Evaluating the Impact of Microcredit on Women's Empowerment in Pakistan

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Preface

Centre for Research in Economics and Business (CREB) was established in 2007 to conduct policy-oriented research with a rigorous academic perspective on key development issues facing Pakistan. In addition the Centre (i) facilitates and coordinates research by the faculty at the Lahore School of Economics, (ii) hosts visiting international scholars undertaking research on Pakistan and (iii) administers the postgraduate programme leading to the M Phil and PhD Degree at the Lahore School.

An important goal of the Centre is to promote public debate on policy issues through conferences, seminars and publications. In this connection, the Centre organizes the Lahore School's Annual Conference on the Management of the Pakistan Economy. The proceedings of which are published in a special issue of the Lahore Journal of Economics.

The CREB Working Paper Series has been started to bring to a wider audience, the research being done at the Centre. It is hoped that these Papers will promote discussion on the subject and contribute to a better understanding of economic and business processes and development issues in Pakistan. Any comments and feedback on these Papers will be appreciated.

Acknowledgments

Abstract

In this paper we evaluate the impact of microcredit on indicators of women's empowerment in the urban slums of the Lahore district of Pakistan. A household level instrument that contains information on different dimensions of household decisions: child related, health, social mobility, economic and major household purchase decisions was specifically designed and implemented to explore the link between microcredit and women's empowerment. After controlling for endogeneity in our estimation by using proxies for initial levels of empowerment, matching the controls and treated units on observable characteristics and finally instrumenting for the treatment, this paper finds no difference between the level of empowerment of treated and control units. Participation in the microcredit program is found to be insignificant in explaining all the outcome indicators of empowerment for the sampled households.

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1. Introduction

Women's empowerment is increasingly being viewed as one of the key constituent elements of the poverty reduction strategy. It is not only seen as a development objective in itself but as a means of promoting growth, reducing poverty and promoting better governance (World Bank, 2001). Specialized microcredit institutions providing loans to poor women all over the world, since the 1980s, have been widely associated with their potential to 'empower' women[1]. The link between microcredit and empowerment, though repeatedly emphasized by donors and practitioners in conferences and summits, is a controversial area of empirical research. Isolating and teasing out the effects of microcredit on empowerment alone is often very difficult to assess empirically. Data sets measuring empowerment are scarce, and the kind of treatment and control groups required to provide significant statistical insights for the differences in household decision making between participants and non-participants are hard to find.

This study attempts such an impact evaluation. It evaluates the impact of microcredit institutions operating in the urban slums of Lahore district of Pakistan on indicators of women's empowerment. Specifically, it explores the effect of giving out small loans to poor women on intra-household decision making. This is made possible by a comprehensive questionnaire designed and implemented by the author, in these areas during last December, to specifically measure empowerment of microcredit clients. This specially designed instrument allows us to

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[1] Mizan (1993) finds significant and positive effects of participation in Grameen Bank program on women's decision making within the household; Hashemi et al. (1996) find that in the context of rural Bangladesh microcredit empowers women in all but two empowerment domains by increasing their economic contribution within their families. The results in Khandker et al. (2003) are consistent with the findings of these earlier studies in Bangladesh that women's participation in micro-credit programs helps to increase their empowerment. Montgomery (2005) finds that participation in Khushali Bank's microcredit program in Pakistan 'increases the degree to which women's decisions are taken into account in child-rearing, community participation and financial matters.'

identify treatment effect of credit on measures of women's empowerment. In doing so it aims at addressing two key questions: *i) what is the effect of microcredit on the empowerment of women in the urban slums of Pakistan? and ii) is this impact uniform across all the domains of household decisions?* The causal effect of credit on empowerment is identified using quasi experimental setting with proxy controls for latent heterogeneity between the control and treatment groups. Matching methods and instrumentation are used to further address concerns for endogeneity of the treatment.

In addressing those questions the research responds to several motivations. Firstly, in the context of Pakistan, where social and economic empowerment of women is still very low, it is important to identify interventions through which this empowerment deficit could be reversed. In the Global Gender Gap Report, published by the World Economic Forum in 2007, Pakistan is ranked at 126 amongst a total of 128 countries. This, in itself, is quite a significant motivation to statistically test the validity of the anecdotal claims made by microcredit institutions about women's empowerment in Pakistan. Secondly, the study coincides with the financial inclusion program being developed by the Department for International Development (DFID) in collaboration with the State Bank of Pakistan, and hence, will provide valuable contributions and insights for determining the future course of financial sector reforms in Pakistan. In recent years, there has been a lot of debate regarding the subsidies and gender-specific approach to microcredit. This impact evaluation will potentially provide valuable insights into this ongoing debate. Finally, urban slums in the Lahore district, being the hub of microcredit activity in Pakistan, are a suitable environment for analyzing impact. Micro finance institutions have existed in Lahore for more than ten years which is a sufficient period to evaluate the impact of any intervention.

Empirically, this is the first independent academic study, to the best of the author's knowledge, which attempts to measure women's empowerment and statistically evaluate the link between credit and women's empowerment in the context of the urban slums of Pakistan. A household level instrument was specially designed and implemented by the author in the urban slums of Lahore to collect the primary data necessary for analyzing the impact of microcredit intervention. Empirical results show that participation in microcredit program has no effect on women's 'say' in all but *one* domain of household decisions, i.e.

decisions regarding major household purchases. However, this positive and significant effect disappears when we instrument for treatment in our proposed model. This result is important for showing that microcredit in the urban slums of Pakistan, at best, is empowering women only in *one* domain of household decision making. Our results suggest that at least in the urban slums of Lahore, the link between microcredit and women's empowerment is not as strong as *perceived* by the donor agencies and microcredit practitioners.

