations included areas that were planned to benefit from the expansion of micro-credit and family planning programs (FPPs) supported by the David and Lucille Packard Foundation Population Program. The main purpose of the RCT was to evaluate whether linking credit and FPPs could have a larger impact on contraceptive use than either program operating on its own. The study was conducted over a large area encompassing 133 administrative areas called 'peasant associations' (PAs), using a 2×2 factorial design where PAs were randomly assigned to one of four group: micro-lending only, FPP only, both, or none (control). Desai and Tarozzi (2011) show that neither type of program, combined or in isolation, led to an increase in contraceptive use beyond what was observed in the control group.

Although the RCT was conducted for the purpose of gauging the impact of the interventions on contraceptive behavior, household surveys at baseline and follow-up measured a broad range of outcomes, including income-generating activities, livestock ownership, schooling and measures of women's empowerment. The study design offers thus the possibility of estimating the impact of microfinance on a large number of socio-economic outcomes. This makes ours one of the first RCTs to examine this important mechanism for improving credit access for the poor after the early contribution of Banerjee et al. (2013), who examine the impact of increased access to microfinance in urban slums in Hyderabad (India). This latter study, like ours, evaluates the impact of introducing microfinance at the community level, a feature also shared by the RCTs evaluated in Angelucci et al. (2013) (rural and urban Sonora, Mexico), Attanasio et al. (2013) (rural Mongolia) and Crépon et al. (2011) (rural Morocco).[1] Other studies have estimated the impact of access to microcredit by using randomization among micro-credit clients close to the threshold of eligibility for loans, see Karlan and Zinman (2010) (urban South Africa), Karlan and Zinman (2011) (urban Philippines) and Augsburg et al. (2012) (Bosnia-Herzegovina).

Our study relies on data gathered in study areas during a pre-intervention household survey

[1] Pronyk et al. (2006) randomized access to loans for women (together with a gender and HIV training curriculum) in a randomly chosen half of study villages in rural South Africa. However, only eight villages were included in the study, and key results are not based on a treatment-control comparison but rather on comparing women who self-selected into the program in treated areas with others from control communities matched based on observed characteristics.

carried out in 2003 ('baseline') and a post-intervention survey completed three years later ('follow-up'). Each survey was conducted by interviewing about 6,000 households, with similar sample size from the two regions of Amhara and Oromiya. At follow-up, interviewers re-visited the same study villages, but they did not seek to re-interview the same households, so our data constitute a panel of villages but not of households. Baseline and follow-up samples were drawn independently of each other, and independently of program participation. The RCT was conducted in poor rural areas where agriculture and animal husbandry represented the bulk of the local economic activities. Borrowing was not common and at baseline just above one household every ten had any outstanding loans. The credit operations introduced in treated communities were implemented in the two study regions by the Amhara Credit and Savings Institute (ACSI) and by the Oromiya Credit and Savings and Share Company (OCSSC).

In several respects, both institutions were typical micro-lenders that granted small loans to small and self-formed groups of borrowers who took joint responsibility for loan repayment. Loan eligibility was supposed to be selected on the basis of several criteria, of which the presence of a viable business plan and poverty status were the more salient ones. Lending was supposed to especially target women, although guidelines in this regard were loose, and indeed we find that a majority of loans were initiated by men. In addition, unlike many micro finance institutions (MFIs), OCSSC and ACSI often required some forms of collateral from borrowers (as in the program studied in Attanasio et al. 2013), making it harder for the very poorest households to have access to loans. Despite such requirements, we will show that borrowing increased substantially more in treated relative to control communities, both on the intensive and the extensive margin. Almost all the increase in borrowing was due to micro-loans from ACSI/OCSSC, suggesting that rather than displacing other forms of pre-intervention borrowing, the introduction of micro-lending led to substantial relaxation of credit constraints. However, we also find that for a large majority of socio-economic outcomes the null of no impact cannot be rejected, although in a number of cases the point estimates are substantively large but imprecisely estimated.

The rest of this paper is organized as follows. Section 2 describes the details of the intervention and the study design. Here we also discuss how the micro-lenders partly deviated from the experimental protocol, starting operations in some areas assigned to control while doing the opposite in some PAs assigned to treatment. Because such deviations from protocol potentially invalidate the exogeneity of treatment, we estimate program impacts using two-stage least squares (2SLS), using randomly assigned treatment as an instrument for actual treatment. We include the details of the

estimation strategy in Section 3, where we also describe the results. Finally, Section 4 concludes, after discussing our findings in relation to the literature.

2 Study Design and Baseline Summary Statistics

The study was implemented over a large geographical area in rural western Ethiopia, spread over 133 administrative units called *kebeles* or 'peasant associations' (PAs) from two 'zones' of the Oromiya region and two zones in the Amhara region, about 300-400 Kms respectively west and north of the capital Addis Ababa, see the map in Figure 1.[2] All study areas were primarily reliant on agriculture and animal husbandry, which in some cases were supplemented with small-scale retailing activities or day labor. Unlike the arid eastern regions, the study locations usually benefit from plentiful precipitation, with an average of 1,200-2,000 millimeters of rainfall per year in 1971-2000 in both regions.

The study area was identified in the context of the expansion of micro-credit and FPPs supported by the David and Lucille Packard Foundation Population Program. The expansion was implemented in the field respectively by the Oromiya Credit and Savings Share Company (OCSSC) and the Oromiya Development Association (ODA) in Oromiya and by the Amhara Credit and Savings Institute (ACSI) and the Amhara Development Association (ADA). Within this context, the research team identified 191 villages in 78 PAs where OCSSC and ODA planned to expand in the coming years in Oromiya and 162 villages in 55 PAs where ACSI and ADA planned to expand.

In each of the 133 study PAs, interview teams obtained a complete list of all villages with an estimate of the total number of households in each village. If the PA had more than 400 households, three villages were randomly selected. If the PA had fewer than 400 households, two villages were selected at random. Within each selected village, interview teams conducted a complete enumeration of households, and a random sample of households was chosen to participate. In all, 3240 households were selected and 3216 interviews (99.3%) completed in Oromiya. In Amhara, 3200 households were selected and 3068 interviews (96%) completed. The sample is not self-weighted

[2]Peasant associations are the smallest local unit of government in Ethiopia and comprise a number of villages. PAs are then grouped into 'woredas', which are then aggregated into 'zones', and zones into regions (Ofcansky and Berry 1991). The eight study woredas in Oromiya are Anfillo, Ayra Guliso, Haru, Mana Sibu, Nedjo and Seyo in West Wellega zone, and Metu and Chora in Illubabor zone. In Amhara, they are Bugna, Gidan and Meket Delanta in the Semien (or 'North') Wollo zone, and Metema, Chilga, Alefa Takusa, and Lay Armachiho in North Gonder zone.

and therefore sampling weights are required to produce unbiased estimates of population statistics. We use sampling weights throughout the paper, although the un-weighted results are generally very similar.

2.1 Experimental Design

The data used in this paper were collected *as two independent cross-sections from the same villages* in 2003 and 2006 as part of the evaluation of a cluster randomized controlled trial (RCT) conducted by Family Health International. The main focus of the RCT was on fertility choices and contraception, and its primary purpose was to determine whether linking microcredit with family planning services could increase the use of contraceptives beyond what each program could accomplish separately. As part of this evaluation, after the completion of the baseline survey the 78 PAs in Oromiya were randomly scheduled to see the introduction of microcredit (20 PAs), family planning services (18), both (20) or neither (20). The 55 PAs in Amhara were assigned as follows: microcredit (14), family planning services (13), both (15), or neither (13). Randomization into the three treatment groups and one control group was completed independently in each of the two regions at the PA level, meaning that all villages selected in each PA (and all selected households in each of these villages) were assigned to the same group. Randomization was done to produce roughly 800 households in each of the four original treatment groups.

The community-based FPPs were based on women from local communities trained and remunerated to make house-to-house visits, provide fertility-related information and offer contraceptives at no cost. In areas where both services were introduced, credit officers also provided information on family planning methods to women borrowers (but did not offer contraceptives). In principle, the FPPs could have had an impact on economic activities via a change in family planning. However, Desai and Tarozzi (2011) show that the programs (both in isolation and when jointly present) failed to modify contraceptive behavior, and were only weakly associated with changes in fertility.[3] For this reason, in this paper we choose to focus only on the impact of increased access to microcredit, although in our preferred estimates we also control for the presence of FPPs, in isolation or in addition to micro-lending, see Section 3 for details.

Both ACSI and OCSSC, the two micro-finance institutions (MFIs) that partnered with Packard for this evaluation, are development-oriented institutions with strong links to the Government.

[3]The most likely reason is that the FPP did not provide injectables (the main contraceptive method in these regions), although referrals to clinics were provided.

Prior to the end of the civil war in 1991, all banking and insurance activities were monopolized by the Ethiopian government. Proclamation No. 84/94 was later issued allowing private domestic investors to also participate in these activities, but the government maintained a strong involvement in the evolution of Ethiopian MFIs, which overall operate is a non-competitive environment (Wolday 2002). At the time of writing, government-supported micro-enterprise lending program encompasses about 30 MFIs registered, licensed, and regulated by the National Bank of Ethiopia, including ACSI and OCSSC.[4] ACSI began as a project of the NGO Organization for the Rehabilitation and Development in Amhara, and was officially established as a microfinance institution in 1997.[5] Its stated mission is to "improve the economic situation of low income, productive poor people in the Amhara region through increased access to lending and saving services". OCSSC was also established in 1997, and was born out of the Oromiya Rural Credit and Savings Scheme Development Project, with the stated mission to "provide need-based micro financial services to strengthen the economic base of the low-income rural and urban people in Oromiya through increased access to sustainable and cost efficient financial services".

