Demasking the impact of microfinance

Helke Waelde*

November 9, 2011

Abstract

We reconsider data from a randomized control trial study in India. The data reveal the impact of a microloan program. We extend the often used randomized impact evaluation and difference-in-difference approach by quantile regression and the consideration of the quantile treatment effects. The use of additional, more advanced, evaluation methods allows a more detailed consideration of borrowers at the lower and at the upper end of the wealth distribution. We find a strong negative and significant time-trend. Furthermore, we observe a negative impact of the provision of microfinance loans such that the overall impact is even more negative. This is particularly well seen for entrepreneurs in the lower and in the higher quantiles. As we learn that poor entrepreneurs use microloans for consumption, we doubt that microfinance is the right instrument for them. The data suggest that providing microloans for average entrepreneurs, who can hire very poor entrepreneurs, might be an effective solution for that dilemma.

*Gutenberg School of Management and Economics, University of Mainz, Jakob-Welder-Weg 4, 55128 Mainz, Germany, helke.waelde@uni-mainz.de, www.helke.waelde.com, Phone +49.6131.39-23969, Fax +49.6131.39-25053. We would like to thank Amelie Wuppermann for helpful comments and the Johannes Gutenberg-University for the financial support of this paper by a research grant.

1 Introduction

The evaluation of treatments is very important and fundamental for the treated and the not treated. In some cases, especially in the field of medicine, it could be life saving or life destroying. To evaluate the effect of a treatment, we have to apply the treatment, a pill or something similar, to a group of randomly chosen patients, i.e. the treatment group. In a perfect setup, we would observe the outcome, the wellbeing of the patient, and turn back time. Then we observe the same patient when she does not receive the treatment. After that we would know whether the pill was effective or not. Unfortunately, we are not able to turn back time. Instead of that we match similar patients and choose at random which one of them will receive the treatment and which will not. The patients are not aware of the fact that they did not receive the medicine as they received placebos. As psychological aspects play an important role in the wellbeing of people, the assumption of random application of the treatment is essential. Beside the huge insights that scientists can learn from such an evaluation, we have ethical concerns about this method. How could we randomly decide who is worth the treatment and who is not? Are we eligible to make such decision over other people's live?

However, this method is also applied in other disciplines aside from medicine. In the development context, we try to evaluate development aid programs with the treatment evaluation approach. Goldstein and Karlan (2007) and Goldberg and Karlan (2008) provide an overview of the evaluation of impact for microfinance. Ideally, we match some pairs of similarly poor entrepreneurs and randomly offer one of them a microloan. The other poor entrepreneur does not receive an offer and continues life without a microloan. This must not necessarily mean that this person does not receive any loan as there is a huge mass of informal finance in developing countries.

The first problem arises in the assumption of the randomly distributed treatment. In microfinance the treatment is often loans that are given to groups. In many studies, the borrowers form group themselves. We call this mechanism assortative matching, firstly described by Ghatak (1990). Thus, we are not able to randomly choose one target person for a loan. To solve this problem, we could compare villages. In some villages we could introduce microfinance, in others not. But nevertheless, applying the treatment to randomly chosen villages is not always possible. Microfinance institutions have limited resources in financial and personal terms. Furthermore, we still face the ethical concerns mentioned above. To summarize, we see that the treatment is not applied to the same person twice, the application is most of the time not random, and looking at longitude data, control units often become treated units during the survey time. Therefore, we see that we have a problem with control groups in microfinance as the results might be biased in various ways.

There are many data sets with microdata available for development issues and they are becoming more and more prevalent. Unfortunately, most of the data lack on a long-term control group like the World Bank data set used by Khandker (2005), the USAID data considered by Barnes et al. (2001) and the IFPRI data studied by Behrman (2010). An unbiased consideration is hard to defend.

Therefore, we ask: Is there a way to evaluate the effect of a treatment more effectively? We would like to apply different methods which are commonly used in economics for evaluating a microfinance program in India. We start with (i) the simple randomized impact evaluation and (ii) the difference-in-difference approach by Ashenfelter (1978) and Ashenfelter and Card (1985). Furthermore, we extend the scope of the methods by (iii) the quantile regression (Koenker and Basset, 1978) and (iv) the quantile treatment effects (Firpo, 2007).

We use data from the IFRM-Centre of Microfinance[1] which were also used by Banerjee et al. (2009). The outstanding characteristic of this data is that the control group remains untreated over time[2]. Another much appreciated characteristic of this data set is that the application of the program was random. Thus, we do not have to handle very harsh biases. We consider the expenditure of households depending on their participation in the program. We would like to quantify coefficients and compare their robustness. To achieve deeper insights we decompose the results into coefficients coming from private expenditure and coefficients coming from business expenditure.

We find a strong negative and significant time-trend and no impact or a negative impact of the microloan program. Using quantile regression, we learn that especially the poor entrepreneurs suffer from a negative impact of the treatment. The average individual seems to decrease private expenditure when increasing business expenditure and does not experience any impact from microloans. Entrepreneurs at the upper end of the distribution have again negative impacts of the treatment when considering the business expenditure. We state that the microfinance program has at least no positive

[1] The data are available on www.ifmr.ac.in.

[2] The microfinance program from one MFI is not applied to the control group. Unfortunately, other MFI's were present in the control villages. However, the level of microfinance in the control villages remains significantly lower than in the treated villages (Banerjee et al., 2009).

impact on entrepreneurs. We suggest to adjust programs more to fit specific groups of entrepreneurs and their needs.

Compared to Banerjee et al. (2009), we find more detailed results from our consideration according to the treatment. Additionally, we are able to extract a negative time-trend. Furthermore, by using quantile regression, we can firstly differentiate between the impact of microfinance on very poor entrepreneurs and very rich entrepreneurs. On the other hand, we are only focusing on the impact of the treatment, while Banerjee et al. (2009) consider also further important variables like gender and age.

Coming back to the control group dilemma, we would like to mention several other approaches that try to solve the control group problem. Abadie et al. (2010) try to solve the dilemma by creating artificial control groups. However, we still have the problem to find enough similar individuals who live under similar conditions to form such a group. Todd and Wolpin (2006) find a way to evaluate a program without the need for a control group. They carry out a structural forecast of the impacts of a school subsidy program in Mexico and compared it to the experimental outcome. The forecast in their model had a reasonably good result. For completeness we have to mention that there is no comparison of an ordinary treatment evaluation and a structural estimation in the literature. Therefore, we can not comment on the results of Todd and Wolpin (2006) at this stage of our research.

The paper is organized as follows. In section 2, we describe the data used. Section 3 provides information on different data levels and in section 4, we outline the methods we use. We then describe the empirical results in section 5 and interpret them in section 6. We conclude with a short comment in section 7.

2 Survey design

The IFMR-Centre of Microfinance[3] provides comprehensive data from a microloan program in Hyderabad, India. The considered program is the microloan program from Spandana, a large Indian microfinance lender. The characteristics of the microloan program from Spandana[4] are

- group-based lending,

[3] www.ifmr.ac.in

[4] www.spandanaindia.com

- with a duration of 50 weeks,
- a loan size from Rs 2,000 to Rs 25,000, and
- weekly repayment schedules.

The general loan, Abhilasha, is supposed to finance small ventures. There are two subsequent loans. Samruddhi, which is an income generating group loan and Pragathi, which is offered to entrepreneurs who want to launch a microenterprise. To be eligible for a loan from Spandana, clients must

(a) be a female,

(b) between 18 and 59 years old,

(c) live for at least one year in the area, and

(d) have an identification and residential proof.