Methodologically, the paper is an attempt to *challenge* the presumption in the literature that cross-sectional data from a non-randomized experimental study cannot be used to draw conclusive inference about program impacts[2]. We recognize the limitations of a single round of non-experimental data in obtaining precise estimates for treatment effects. Ravallion (2006) argues that though "economists have sometimes been too uncritical of their non-experimental identification strategies, credible means of isolating at least a share of the exogenous variation in an endogenously placed program can still be found in practice."A carefully designed research study with knowledge of how the program works on the ground, combined with methods that mitigate differences between control and treatment groups is capable of addressing concerns for selection bias. This study is the first rigorous methodological attempt to address concerns both for observed and latent heterogeneity in analyzing the impact of microcredit institutions without exploiting some particular *anomaly* in intervention design[3]. We claim that the methodology proposed in this study provides reliable estimates of treatment effects within the constraints imposed by the data. The proposed methodology is both resource efficient and cost effective, and unlike other studies can easily be replicated elsewhere.

The paper is organized as follows. Section Two of the study sets out the

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[2] Karlan (2001) and Karlan and Alexander-Tedeschi (2008) argue that the failure to control for attrition, selection and program placement biases in non-experimental set-ting seriously undermine the credibility of estimated results; Glazerman et al. (2003) and Glewwe et al. (2004) find that estimates of treatment in non-experimental settings are biased for impact evaluations of training and education programs respectively.

[3] For instance Coleman (1999) constructed his control group by using prospective clients in villages of Northern Thailand who signed up a year in advance to participate in village banks; Khandker and Pitt (1998) used the eligibility criterion that requires program participants to have less than 0.5 acres of land to identify estimates of treatment effect of credit program participation in Bangladesh.

background and context for this study. It summarizes the evolution of microfinance sector in Pakistan and the primary goals targeted by these institutions. Section Three lays down the theoretical foundation to identify ''direct' and ''indirect' channels through which changes in intra-household bargaining occur through giving small loans to poor women. In the direct transmission mechanism, the ability of a woman in bringing *credit money* to the household is sufficient to increase her worth, giving her greater bargaining power in intra-household decisions. However, the indirect channel emphasizes that the control over resources generated from borrowing money, in fact, leads to greater economic and financial independence of a woman, giving her greater "say" in decision making process. *Empirical testing of these direct and indirect effects of microcredit on empowerment is the main motivation of the study.*

Section Four outlines the empirical methodology for testing this hypothesis. The section sets out the difficulties involved in controlling for biases, and explains the concern for endogeneity in this context. The section conceptualizes treatment measure as participation in a microcredit program, for at least one complete loan cycle. Using a comparable group of prospective clients who have accessed loans for the first time in the past month we are able to estimate the impact of treatment on indicators of women's empowerment. Estimators for treatment effects are outlined. Further, matching and instrumenting is proposed to address problems stemming from endogeneity and simultaneity bias. Section Five describes the data collected from a cross section of two hundred and seventy households sampled for this study. Section Six applies the empirical methodology outlined in section four to the available data; empirical results are discussed. Section Seven concludes.

2.   Context: Microfinance Sector in Pakistan

This section summarizes the concept and practice of microfinance in Pakistan. In doing so, it seeks two aims: firstly, it identities the specialized providers of microfinance services in Pakistan. This is important in measuring impacts, as non-specialized institutions may have other services bundled with microcredit that might explain the resulting impact. Secondly, it provides the background of the microfinance institutions that have been carefully sampled for this study. This background provides important insights into the selection mechanism employed by these institutions and allows the researcher to better understand the concerns for endogeneity and address them accordingly. The institutional

background is also fundamental in motivating the choice of institutions deemed appropriate for measuring impacts. These are the institutions that specialize in microcredit and are predominantly working with solidarity group models of lending to poor women in Lahore.

2.1.   *Evolution and Nature of Microfinance Institutions in Pakistan*

Inspired by the Nobel Peace Prize winning Grameen Bank in Bangladesh there has been a mushrooming growth of microfinance institutions in developing countries; Pakistan is no exception. Growth and diversity in its microfinance sector have been encouraged by the microfinance Ordinance 2001, which resulted in the establishment of the First Microfinance Bank in Pakistan. Before the promulgation of the microfinance ordinance, the main providers of microfinance services in Pakistan were NGOs and government sponsored rural support programs. Table 2.1 summarizes the nature of different microfinance institutions operating in the Punjab province of Pakistan, the year in which they were operationalized and the concentration of operations in rural or urban settings.

It is clear from the table that most of the NGO based institutions have a gender specific solidarity group approach to microcredit, where they are catering predominantly to female clients. Most of the microfinance institutions working in Punjab province, except for the rural support programs, are either specialized institutions or have a specialized window for micro credit. Except for ASASAH, most of the institutions have been in business sufficiently long to warrant an impact evaluation. For this study we are interested in specialized institutions using a group solidarity model with a women-specific focus. Thus, the province of Punjab was an obvious choice with the most well recognized and established NGO based microfinance institution (MFI), Kashf Foundation, operating out of this province. Given the time and resources available for the study, we have limited our attention to the Lahore district of Punjab, which is the hub of urban microfinance activity in Pakistan, accounting for more than half of the total borrowers in the province.

Within the group of NGOs identified to be appropriate for this study, only Kashf Foundation and Community Support Concern (CSC) had agreed to collaborate with the research team and hence the others were