Both ACSI and OCSSC operated on the basis of group lending. Small and self-formed groups of borrowers, who took on collective responsibility for repayment of loans, were selected on the basis of several criteria, of which business plan and poverty status were the more salient ones. Loans were made for one year at interest rates reflecting market conditions. Based on OCSSC and ACSI records, the interest rate in 2002-2003 was 12% per year on average. Credit officers helped fill out loan applications and monitored the groups. Borrowers were expected to make regular deposits and repayments. Both OCSSC and ACSI reported repayment rates higher than 95% in the years before the intervention. In both regions, the credit program expansion was supposed to target poor women borrowers, but in reality no strict guidelines about the gender of the borrowers was issued. For this reason, as we will show, loans were often granted to individuals of both genders. The guidelines of both micro-lenders mentioned that no collateral was required in order to have access to loans, although our post-intervention data suggest otherwise, with a majority of borrowers indicating that they had been asked for collateral.

Before discussing the baseline summary statistics, it is important to highlight limitations of this study related to statistical power. Although both pre and post-intervention surveys recorded a wealth of information about households' socio-economic conditions and income-generating activ-

[4]See http://www.aemfi-ethiopia.org/site/membership.html.

[5]For more information see http://www.acsi.org.et.

ities, sample size was determined specifically to ensure sufficient power to detect changes in rates of contraceptive usage, which was initially the key outcome of interest.[6] An implication of this is that statistical power was ex-ante relatively low for outcomes such income or wealth indicators, outcomes which are usually characterized by large variability and measurement error. We will return to this point when we discuss the results.

2.2 Baseline Summary Statistics

The randomization was overall successful at producing balance in a broad range of statistics among the four original treatment groups (Desai and Tarozzi 2011, Table 2). Because in this paper we focus on the impacts of micro-credit, in Table 1 we show summary statistics calculated separately for communities where micro-lenders were assigned to start operating ('assigned to treatment') versus others assigned to receive either FP programs or no intervention ('assigned to control'). Overall, the figures show good balance, with differences between arms generally small and significant at standard levels for only one of 35 variables.

The summary statistics document the poor overall socio-economic status of sample households. Households were large (about 5 members on average) and most household heads had low levels of schooling. Most study communities were remote, on average more than an hour away from the nearest market or health center. More than a quarter of households used surface water (from rivers, lakes etc.) as main source for drinking needs. Food scarcity was also common, with respondents reporting on average more than two months of insufficient food in a typical year.

Agriculture was the main economic activity for almost 90% of households. In control areas revenues from crops during the 12 months before the interview were 203 Birr on average (or about 90 USD using the PPP exchange rates in World Bank 2008), while expenditures for such production were 90 Birr. Animal husbandry was also important, both as an income-generating activity and as a form of asset accumulation: in control areas, on average households owned almost three large animals such as cows or oxen, the total value of livestock was 1500 Birr (about 670 USD, more than seven times the average revenues from crop cultivation) and sales of animals amounted to 160 Birr in the previous 12 months. Other sources of income included wage labor (174 Birr per

[6] To be specific, sample size was determined to ensure an 80 percent probability of rejecting the null of no effect at the 5 percent significant level, assuming a baseline contraceptive rate of 6 percent (estimated from the 2000 Demographic and Health Survey of Ethiopia), an intra-class correlation of 0.05 and a 12 percentage points difference in contraceptive behavior between any two of the four experimental arms.

household on average during the previous 12 months), transfers in cash or kind (115 Birr) and sales from non-farm business activities (310 Birr), although households only owned 0.1 such activities on average.

Baseline information also shows that borrowing was very limited in the area. Only 13 percent of households borrowed in Control areas, and the average amount of outstanding loans (including zeros for non-borrowing households) was less than 40 Birr. Most households borrowed from informal sources, while less than 3 percent had loans from formal institutions such as banks and cooperatives. Micro-finance institutions or revolving credit associations were also rare, with about two percent of sample households having funds from such sources.

In principle, the very low prevalence of borrowing at baseline may be consistent with low demand for credit, but several indicators suggest that limited access to credit had negative implications for households' income generating activities and consumption smoothing ability. First, we have seen that on average households experienced more than two months of insufficient food. Indeed, only 27 percent of respondents said that their household always had enough to eat, while 45 percent stated that food was not sufficient for 2-3 months in a typical year and about one quarter said that food scarcity was a problem for four months or longer. Second, limited access to credit was mentioned among the three most important factors limiting income growth in agriculture and non-farm business activities by about 20 percent of households involved in such activities.[7] Limited access to credit may also have contributed to the fact that only 11 percent of households had any non-farm business, although our data do now allow us to test this conjecture.

2.3 Deviations from the Experimental Protocol

We have seen that the results in Table 1 show a good degree of balance in observed characteristics between areas assigned to treatment and control. However, the implementation agencies did not always comply with the experimental protocol. In fact, actual treatment coincided with the randomly assigned treatment in only 104 of the 133 PAs, that is, in 78 percent of cases. Specifically, 8 of the 69 PAs where micro-credit was supposed to be introduced did not see it happening, while ACSI/OCSSC started to operate in 15 of 64 areas where they had not been assigned to do so.[8] In

[7] Insufficient credit was not listed as a possible option limiting income growth in livestock activities, so we do not have clear information about the role of credit constraints for this activity, although we know that insufficient grazing land was mentioned as the key limiting factor in 83 percent of cases.

[8] In the end, both microfinance and FPP were introduced in 43 PAs, microfinance only was introduced in 33, FPP only in 20 and neither in 37.

Table 2 we show the results of a linear probability model where we regress a dummy for (actual) treatment (let this be denoted by $T = 1$) on a dummy for randomly assigned treatment ($D = 1$) and a list of observed household characteristics.

Although as expected assigned treatment is the strongest predictor of treatment, a number of other coefficients are large and significant at standard levels. In particular, borrowing from informal sources decreases the predicted probability of treatment by 16.5 percentage points (p-value < 0.001), and income from wages or transfers also decreases significantly such probability, although the corresponding slopes are very small. On the one hand, the results of the regression do not show an overall unambiguous picture of purposely selective program placement, for instance towards areas that were richer or poorer, or towards areas with more or less access to formal credit. On the other hand, the presence of some large and significant coefficients suggests that deviations from protocol were not completely random. This is also confirmed by the result of a joint test of significance of all slopes except the one corresponding to D: the p-value of such test is 0.011, so the null is rejected at the 5 percent level. As a consequence, as we describe below, in estimating impacts we will use two-stage least square estimators, using assignment to treatment as instruments for actual treatment. Random assignment ensures the exogeneity of the instrument, while the relatively limited departures from the experimental protocol rule out concerns related to weak instruments.

2.4 Migration

Before discussing the results, we briefly address the potential problem of population changes between the baseline and the follow-up, carried out three years later. Because both surveys were conducted by drawing independently from a separately generated village census, the estimated treatment effects could be biased if local populations had changed between baseline and follow-up. For instance, access to micro-credit could have changed the likelihood of migrating from (or into) a village differentially by treatment status. The lack of a true panel of households does not allow us to examine out-migration conclusively. However, information about the length of time spent by sample households in their village of residence allows us to gauge the extent of in-migration.

Only 80 of 6263 respondents in the post-intervention survey (1.3 percent) reported having lived in their village for less than four years (recall that the baseline was completed three years earlier). Information on the reason for migration is available for only 46 of these households, but in no case was the reported reason directly related to the availability of microcredit, although 23 respondents

reported migration was due to 'work'. Of these 23, 18 moved into control areas, so most of these re-locations were not due to earning opportunities opened up by the increased availability of credit. Overall, these estimates suggest that migration rates were very low and unlikely to bias estimated impacts in a substantial way.

3 Estimation Methodology and Results

The lack of a panel of households does not allow us to look at changes in outcomes at the household level, but because the same villages were surveyed before and after the intervention we can control for the presence of time-invariant location-specific fixed effects. The equation being estimated is thus the following

$$y_{pi,t} = \alpha_p + (\beta_{Post} + \beta_{MF} MF_p + \beta_{FP} FP_p \times (1 - MF_p) + \beta_{Both} \times FP_p \times MF_p) \times Post_t + u_{pi,t}, \quad (1)$$

where $y_{pi,t}$ denotes an outcome for household (or individual) i in PA p and time t (where $t = 0$ denotes baseline and $t = 1$ follow-up), α_p is a PA fixed effect and $Post_t$, MF_p and FP_p denote binary variables equal to one when, respectively, $t = 1$ and when microcredit or FPP were introduced in PA p. The residual $u_{pi,t}$ is allowed to be correlated within PA, so all standard errors and tests will be robust to intra-PA correlation. The coefficient $\hat{\beta}_{MF}$ is thus the main object of interest. With randomly allocated treatment (a condition that fails in our empirical context), such parameter would measure the intention-to-treat (ITT) in the study area, that is, the impact of introducing micro-lending, regardless of actual household borrowing (Heckman et al. 1999, p. 1903). Note that equation (1) also controls for the introduction of family planning, either in isolation or in addition to micro-lending. The coefficient β_{FP} measures the impact of introducing only family planning operations without micro-lending (that is, $FP_p = 1$ and $MF_p = 0$), while β_{Both} measures any differential impact of micro-lending when family planning operations were present (that is, $FP_p = 1$ and $MF_p = 1$). In light of the fact that the family planning services were not effective at changing contraceptive usage (the main outcome targeted, see Desai and Tarozzi 2011) our prior was that $\hat{\beta}_{FP}$ and $\hat{\beta}_{Both}$ would be generally small and not statistically significant. This prediction is mostly but not always borne out in the data. Still, because the main objective of the paper is to gauge the impact of microfinance we choose to focus on $\hat{\beta}_{MF}$, although we report the full results in the Appendix, where we also show the estimates when the presence of family planning services is ignored (that is, when we impose $\beta_{MF} = \beta_{FP} = 0$).