(e) Groups are formed by themselves and

(f) at least 80 % of a group must own their home.

Spandana also offers individual loans, but only to men who have a monthly source of income. Conditions (b) to (d) also apply to men. As 96,5 % of the borrowers from Spandana are women, we disregard individual loans. Spandana offers some side-products like safe drinking water inventions, renewable-energy product portfolios, life-insurance and new market linkages. But these products are not mandatory to the borrowers[5].

At the beginning of the experiment, Spandana selected 120 slums in which they would like to open branches. Only slums with no pre-existing microfinance lending schemes and with potential borrowers that were poor but not at the lowest level of poverty were selected. Slums with a large amount of construction workers were excluded as they seem to be more willing to apply for a microloan than other entrepreneurs. The population in the slums ranges from 46 to 555 households.

After the first survey, 16 slums were dropped, as these contained a large number of migrant-worker households. However, 104 slums remained. These slums were paired based on their per capita consumption, their fraction of households with debt, and their

[5] www.spandanaindia.com

fraction of households with a business. Then one of these slums was randomly assigned to the treatment, i.e. Spandana started providing group loans to the borrowers in the treated areas. The second survey took place at least 12 months after the assignment, but on average after 15 to 18 months.

In the first wave in 2005, the IFMR surveyed 2,800 households living in slums. The selected households are supposed to have at least one woman between 18 and 55 years, as the group loans from Spandana mainly target women. The questionnaire requests information on household composition, education, employment, asset ownership, decision-making, expenditure, borrowing, saving, and businesses. In 2007 and 2008 the slums were resurveyed. At this time, information from 6,798 households was collected. The survey did not intend to resurvey exactly the same households in the slums (Banerjee et al., 2009).

Summarizing the characteristics of the data set, we see that the assignment of treatment to 52 of the 104 slums was applied randomly and that the control slums remained untreated over time at least for microfinance loans from Spandana[6].

3 Different data levels

We will consider the data on two different levels to obtain driving factors of the changes in wealth. First we study the data at the slum level in a panel data structure. Afterwards, we consider the data at the individual level in form of repeated cross-section data.

3.1 Panel data at the slum level

In the data we obtain information on individuals who live in one slum. As we can follow the slums perfectly over two periods in time we average the values for expenditure of all individuals in one slum. We therefore obtain two values of expenditure, one for 2005 before the microfinance program was launched and one for 2007 for each slum when the program finished.

A first advantage of using panel data methods are more precise standard errors compared to ordinary least square regression (OLS) for pooled data. The OLS seems to underestimate standard errors and thus t-statistics were overestimated (Cameron and

[6]Other MFI's provides microloans in the control area during the time of the survey. But as Banerjee et al. (2009) argue, the probability to receive a loan was still higher in treated areas than in control areas.

Trivedi, 2005). Second, as the treatment is said to be random in our data set we can estimate them in a fixed effects model. As a result, we account for unobserved heterogeneity between the slums that stays constant over time and might be correlated with regressors. This problem might appear small in our data set as we have only two periods of time. The opposite to fixed effects are random effects. We describe unobserved heterogeneity as random effect when it is independently distributed from the regressors. But following Cameron and Trivedi (2005), economists regret the possibility of a random effect model to represent the true model. As a third advantage, we can follow the panel data over time and learn something about the dynamics of the dependent variable, when assuming no correlation between the regressors.

To account for the disadvantages of panel data, we suggest a paper by Bertrand et al. (2004). They find that because of serial correlation in repeated panels, the ordinary standard errors might understate the deviation of the treatment effect and as a result still overestimate the t-statistics and their respective significance levels. They suggest different methods such as block bootstrapping to avoid the problem. It also becomes clear that the problem arises in data sets which are recorded at more than two points in time. In our data we are faced with a so called short panel which means we have a large population but only two periods of time. In short panels, a biased estimation as mentioned above is unlikely. However, we will report a bootstrap regression as a comparison for each regression.

3.2 Repeated cross-section data at the individual level

By focusing on panel data, we lose some information on individuals and the structure of the slum. However, we enrich our consideration by estimating models based on individual data in a repeated cross-section analysis. We obtain $2,440$ observations in 2005 and $6,763$ observations in 2007. Before obtaining these final numbers, we remove missing data. The treatment variable was missing for 360 individual data out of $2,800$ individual data in 2005. Therefore we had to delete these data. In 2007, all individual data remained.

The advantage coming from repeated cross-section analysis is that we have a larger number of observations. The problem arising with that is, however, that a repeated cross-section analysis can lead to inconsistent estimates (Cameron and Trivedi, 2005). To check the robustness of the estimates, we can not do a fixed effects regression as we do not have a panel structure here. To solve this problem we form a pseudo panel, where we pool information on slum level (Khandker et al., 2010) to do a fixed effects model regression. So we obtain robust estimates again. As we do not consider a panel data structure here, we can report results without bootstrapping estimations. Furthermore, as we consider many entrepreneurs from each slum, the error terms of the individuals in one slum can be correlated. The standard errors of these individuals will be clustered to account for the problem.

4 Different evaluation methods

As we would like to measure whether the availability of a microloan program has a positive impact on wealth, we have to identify variables to measure wealth from the data set. As there is no consistent information about the assets of an individual, we use expenditure as such. The more an individual is spending, the more wealth the individual is supposed to have. For a nearer consideration of the data, we allow for a disaggregation of the expenditure in private expenditure and expenditure for business investments.

A census was taken in India in 2007. Banerjee et al. (2009) find that Spandana borrowers were oversampled in that census. Thus, we have to adjust for the oversampling by weighting our results, as we only want to obtain the effect of the treatment and do not claim to reflect the true model.

4.1 Randomized impact evaluation

As the treatment is said to be randomly assigned to 52 of the 104 slums, the randomized impact evaluation (RIE) would be the simplest way to evaluate the program. We assume no selection bias in the data given the random assignment. Usually, the RIE is applied to cross-section data for the second period in time, only. As a result we observe the impact after the treatment was applied. This method consists of a t-test or alternately of an ordinary least square regression (OLS). The regression is given by

$$\ln Exp_i = \alpha + \beta Treat_i + \varepsilon_i \tag{1}$$

where i denoted the individual. The expenditure is the dependent variables on the left-hand side and the assigned treatment is the only explanatory variable on the right-hand side. We test whether the coefficient β of the treatment variable is significantly different for the treated slums and the non-treated slums.