Table 2.1: Microfinance Institutions in Punjab Province of Pakistan

Institution	*Bank/NGO/RSPs*	*Focus on Women*	*Target Areas*	*Starting Year*	*Objectives*
Khushali Bank	Bank (Specialized)	No	Rural and Urban Slums (All Provinces)	2000	Operational and financial self sufficiency
KASHF Foundation	NGO (Specialized)	Yes	Urban slums (Punjab)	1996	• Women Empowerment • Poverty Alleviation
ASASAH	NGO (Specialized)	Yes	Urban slums (Punjab)	2003	• Financial Sustainability • Poverty Alleviation
DAMEN	NGO (Specialized Microcredit Division)	Yes	Rural (Punjab)	1996	Microcredit for Poverty Alleviation
AKHUWAT	NGO (Specialized)	No	Urban slums (Punjab)	2001	Poverty Alleviation
Community Support Concern	NGO (Specialized Microcredit Division)	Yes	Urban slums (Punjab)	1999	Microcredit for Women Empowerment
National Rural Support Program	RSP	No	Rural (All Provinces)	1991	Multi-Sector Development Program: Health, Education and Microcredit for Social Mobilization
Punjab Rural Support Program	RSP	No	Rural (Punjab)	1998	Replication of NRSP at Provincial Level: Integrated Multi-Sector Approach
First Microfinance Bank	Agha Khan RSP transformed into a Bank	No	Urban and Rural (All Provinces)	2002	AKRSP Development Agenda: Sponsors' mandate
Center for Women Cooperative Development	NGO	Yes	Urbanslums (Punjab)	1999	Economically Empower Poor Communities: Poverty Alleviation

Starting year corresponds to the year in which a specialized microcredit division was operationalized in case of multi-sector development NGO.

left out[4]. Moreover, it should also be noted that Khushali Bank does not have a gender-specific program in rural areas but works with a third party retail organization, Family Planning Association of Pakistan (FPAP), to give loans to poor women in urban slums of Lahore. However, in an earlier survey of Khushali Bank clients, the non-repetitive nature of Khushali Bank's group lending model created difficulties in chasing out a large number of old inactive borrowers, creating a strong attrition bias. Moreover, it is also difficult to isolate the impact of microcredit from other programs run by FPAP, and the selection effects are likely to be stronger with cultural stigma stemming from an association with a family planning institute. Learning from these reasons identified in an earlier survey conducted by the author, in August 2006, Khushali Bank clients were strategically left out for this study.

2.1.1. KASHF Foundation

Kashf Foundation, a non-profit NGO based MFI, started its operations in Lahore District in 1996. Kashf started with the mission to 'provide quality and cost effective microfinance services to low income households, especially women, in order to enhance their economic role and decision making capacity[5]. Kashf provides four types of loan products: firstly, is the general loan; secondly, it has an emergency loan which is confirmed only if the credit committee takes responsibility for repayment; thirdly, there is the home improvement loan for old and reliable clients; and fourthly, Kashf has introduced a business loan for the missing middle market. The most popular one is the general loan, which has to be repaid over a period of 12 months at a flat interest rate of 20%. For our study we limit our attention to the impact of the general loan taken by all participants on outcome indicators of empowerment.

Kashf's solidarity group lending model is a Grameen Bank replication with some adaptations. At the first stage, the branch officers find women in the local area who want to establish a center. A 'center' is defined as a group of 25 women who take the collective responsibility for loan repayment. The center is sub divided into five groups. Each group has a group leader, and there is one center manager and center secretary. Together, these seven women form the credit committee and are

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[4] The timing of this research coincided with the year-end closing of these institutions which made it difficult for them to participate in this research exercise.
[5] Kashf's mission statement available at kashf.org.pk

responsible for maintaining credit discipline in the center. According to the loan officers (LOs), when people see a successful center being run in their neighborhoods, they try to approach them to set a center in their respective houses. The clients are assisted by the loan officers in selecting the clients. From the pool of potential clients the loan officers screen out the ones failing to fulfill the eligibility criteria. For instance, as a general rule, not more than five to seven clients on rented accommodation are admitted to be part of the center. Moreover, the National ID card number is required, though this requirement has been relaxed in the past couple of years with intense competition from rival MFIs entering the market. Once the initial screening is done by the LOs, application forms are filled and verified by the pair LO.

Based on our observations from the field, the credit committee looks for women who have some dependable source of family income and sound reputation for credit worthiness within the neighborhood. It is obvious that the process of client selection is non-random, and studies by Coleman (2002) and Hashemi (1997) suggest that microcredit clients might have some initial advantages over non-clients: for instance women selecting themselves into the program might be more enterprising and hence independent, as compared to non-participants, and the impact of microcredit on empowerment might be overestimated without controlling for these initial biases.

2.1.2.   Community Support Concern (CSC)

CSC is a non-governmental, non-profit organization established in 1989 with a mission to provide quality health and social services to underserved communities. One of the core objectives outlined by CSC is the empowerment of women. In 1999, the organization launched its microcredit program, as part of its commitment to work towards the training of women as agents of socioeconomic development. CSC provides only one type of loan to its clients: the basic general loan for a twelve month period with a flat rate of interest of 20%. The only difference between Kashf and CSC is that the latter operates on a monthly repayment schedule as opposed to the bi-weekly payments administered by Kashf. Since microcredit is *one* of the many independent programs run by CSC, the scale of operations is relatively small with the total number of active borrowers for CSC standing at 7,525 compared to more than 100,000 borrowers for Kashf. This is important for including institutional

level dummies to account for the possible heterogeneity of treatment stemming from scale differences between the two institutions.

CSC follows a very similar group solidarity model as used by Kashf, and the only possible difference is in the group size. Unlike Kashf, the CSC's groups vary in size from 15 to 25 members and are far more geographically dispersed. It was observed during the field survey, that the loan officers were colluding with influential local agents who were responsible for finding clients and setting up centers. There were also reports that the local agent, who sometimes happened to be the center manager, charged commission from the people selected into the program. It was also the case that the same center manager was running centers for more than one microfinance institution. Thus, in the case of CSC, it is more likely that the people who select themselves into the program are those who need credit the most, as they are willing to bear the extra commission costs to take up the loans.

3. Conceptual Framework

3.1. Motivating Theoretical Framework

This section draws on the existing literature to develop a theoretical framework for testing the link between credit and empowerment. Though women's empowerment defies a 'universal' definition that could be applied across all disciplines, there is greater consensus on its conceptualization in literature than expected. Often the range of overlapping terms used in the literature including choice, control and power, refer simply to women's ability to make decisions and affect outcomes of importance to themselves and their families. Control over one's own life and resources is often stressed (Malhotra et al., 2002). From this growing literature a common concept of women's empowerment can be distilled, and is used here: women's empowerment is defined as the expansion of choices for women and an increase in women's ability to exercise those choices in the household (UNDP, 1995).