To overcome the potential endogeneity of treatment that derive from imperfect compliance with the experimental protocol, we estimate model (1) with 2SLS, using as instruments the interactions between $Post_t$ and three dummies constructed as in (1) but using randomly assigned treatment status instead of actual treatment.[9] Because randomly assigned treatment status is an exogenous and strong instrument for treatment, 2SLS will estimate the ITT parameter if program impact is homogeneous across areas. However, in the common situation where program impacts are heterogeneous, and under the plausible assumption that assignment to treatment (weakly) increases the probability of treatment in all PAs, 2SLS will only estimate a local average treatment effect (LATE, Imbens and Angrist 1994). The LATE can thus be interpreted as the impact only for 'complier' PAs, defined as those that saw the introduction of micro-lending only because they were assigned to see it. Despite this limitation, the estimated parameter remains of policy interest, but in interpreting the results it should be borne in mind that the estimates are not relevant for the (unobserved) sub-populations of PAs where micro-lending would have (or would have not) been introduced in any case, regardless of the result of the randomization. Finally, we stress again that our estimates will measure the impact of *access* to micro-loans in complier PAs, and not the average impact on households who actually borrowed in such communities. The latter parameter is certainly of interest and in the absence of spillovers it could be estimated by dividing the intent-to-treat by the fraction of borrowers. However, the introduction of microfinance is likely to generate general equilibrium effects that will affect non-borrowers as well (Kaboski and Townsend 2012, Buera et al. 2013), so we do not pursue this estimation strategy here.

3.1 Impact on Borrowing Behavior

We base our estimates of impacts on borrowing behavior solely on data collected either in the pre-intervention (2003) or in the post-intervention surveys (2006). We thus have no detailed information provided directly by the MFIs about variables such as total loans disbursed or clients served by PA/year. However, service data collected in all PAs to verify the extent to which program implementation deviated from the experimental protocol indicate that by the end of 2003 ACSI/OCSSC were already granting loans in 63 percent of the PAs where they eventually entered before the follow-up survey, and the proportion grew to 82 percent by the end of 2004. In a large

[9] Recall that equation (1) includes location-specific dummies, so the arm-specific dummies not interacted with time are absorbed by the PA fixed effects α_p. Program impacts are thus identified by treatment status interacted with the $Post_t$ dummy, as in a standard difference-in-differences model.

majority of communities, program exposure was thus as long as 2-3 years.

In Table 3, we demonstrate that the intervention led to substantive and statistically significant increases in borrowing. The figures in column 1, panel A, show that the fraction of households with any outstanding loans from ACSI/OCSSC increased from almost zero to 36% following the introduction of micro-lending.[10] We also find no evidence of crowding out of other forms of borrowing: the frequency of loans from NGOs, banks and cooperatives increased by 3-4 percentage points and that of loans from informal sources decreased by 1 percentage point, but none of these is significant at standard levels (column 2-4). As a consequence, the increase in loans from the micro-lenders is almost identical to the overall increase in the frequency of borrowing (column 5). We also see that, despite the lack of strict guidelines about targeting women for loans, female borrowing saw very substantial increases: while in control areas less than 3 percent of households had loans initiated by women, the 2SLS estimates show an 11 percentage points increase in treatment areas.[11]

The estimates in the bottom panel B of Table 3 show that not only the frequency of borrowing increased, but the amounts involved were substantial. The estimated increase in borrowing caused by the introduction of micro-lending was 460 Birr, that is, more than twice the average revenues from crop sales in control areas at baseline and about one third of the average value of all livestock (see Table 1). When we look only at households who borrowed from ACSI/OCSSC (results not in table), we estimate that the median loan at follow-up in treated areas was approximately 1,200 Birr (about 500 USD), with only about 10% of loans smaller than 700 Birr and about 10% larger than 2,000 Birr. To put these figures in perspective, the official poverty line, expressed in total consumption per adult/year in 2006, was close to 1,500 Birr, while the mean amount of total outstanding loans among households who borrowed (from any sources) at baseline in control areas was about 300 Birr (in 2006 units).

As we discussed earlier, in a subset of PAs the micro-lending operations were accompanied by the introduction of community-based FPPs, conducted by other independent organizations.

[10]The figure of 0.0198 represents the fraction of households borrowing from 'revolving credit associations' (RCA) at baseline in control areas. In the section of the post-intervention questionnaire were outstanding loans were listed borrowing from ACSI/OCSSC was coded as borrowing from RCAs and so we use the two terms as identical, although, especially at baseline, in some cases the loans may have been obtained from sources different from ACSI/OCSSC. In any case, our data indicate that the large majority of loans from RCAs at follow-up were indeed from ACSI/OCSSC.

[11]Note also that, because the follow-up was conducted in the same villages as the baseline, model (1) can be estimated using village, rather than PA, fixed effects. In Appendix Table A.4 we show that the two results are almost identical in terms of both point estimates and standard errors, and so in the rest of the paper we will only focus on the PA-fixed effects results.

In Table Appendix A.4, we show that ignoring the presence of such programs leave the results substantively unchanged, although both the point estimates and the standard errors of the impacts on the amounts borrowed become larger. As expected, in all regressions the coefficients β_{FP} and β_{Both} are not significant at standard levels, although in some cases the point estimates are not small.

In Table 4 we show the results of a simple regression where we analyze what predicts borrowing from micro-lenders in treated areas. Of course these estimates are not to be interpreted causally because all regressors are likely endogenous, being possibly correlated with sources of unobserved heterogeneity such as entrepreneurship, impatience and risk aversion among the others. Still, we chose regressors that were unlikely to have been affected by the availability of credit, so that at least we can exclude reverse causality from borrowing to the covariates. Recall also that we do not have a panel of households, so we do not have any household-specific data recorded for these same households before the intervention. We also included in the regressions PA fixed effects, so that the estimates control for all PA-level characteristics that may enter in an additive, linear way in the prediction. The results show that a number of variables are very strong predictors of borrowing from micro-credit. In particular we find evidence that households with low socio-economic status were less likely to borrow from ACSI/OCSSC: everything else being the same, the presence of a head with no formal schooling reduces the likelihood of borrowing by 7 percentage points (p-value 0.002), while the predicted probability increases by 2 percentage points for every additional hectare of cultivable land ($p < 0.01$) and for every additional sleeping room ($p = 0.103$). Households that have resided in the location for less than four years were 14 percentage points less likely to borrow ($p = 0.034$). Overall, this is consistent with ACSI/OCSSC lending preferentially to households more likely to be able to repay the loans and possibly more likely to offer collateral. One additional important observation is that households who had a non-farm business that started at least four years before the follow-up (that is, before the intervention) were six percentage points more likely to borrow ($p = 0.013$).

Respondents reported that a large majority of loans from ACSI/OCSSC were utilized for productive purposes. Of a total of 1,682 micro-loans at follow-up, 1,388 (83%) were reported as having been initiated to pay for 'working capital' or 'basic investment'. According to the interviewers' manuals, such categories encompassed production-related items such as wages for hired labor, rents for land and equipment, cost of seeds, fertilizers and pesticides, fees for veterinary services, purchase of animals, land, equipment etc. In contrast, only 25 loans were initiated to repay other loans,

and a total of 144 (9%) were used to pay for consumption, schooling or ceremonies. We also find that most loans were initiated to fund crop cultivation or animal husbandry, with 80 percent of the 1,388 loans used for working capital or investment in these sectors and only 235 (17 percent) used for 'trading and services'.[12]

3.2 Impact on Households' Economic Activities

Next, we turn to the analysis of impacts of ACSI/OCSSC operations on households' economic activities. Both baseline and follow-up surveys included information on sales as well as input purchases for farm and livestock activities and for non-farm self-employment businesses. No information was collected on family employment or home consumption, so we cannot estimate a measure of profit. We focus then on measures of 'net sales', calculated as differences between yearly revenues and input purchases. Expenditures and sales related to these activities were recorded with a 12-month recall period, see Appendix A.1 for details. The one-year reference period likely reduced concerns about seasonality, but it may have exacerbated recall errors that are common in context such as ours were record-keeping is rare.[13]

In panel A of Table 5, we show that none of the impacts on business activities (that is, non-farm self-employment) was significantly different from zero at standard levels, although some of the estimates are large in magnitude. A key result is that we find no evidence that micro-credit led to an increase in the fraction of households with a non-farm business or even a female-led non-farm business (columns 1 and 2). The impacts on the latter outcome is actually negative (-0.037), although not significant at standard levels. Similarly, we find no evidence that micro-loans increased the probability of starting new businesses. The treatment effect is again negative (-0.02) and not significant. When we look at business-related monetary outcomes we find that, while again not significant at standard levels, the impacts are large in magnitude, see columns 4 to 7. In particular, the impact on 'net revenues' is 805 Birr, that is, almost three times the initial average of the variable and about half of the mean value of all livestock owned at baseline in control areas. However, the large standard error implies that a 95% confidence interval is very wide, with a lower and upper bound of -398 and $2,008$ Birr respectively.