4.2 Difference-in-difference approach

To obtain deeper information and to be able to analyze the robustness of the coefficients, we apply a difference-in-difference approach (DiD). Suppose we consider a correct control group, selection biases should not be a problem. We consider the different individuals or slums at different periods of time. By using double differences, we remove time-invariant socioeconomic characteristics that might be different for the considered slums or individuals. Starting with the control group, we consider the expenditure of an individual in 2005 as

$$E\left(\ln Exp_{it} \mid Treat_{it}=0, Year_{it}=2005\right)=\alpha \tag{2}$$

and in 2007 as

$$E\left(\ln Exp_{it} \mid Treat_{it}=0, Year_{it}=2007\right)=\alpha+\beta_Y. \tag{3}$$

where i denotes the individual and t denotes time. Then we take the first difference

$$\begin{aligned} & E\left(\ln Exp_{it} \mid Treat_{it}=0, Year_{it}=2007\right) \\ & -E\left(\ln Exp_{it} \mid Treat_{it}=0, Year_{it}=2005\right) \\ & =\beta_Y. \end{aligned} \tag{4}$$

Now we consider the treated group in 2005, i.e. before the treatment, as

$$E\left(\ln Exp_{it} \mid Treat_{it}=1, Year_{it}=2005\right)=\alpha+\beta_T \tag{5}$$

and in 2007, after the treatment, as

$$E\left(\ln Exp_{it} \mid Treat_{it}=1, Year_{it}=2007\right)=\alpha+\beta_T+\beta_Y+\beta_{TY}. \tag{6}$$

We consider the first difference as

$$\begin{aligned} & E\left(\ln Exp_{it} \mid Treat_{it}=1, Year_{it}=2007\right) \\ & -E\left(\ln Exp_{it} \mid Treat_{it}=1, Year_{it}=2005\right) \\ & =\beta_Y+\beta_{TY}. \end{aligned} \tag{7}$$

Equation (4) and equation (7) give the change in expenditure for the control group and the treated group. To obtain significant differences in this change we have to take the second difference as

$$\begin{aligned} & E[(\ln Exp_{it} \mid Treat_{it} = 1, Year_{it} = 2007) \\ & -E\left(\ln Exp_{it} \mid Treat_{it} = 1, Year_{it} = 2005\right)] \\ & -E[(\ln Exp_{it} \mid Treat_{it} = 0, Year_{it} = 2007) \\ & -E\left(\ln Exp_{it} \mid Treat_{it} = 0, Year_{it} = 2005\right)] \\ & = \beta_{TY}. \end{aligned} \tag{8}$$

The resulting coefficient β_{TY} measures the difference caused by the treatment (Imbens and Wooldridge, 2009). We estimate the coefficient by using

$$\ln Exp_{it} = \alpha + \beta_Y Year_{it} + \beta_T Treat_{it} + \beta_{TY} \left(Treat * Year\right)_{it} + \varepsilon_{it} \tag{9}$$

where the expenditure are the dependent variable on the left-hand side and year and treatment are the dummy variables on the right-hand side. The $Year$ is coded with 0 for 2005 and 1 for 2007. The variable $Treat$ is 0 for an individual in the control group and 1 for an individual in the treatment group. The variable $Treat * Year$ is 1 if and only if the individual is treated and considered in 2007. Otherwise this variable equals 0.

The coefficient β_{TY} is the variable of interest to us. If that coefficient is significant, there might be a positive or negative impact from the microloan program on the expenditure. Furthermore, we look at β_Y to analyze whether there is a time-trend in the data or not. The coefficient β_T is expected to be insignificant or zero as the treatment is said to be randomly applied. We weight the results to be able to interpret the results for the whole population in slums.

4.3 Quantile regression with the DiD approach

The DiD estimation calculates the effect on the average expenditure. By using quantile regression (QR), we can find the effects at different quantiles of the expenditure distribution. As a result, we can consider the changes for very poor individuals or slums and also for very rich individuals or slums, which are caused as a result of using of Spandana microloans. In contrast to the OLS estimation, the quantile regression minimizes absolute values of errors. The errors were asymmetrically weighted dependent on the quantile we are about to estimate. The quantile regression estimator $\widehat{\beta}_q$ is minimizing over β_q

$$Q_N\left(\beta_q\right) = \sum_{i:y\geq x_i'\beta}^{N} q\left|y_i - x_i'\beta_q\right| + \sum_{i:y<x_i'\beta}^{N}\left(1-q\right)\left|y_i - x_i'\beta_q\right| \tag{10}$$

where the absolute error is given by

$$e = y - x'\beta_q \tag{11}$$

(Cameron and Trivedi, 2005). The variable q denotes the q^{th} quantile of the distribution. The first term on the right-hand side of equation (10) summarizes the underestimated errors, and the second term summarizes the overestimated errors. By using this method, we can see whether the microfinance program has a positive or negative effect on slums at the lower level of the distribution of expenditure or at the other end of the distribution. This estimation is more robust to outliers and the coefficients are consistent under weaker assumptions than in the OLS estimation (Cameron and Trivedi, 2005). Following Khandker et al. (2010) we can estimate the coefficients using

$$\ln Exp_{it} = \beta_q X_{it} + \varepsilon_{qit}, Q_t\left(\ln Exp_{it}|X_{it}\right) = \beta_q X_{it}, q \in (0,1). \tag{12}$$

As we would like to concentrate on the effect of the treatment, we apply two variations of this method to our data set, first the quantile regression with DiD approach and secondly the quantile treatment effects estimation. The first approach gives us information on the impact of the treatment on different groups at the lower end or upper end of the distribution of the expenditure. The second one extracts information on the absolute difference in expenditure between the treated group and the control group in a special quantile. Taking both methods together, we can draw a clearer picture of the impacts of the microloan program over the whole distribution.

We follow Athey and Imbens (2006) by estimating a DiD approach using the advantages of the quantile regression (QDiD) . They show that the distribution of the counterfactual is given as

$$QDiD_{\ln Exp(q)} = \ln Exp_0^T\left(q\right) + \left(\ln Exp_1^C\left(q\right) - \ln Exp_0^C\left(q\right)\right). \tag{13}$$

At first the difference in $\ln Exp$ over time at the q^{th} quantile of the control group is calculated and then added to the q^{th} quantile of $\ln Exp$ from the treated group. One very limiting assumption of this approach is that the distributions of $\ln Exp$ for the treated is equal to the distribution of the difference of expenditure of the control group (Khandker et al., 2010). Under such an assumption, Athey and Imbens (2006) treat the DiD approach as a special case of QDiD. However, we estimate

$$\begin{aligned} \ln Exp_{it} &= \alpha(q) + \beta_Y(q)\, Year_{it} + \beta_T(q)\, Treat_{it} \\ &\quad + \beta_{TY}(q)\,(Treat * Year)_{it} + \varepsilon_{it}(q) \end{aligned} \tag{14}$$

where the q^{th} quantile is given by $q \in (0,1)$ and focus on the coefficient of $Treat * Year$. We weight the results to apply a general interpretation for all individuals in the groups.

Abrevaya and Dahl (2008) showed a different approach, which specifies the unobserved fixed effects as a linear function of other covariants. This deviation is then denoted as pooled linear quantile regression. As we focus on the treatment and do not consider covariants at the moment, we refrain from showing this method in this context. An application of this method can be seen in Khandker et al. (2009).

4.4 Quantile treatment effects

Estimating quantile treatment effects (QTE) allows for the quantification of quantile regression results. In principal, the QTE of a program can be calculated when the assignment of the treatment was applied randomly (Heckman et al., 1997). These effects are defined as

$$QTE = \ln Exp_q^{Treat} - \ln Exp_q^{Control} \tag{15}$$

which indicates the difference in expenditure of the treated individuals, $\ln Exp_q^{Treat}$, and the expenditure of the control individuals, $\ln Exp_q^{Control}$, in the quantile q of the distribution of expenditure. We have to keep in mind that the QTE is not able to show the distribution or the impact of the treatment effects like the upper QDiD approach does (Bitler et al., 2008). More technical information on the distribution of quantile treatment effects are provided in the appendix.