3.1.1. Collective Household Model

Improvements in women's access to credit through microfinance institutions expand this 'choice set' available to women by providing them with greater access to economic opportunities. This leads to

economic empowerment that transpires into greater bargaining power in the intra-household decision making process. Khandker et al. (2003) draw on the 'collective household model' to motivate this change in gender relations brought about through access to credit. In this simple collective household model the two partners look for a pareto equilibrium (Browning and Chiappori, 1998). They maximize their own utility taking into account the utility of the other partner. Thus, the household simply optimizes a weighted utility function of the form:

$$\Omega = \theta u_1(x) + (1 - \theta)u_{2(x)}$$

where $\theta \in (0,1)$ stands for the relative power of the female compared to male partner in the household decision making process; $u_1(x)$ captures the preferences of the female over a bundle of consumption goods 'x', and $u_2(x)$ are the male preferences over the same consumption bundle.

The weight 'θ' can be thought of as representing the power distribution within the household. With '$\theta = 0$' women's preferences are given no weight and the household collective utility function is identically that of the male. Thus, the parameter 'θ' indexes the extent to which women's preferences are reflected in intra-household decision making. These preference weights in turn determine resource allocation within a household. The power coefficient is identified in the collective model as a scalar valued function of intra-household power distribution (Browning and Chiappori, 1998). This means that the estimate of the weight parameter 'θ is assumed to reflect women's bargaining power in all the domains of household decisions and is indexed by the same value of 'θ' across *all* household decisions.

3.1.2.   Micro Credit and Women's Empowerment

Most of the empirical literature recognizes that the value of power coefficient 'θ' is increasing in the relative share of women's earned income in the household [see Duflo and Udry (2003), Anderson and Eswaran (2005) and Basu (2004)]. Credit is seen as a critical input for increasing the employment of women in homestead income generating activities or enhancing the productivity of women's enterprizes through the adoption of an improved technology. In either case there is a likely increase in the share of female earned income that manifests itself in greater 'power' within the decision making process. Thus, it is claimed that access to microcredit through increasing women's income leads to empowerment.

Goetz and Gupta (1996) point to another less developed link in the literature on credit and empowerment. They argue that the ability of women in bringing credit--a valuable and productive resource to the household--may enhance their position within the family, resulting in economic empowerment. However, they completely ignore this *lead* in their empirical approach of measuring and quantifying empowerment. Using an index reflecting the degree of control the women have on the loans that they take, they conclude that most women have minimal control over their loans. We agree with the authors insofar that credit might be fungible within the household, resulting in a lesser degree of control over loan use; but it remains to be seen empirically whether this fungibility translates into a lesser degree of 'autonomy' within decision making process. This study tests this hypothesis across different dimensions of decision making.

The literature surveyed suffices to provide the theoretical foundations for empirically testing the link between microcredit and women's empowerment. Drawing on the key insights from the literature, Fig 3.1 describes the three channels through which we expect microcredit to improve the position of women within the household: i) firstly, empowerment brought about by increases in independent income; ii) secondly, empowerment through 'control' over credit and savings decisions; and iii) thirdly, through the increased worth of women within the family, stemming from their ability to bring a 'valuable' asset to the household economy.

The identification of these three mechanisms in generating 'empowerment' effects is critical for motivating the research design for this study. The focus on any one of these mechanisms alone will fail to provide a complete perspective on women's empowerment. For any empirical study that aims to evaluate the empowerment potential of credit, it is necessary to look at the 'outcomes' associated with the empowerment effects rather than looking only at the 'processes' through which those empowerment effects were generated. Process based indicators like the accounting knowledge (Ackerly, 1995) and managerial control (Goetz and Gupta, 1996) do not suffice, on their own as evidence for or against empowerment (Kabeer, 2001).

Figure 3.1: Framework of Women's Empowerment through Microcredit
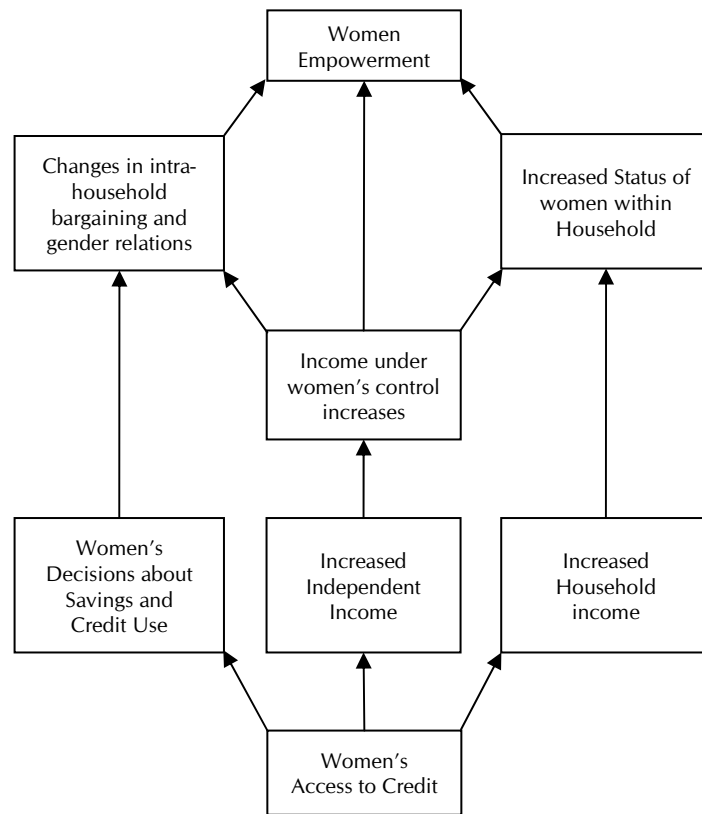
3.1.3.   Previous Empirical Studies