[12] The remaining 40 loans were used for hard to categorize agricultural and non-agricultural 'processing' and for 'production'.

[13] However, de Mel et al. (2009) find that in a sample of micro-enterprises in Sri Lanka reports were similar when using a 12-month recall as compared to the sum of monthly data collected four times during the year, although they also find evidence of mis-reporting with both methodologies.

In panel B we show estimated impacts on crop cultivation and livestock activities during the 12 months before the survey. Once again several estimates are noisy and thus not significant, although the null of no impact can be rejected at the 5 percent level for crop-related expenses and at the 10 percent level for livestock sales. The impact on crop-related expenditures (+241 Birr) is difficult to interpret because we do not have information on total quantities produced or consumed but only on amounts sold. The questionnaire, however, recorded the fraction of the production sold, and so we estimate net sales as the difference between total cash sales and total crop-related expenditures multiplied by the share sold, see Appendix A.1. The impact on total revenues is large, with a magnitude similar to that of total sales in control areas in 2003, but the standard error is twice as large and the null of no impact cannot be rejected. The estimate for net sales and land cultivated are similarly positive, but are not large and are again not significant. The last columns of panel B show some evidence that access to micro-loans increased the stock of animals owned and the value of their sales, although only the latter is significant, at the 10 percent level. Although not significant, the estimated impact on the value of livestock is large, at 239 Birr, almost 20 percent of the average value at baseline in untreated areas.

Next, we show that the introduction of micro-loans was not associated to substantive or statistically significant changes in income from wages or in transfers from any source (panel C, columns 1 and 2). The latter results in particular is interesting, because it suggests that the large increase in borrowing documented in Table 3 did not come at the expense of reduced transfers from any existing transfer network that pre-dated the program. This finding is also consistent with the earlier result that the introduction of micro-finance did not appear to crowd out existing borrowing from informal sources.

Finally, in columns 3 to 7 of panel C we find little evidence of impacts on labor supply of working-age adults, defined here as individuals of age 16-75, regardless of whether this is estimated for self-employment or outside activities. All estimates are close to zero in magnitude and not significant and this also holds true if we only look at women (columns 6 and 7), which may have been expected to increase labor supply if micro-loans favored the creation or expansion of women-led activities.[14]

Overall, then, we find that in our study areas the introduction of micro-lending left adult labor

[14]Note that the low numbers of hours worked on average at baseline in control areas are *unconditional* means that also include individuals who did not work, most of which were women. If we estimate this statistic using only individuals for whom work was the primary activity, the averages become 39 hours/week for men and 26 for women.

supply and the prevalence of non-farm self-employment activities largely unaffected, while there is some evidence of increases in livestock ownership and sales as well as in net sales from non-farm businesses. Most impacts are imprecisely estimated, though, so that all but two of the impacts are not statistically significant, and none is significant at the 1 percent level.[15] Despite this, it is interesting to note that the sum of the estimated impacts on all categories of 'production costs' is lower but not far from the impact on the amounts borrowed from micro-lending. The latter is 460 Birr (see Table 3) while the sum of impacts on expenses for non-farm business (112 Birr) and expenses for crop cultivation (241) is 353 Birr.[16] Although all estimates are noisy, this is thus consistent with most loans being initiated for productive purposes, as indicated by respondents.

3.3 Impacts on Child Schooling and Other Socio-economic Indicators

Next, in Panel A of table 6 we turn to the analysis of child schooling. Ex-ante, it is not clear that improvements in such indicator should have been expected. In fact, while income effects and reductions in credit constraints will *ceteris paribus* typically raise child schooling, the opposite effect may arise if access to credit increases sufficiently the returns to child labor (Wydick 1999).

In Ethiopia, public primary and secondary schools are nominally free, although associated costs such as textbooks and uniforms must be born by the families.[17] In rural areas it is common for children to start school late. At baseline, only 15 percent of 7-year old children were attending school, while the proportion increased monotonically to 54 percent at age 14 and declined afterwards. Such staggered start of school leads to the lack of clear jumps in enrollment at specific ages, and to a large variation in the age of children enrolled in the same grade. We thus analyze separately impacts for children 6 to 15 and for individuals 16-20. For each individual in the household roster, enumerators recorded years of school completion as well as school attendance at the time of the interview. Attendance was measured in a binary way, simply asking about whether the individual was 'currently attending school' while no information was collected on learning outcomes such as test scores. For individuals 10 or older, the questionnaire also included separate questions about

[15]The estimates in Appendix Table A.5 show that the presence of family planning programs had similarly mixed impacts on income-generating activities. The most notable results are that areas where FPPs were offered in addition to microcredit saw relatively *less* growth in non-farm business and crop-related activities and larger increases in livestock, although most coefficients are not significant at standard levels.

[16]If we use as dependent variable the sum of the two cost categories the estimate is again not significant at standard levels.

[17]See Appendix A.1 for additional details about schooling in Ethiopia.

time allocation, so we will also look at labor supply and domestic work among children 10-15.

In column 1, we see that the estimated program impact on schooling attendance of children 6-15 is positive but very small (0.023) and not significant. This compares to a very large increase in schooling attendance that took place in this age group in untreated areas, where the indicators increased from 37 to 54 percent.[18] When we look at time allocation among the subsect of these children for whom such data were collected (that is, children of age 10-15) we find similarly small and not significant impacts in the number of hours worked, regardless of whether we look at outside of self-employment activities. We find instead a 5 percentage points increase in the fraction of children for whom domestic chores was the primary activity. Such increase is large relative to the level at baseline in control areas (13 percent) but is estimated imprecisely and is only significant at the 10 percent level.[19] In addition, when we re-estimate the model in column 1 including only children in this age group, we find a small but not significant *increase* (0.015) in school attendance, suggesting that these children were not becoming more likely to drop out of school. In column 5 we also show that we do not find any evidence of increases in schooling among older cohorts.[20]

Next, we look at indicators of women's empowerment. When a woman of age 15-49 was present in a male-headed household (about 90 percent of households were, see Table 1), surveyors asked the woman about who in the household was involved in decision-making related to 20 different topics, ranging from children's health and education, to contraceptive use, to savings, to the woman's involvement in the labor market, see Appendix A.6 for details. We thus construct a measure of empowerment as the fraction of decisions where the woman stated she was involved, and a separate measure calculated taking into account only seven domains—such as savings decisions or labor market participation—that we categorized as being more clearly 'economic' in nature. Interviewers were instructed to complete the corresponding section of the questionnaire without the spouse being present during the interview, to enhance the truthfulness of the responses.

Both indicators show that in control areas women were on average involved in about 85 percent

[18]Such overall large increase in school attendance was broadly consistent with nationwide trends. For instance, data from the Ethiopia Demographic and Health Surveys (DHS) show that between 2000 and 2005 the percentage of 10-14 year old with some primary education increased from 37 to 55 percent among girls and from 47 to 58 percent among boys, see Macro International Inc. (2007).

[19]The increase is larger for girls (0.07) than for boys (0.03), but neither gender-specific impact is significant, results not shown.

[20]In treated areas that also saw the introduction of family planning programs we find relatively larger enrolment rates for both 6-15 and 16-20 age groups, although the estimates are not significant at standard levels, see Appendix Table A.6.

of decisions, suggesting a high degree of participation in the study areas. The estimates in columns 1 and 2 of panel B show that we find no evidence of improvements in either indicator. Both estimates are relatively small and actually negative (-0.07) although neither is statistically significant at standard levels. As we mentioned earlier, the lack of beneficial program impacts is perhaps not surprising given that, unlike what is often observed in many micro-finance institutions worldwide, ACSI/OCSSC did not lend exclusively or predominantly to women.[21]

In column 3 of panel B we also find no significant impact on the resale value of a non-exhaustive list of assets, which included radios, electric stoves, kerosene/pressure lamps, beds, tables, chairs, bicycles, motorcycles/scooters, cars and trucks. In column 4 we see that microcredit had barely any impact on the fraction of households where at least one member was 'seriously ill' during the previous three years (that is, in the period of time between baseline and follow-up surveys). This is of course an extremely coarse health indicator, and in addition it was only measured at follow-up, so in this case we apply 2SLS to a version of model (1) in levels.[22] Finally, in column 6 of panel B we see that microcredit was associated with an *increase* in the number of months of food insecurity. The estimate is large (0.7 months) and significant, although only at the 10 percent level. Given the coarse way this variable was measured it is hard to exclude that this result is spurious, also because the results on economic activities, while mostly not significant, suggested if anything some improvements in areas with increased access to credit.

4 Discussion and Conclusions

In this paper we have evaluated the impact on several socio-economic outcomes of the introduction of microfinance in rural areas of Amhara and Oromiya (Ethiopia) in the context of a clustered randomized field trial carried out between 2003 and 2006. Our results should be a useful addition to a still limited number of RCTs that evaluate the impact of *introducing* microloans in communities who had previously no access to it. Our empirical framework is perhaps closer to that in Crépon et al. (2011), who describe an RCT conducted in Morocco: the study area was rural and with

[21] The addition of family planning programs changed little to the impacts on women's empowerment. Somewhat surprisingly, the presence of FPPs only (that is, without micro-lending) is associated with substantively lower indicators, with impacts significant at the 10 percent level.