Froelich and Melly (2008) show the implementation of the estimation method for Stata, which we use for our estimations. For finding the correct method to estimate the QTE, we have to distinguish between conditional and unconditional effects, and additionally, between endogenous and exogenous treatment. Assuming conditional treatment effects, the outcome is dependent on the value of the regressors while for unconditioned effects the outcome is independent from the regressors. An endogenous treatment is assumed as long as the treatment is assigned randomly. An exogenous treatment explains the assignment of the treatment depending on observable characteristics of the individuals (Froelich and Melly, 2008). However, given different treatment effects we have to apply different estimators as shown in Froelich and Melly (2010). The methods are summarized in table 1. In our setup, we have unconditional exogenous treatment effects, as the

	conditional effects	unconditional effects
endogenous treatment	Abadie et al. (2002)	Froelich and Melly (2008)
exogenous treatment	Koenker and Bassett (1978)	Firpo (2007)

Table 1: Overview over methods to estimate QTE

assignment was random and we can observe characteristics of the individuals. We use the technique of Firpo (2007) for our estimation.

5 Empirical results

5.1 Panel data at the slum level

5.1.1 Randomized treatment evaluation

As the treatment was said to be randomly assigned, we establish an ordinary least square estimation (OLS) using the data from 2007 first. We weight our results because Spandana borrowers were oversampled. Weighted results can be interpreted as correct coefficients for the complete population. Therefore, we regress

$$\ln Exp_i = \alpha + \beta Treat_i + \varepsilon_i \tag{16}$$

where $i = 1, ..., 104$. We obtain the results in table 2. As the coefficient estimates are not significant coefficients for the treatment ,we have to be careful in interpreting these results. We do not observe a direct impact of a microfinance treatment from Spandana on slums. However, for this result and other results at the slum level we have to mention that we consider averaged data. Even if we break down the data set into private expenditure and business expenditure, we find no significant results. Therefore, we take advantage of

OLS	Complete	Private	Business
α_i	9.288^{***}	8.762^{***}	9.120^{***}
	(0.110)	(0.023)	(0.165)
$Treat$	-0.027	0.024	0.027
	(0.128)	(0.032)	(0.209)

*,**,*** significant at 10, 5, and 1 percent.
Standard errors are shown in parentheses.

Table 2: OLS coefficients at the slum level

another characteristic of this data set. At the slum level, we are able to form a panel data set in which we can follow specific slums over two time periods.

5.1.2 Difference-in-difference approach

As a next step we study the data set with the difference-in-difference method (DiD). We estimate the basic DiD model as

$$\ln Exp_{it} = \alpha + \beta_Y Year_{it} + \beta_T Treat_{it} + \beta_{TY} \left(Treat * Year\right)_{it} + \varepsilon_{it} \tag{17}$$

where $i = 1, ..., 104$ describes the number of the slums and $t = 2005, 2007$ denotes the year. Afterwards, we calculate two alternative regressions. First, we estimate the model with fixed effects for the slums. With this modification, we account for specific characteristics of the single slums that do not change over time. This method is also called the least square dummy-variable regression. The estimator is called within estimator and performs a OLS estimation on the mean-differenced data. Therefore, this method is not able to estimate the effect of any time-invariant variable. We have to mark that the α_i, which denotes the fixed effect in that alternative is not consistently estimated in short panels. However, the coefficient of the dummy-variable $Treat * Year$ is consistently estimated (Baltagi, 1995; Cameron and Trivedi, 2010). By estimating a fixed effect model, we can account for unobserved individual heterogeneity that might be correlated with the regressor. Using a fixed effects method, regressors that are individual-invariant can not be identified. For completeness, we also carry out a DiD estimation with random effects, which would take into account a changing environment in each slum. The estimator is called the between estimator and only considers cross-section variation of the data. The random effects model is often criticized by economists for not showing the true model. Nevertheless, we show this approach to give a complete picture. At the end we establish a bootstrap estimation, which is able to adjust standard errors in a robust way. Table 3 summarizes the results for the basic estimation, the fixed effects model, the random

DiD	DiD	DiD_fe	DiD_re	BS_50
α_i	9.942***	9.982***	9.942***	9.948***
	(0.064)	(0.037)	(0.078)	(0.057)
$Year$	−0.654***	−0.654***	−0.654***	−0.772***
	(0.124)	(0.124)	(0.102)	(0.092)
$Treat$	0.063	omitted	0.063	0.014
	(0.092)		(0.111)	(0.110)
$Treat * Year$	−0.116	−0.111	−0.116	−0.032
	(0.144)	(0.145)	(0.144)	(0.143)

*,**,*** significant at 10, 5, and 1 percent.
Standard errors are shown in parentheses.

Table 3: DiD coefficients at the slum level

effects model and the bootstrap estimation. Considering the coefficient of $Treat * Year$, we are faced with the same result as in the OLS estimation. We see a negative, but insignificant coefficient. Furthermore, we consider standard errors that are larger than the coefficient in each estimation alternative. Nevertheless, we see all coefficients point in the same direction. We suggest that there might be a negative impact of the microfinance program on average expenditure in the slums.

Surprisingly, we see a highly significant and negative effect of the dummy for the $Year$. To repeat the coding, we use 0 for the time before the treatment in 2005 and 1 for the time after the treatment in 2007. Our result suggests that in 2007 each slum has lower expenditure than in 2005. For the coefficient of the variable $Treat$, we see positive coefficients that are not significant. We have to mention that the coefficient in the estimation with fixed effect is omitted as we do not have variation in this variable. To conclude, we seem to have a strong and negative underlying time-trend. The effect of the treatment is negative in each regression, while it is not significant.

The average expenditure in a slum consist of private expenditure and business expenditure. It would be interesting to decompose the impact from the treatment for the type of expenditure. By decomposing the results from table 3, we get the results given in table 4. Here we can see that the microfinance treatment might have a negative effect for private expenditure, while it might have a positive effect on business expenditure. That also applies to the results stated by Banerjee et al. (2009). However, coefficients remain insignificant.

DiD	Complete	Private	Business
α_i	9.942^{***}	9.798^{***}	8.823^{***}
	(0.078)	(0.061)	(0.178)
$Year$	-0.654^{***}	-1.036^{***}	0.297
	(0.102)	(0.062)	(0.201)
$Treat$	0.063	0.084	-0.157
	(0.111)	(0.088)	(0.249)
$Treat * Year$	-0.116	-0.072	0.140
	(0.144)	(0.088)	(0.271)

*,**,*** significant at 10, 5, and 1 percent.

Standard errors are shown in parentheses.

Table 4: Decomposed DiD coefficients at the slum level

Considering the coefficient of $Year$, we see that it is significantly negative for private expenditure and insignificant and positive for business expenditure. We believe the time trend effects the private expenditure more than the business expenditure. Considering the variable $Treat$, we can not observe a significant coefficient for private expenditure and for business expenditure.