There is very little empirical evidence in the literature on the effect of credit on the 'outcome' indicators of empowerment. The two widely cited studies are Hashemi et al. (1996) and Khandker et al. (2003). Both of these studies find significant positive effects of membership in Grameen Bank and BRAC on empowerment. There are two theoretical challenges that a good empirical assessment should aim to address: firstly, the problem of selection bias and secondly, the validity of outcome indicators of empowerment. Hashemi et al. (1996) score well on the second where their indicators of empowerment including female mobility in public domain, ability to make large and small purchases, ownership of productive assets, involvement in decision making and the ability to make choices regarding money and travel decisions are valued outcomes in their own right and are linked to the underlying structures

of constraints that give rise to gender inequality in rural Bangladesh (Kabeer, 2001). However, the authors acknowledge the methodological limitation of the study in their failure to address unobserved heterogeneity, or simply the fact that the endogenous decision to join the program may be correlated with the unobservable empowerment endowment, which could seriously bias the results [see (Signe-Mary, 2001)]. Moreover, they also fail to account for any village-level heterogeneity between program villages and non-program villages[6]. The study by Khandker et al. (2003) does better on the methodological rigour. They take advantage of the program design of microfinance institutions in the context of rural Bangladesh to identify unbiased estimates of treatment effects. They exploit the exogenous eligibility rule used by the Bank to select their clients in designing the sample of eligible and ineligible households in the program villages. This eligibility rule serves as a key identifying assumption for disentangling the effects of credit on outcome variables from the confounding village level fixed effects. Their exogeneity assumption has come under criticism in the literature [see (Morduch, 2005) and (Ravallion, 2006)].[7] As far as the validity of outcome indicators is concerned they use a considerably exhaustive list of variables that capture almost all the different dimensions of women's empowerment mentioned in the literature. Building on their earlier work they report positive and significant effects of credit on indicators of empowerment. It must be noted however that the conclusions drawn from their work rest critically on the identifying assumptions used, and their methodological approach is not practically replicable in other settings[8].

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[6] Khandker et al. (2003) find strong evidence of program placement bias in their sample. A fixed effects model at the sub-district level is even insufficient in correcting for the significant heterogeneity bias in this study.

[7] Their estimate of the impact of participation in the credit program is actually an estimate of the `returns to land of taking away village level access to Grameen Bank. It is perfectly plausible to imagine that the Grameen Bank might have targeted villages with unusually low returns to land which might explain why they were poor in the first place. These people with non-farm activities were an obvious choice for Grameen style

[8] All three programs in Pitt and Khandker (2005) study define eligibility in terms of land ownership: only functionally landless household owning less than half an acre of land could borrow from this. This quasi-experimental setting is seldom available with programs implemented elsewhere and even the actual implementation of this eligibility criterion in Bangladesh has been questioned by numerous authors [see (Hossain, 1988); (Morduch, 1998); (Roodman and Morduch, 2009)]

3.2.   Motivating Methodological Framework

In this study we suggest and implement a method that addresses the concerns about selection bias in a cross sectional setting. The ex-ante knowledge on the determinants of program placement both on the sides of the program administrator and participants allows us to convincingly capture these determinants with a single round of data collection. We obtain a relatively clean estimate of treatment effects in these constrained settings using propensity score matching and instrumental variable methods. As far as the validity of outcome indicators is concerned, we rationalize the selection of these variables both from the theoretical perspective and the underlying structural gender inequalities prevalent in the country.

We want to estimate the impact of participation in the credit program against the counterfactual of how those same individuals would have fared in the absence of the program. A typical formulation of evaluation problem follows Rubin (1974) in postulating two possible outcomes for individuali; the value of Y_i under treatment is denoted by Y_i^1 while it is Y_i^0 under the counterfactual of not receiving the treatment. The gain from treatment or the 'causal' effect of participating in the program for individual i is $G_i = Y_i^1 - Y_i^0$. In the context of this study, we are interested in estimating the average gain in empowerment outcomes from treatment on those who participated in the program, conditioning on the covariates X, which might also influence the outcome. This conditional mean impact is called the Average Treatment Effect on the Treated (ATT) in the literature and is ideally measured as: $E(G \backslash X, T = 1)$.

This is obviously problematic in practice as it is not possible to observe the same individual in a program to be a participant and non-participant simultaneously. In settings, where intervention has already taken place, the researcher must settle for estimates of the average impact of the program on the participants–treatment group compared with a credible comparison group–control group. The single difference in mean outcomes between program participants and the control group fails to give us an unbiased estimate for treatment effect as shown in the equation below:

$$
\begin{aligned}
D &= E[Y^1|X, T = 1] - E[Y^0|X, T = 0] \\
&= E[Y^1 - Y^0|X, T = 1] + E[Y^0|T = 1] - E[Y^0|T = 0] \\
&= ATT + \text{Selection Bias}
\end{aligned}
$$

In the context of our study, there are three potential sources of selection biases in measuring program impact: firstly, program participants are likely to be different from their control comparators in terms of their distribution of observed covariates, which is known in the literature as 'selection on observables'. Such a bias is likely to arise because of any changes in the screening criteria used by microfinance programs over the year[9]. We control for this selection on observable in two ways. First, in the sample design, we construct a group of pipeline borrowers. In doing so, we compare the treatment group of participants with a comparison group of incoming borrowers who have applied for microcredit but have not yet received the loan. The prior selection process means that the successful applicants will tend to have similar observed characteristics whether or not they have actually received the treatment. Moreover, both incoming and old borrowers are purposively drawn from the same geographical area so that they do not differ much in terms of the socioeconomic and demographic characteristics. Secondly, as discussed in the next section we use both regression methods and propensity score matching to control for observed differences between program participants in the control and treatment groups. These approaches provide an unbiased measure of program impact under the assumption of 'ignorability of treatment', whereby the preprogram outcomes are assumed to be independent of treatment, conditional on the controls used in regression or matching.