[22] The estimates are similarly negative but close to zero and not significant if we look at the probability of a child under six being seriously ill in the previous three years, or at health expenditures for serious illness during the same period (results not reported).

little access to credit at baseline, the interest rate charged by the micro-lenders was relatively low, loans were granted to both men and women and used mostly to fund crop production and livestock activities.

ACSI and OCSSC, the two micro-finance institutions MFIs involved in our evaluation, did not always comply with the experimental protocol so that actual and randomly assigned treatment coincide in only 78 percent of the 'peasant associations' (PAs) included in the study. Our estimates were thus obtained using 2SLS, using assigned treatment as instrument for the actual presence of microfinance and thus measure the intent-to-treat with microfinance only for the sub-population of 'complier communities', that is, excluding PAs where microcredit would have been introduced or not introduced anyway, regardless of treatment assignment. Three years after the start of the trial we estimate that the entry of the two MFIs increased the fraction of households with loans by 35 percentage points relative to control areas. These were very large increases, especially relative to pre-intervention borrowing rates of 13 percent in control areas.[23] The average loan size among borrowers was large, being close to 80% of the poverty line in Ethiopia in terms of adult consumption per year. We also do not find evidence that the micro-loans supplanted pre-existing sources of credit.

The magnitude of the increase in the prevalence of borrowing is large also when compared to other recent RCTs that evaluated impacts of microfinance in other locations. The intervention evaluated in Crépon et al. (2011) increased access to credit by 13 percentage points relative to control area, in Hyderabad (India) Banerjee et al. (2013) found that after about 1.5 years program areas saw 9 percentage points more borrowing from MFIs but also 5 percentage points less borrowing from informal sources, while credit usage increased by 12 percentage points in a Mexico-based RCT (Angelucci et al. 2013) and by 24 percentage points in Mongolia (Attanasio et al. 2013), although in this latter case the study population was composed of women who had expressed an interest in borrowing. Our respondents reported that a large majority of loans were used to fund production activities although, unlike in the most common narrative of microfinance, investment in non-farm small businesses played only a minor role, with most funds reported as being invested for crop cultivation and livestock-related activities.

Despite the remarkable increase in borrowing, we do not find unambigious evidence of widespread improvement in socio-economic indicators in treated areas. Of a total of 31 outcomes, including

[23] If we re-estimate model (1) using OLS, the ITT impacts on borrowing from any source remain very large, with $\hat{\beta}_{MF}$ equal to 0.21 or 0.25 depending on whether we use actual or randomly assigned treatment dummies as regressors.

input and output measures of income-generating activities, labor supply, child school attendance, indicators of women's empowerment and (coarse) indicators of health and food adequacy, only four are significant, and none of these signals a clear improvement in household welfare. The null of no impact can only be rejected for expenses for crop cultivation (at the 5 percent level), livestock sales (10 percent), fraction of children 10-15 for which housework is the primary activity (10 percent) and months of food insecurity (10 percent), all of which increased in treated relative to control areas. But given the very large number of outcomes analyzed, mere chance may have produced such rejections of the null hypothesis of no impacts even if the null had been true for all of them. Had we used more conservative, Bonferroni-type tests to evaluate significance in individual equations, the null of no impact would not have been rejected in most cases, with the major exception of the estimated impacts on borrowing behavior.[24]

Despite these arguments, the overall picture of the program impacts is more positive if we focus on the point estimates of a number of indicators, although we reiterate that for none of them the null of no impact can be rejected at standard levels. The introduction of microfinance was not associated with an increase in non-farm business activities, but revenues from such activities (both gross and net of estimated costs) increased substantially: the estimated impact on net revenues was 54 percent of the average value of livestock owned at baseline in control areas. That microfinance acted more on the intensive than on the extensive margin of small businesses is consistent with what found in other RCTs (Banerjee et al. 2013). We also find small but positive impacts on cash revenues from crops, land cultivated, livestock owned and livestock sold, although as we mentioned earlier the null of no impact can be rejected only for the last variable. Impacts on wage income, adult labor supply (including time spent working as self-employed) as well as on child schooling and time allocation are instead generally close to zero. In light of these overall positive impacts on income-generating activities the finding that food scarcity was *more* common in treated areas is surprising and perhaps it should be dismissed as a spurious results. Finally, like in Crépon et al. (2011) and Banerjee et al. (2013), we find that microfinance did not improve significantly indicators of women's empowerment (our point estimates actually indicate a small decline), but it should be recalled that in our as well as in these two other studies micro-lenders did not target exclusively women's borrowers. We also find relative improvements in school attendance of children 6-15 in treated areas but the changes were very small and not significant.

[24]For recent examples of program evaluations that take explicitly into account this multiple inference issue see for instance McKenzie et al. (2009) and Angelucci et al. (2013).

The time interval between pre and post-intervention surveys was relatively long, approximately three years. This is potentially important, because the literature has highlighted that the need for lumpy investments may actually *decrease* certain welfare indicators in the short term, before investments have paid off (see for instance Banerjee et al. 2013 or Fulford 2011). On the other hand, and unlike Banerjee et al. (2013), we do not have data from the *interim* period, so we cannot gauge to what extent the immediate impacts differed from those observed at endline.

An additional shortcoming of our study is that, although households from the same villages were surveyed before and after treatment, it was not the same households that were surveyed. We thus have a panel of villages but not of households. Having baseline data is still useful because it allows us to gauge to what extent observed household characteristics were balanced across different experimental arms at baseline, and also allows us to control in the estimation for any time-invariant community-level confounder. However, the lack of baseline data limits severely our ability to estimate heterogeneous treatment effects. This is potentially very important, because the existence of heterogeneous impacts is a common theme among RCTs evaluating other microfinance programs. A related limitation of our study is that consumption data were not collected, so we cannot verify whether the lack of clear impacts on this variable observed in other studies also occurred in our study areas.

A final but crucial cautionary note is that the failure to identify statistically significant impacts on key outcomes such as net revenues or livestock ownership may also have been the result of measurement error or insufficient statistical power.[25] Revenues and costs are notoriously difficult to measure in household surveys (de Mel et al. 2009) and measurement error of a non-binary dependent variable, while not causing estimation bias under certain conditions, will increase the standard errors of the estimates. The figures in Table 1 clearly show that standard deviations were large for outcomes expressed in monetary terms. For instance, mean revenues from non-farm business in control areas at baseline was 310 Birr with a standard deviation about 20 times as large, while the mean value of livestock was 1,502 Birr, with a standard deviation of 2,021 and a very high intra-PA correlation of about 0.20. The point estimates for the DD in Table 5 show impacts of about 240 Birr, which correspond to a small effect size of about 0.12. Under such scenario, and taking into account a total of 60 households in each of 60 clusters, the probability of

[25]Recall that the data used in this paper come from a randomized controlled trial whose primary purpose was the evaluation of FPPs and micro-loans on contraceptive choices. The power calculations were thus conducted in relation to such fertility-related outcomes.

rejecting the null of no impact using a 10 significance level would be only 26 percent.[26] The fact that we used 2SLS to address potential endogeneity concerns raised by imperfect compliance with the experimental protocol imposed further costs on power, although the first stage is very strong.

While these caveats need to be kept in mind, our results are overall consistent with the broad framework described in the survey by Banerjee (2013), with increased access to microfinance associated with some improvements in living standards of beneficiary communities, but without compelling evidence of a true transformative power of microfinance. Our study thus provides additional power to the cautionary note sounded early on by Morduch (1999) in relation to the potential of microfinance as a development tool against poverty.

[26] We calculated power using the Optimal Design software, version 1.56.

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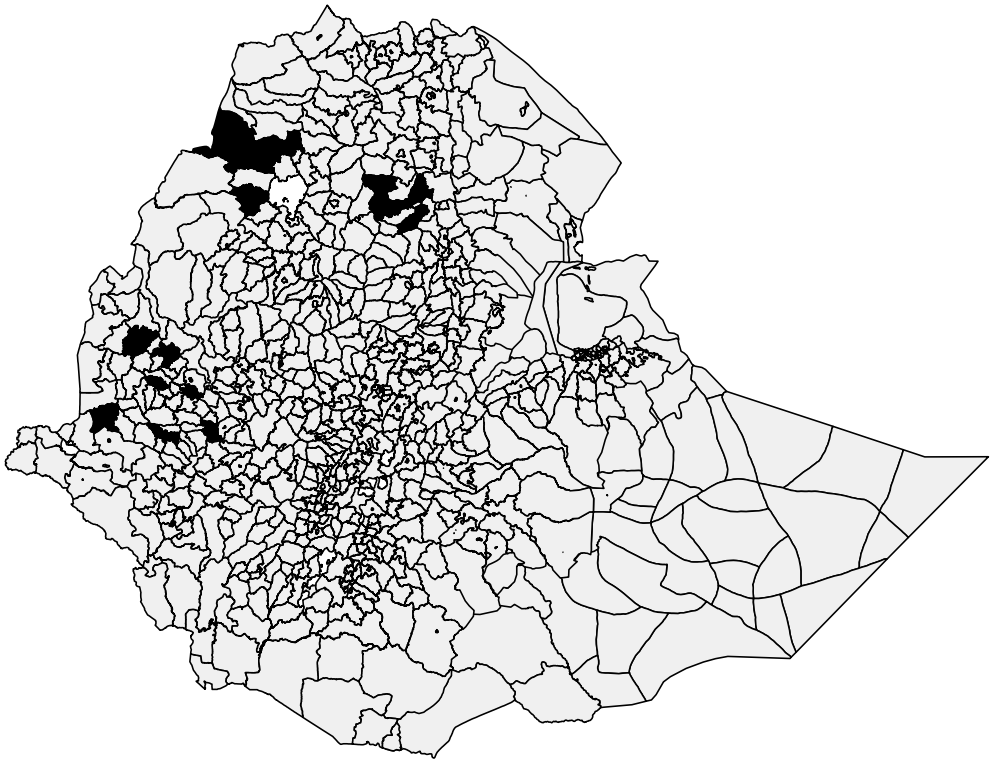
Figure 1: Study Areas



Source: Geo-spatial data from http://maps.worldbank.org/. Each contour represents an administrative unit called a *woreda*. The woredas where study PAs were located are shown in black. The northern-most woredas are those in Amhara and the southern-most ones are those in Oromiya.

