Pre-Analysis Plan for "The intergenerational impacts of capital for microentrepreneurs: long-run evidence from a flexible credit contract intervention in India"[*]

Patrick Agte (Princeton)   Arielle Bernhardt (Harvard)   Erica Field (Duke)

Rohini Pande (Harvard)   Natalia Rigol (MSR, HBS)

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[*]Contact information: pagte@princeton.edu; abernhardt@g.harvard.edu; emf23@duke.edu; rohini_pande@harvard.edu; nrigol@hbs.edu.

1   Introduction

This pre-analysis plan is for a study of the long-term, intergenerational impacts of a capital shock intervention for low-income households in Kolkata, India.

Using a field experiment, Field et. al (2013) examined if the repayment requirements of the classic Grameen microfinance contract inhibited investment in high return but illiquid business opportunities among the poor. In partnership with Village Financial Services (VFS), a microfinance institution, Field et. al (2013) selected a sample of 845 women in Kolkata to receive individual-liability loans. The authors compared household and enterprise outcomes for households that received the classic contract (which requires repayment to begin immediately after loan disbursement) to outcomes for households that received a contract with a 2-month grace period. The provision of a grace period increased short-run business investment and, three years post-intervention, household income for treatment group women was 20% higher than for control group women.

Our current analysis centers on data collected through a ten-year follow-up survey of the grace period study households. Seventy-percent of households in our sample had children 0-16 at the time of the intervention. Increased household income as a result of the grace period intervention may have led households to increase investments in the quality and quantity of children's education and access to healthcare. Through improving children's health and educational attainment outcomes, these intergenerational transfers may have had significant impacts on children's later economic activity, marital status, and fertility.

2   Experimental design

We use a randomized controlled trial (RCT) to evaluate our research questions. Starting in 2006, we conducted three field experiments to understand repayment flexibility. In the first (2006 - 2007), clients were randomly assigned to either a weekly or monthly repayment schedule. In the second (2007 - 2008), clients were randomly assigned to either begin loan repayment 2 weeks or 2 months after loan disbursal. In the third (2008 - 2009), the first experiment was repeated with a more heterogeneous sample to measure the interaction between repayment flexibility and client characteristics. The random treatment assignment in the second and third round was stratified by previous treatment assignments to ensure interpretability of the results. Here, we are interested in the long-term impact of the second intervention, the grace period intervention.

Our study was conducted with Village Financial Services (VFS), an MFI that makes individual-liability loans to women in low-income neighborhoods of Kolkata. Between March and December 2007 VFS formed 169 five-member loan groups designated for inclusion in the study, giving us a study sample of 845 clients. Each client received an individual-liability loan varying in size from Rs 4,000 (USD 90) to Rs 10,000 (USD 225) with a modal loan amount of Rs 8,000. After group

formation and loan approval, but prior to loan disbursement, groups were randomized into one of two repayment schedules. Eighty-five groups were assigned to the regular VFS debt contract with repayment in fixed installments starting two weeks after loan disbursement, and 84 groups were assigned an analogous contract that also included a grace period of two months. Other features of the loan contract were held constant: Once repayment began, all groups were required to repay fortnightly over the course of 44 weeks. Repayment occurred in a group meeting conducted every two weeks by a loan officer in a group member's home (on group-meetings also see Feigenberg, Field, and Pande 2012). Both groups faced the same interest charges. However, longer debt maturity (55 as opposed to 44 weeks before the full loan amount was due) combined with the same total interest charges implied that grace period clients faced a slightly lower effective interest rate on the loan. Treatment status was assigned within batches of 20 groups, determined by timing of group formation (the final batch was smaller with nine groups). No clients dropped out between randomization and loan disbursement.

After the 2008 second endline data collection, the 845 clients we re-surveyed in 2010 and 2012 to build a panel data set. Between March and August 2018, we conducted the last round of follow-up, collecting data for 729 of the original 845 clients. As part of the 2018 survey, we also collected biomarkers, health and business outcomes data for all children in the household and detailed education data for all children the respondent ever had.

Seventy percent of households in the sample had children ages 0-16 at the time of the intervention and, in 2018, 73% of households still have children living in their household.