The second source of bias is that the treatment group might differ from the control group in the distribution of unobserved characteristics. In the context of this study the person-specific unobserved determinants of empowerment might give rise to such a bias[10]. Invoking a political economy argument, it is perfectly plausible that the institutions aspiring to show empowerment potential of their programs to attract subsidies from the donors were initially targeting relatively poorer women with a lesser degree of autonomy. This emphasis of targeting less empowered women might have changed over time as institutions became more self-reliant and less dependent on donor subsidies. If this is the case, then selection into the program is determined by the pre-existing level of women's empowerment

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[9] Qualitative interviews of branch managers and field staff, and, screening manuals didn't reveal any significant changes in client selection methodology over the years.

[10] The correlation between the treatment and location specific unobserved determinants of empowerment did not pose much of a challenge for this study because both the program participants and the comparison group are drawn from the same geographical location ruling out any unobserved location specific heterogeneity.

and autonomy, which cannot be controlled for with the observed data. Other factors contributing to unobserved heterogeneity might include unobserved attitudes, and characteristics of husbands and in-laws.

Again, we try to control for selection on unobservables in three ways. First, the pipeline comparison method to some extent addresses the problem of latent heterogeneity (Ravallion, 2006). Since, both groups are self-selecting themselves into the program, it is argued that they might have similar distribution in terms of unobserved characteristics as well. However, this argument ignores the time variant latent heterogeneity stemming from the timing of the decision to join the program. There may be unobservable reasons why incoming clients may differ from the clients who chose to enroll in the program at an earlier date. It could be argued that women who were more autonomous and empowered might have been the ones that joined the program earlier. Second, we use an observable proxy to capture pre-existing levels of empowerment for both the treatment and control groups. Third, we use an exogenous variable uncorrelated with the errors of the structural equation to instrument for the treatment. The econometric details of these methods are spelt out in the next section.

A final source of bias can arise if participation in the program has positive externalities on those who joined the program at a later date (control group). Such a bias is likely to be inconsequential for the current study as most of the clients in the control group are located at a fair distance from the treated locations. This rules out any possibility of shared businesses and economic links amongst the clients in the two groups. In the absence of such economic links it is improbable that incoming borrowers will have any effect on empowerment indicators with simple access to microcredit services. Hence, in the context of this study we can confidently assume that such bias, if it exists is negligible.

3.2.1. Methodological Framework

This study estimates the conditional demands[11] for empowerment indicators conditioned on the individual's participation in the credit program. The reduced form system of equations that we want to estimate are:

$$C_i \; = \; \alpha_c X_i \; + \; \pi Z_i \; + \; \epsilon_i \qquad\qquad (3.1)$$

$$Y_i \; = \; \alpha_y X_i \; + \; \delta_y C_i \; + \; \epsilon_j \qquad\qquad (3.2)$$

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[11] Following the terminology used by Khandker et al. (2003).

Where C_i is the binary treatment measuring participation in the program, X_i is a vector of household and borrower's characteristics (age, education, children, etc.), Y_i is the conditional demand for women's empowerment outcomes (such as level of involvement in the decision making or ability to make decisions without someone's permission), Z_i is the set of exogenous instruments distinct from $X's$ that affect C_i but not household behavior Y_i conditional on C_i, α_c, π, α_y and δ_y are the parameters to be estimated and ϵ_i and μ_i are errors representing unmeasured household level characteristics that determine participation in the program and outcomes respectively. 'δ_y' is the parameter of interest that measures the impact of participation in the program on the outcome. The estimation problem arises because of the possible correlation between ϵ_i and ϵ_j due to the endogeneity of treatment. In the absence of exogenous instrument vector Z_i, strong assumptions are required to address concerns regarding endogeneity.

3.3. Outcome Indicators

Finally, we justify the empowerment indicators used to measure and quantify the outcome variable 'Y' in the above specification. We quantify how decisions in the household are arrived at, rather than simply focusing on who is responsible for making the decisions. In doing so, we are interested in measuring the extent to which women's preferences are reflected in household decision making. This attempt draws inspiration from an earlier work by Pitt and Khandker (1995) where they attempt to capture intra-household power relations by giving weights to the preferences of different household members in decision making. Here, we develop the literature, and allow our respondents to rank, in an ordinal manner, the extent to which her preferences are taken into account in intra-household decisions. This approach addresses the two key concerns raised in the literature in conceptualizing women empowerment. First, if the women voluntarily transfers the loan to her husband and has no managerial control over loan use, we are still able to ascertain 'degree of empowerment' from her level of participation in household decision making. Second, this approach tries to address concerns for 'structural deductivism' where male dominance within the household is deduced from the formal structures prevalent in the society rather than based on direct evidence of how power is exercised within a household (Kabeer, 1999). In the context of the urban slums of Pakistan there are certain structural inequalities and prescribed social norms that reinforce the subservient

position of women within the household. We are not setting our hopes unrealistically high in expecting microfinance institutions to completely reverse these structural inequalities prevalent in the society. What we expect, however, is that access to credit will help women in contesting those prescribed gender rules. And, this could be measured by changes in the degree of female participation brought about by credit in decisions concerning household and women's welfare. We use ordered probit framework to capture these ordinal preferences in our estimation strategy detailed in the next section.

For the sake of completeness we also use the traditional definition of autonomy to see whether our results are consistent across both sets of questions. These questions follow the standard framework of the feminist paradigm where we measure empowerment in terms of 'individuality' and independence in the decision making process (Evans, 1977). To measure this degree of autonomy we ask our respondents whether they require the permission of any member in the household in making decisions about purchasing things for themselves, their children and the household. Starting from very basic decisions like purchasing grocery for the household we move to higher order decisions such as the purchase of durable goods and assets. This allows us to empirically test our earlier conjecture that prescribed norms of gender inequalities are unlikely to be affected by participation in the credit program. As independence in decision making is measured as a binary variable we use the probit framework to analyze these set of questions.