5.1.3 Quantile regression with the DiD approach

As our estimations above are based on averaged data, it might be useful to split the consideration to convey the "hidden" information. Therefore, we extend the consideration of Banerjee et al. (2009) and use the method of quantile regression. A slum contains all kinds of entrepreneurs ranging from very poor to very rich. It is not very likely that each slum consists of the same distribution of wealth. Rather, we expect to find some very poor slums and some wealthier slums over the whole data set. Quantile regression gives us an instrument to consider impacts of the treatment on each quantile of the distribution separately. First we consider poor slums at the 5% quantile, then the average slums at the 50% quantile and finally the richest slums at the 99% quantile. We do so by estimating

$$\ln Exp_{it} = \alpha (q) + \beta_Y (q) Year_{it} + \beta_T (q) Treat_{it} + \beta_{TY} (q) (Treat * Year)_{it} + \varepsilon_{it} \quad (18)$$

where $i = 1, ..., 104$ describes the number of the slums and q denotes the considered quantile. To account for the possible biases in standard errors, we refer to the bootstrap estimation for each quantile. In general, we see that the bias is not large, but for some coefficients significance levels change. Table 5 illustrates the obtained results. First we

QDiD	QDiD_05	BS_05	QDiD_50	BS_50	QDiD_99	BS_99
α_i	8.979***	8.979***	9.948***	9.948***	10.902***	10.902***
	(0.087)	(0.189)	(0.071)	(0.057)	(0.011)	(0.103)
$Year$	−0.251*	−0.251	−0.772***	−0.772***	3.319***	3.319
	(0.122)	(0.196)	(0.101)	(0.092)	(0.016)	(1.995)
$Treat$	0.323*	0.323	0.099	0.014	0.452***	0.452*
	(0.130)	(0.223)	(0.101)	(0.110)	(0.016)	(0.199)
$Treat * Year$	−0.378*	−0.407*	−0.113	−0.032	−3.808***	−3.808
	(0.177)	(0.128)	(0.143)	(0.143)	(0.023)	(2.059)

*,**,*** significant at 10, 5, and 1 percent. Standard errors are shown in parentheses.

Table 5: Comparison of QR coefficients at the slum level

consider the coefficient of $Treat * Year$. We emphasize that this coefficient measures the effect for treated slums after treatment. We learn that the results become more interesting and significant. We find negative impacts of the treatment for very poor slums and for very rich slums. At the 99% quantile, the coefficient is not significant when we apply the bootstrapping adjustment of the standard errors. That might by caused by the high variation at the upper end of the distribution. Furthermore, this could be due to the fact that we do not have enough observations at this quantile to obtain robust estimations. However, for poor slums the result remains significant. We learn that the microfinance program seems to have a negative effect on very poor slums that participate in the treatment compared to poor slums that did not have the chance to participate in the microfinance program. For the 50% quantile, the coefficient for $Treat * Year$ is not significant.

Considering the other coefficients in the 5% quantile and the 50% quantile, we can not see a unique direction. For the poor slum and the average slum, the coefficient for $Year$ is significantly negative. We suggest that all slums have lower expenditure in 2007. We would like to mention that the coefficient for $Treat$ is positive for poor slums. That suggests that in these quantile the treatment might not be completely randomized. Together with our results for the combined coefficient of $Treat * Year$, we propose that poor slums are faced with a significantly negative time-trend and a negative impact from the microfinance program. Average slums are faced with the negative time-trend, but have no impact from the treatment at all.

For the 99% quantile, the coefficients of $Year$ is positive, which means that rich slums seems to spend more over time. The coefficient of $Treat$ is also positive and significant, which would support the suggestion made above that treatment was not completely randomized in the higher quantiles, too. However, we observe that the treatment effect coefficient $Treat * Year$ points in the opposite direction at a high significance level. That would suggest that a participation in the program has a negative impact for rich slums. As we see that the significant coefficient becomes insignificant when using bootstrapping methods, we have to be careful in interpreting this result.

We obtain an overview of all quantiles by looking at figure 1. The dashed line depicts

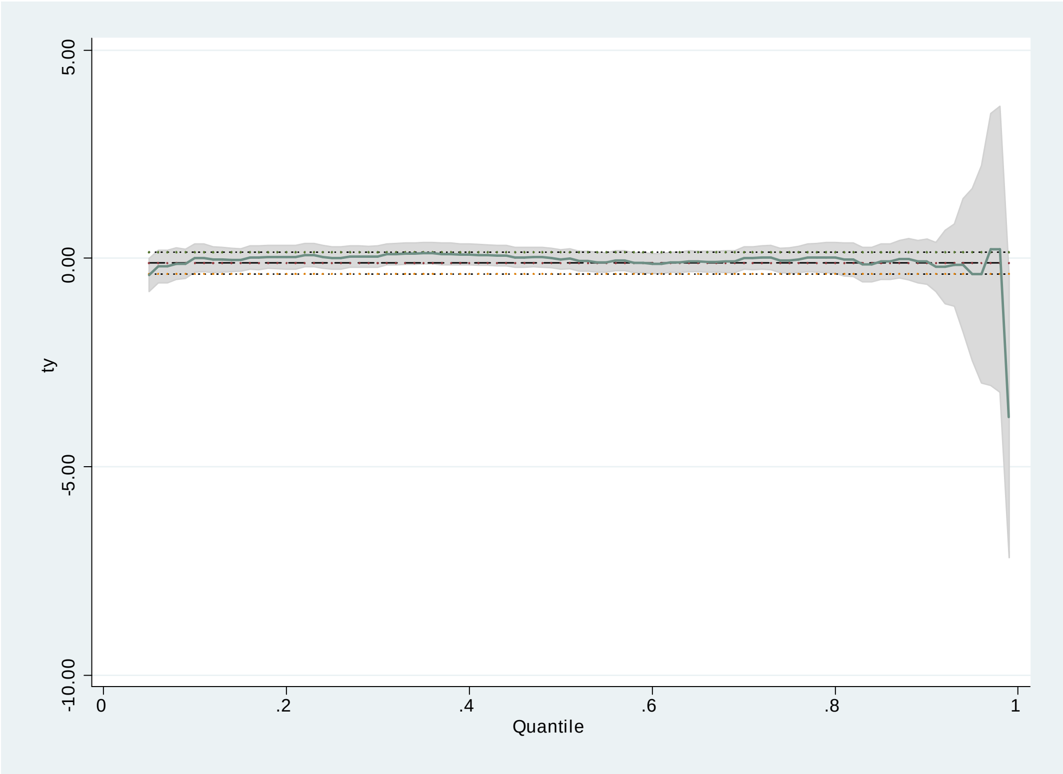


Figure 1: Comparison of $Treat * Year$ for DiD and QR at the slum level

the DiD estimator and the dotted lines show the 10% confidence interval. The straight line illustrates the quantile regression estimator while the grey area around it gives the confidence interval. We witness a more precise estimator in the QDiD, whereas it stays in the confidence interval of the DiD estimation up to the highest quantiles. Therefore, the QDiD does not contradict the DiD. At the end we see a strong negative outlier, which repeats the estimation we interpreted above. However, as the confidence interval increases at the upper quantile, we will not interpret this result.

In the following, we decompose the results into effects from only private expenditure and from only business expenditure to compare the results and extract the driving forces. A comparison of the coefficients of the complete model, only private expenditure and only business expenditure, is shown in table 6. We consider only the coefficient of $Treat{*}Year$.

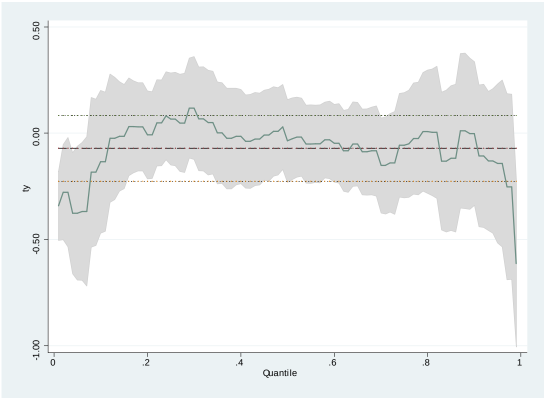
$Treat * Year$	Complete	BS	Private	BS	Business	BS
Quantile 0.05	-0.378^{*}	-0.407^{*}	-0.369^{*}	-0.377^{*}	0.635	0.635
	(0.177)	(0.128)	(0.149)	(0.174)	(1.243)	(0.887)
Quantile 0.50	-0.113	-0.032	-0.008	-0.037	-0.146	-0.142
	(0.143)	(0.143)	(0.118)	(0.108)	(0.297)	(0.294)
Quantile 0.99	-3.808^{***}	-3.808	-0.613^{***}	-0.613^{*}	-2.667^{***}	-2.667
	(0.023)	(2.059)	(0.006)	(0.247)	(0.075)	(2.231)

*,**,*** significant at 10, 5, and 1 percent. Standard errors are shown in parentheses.