Table 1: Baseline Summary Statistics and Tests of Balance

	(1)	(2)	(3)	(4)
	Assigned Control ($T = 0$)		Assigned Treatment − Control	
	Mean	St. Dev.	Coeff.	p-value
Household composition				
# members	5.22	2.14	-0.046	0.750
# adults ($\geq$ 16 years old)	2.43	1.01	0.040	0.522
# children ($<$ 16 years old)	2.79	1.78	-0.086	0.490
Male head	0.873	0.333	-0.003	0.857
Head age	40.9	13	-0.556	0.363
Head with no education	0.734	0.442	-0.019	0.697
Access to credit:				
Loan from RCA	0.021	0.142	-0.006	0.208
Loan from other MFI	0.005	0.0737	-0.002	0.547
Loan from a Banks and cooperatives	0.026	0.16	0.000	0.951
Informal loan	0.076	0.264	-0.012	0.592
Any type of loan	0.131	0.337	-0.011	0.623
Any type of loans initiated by a woman	0.017	0.13	0.001	0.882
Amount borrowed from (in 2006 Birr):				
Loans from RCA	11.0	94.8	-3.260	0.377
Other MFI	1.4	26.0	-0.522	0.570
Banks and Cooperatives	8.4	79.2	0.199	0.957
Informal loan	14.9	79.2	1.210	0.801
Total	36.6	149.0	-0.488	0.951
Loans initiated by woman	7.3	85.6	-3.790	0.372
Income-generating activities				
Agriculture is main economic activity of household	0.855	0.352	-0.0185	0.369
Total revenue from crop sales last 12 months	203	650	-38.3	0.485
Total expenditure for crops last 12 months	89.9	977	-10.3	0.715
# of large animals owned	2.84	5.37	-0.374	0.117
Total value of livestock	1502	2021	-122	0.426
Total revenues from livestock sales last 12 months	160	423	12.8	0.714
Total sales from non-farm self-employment last 12 m.	310	6804	-147	0.318
Total costs for non-farm self-employment last 12 m.	17.2	144	6.51	0.497
# of non-farm self-employment activities	0.108	0.333	0.017	0.591
# of non-farm self-empl. act. managed by women	0.042	0.212	0.011	0.488
Transfers in cash or kind last 12 months	115	443	28.2	0.454
Income from wages last 12 months	174	1100	-0.709	0.990
Other Indicators of Socio-Economic Status				
Total value of selected assets†	36.9	62.7	3.57	0.564
Surface water as main source for drinking	0.264	0.441	0.091	*0.093
Number of months of insufficient food in a typical year	2.4	1.9	-0.2	0.224
Distance to nearest health facility (minutes)	89.7	90.6	11.5	0.507
Distance to nearest market (minutes)	79.0	68.6	4.6	0.661

Notes: Data from baseline (2003) survey. All statistics are calculated using sampling weights. The figures in the last column are p-values for individual tests of equality of means between treatment and control areas, robust to intra-PA correlation. Asterisks denote statistical significance at the 10(*), 5(**) or 1(***) percent level. All figures expressing monetary values have been converted into 2006 Birr using region-specific consumer price indexes (CPIs) constructed by the Central Statistical Agency of Ethiopia. In Amhara, the CPI increased from 114.6 in January-May 2003 to 158.1 in March-July 2006 (a 38% increase), while in Oromiya the increase was from 122.8 to 156.8 (a 28% increase). The PPP exchange rate according to the latest World Bank figures is 2.25 Birr/1USD (World Bank 2008). † Estimated from the resale value of the following items owned by the household: radio, electric stove, lamps, beds, tables and chairs, bicycles, motorcycles, cars and trucks.

Table 2: Predictors of Actual Treatment Status

Dependent Variable: Treatment = 1	Coefficient	s.e.
Randomly assigned to treatment	0.647	0.070***
# adults in households	0.009	0.012
# children in households	0.004	0.005
Household head is male	-0.001	0.022
Age of household head	-0.001	0.001
Household head has no formal education	-0.025	0.031
Household has loans from banks/cooperatives	0.010	0.042
Household has loans from informal lenders	-0.165	0.046***
At least one loan initiated by a woman	-0.056	0.045
Agriculture is main economic activity of household	-0.046	0.028*
Total revenue from crop sales last 12 months	0.002	0.001
Total expenditure for crops last 12 months	0.000	0.001
# of large animals owned	0.003	0.002*
Total value of livestock	0.001	0.001
Total revenues from livestock sales last 12 months	-0.001	0.001
Total sales from non-farm self-employment last 12 m.	0.000	0.000
Total costs for non-farm self-employment last 12 m.	0.002	0.002
# of non-farm self-employment activities	-0.041	0.041
# of non-farm self-employment activities	0.027	0.034
Transfers in cash or kind last 12 months	-0.004	0.002***
Income from wages last 12 months	-0.002	0.001**
Total value of selected assets	0.002	0.004
Surface water as main source for drinking	-0.037	0.032
# of months of insufficient food	-0.008	0.007
Distance to nearest market (minutes)	0.001	0.000
Constant	0.268	0.072***
Observations	6410	
R-squared	0.46	
P-value, H_0 : all slopes equal to zero	< 0.001	

Notes: Data from baseline (2003) survey. The figures are regression coefficients from an OLS regression of a dummy for actual treatment status on a dummy for the randomly assigned treatment status and a series of predictors. Standard errors (in brackets) and tests are robust to intra-PA correlation. All statistics are calculated using sampling weights. Asterisks denote statistical significance at the 10(*), 5(**) or 1(***) percent level. All figures expressing monetary values are quartic roots of values in 2006 Birr.

Table 3: Impacts on Borrowing

	(1) ACSI & OCSSC	(2) NGOs	(3) Banks & Cooperatives	(4) Informal Sources	(5) All sources	(6) All sources Women
Panel A. Credit access	Any loan from					
Treated village	0.357***	0.042	0.0301	-0.0149	0.354***	0.105***
	(0.0932)	(0.0507)	(0.0599)	(0.0339)	(0.0984)	(0.0349)
Observations	12687	12687	12687	12687	12687	12687
Control mean	0.0198	0.00663	0.0239	0.101	0.161	0.0245
Panel B. Loan amounts	Loan amounts (in 2006 Birr)					
Treated village	460***	36.6	-44.2	5.33	451***	136***
	(120)	(48.2)	(104)	(12.5)	(159)	(75.8)
Observations	12687	12687	12687	12687	12687	12687
Control mean	10.5	1.67	8.7	19.4	43	8.83

Notes: Data from 2003 and 2006 surveys. Standard errors (in brackets) and tests are robust to intra-PA correlation. 2SLS Estimates of β_{MF} in model (1) in the text. The dependent variables in panel A are defined as follows: a dummy for whether the household had an outstanding loan from ACSI/OCSSC (column 1), or from NGOs (2), or from a bank or cooperative (3), or from informal sources such as money lenders or other individuals (4), or from any sources (5), or if a woman in the household had a loan from any source (6). The dependent variables in panel B are the amounts corresponding to the loans defined in the column headers. The control means reported at the bottom of each panel are calculated for non-treated areas ($T = 0$) at baseline. All statistics are calculated using sampling weights. Asterisks denote statistical significance at the 10(*), 5(**) or 1(***) percent level. All figures expressing monetary values are in 2006 Birr. The PPP exchange rate according to the latest World Bank figures is 2.25 Birr/1USD (World Bank 2008).

Table 4: Predictors of Borrowing

Household head is male	0.0656	(0.0221)***
Age of household head	-0.0004	(0.0006)
Head has completed at least primary schooling	-0.0040	(0.0278)
Head has no formal schooling	-0.0664	(0.0202)***
# Adults in household	0.0091	(0.0071)
# 6-15 year old children in household	0.0220	(0.0055)***
# children below age 6 in household	0.0199	(0.0083)**
Household has resided in village for less than 4 years	-0.1384	(0.0642)**
Cultivable land owned (Hectares)	0.0205	(0.0042)***
# Sleeping Rooms in dwelling	0.0239	(0.0145)
Household has non-farm business more than 3 years old	0.0622	(0.0245)**
Intercept	0.1119	(0.0416)

Notes: Data from treated areas only ($T = 1$), 2006 (post-intervention) surveys. Standard errors (in parenthesis) are robust to intra-PA correlation. The figures correspond to OLS estimates with sampling weights of a linear probability model where the dependent variable is a dummy = 1 if the household has an outstanding micro-loan at follow-up. The regression also includes PA fixed effects. None of the predicted probabilities of borrowing lie outside the unit interval (min = 0.034, max = 0.808, mean= 0.268). Sample size $n = 3,528$, with 23 observations ($< 1\%$)) dropped because of missing values in one or more of the predictors. Asterisks denote statistical significance at the 10(*), 5(**) or 1(***) percent level.