3.4.   Conclusion

Thus microcredit improves the position of women within the household through i) access to independent income, ii) control over savings and credit use, iii) ability to bring productive asset to household economy. These three channels of women empowerment necessitates looking at the 'outcomes' associated with empowerment instead of focusing on the processes of empowerment alone. This analysis is used to motivate the empirical testing of the link between participation in credit program and outcome indicators of women empowerment. The section seeks to address the concerns for all possible sources of selection biases that could distort our estimation. Finally, the choice of empowerment indicators used for this study are given theoretical and structural contextualization.

4. Testing Strategy

This section explains and justifies the econometric methodology used to test the hypothesis of interest. First, it briefly outlines the hypothesis and the estimation strategy used to test it. The latter section discusses and justifies the assumptions and methodologies used to obtain consistent estimators to test the desired hypotheses.

4.1. Preference Based Indicators

We test the hypothesis whether participation of women in microcredit program increases their 'say' in:

1.1:   Child Related Decisions
1.2:   Health Related Decisions
1.3:   Economic Decisions
1.4:   Social Mobility Decisions
1.5:   Resource Allocation Decisions

The ordered outcome variables measuring the extent to which their preferences are reflected in these domains of household decisions are regressed against the treatment dummy using an ordered probit model. IV method and PSM is used to check the robustness of our ordered probit estimator.

These hypotheses empirically test the *immediate* changes in gender relations brought about by participation in a microcredit program. We expect that if the position of the women within the household has improved after taking a loan then power distribution should change across some domains of decision making within the household.

4.2. Autonomy Based Indicators

We test the hypothesis whether participation of women in microcredit program gives them *independence* in[12]:

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[12] As reported earlier there is limited variation in decisions relating to major household purchases. We tried to pool the data on these questions but still the variation was too little to properly identify the model. The descriptive statistics make it clear that these decisions clearly fall within the ambit of male-dominated decisions; only a handful of women in our sample claim to make these decisions independently.

2.1.   Making Small Household Purchases.
2.2.   Child Related Decisions.

A binary treatment dummy is used to capture participation in the program. The outcome variable measuring women's ability to make independent decisions (whether they require someone's permission or not for a number of decisions concerning their own and the household's welfare) for both set of decisions are regressed upon the treatment dummy and household controls using probit model. Robustness of our estimators is checked using PSM and IV methods.

These hypotheses go a step further than the ones reported earlier. They test whether microcredit is enabling women in contesting their way out of the prescribed norms of gender inequality embedded in the society. They serve as a formal test of our earlier intuition that interventions like microcredit only empower women within the constraints of patriarchy imposed by the society.

The following section details the econometric methods used to test these hypotheses.

4.3.   Estimation Strategy

We use three different estimators and find that our results are robust across all methods used for estimating the impact. We obtain our estimators using the class of probit and bivariate probit models as outlined in the following sections.

4.3.1.   Regression Methods

First, we use a simple parametric framework of conditional mean independence in order to measure impact. As outlined earlier, the endogeneity of treatment in our specification necessitates making the following assumption:

Assumption 1.1
Ignorability of treatment

Conditional on observed individual characteristics X_i, outcomes (Y^0, Y^1) are independent of the treatment T. This assumption implies conditional mean independence of outcomes:

$$[E(Y^0|X,T) = E(Y^0|X)] and [E(Y^1|X,T) = E(Y^1|X)]$$

Assumption 1.2
Stable Unit Treatment Value Assumption (SUTVA)

Treatment of unit 'i' affects only the outcome of unit 'i' (Wooldridge, 2002). This assumption rules out any general equilibrium effects where the treatment of one unit affects another's outcome. By assuming that we have an independent and identically distributed random sample of treated and controlled units from the population we satisfy SUTVA. The stronger assumption of random sampling used in turn implies SUTVA (Wooldridge, 2002).

These assumptions *must* be valid for the simple parametric approach to give us an unbiased estimator of the program participation on women's empowerment. In the absence of a random assignment of treatment this weaker assumption of 'ignorability of treatment' allows us to consistently estimate the ATT in a cross sectional data. There are no formal tests available, however, to test the validity of this assumption. We motivate the estimation strategy by imposing a linear relationship for conditional mean independence; the two outcome regimes of interest are:

$$E(Y^0|X) = \gamma_0 + \beta_0(X - \bar{X}) \tag{4.1}$$

$$E(Y^1|X) = \gamma_1 + \beta_1(X - \bar{X}) \tag{4.2}$$

Where X is the vector of covariates with the average value $\bar{X}$ in the treated population.

In this generic heterogenous treatment effects model we allow both the parameters of slope (β_0, β_1) and intercept term (γ_0, γ_1) to vary across the outcome regimes. This switching regression model can be reduced to a single equation giving the expected empowerment outcome Y conditional on X,T:

$$E(Y|X,T) = \mu_0 + \alpha T + \beta X + T(X - \bar{X})\delta \tag{4.3}$$

Where $Y = (1 - T)Y^0 + TY^1$ is the observed outcome (equal to Y^1 for participants in the treatment group and Y^0 for participants in the control group). For the case when Y is binary, running a probit regression of Y on X, T and $X - \bar{X}$ we can consistently estimate the parameters and obtain an estimator for the $ATE(X)$:

$$\widehat{ATE}(X) = \hat{\alpha} + (X_i - \bar{X})\hat{\delta} \tag{4.4}$$

If we assume a 'common impact model' or homogeneity of treatment across the covariates then the interaction terms in the equation simply drop out and the parameter $\hat{\alpha}$ measures the mean impact of participating in the program. In our context we maintain 'common impact' assumption across all participants[13].