3 Experimental integrity

As reported in Online Appendix Table 1 of Field et al. (2013), treatment and control groups are balanced on key time-invariant client- and household-level characteristics. In the current analysis, we will test for differential attrition rates across treatment and control groups.

4 Empirical Strategy

$$Y_{ihg} = \alpha + \beta T_g + \alpha_g + \gamma X_{ihg} + \epsilon_{ihg}. \tag{1}$$

where Y_{fhg} denotes the outcome of individual i who lives in household h and belongs to microfinance group g. T_g indicates whether the individual was in a loan group which received the grace period treatment and α_g are stratification dummies (consisting of loan group formation date fixed effects). X is a set of baseline control variables selected via the double lasso approach developed by Belloni et al. (2014). Standard errors are clustered at the loan group level. We will present results with and without baseline covariates.

Our primary specification will be at the individual child-level when applicable. We will also

report household-level results as a robustness check.

5   Hypotheses and Outcomes

Our analysis centers on data collected through a ten-year follow-up survey of female clients, their spouses, and their co-residing children. Due to the long gap between our study baseline and this follow-up survey, we are faced with several potential sources of selection bias and measurement error. First, if the grace period intervention impacted levels of child mortality among treated households, results from our main intent-to-treat specification for individual- and household-level outcomes may be biased. Similarly, children in treated households may stay in school longer and be less likely to have entered the labor force which may bias our results on children's economic activity. We will also need to consider possible selection bias on health outcomes if there are effects on children's co-residency status, since we only collected data for child health outcomes for children living in the household.

All hypotheses and outcomes were pre-specified prior to analysis. Since each hypothesis involves assessing impacts on multiple outcomes, our analysis will incorporate corrections for multiple-hypothesis testing.

5.1   Impact on demographic outcomes of client households

We hypothesize that being assigned to the treatment intervention may alter children's demographics and household composition relative to the control group. Household-level outcomes may include the following:

1. Total fertility

2. Child mortality

3. Residency of children within the household

5.2   Impact on children's health outcomes and investments in children's health

We hypothesize that being assigned to the treatment intervention may alter children's health outcomes and investments in children's health. For this part of the analysis, the sample will be restricted to children who co-reside with the client, since health data was only collected for this subsample.

5.2.1   Children's health outcomes may include the following:

1. Number of work/school days missed because of illness in the past 30 days

2. Self-reported health level (scale 1-10)

3. Symptom Index

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4. Illness Index

5. Child height

6. BMI

5.2.2 Health investment outcomes may include:

1. Healthcare expenditures

2. Characteristics, quality and costs of healthcare provider

3. Healthcare utilization, including frequency of visits.

We will test impacts on standardized indexes of sub-outcomes for measures of children's health and investments in children's health.

5.3 Impact on children's educational attainment and households' investments in children's education

We hypothesize that being assigned to the treatment intervention may alter children's educational attainment, households' investments in their children's education, and intergenerational mobility relative to the control group.

Educational attainment and investment outcomes may include

1. Years of schooling

2. Performance and reasons for dropping out of school

3. Cost, quality and quantity of education and extracurricular activities

We will test impacts on standardized indexes of sub-outcomes for measures of educational attainment and investments. The child-level measures of intergenerational educational mobility will be based separately on mothers' and fathers' education levels.

5.4 Impact on children's economic activity

We hypothesize that being assigned to the treatment intervention may impact children's economic outcomes relative to the control group. Measures of economic activity may include labor force participation, occupation, and income. We also collected measures of business outcomes for children who co-reside with the client. For these children, economic outcomes may include existence of enterprises, sector of enterprise activity, and enterprise income, assets, and employees.

5.5   Impact on children's demographic outcomes

We hypothesize that being assigned to the treatment intervention may alter demographic outcomes for children in treatment households relative to children in control group households. Individual child-level outcomes may include marital status, fertility, and child mortality (of clients' children's own daughters and sons).

6   Heterogeneity Analysis

We will analyze heterogeneous treatment impacts over the following categories:

1. Child gender

2. Client's decision-making power in the household

3. Client's completed fertility at baseline

References

Belloni, A., V. Chernozhukov, and C. Hansen (2014). Inference on treatment effects after selection among high-dimensional controls. *The Review of Economic Studies 81*(2), 608–650.

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