Table 5: Impacts on Income Generating Activities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. *Self-employment activities*							
	Has non-farm Business	Has female-led non-farm Business	Started business last 3 years	Revenues last 12 months	Investment last 12 months	All expenses last 12 months	Net Revenues last 12 months
Treated village	0.009	-0.037	-0.020	918	16.9	112	805
	(0.064)	(0.049)	(0.024)	(610)	(15)	(184)	(614)
Observations	12687	12687	12687	12687	12687	12687	12687
Control mean	0.115	0.05	0.03	351	1.28	18.6	332
B. *Crop Cultivation and Animal Husbandry*							
	Cash revenues from crops	Expenses for crop cultivation last 12 m	Net revenues from crop last 12 m	Land cultivated last 12 m (hectares)	Value of livestock owned	Value of large animals owned	Livestock sales last 12 m
Treated village	145	241**	51	0.157	239	225	112*
	(313)	(106)	(256)	(0.217)	(389)	(365)	(60)
Observations	12687	12687	12687	12348	12687	12687	12687
Control mean	159	81.6	147	0.842	1318	1177	155
C. *Wages, Transfers and Labor Supply*							
			Hours work / week, Household members 16-75				
	Wages last 12 m	Transfers last 12 m	All adults All Activities	All adults Self employment	All adults Outside employment	Women Self employment	Women Outside employment
Treated village	71	-6	1.9	1.67	0.23	-1.52	1.36
	(127)	(30)	(2.17)	(1.88)	(1.01)	(1.54)	(1.53)
Observations	12676	12687	31769	31769	31769	16051	16051
Control mean	192	32	22.1	19.6	2.57	7.37	2.82

Notes: Data from 2003 and 2006 surveys. Standard errors (in parenthesis) are robust to intra-PA correlation. 2SLS estimates of model (1) in the text. All regressions use sampling weights. Asterisks denote statistical significance at the 10(*), 5(**) or 1(***) percent level. The control means at the bottom of each panel are calculated for non-treated areas at baseline ($T = 0$). Net revenues from crop cultivations are estimated as the difference between revenues and the fraction of costs imputed for the crop share sold by the household, see Appendix A.1 for details. All figures expressing monetary values are in 2006 Birr. The PPP exchange rate according to the latest World Bank figures is 2.25 Birr/1USD (World Bank 2008). In column 5 of panel A, 'investment' refers to expenditures in 'equipment, machinery, assets' for non-farm businesses. The figures in column 6 ('all expenses') add to such expenditure the amounts spent for hired labor, raw material, transport, storage and 'other items'.

Table 6: Impacts on Child Schooling and Other Socio-economic Indicators

	(1)	(2)	(3)	(4)	(5)
Panel A	School attendance and time allocation of children				
	% of children 6-15 in school	Average hours/week worked by children 10-15		% of 10-15 children for whom housework is primary activity	% of 16-20 in school
		Self employment	Outside activities		
Treated village	0.023	-0.2	0.2	0.053*	-0.0005
	(0.064)	(2.5)	(0.9)	(0.032)	(0.043)
Observations	22071	11774	11774	11959	7234
Control mean	0.365	16.6	1.3	0.128	0.283
Panel B	Other indicators				
	Empowerment: % decisions with woman's involvement		Value of selected assets	Someone seriously ill last 3 years	# Months of food insecurity
	All	Economic			
Treated village	-0.071	-0.070	-3	-0.018	0.735*
	(0.060)	(0.065)	(18.6)	(0.052)	(0.42)
Observations	10500	10497	12676	6264	12676
Control mean	0.848	0.851	34.7	0.47	2.51

Notes: Data from 2003 and 2006 surveys. Standard errors (in parenthesis) are robust to intra-PA correlation. 2SLS Estimates of model (1) in the text, except in column 4, Panel B, because the dependent variable was measured only at follow-up. All regressions use sampling weights. The unit of observation is a child in all regressions of panel A, a woman in columns 1 and 2 or Panel B, and a household in columns 3 to 5 of Panel B. Asterisks denote statistical significance at the 10(*), 5(**) or 1(***) percent level. The control means reported at the bottom of the table are calculated for non-treated areas at baseline ($T = 0$), except in column 4, Panel B, where the mean was calculated at follow-up.

A Appendix

A.1 Detailed Description of Outcomes

Outstanding Loans: A household is labeled as having an outstanding loan if any household member owes money or goods to anyone at the time of the interview, or if such loans existed in the 12 months before the interview. At the time of the follow up survey, 440 of 4,164 households (10.6%) with no current outstanding loans had extinguished loans in the previous 12 months. Women's borrowing is identified by specific questions about the individual who contracted the loan.

Revenues and Costs from Non-farm Self-employment: These activities include "non agricultural enterprise which produces goods or services (for example, artisan, metalworking, tailoring, repair work; also include processing and selling your outputs from your own crops if done regularly)", shops or trading business. Respondents were asked to report for how many days/weeks/months the business operated in the previous 12 months, and the total revenues from sales per unit of time. Total monetary costs incurred during the previous 12 months were also recorded separately for hired labor, raw materials, equipment/machinery, transport/packing/storage and other items. As an example, suppose that a business operated for 3 months, with weekly earnings of 100 Birr/week, and with total costs of 500 Birr. Then we estimate 'net revenues' to be equal to 700 Birr ($= 100 \times 4 \times 3 - 500$). Separate information was collected for each existing business separately. The survey did not collect information on family labor or self-consumption of goods produced by the business.

Animal Husbandry: The value of animals owned is derived from questions about the expected revenues from their hypothetical sale at the time of the interview. The value of sales (in Birr) is the total revenue from actual sales of animals over the previous year. Separate information was collected for each animal type separately (types included cows, oxen, calves, bulls, camels, horses, donkeys, mules, sheep, goats, chicken and 'others'). No information was collected about costs for hired labor, veterinarian services, feed, etc. so for this economic activity we cannot calculate a value for 'net sales'.

Revenues and Costs from Crop Cultivation: Information was collected separately for each crop type (the principal crops were wheat, barley, teff, maize, sorghum/millet, beans and—in Oromiya—coffee. For each crop, the questionnaire recorded the total revenues from sales (in Birr) over the last 12 months, the share of the total crop sold, and the total amount of expenses incurred for cultivation and sales. We calculate net revenues from sales as the difference between revenues and the corresponding imputed costs, estimated as total costs multiplied by the fraction of the crop sold. So, for instance, if a household incurred a total cost of 500 Birr for cultivation and sold 50 percent of the quantity produced for a total of 300 Birr, net crop sales are calculated as $300 - 0.50 \times 500 = 50$ Birr.

Other Sources of Income: Income from wages is reported as total earnings (in Birr) in monetary or in-kind terms, for work conducted for someone else over the previous 12 months.

Hours of Work: For each individual of age 10 and above, the survey recorded the two most important activities the individual was involved in during the past 12 months. For each activity, records were then taken about the number of weeks spent in such activity, the number of days usually spent per week as well as the number of hours spent per day. Hours were counted as 'work'

if they were related to one of the following activities: crop cultivation, care of livestock, fishing, mining, manufacture and processing, retail and wholesale trade, finance, public administration, education, health, and social services or other services. Hours spent in school or in domestic work were listed separately.

A.2 Schooling in Ethiopia

Primary school covers grades 1 to 8, and by the end of eighth grade pupils must pass a national examination before they are allowed to start secondary school (grades 9 and 10). Students that pass another national examination at the end of the 10th grade are allowed to enroll in two years of 'preparatory' school (grades 11 and 12) and those who also pass a 12th grade exam are eligible to enroll in public universities.[27]

A.3 Indicators of Women's Empowerment

When a woman of age 15-49 was present in a male-headed household (almost 90 percent of households were male-headed, see Table 1), interviewers asked the woman to list all members involved in decision-making related to 20 different issues. Such issues included the following: 1. Food eaten at home; 2. Routine purchases for household items such as cleaning supplies; 3. The woman's own clothes; 4. The clothes of the woman's spouse; 5. Children's clothes; 6. Children's education (to attend, and then continue); 7. The woman's health; 8. The health of the spouse; 9. Children's health; 10. Large expensive purchases for the household; 11. Giving money to the woman's parents/family; 12. Giving money to the spouse's parents/family; 13. Gifts for special occasions; 14. Monthly savings; 15. Sale of cattle; 16. Time the woman spends socializing; 17. Whether the woman works outside the household; 18. Number of children to have; 19. Contraceptive use; 20. When daughters can marry.

We construct two indicators of empowerment. The first is the fraction of domains for which the woman is included as one of the decision-makers, while the second is calculated in the same way but including only domains with a more distinctly 'economic' content, which we roughly identify to be items 10-15 and 17 in the list above. In several instances a specific decision was not relevant for the household, in which case the decision was not considered in calculating the indicator. For instance, in a family with no children nor cattle, items 5, 6, 9, 15 and 20 would not be relevant, so the indicator would be calculated as the fraction out of 15 decisions where the woman was involved as a decision-maker.

[27] See Section P of the 2008 Statistical Abstract of Ethiopia, http://www.csa.gov.et/surveys/Nationalstatistics/nationalstatistics2008/SectionP-Education.pdf.