Table 6: Decomposition of QR coefficients at the slum level

The given results suggest that treated slums are worse off over nearly all quantiles when we look at private expenditure or business expenditure. We find significantly negative results for the very poor slums for private expenditure and for all expenditure, while the coefficient for business expenditure is not significant in the bootstrap estimation. For average slums, the results are negative but not significant. At the upper end, we see that microloans have a negative impact on private expenditure. Standard errors are too high to interpret the results at the other columns. We learn more about the effects caused by private expenditure or by business expenditure overall quantiles when looking at the decomposed pictures in table 7. We have to pay attention to the different scaling on the y-axis. The scaling in the left figure is much smaller than in the right figure. That suggests that the effects from business expenditure are larger in range. For private expenditure, the QDiD estimator is significantly lower and exceeds the confidence interval of the DiD estimator for poor slums and rich slums. This supports the negative coefficients in our estimation. In the right picture, we see the business estimators for DiD and QDiD. We learn that the treatment might have a positive effect for very poor slums as the estimator exceeds the confidence interval of the DiD estimator. Furthermore, we become more sensitive to the effect on the rich slums as the estimator also breaks out of the confidence interval here .

Private



Business

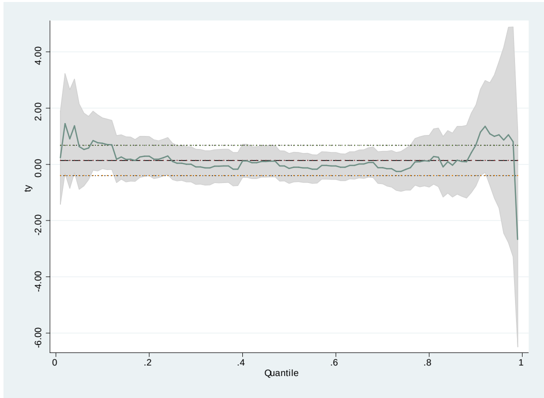


Table 7: Private expenditures and business expenditures at the slum level

5.1.4 Quantile treatment effects

Quantile treatment effects (QTE) are an interesting instrument to quantify the difference between a treatment group and a control group without distortion of strong underlying trends. Table 8 illustrates the unconditional exogenous treatment effects. Here we find

$\ln Exp_q^{Treat} - \ln Exp_q^{Control}$	Complete	Private	Business
Quantile 0.05	0.048	-0.022	-0.616
Quantile 0.50	-0.196	0.138	0.107
Quantile 0.99	0.233	0.296	-0.491

Table 8: QTE at the slum level

that for very poor people the overall effect should be positive, while the decomposed effects are negative. This might be caused by the averaging of the data. Poor slums are not increasing their expenditure at all. Slums in higher quantiles of the distribution seem to be able to gain positive effects from the possibility of participating in a group as they increase private expenditure or both types of expenditure. The effect states that the rich slums are not better off because they are able to participate in a microloan program when considering business expenditure. Overall the effects are ambiguous. We will come back to this in the next section, when we consider individual data to obtain more robust information and compare the individual results to the results from the slum level.

5.2 Repeated cross-section data at the individual level

5.2.1 Randomized treatment evaluation

In the following section, we repeat our estimations but change the basis of the underlying data. We use individual, repeated cross-section data. We have $2,440$ observations in 2005 and $6,763$ observations in 2007. As a first step we conduct an OLS estimation. Therefore we regress

$$\ln Exp_i = \alpha + \beta Treat_i + \varepsilon_i \tag{19}$$

where $i = 1, ..., 9, 203$. We estimate the expenditure as a complete sum, and also decomposed as private expenditure only and business expenditure only. We cluster the data at the slum level to account for correlation of errors of individuals in the same slum and weight the results to be able to interpret the results for the whole population. The results are summarized in table 9. We observe no significant coefficients for $Treat$. When decomposing the results, we do not obtain significant results. The coefficients remain positive. In the next sections we will apply more advanced methods to quantify the impact and be able to compare individual results to the aggregate result considered above at the slum level.

OLS	Complete	Private	Business
α_i	8.781***	8.589***	7.021***
	(0.028)	(0.024)	(0.129)
$Treat$	0.029	0.015	0.155
	(0.043)	(0.033)	(0.173)

*,**,*** significant at 10, 5, and 1 percent.
Standard errors are shown in parentheses.

Table 9: OLS coefficients at the individual level

5.2.2 Difference-in-difference estimation

We apply the DiD method at repeated cross-section data. The interpretation of the coefficients remains. We estimate

$$\ln Exp_{it} = \alpha + \beta_Y Year_{it} + \beta_T Treat_{it} + \beta_{TY} \left(Treat * Year\right)_{it} + \varepsilon_{it} \tag{20}$$

where $i = 1, ..., 9, 203$ describing the number of the individuals. As we do not obtain panel data, we have to take care of the robustness of the coefficients. Therefore, we cluster error terms of the individuals in each slum and weight results again. Furthermore, we are not able to obtain a fixed effects model at the individual level. However, we can use the slums as a basis for the fixed effect. Table 10 summarizes the results. We do not find

DiD	DiD	DiD_fe (on slum)
α_i	9.461^{***}	9.949^{***}
	(0.038)	(0.023)
$Year$	-0.680^{***}	-0.685^{***}
	(0.042)	(0.042)
$Treat$	0.088	omitted
	(0.055)	
$Treat * Year$	-0.077	-0.025
	(0.061)	(0.063)

*,**,*** significant at 10, 5, and 1 percent.
Standard errors are shown in parentheses.

Table 10: DiD coefficients at the individual level

a significant coefficient for $Treat * Year$. All coefficients for $Year$ are significant and negative. All individuals reduce their expenditure from 2005 to 2007. When we consider the coefficient of $Treat$ we can not find significant coefficients.

Decomposing the result might shed some light on the underlying mechanisms. We compare results from all expenditure to only private expenditure and only business expenditure in table 11. We learn that all coefficients point in the same direction and seem

DiD	Complete	Private	Business
α_i	9.461^{***}	9.370^{***}	7.731^{***}
	(0.038)	(0.036)	(0.154)
$Year$	-0.680^{***}	-0.781^{***}	-0.710^{***}
	(0.042)	(0.037)	(0.172)
$Treat$	0.096	0.085	0.228
	(0.055)	(0.049)	(0.217)
$Treat * Year$	-0.067	-0.082	-0.133
	(0.062)	(0.054)	(0.231)

*,**,*** significant at 10, 5, and 1 percent.
Standard errors are shown in parentheses.

Table 11: Decomposed DiD coefficients at the individual level

to support the interpretation made at the slum level, even if they are insignificant. We do not find strong differences between private expenditure and business expenditure.

5.2.3 Quantile regression with the DiD approach

By studying single quantiles of the distribution, we are able to obtain the impact of the program for the very poor individuals at the lower end, for average individuals, and for richer individuals at the upper end. We estimate

$$\ln Exp_{it} = \alpha\left(q\right) + \beta_Y\left(q\right) Year_{it} + \beta_T\left(q\right) Treat_{it} + \beta_{TY}\left(q\right)\left(Treat * Year\right)_{it} + \varepsilon_{it} \quad (21)$$

where $i = 1, ..., 9,203$ describes the number of the slums and q gives the quantile. Table 12 summarizes the obtained results. The big picture remains. We consider negative,

QDiD	QDiD_05	QDiD_50	QDiD_99
α_i	8.300***	9.308***	12.320***
	(0.038)	(0.022)	(0.963)
$Year$	−0.526***	−0.636***	−1.053
	(0.045)	(0.025)	(1.133)
$Treat$	0.131**	0.072*	0.339
	(0.054)	(0.031)	(1.367)
$Treat * Year$	−0.107	−0.054	−0.082
	(0.063)	(0.036)	(1.607)

*,**,*** significant at 10, 5, and 1 percent.
Standard errors are shown in parentheses.

Table 12: Comparison of QR coefficients at the individual level

mostly significant coefficients for $Year$, positive coefficients for $Treat$, and negative but not significant coefficients for the combined variable $Treat * Year$. The negative time-trend seems to be supported again. Individuals reduce expenditure from 2005 to 2007. We observe significant positive coefficients for $Treat$, which should not appear when the application of the treatment is randomized. Furthermore, we learn that we can not interpret the treatment effect. Therefore, we summarize that we can not find an impact on the microloan program. The coefficients over all quantiles are graphically shown in figure 2. The dashed line shows the DiD coefficient and the dotted line depicts the 90% confidence interval. The straight line shows the quantile regression coefficients for $Treat * Year$ and the grey area the associated confidence interval. At the lower end and the upper end of the distribution, we find quantile regression coefficients for $Treat * Year$ which exceed the confidence interval of the DiD estimation. However, as coefficients did

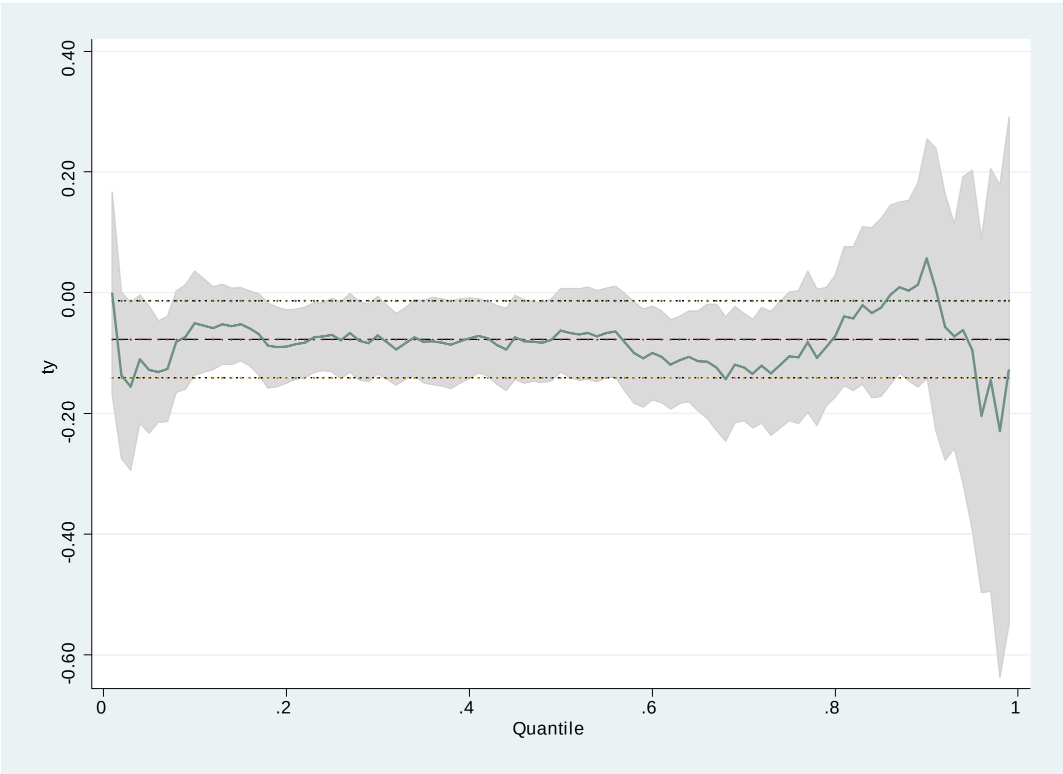


Figure 2: Comparison of $Treat * Year$ for DiD and QR at the individual level

not appear to be significant for all quantiles, we will not interpret this as a strong evidence here. For the average quantile, DiD estimation seems to provide good results.

For further insights, we decompose the expenditure in private expenditure and in business expenditure. We show the coefficient of the combined variable $Treat * Year$ in table 13. We find that all coefficients are not significant, except the coefficients for

$Treat * Year$	Complete	Private	Business
Quantile 0.05	-0.107	-0.100	-0.511^{***}
	(0.063)	(0.064)	(0.000)
Quantile 0.50	-0.054	-0.049	0.086
	(0.036)	(0.031)	(0.327)
Quantile 0.99	-0.082	-0.142	0.061
	(1.607)	(0.367)	(1.771)

*,**,*** significant at 10, 5, and 1 percent.
Standard errors are shown in parentheses.

Table 13: Decomposition of QR coefficients at the individual level

very poor business expenditure. That might be a sign that poor entrepreneurs reduce their business expenditure to secure their daily private consumption. For the average individuals and the rich individuals, we see positive impact on business expenditure. We interpret this result such that the individuals above the very poor quantile in the distribution increase business expenditure while perhaps reducing private expenditure. They might be able to do so, as they have sufficient assets to cover the requirements of everyday life. Nevertheless, because of small significance levels, we have to be careful in interpretation. Table 14 shows the quantile regression of private expenditure on the left side and the business expenditure on the right side. Considering private expenditure, we find that there might be a negative impact overall as described above. Furthermore, we see that for very poor individuals and for rich individuals, the negative results are stronger than estimated in the DiD estimation. Considering the business expenditure on the right side, we do not see much outliers, except the ones at the lower end of the distribution. That could state that for individuals who become rich enough to launch a business, the impact might be positive, while for all others the impact seems to not appear or be slightly negative.