A.4 Impacts on Borrowing, Complete Results and Alternative Specifications

Panel A. Credit access	(1)	(2)	(3)	(4)	(5)	(6)
	Any loan from					
	ACSI & OCSSC	NGOs	Banks & Cooperatives	Informal Sources	All sources	All sources Women
	PA Fixed Effects with FP-treatment controls					
Treated	0.357***	0.042	0.030	-0.015	0.354***	0.105**
	(0.093)	(0.051)	(0.060)	(0.034)	(0.098)	(0.035)
Family Planning (no microloans)	-0.041	0.018	0.007	0.019	-0.016	-0.020
	(0.075)	(0.028)	(0.050)	(0.041)	(0.087)	(0.045)
Treated × Family Planning	-0.134	-0.042	-0.025	0.045	-0.106	0.006
	(0.110)	(0.046)	(0.053)	(0.033)	(0.096)	(0.042)
	PA Fixed Effects					
Treated	0.313***	0.0125	0.0146	-0.00319	0.31***	0.119***
	(0.066)	(0.027)	(0.038)	(0.027)	(0.066)	(0.030)
	Village Fixed Effects					
Treated	0.302***	0.009	0.016	-0.023	0.291***	0.120***
	(0.063)	(0.027)	(0.036)	(0.016)	(0.068)	(0.030)
Panel B. Loan amounts	Loan amounts (in 2006 Birr)					
	ACSI & OCSSC	NGOs	Banks & Cooperatives	Informal Sources	All sources	All sources Women
	PA Fixed Effects with FP-treatment controls					
Treated	460***	37	-44	5	451***	136*
	(120)	(48)	(104)	(13)	(159)	(76)
Family Planning (no microloans)	-223	11	-184	21	-382	-178
	(225)	(23)	(173)	(18)	(372)	(162)
Treated × Family Planning	124	-46	216	1	305	212
	(303)	(44)	(279)	(13)	(549)	(230)
	PA Fixed Effects					
Treated	635***	8.8	155	-5.05	795**	330**
	(184)	(25)	(161)	(11)	(325)	(135)
	Village Fixed Effects					
Treated	626***	8.05	159	-3.1	795**	324**
	(183)	(26)	(161)	(9)	(326)	(134)

Notes: Data from 2003 and 2006 surveys. Standard errors (in parenthesis) are robust to intra-PA correlation. The dependent variables are defined as in Table 3. All estimates are obtained from 2SLS. The base estimates are those labeled "PA Fixed Effects with FP-treatment controls", corresponding to model (1) in the text. For these results, the estimates in the row labeled 'Treated' are identical to those in Table 3, while those in the rows below show the results for $\hat{\beta}_{FP}$ and $\hat{\beta}_{Both}$. The estimates labeled "PA Fixed Effects" are obtained ignoring the presence of Family Planning Programs, that is, imposing $\beta_{FP} = \beta_{Both} = 0$ in model (1), with PA fixed effects or with village fixed effects. Asterisks denote statistical significance at the 10(*), 5(**) or 1(***) percent level.

A.5 Impacts on Income-generating Activities, Complete Results and Alternative Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. *Self-employment activities*							
	Has non-farm Business	Has female-led non-farm Business	Started business last 12 months	Revenues last 12 months	Investment last 12 months	All expenses last 12 months	Net Revenues last 12 months
	PA Fixed Effects with FP-treatment controls						
Treated	0.009	-0.037	-0.020	918	16.9	112	805
	(0.064)	(0.049)	(0.024)	(610)	(15)	(184)	(614)
Family Pl. (no microloans)	0.030	0.031	0.008	63.4	-1.82	114	-51.1
	(0.078)	(0.066)	(0.027)	(398)	(5)	(242)	(352)
Treated × Family Pl.	-0.080	0.019	-0.019	-879	-18.6	-257*	-622
	(0.077)	(0.047)	(0.027)	(539)	(14)	(137)	(551)
	No FP-treatment controls						
Treated	-0.045	-0.044	-0.034*	460	8.84	-70.3	531
	(0.058)	(0.043)	(0.020)	(372)	(8)	(156)	(350)
B. *Crop Cultivation and Animal Husbandry*							
	Cash revenues from crops	Expenses for crop cultivation last 12 m	Net revenues from crop last 12 m	Land cultivated last 12 m	Value of livestock owned	Value of large animals owned	Livestock sales last 12 m
	PA Fixed Effects with FP-treatment controls						
Treated	145	241**	51	0.157	239	225	112*
	(313)	(106)	(256)	-0.217	(389)	(365)	(60)
Family Pl. (no microloans)	-202	-74	-185	0.0892	269	300	154
	(237)	(98)	(206)	(0.242)	(750)	(705)	(179)
Treated × Family Pl.	-397	-220**	-285	-0.24	284	307	-31
	(240)	(101)	(197)	(0.204)	(438)	(444)	(93)
	No FP-treatment controls						
Treated	57	172**	9	-0.005	238	219	19
	(187)	(77)	(157)	(0.156)	(433)	(416)	(95)
C. *Wages, Transfers and Labor Supply*							
			Hours work / week, Household members 16-75				
	Wages last 12 m	Transfers last 12 m	All adults All Activities	All adults Self employment	All adults Outside employment	Women Self employment	Women Outside employment
	PA Fixed Effects with FP-treatment controls						
Treated	71	-6	1.90	1.67	0.23	-1.52	1.36
	(127)	(30)	(2.17)	(1.88)	(1.01)	(1.54)	(1.53)
Family Pl. (no microloans)	162	30	-0.32	-1.81	1.49	-1.80	3.15**
	(141)	(33)	(2.19)	(2.61)	(1.24)	(1.81)	(1.56)
Treated × Family Pl.	-10	-41	-2.93	-2.88	-0.05	0.10	0.00
	(107)	(50)	(1.91)	(1.84)	(0.95)	(1.17)	(1.29)
	No FP-treatment controls						
Treated	-17	-41	0.72	1.24	-0.53	-0.585	-0.189
	(100)	(30)	(1.50)	(1.66)	(0.87)	(1.16)	(1.20)

Notes: Data from 2003 and 2006 surveys. Standard errors (in parenthesis) are robust to intra-PA correlation. All estimates are obtained from 2SLS. The base estimates are those labeled "PA Fixed Effects with FP-treatment controls", corresponding to model (1) in the text. For these results, the estimates in the row labeled 'Treated' are identical to those in Table 5, while those in the rows below show the results for $\hat{\beta}_{FP}$ and $\hat{\beta}_{Both}$. The estimates labeled "PA Fixed Effects" are obtained ignoring the presence of Family Planning Programs, that is, imposing $\beta_{FP} = \beta_{Both} = 0$ in model (1), with PA fixed effects. In column 5 of panel A, 'investment' refers to expenditures in 'equipment, machinery, assets' for non-farm businesses. The figures in column 6 ('all expenses') add to such expenditure the amounts spent for hired labor, raw material, transport, storage and 'other items'. Asterisks denote statistical significance at the 10(*), 5(**) or 1(***) percent level.

A.6 Impacts on Child Schooling and Other Socio-economic Indicators, Complete Results and Alternative Specifications

	(1)	(2)	(3)	(4)	(5)
	% of children 6-15 in school	Average hours/week worked by child 10-15		% of 10-15 children for whom housework is primary activity	% of 16-20 in school
		Self employment	Outside activities		
	PA Fixed Effects with FP-treatment controls				
Treated	0.023	-0.2	0.2	0.053*	-0.0005
	(0.064)	(2.5)	(0.9)	(0.032)	(0.043)
Family Pl. (no microloans)	0.054	-4.7	1.5	-0.024	0.027
	(0.072)	(5.0)	(1.0)	(0.041)	(0.054)
Treated × Family Pl.	0.052	-3.5	-0.8	-0.039	0.077
	(0.052)	(3.0)	(0.8)	(0.029)	(0.050)
	No FP-treatment controls				
Treated	0.020	0.5	-0.9	0.047*	0.019
	(0.047)	(3.2)	(0.7)	(0.026)	(0.040)
	Empowerment: % decisions with woman's involvement		Value of selected assets	Someone seriously ill last 3 years	# Months of food insecurity
	All	Economic			
	PA Fixed Effects with FP-treatment controls				
Treated	-0.071	-0.070	-3	-0.018	0.735*
	(0.060)	(0.065)	(18.6)	(0.052)	(0.42)
Family Pl. (no microloans)	-0.123*	-0.178*	-24.1	0.059	-0.0258
	(0.074)	(0.098)	(18.8)	(0.046)	(0.39)
Treated × Family Pl.	0.029	0.048	-24.5	0.004	-0.227
	(0.062)	(0.074)	(19.1)	(0.050)	(0.44)
	No FP-treatment controls				
Treated	0.006	0.045	-2.39	-0.047	12676
	(0.056)	(0.073)	(15.1)	(0.038)	2.51

Notes: Data from 2003 and 2006 surveys. Standard errors (in parenthesis) are robust to intra-PA correlation. All estimates are obtained from 2SLS. The base estimates are those labeled "PA Fixed Effects with FP-treatment controls", corresponding to model (1) in the text. For these results, the estimates in the row labeled 'Treated' are identical to those in Table 6, while those in the rows below show the results for $\hat{\beta}_{FP}$ and $\hat{\beta}_{Both}$. The estimates labeled "PA Fixed Effects" are obtained ignoring the presence of Family Planning Programs, that is, imposing $\beta_{FP} = \beta_{Both} = 0$ in model (1), with PA fixed effects. Asterisks denote statistical significance at the 10(*), 5(**) or 1(***) percent level.