Private expenditure Business expenditure

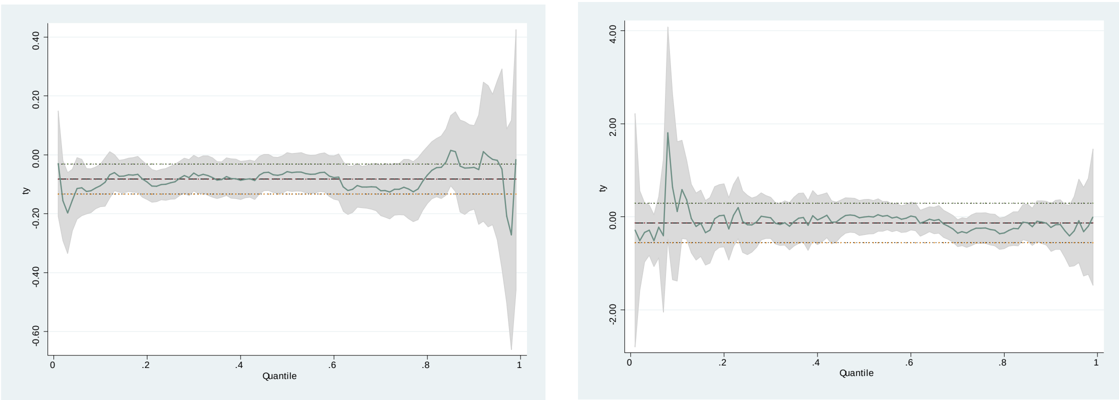


Table 14: Private expenditures and business expenditures at the individual level

5.2.4 Quantile treatment effects

The QTE help us to extract the direct difference between the treatment group and the control group. Table 15 illustrates the unconditional exogenous treatment effects. The

$\ln Exp_q^{Treat} - \ln Exp_q^{Control}$	Complete	Private	Business
Quantile 0.05	-0.020	0.016	0
Quantile 0.50	0.026	-0.003	0.050
Quantile 0.99	0.322	0.137	0.462

Table 15: QTE at the indivdidual level

treatment effects measure the quantitative difference. By doing so, the method weakens the underlying positive or negative trends. We see that over all expenditure, the very poor people are not better off when offered a microloan program. Considering the decomposed effects, the private expenditure increases while nothing happened on the business side. Also, here we see support for the statement that very poor people do not launch a business or increase their business expenditure. For the average individual, we see a positive treatment effect. Decomposing the overall effect we find a negative effect on private expenditure and a positive on business expenditure. Individuals are able to reduce private expenditure for business expenditure by participating in a microloan without impacting on the everyday life consumption. For rich individuals, microloans offer the possibility to increase both kinds of expenditure. Compared to the averaged results at the slum level, we find a positive impact here for rich entrepreneurs and their business expenditure. We did not extract a similar outcome above.

To summarize, we see that microloans do not seem to have a positive impact even when considered independently from the underlying trend. Especially poor entrepreneurs and very rich individuals seem to experience a negative impact as a result of the microloan program of Spandana. One could construct a pseudo panel with the individual data and control the results. As we do not expect a significant change, we leave this consideration for further work.

6 Microfinance amplifies the negative time-trend

Considering OLS regressions for slums, we find that all coefficients are not significant. The coefficient for all expenditure is negative. In the individual consideration, all coefficients are positive but not significant, either. OLS estimations might suggest that the possibility of obtaining a microloan has a negative impact at the slum level and a positive impact at the individual level.

Using the advantages of our data, in particular the panel structure at the slum level, we find negative but not significant coefficients for the combined variable in the DiD approach. Furthermore, we learn that a negative trend in the data might exist, as for this and all the following regressions, the coefficients for the time-variable are highly significant and negative. The treatment effect is negative but not significant at the slum level. At the individual level, the treatment effect is negative and the time-trend highly significant negative. Therefore, the individual DiD approach provides us with the same information as the estimations at the slum level. We find (i) a strong negative time-trend, and (ii) a negative treatment effect. Decomposing the results in private expenditure and business expenditure reveals no new information.

As OLS or DiD regressions estimate one average coefficient for the complete range of the distribution of expenditure, we consider quantile estimates to obtain specific impacts for very poor slums or entrepreneurs and rich entrepreneurs or slums. At the slum level, we find a significant negative impact from the microloan program on very poor slums and rich slums. The treatment effect is negative. Correcting the standard errors by bootstrapping shows that standard errors are heavily underestimated for the high quantile. Furthermore, we find significant coefficients for the treat variable, which would cast doubts on the randomization of the experiment. When considering the individual data, we see evidence for the findings in the slums for very poor people. For richer entrepreneurs, we can not find significant results for the treatment variable while it remains negative. To conclude, we see that at the lower end of the distribution and at the upper end of the distribution the impact of microfinance is negative.

When decomposing the effects we find that very poor slums and individuals have negative effects on both kinds of expenditure. At the slum level, the picture remains the same for the other quantiles. However, considering the individual data we find a decrease in private expenditure while business expenditure increases. This might suggest that microloans boost the businesses of individuals. A further interpretation might be that individuals shift their expenditure into business when not facing daily surviving anymore. This result for richer individuals was also found by Banerjee et al. (2009).

At the end, we look at quantile treatment effects to consider the quantitative impact of the opportunity to participate in a program. Here we find that the treated group is better off when in higher quantiles. At the slum level, we have to mention that for slums at the upper end of the distribution the treatment might have a negative impact on their business expenditure. At the individual level, we find that the poor entrepreneurs can not gain from the treatment, whereas richer might be able. It also comes to our attention that entrepreneurs that are accumulating more than a certain level of wealth shift private expenditure into business expenditure.

7 Concluding comments

Overall, we state that we find negative, but mostly insignificant impact of the microloan program. Furthermore, we see a strong negative time-trend over all data. Looking deeper into the results, we find that very poor entrepreneurs are more heavily faced with the negative time-trend and additionally, microfinance has a negative impact. Average entrepreneurs seems to be unaffected by microloans at all. For entrepreneurs at the upper end, we find a negative impact for business expenditure. We suggest to adjust programs more to specific groups of entrepreneurs and their needs. As we also see that poor entrepreneurs use the microloan for consumption, we doubt that microfinance is the right instrument for them. Finding employment could be the right way for very poor entrepreneurs. We propose that microloans given to more advanced entrepreneurs, who can hire very poor entrepreneurs, could be an effective solution for that dilemma.

We have to mention that the negative line that is drawn with this consideration can have many sources. The most obvious might be the short period of the survey. We have only 15 to 18 months between the first round and the second round. Furthermore, we see strong corrections in the standard errors when using bootstrapping methods. Therefore, we have to be careful especially in the higher quantile with overstating the results.

Further research could apply the quantile regression to similar data sets. Additionally, we found out that a third survey round should be available soon. With this we should be able to extend the consideration. While we can obtain an impact of a treatment with these methods, we can not extract the drivers of this model. For that, one could use another method. Todd and Wolpin (2006) use a structural estimation method and find key mechanisms and more effective ways for implementing the treatment. Another advantage of a structural estimation would be that we do not need control groups for evaluating the impact of a program. A question not yet put forward in the literature is how equivalent a DiD approach would be to a structural estimation when it comes to the robustness of the results. Such a comparison could lead to a new and effective method for evaluating the impact of programs and extracting the mechanisms behind them at the same time.

8 Appendix

Derivation of quantile treatment effects

Technically, we see that QTE's are derived from the marginal distributions

$$F_T(\ln Exp) \equiv \Pr[\ln Exp_i^T \leq y] and F_C(\ln Exp) \equiv \Pr[\ln Exp_i^C \leq y] \tag{22}$$

where y denotes the individual expenditure. Following Khandker et al. (2010) these distributions are known. They argue that assuming the definition of the q^{th} quantile from the distribution $F_t(\ln Exp)\,, t = \{T, C\}$ is given as

$$\ln Exp^t(q) \equiv \inf[Y : F_t(\ln Exp) \geq q] \tag{23}$$

the treatment effect for the q^{th} quantile is just the difference in the quantiles of the two marginal distributions (Khandker et al., 2010, p.125). However, to express the differences in that way, strong limiting assumptions about the joint distribution of the two marginal distributions have to be made (see Bitler et al., 2008).

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