4.3.2.   *Propensity Score Matching*

The closest possible we can get to randomization of treatment in a non experimental observational study is propensity score matching (PSM). Intuitively, the method matches treated units with control compatriots having close to identical values of a scalar-valued function of covariates, measuring the likelihood of participation in the program. In other words, PSM ensures that the conditional probability of participation in the program is uniform between participants and matched comparators. Where as in a randomized trial the participants and non-participants are identical in terms of the distribution of all the characteristics, whether observed or not.

Using a pooled sample of control and treatment groups we estimate a flexible probit model by regressing treatment T_i on covariates Z_i and functions of covariates. The estimated parameters from this regression are used to predict $\hat{P}(Z_i)$ the propensity score for treatment assignment.

$$\hat{P}(Z_i) \ \equiv \ F(Z; \hat{\gamma}) \tag{4.5}$$

where $\hat{\gamma}$ is obtained from a probit regression of T_i on Z_i[14].

Propensity Score Matching uses the values of $\hat{P}(Z)$ to select matching comparison units. The estimates of the program impact are highly sensitive to the choice of the matching method used to construct comparison group (Smith and Todd, 2001). However, Smith and Todd (2001) go on to demonstrate that the robustness of estimators can be enhanced by restricting the matches only to those units in the control and treatment group who have a common support in the distribution of propensity scores. We eliminate all the cases in the control and

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[13] We are only looking at the female participants of microcredit institutions for our study. There is no reason to believe that there will be heterogenous treatment across subgroups of sampled clients.

[14] *Zi* includes pre-treatment control variables and function of these covariates. Also note that the model is intentionally over-parameterized using many variables, quadratic terms and interaction terms to improve the predictions of treatment assignments, critical to matching methods.

treatment group that lie outside the common support condition. We further check robustness of our estimates by using both kernel and radius matching methods.

4.3.3. IV Method

In the case where our treatment and outcome variables are both binary we use the bivariate probit model to estimate program impact. Our bivariate probit model with endogenous treatment is properly identified since we have one exogenous regressor 'distance from the branch', which is correlated with the timing of the decision to participate in the program, but is uncorrelated with indicators of empowerment. The endogenous model in reduced form is:

$$C_i = \alpha_c Z_i + \epsilon_i \tag{4.6}$$

$$Y_i = \alpha_y X_i + \delta_y C_i + \epsilon_j \tag{4.7}$$

Where Z_i is the vector of observables and the system is properly identified if at least one variable in Z_i is not contained in X_i.

To allow for the possibility that the unobserved determinants of participation in the program are correlated with the unobserved determinants of women empowerment we assume that ϵ_i and ϵ_j are distributed bivariate normal with $E(\epsilon_i) = E(\epsilon_j) = 0, Var(\epsilon_i) = Var(\epsilon_j) = 1$ and $Cov(\epsilon_i, \epsilon_j) = \rho$. For the case where both treatment and outcome variables are dichotomous there are four possible states $Y_i = 1$ or $Y_i = 0, C_i = 1$ or $C_i = 0$. The likelihood function corresponding to these set of events is therefore bivariate probit. And, for the case where outcome variable is ordinal, the appropriate model is bivariate ordered probit. We obtain parameters of our model by a maximum likelihood estimation of bivariate probit model with the distance of the client from the microfinance branch used as an identifying instrument.

4.4. Robustness

The triangulation of the results computed using different econometric methods, in itself serves as a robustness check for the estimates reported for the underlying model. However, as stated earlier, the validity of all these estimators hinges critically on the identifying assumptions used for

each of the three methods outlined above. This section specifies the econometric tests used to check and justify the use of these assumptions.

4.4.1.   Testing the Assumptions for PSM

The key assumption in estimating ATE requires that the propensity score satisfies the common support condition. The idea for testing this assumption is to inspect the multivariate covariate distribution of propensity scores for both the control and treatment group. We assess the common support condition by plotting the propensity scores estimated for both the treatment and control group. If there is considerable overlap in the covariate distribution then both the groups are similar in terms of observed characteristics.

Moreover, we test for any systematic differences between the treatment and comparison groups constructed by propensity score matching. We use the balancing test developed by Smith and Todd for this purpose. The 'balancing test' reveals whether the comparison group constructed with PSM sufficiently resemble the treatment group, by testing whether the means of observable variables are significantly different (Smith and Todd, 2001). Within each block of treated and control matches constructed by PSM, a t-test of equality of means is conducted for each variable included in the probit for participation equation. A non-rejection of the null implies that there are no significant differences between the treated and comparison units in their observed characteristics.

4.4.2.   Testing the validity and informativeness of IV

The variety of statistical procedures and matching methods outlined above only account for one type of selection bias that is 'selection on observables'. In the absence of any formal tests to check the validity of unconfoundedness assumption we still fear for endogeneity stemming from latent heterogeneity. The instrumental variable method outlined earlier is the standard approach for testing the robustness of our single-equation model.

As outlined earlier we use distance of the client from microfinance branch as an instrument to identify our bivariate endogenous model. Our results rest critically on the validity of this instrument; that is i) the distance of a client from the branch of an MFI strongly determines the *timing* of the decision to join the credit program; ii) and it is not a determinant of any empowerment outcomes associated with participation in the program.

The instrument must not be correlated with the error term 'ϵ_i' in the outcome equation. It is easy to test the first assumption by using a probit model to regress the participation in the program on instrument and conditional determinants of the outcome that are explicitly controlled for. If the coefficient on the instrument is statistically and economically significant then the instrument is informative.

The second part of the assumption is technically untestable as the error is unobserved. Thus the credibility of our results for bivariate probit turns on our assumption that borrowers who are a few kilometers away from the branch, are no more likely to be empowered than otherwise identical borrowers who are closer to the branch. The design of the study ensures that the structural dynamics of the area are similar for both the control and treated units as both groups are sampled from the same geographical area. Nevertheless, there still remains a possibility that the distance might capture some neighborhood level fixed effects not captured by the model that might drive the empowerment effects.

5. Data

The empirical analysis in this study draws on the primary data for this study collected by the author in December 2007. This section summarizes the research design for the field work carried out for the study. The nature of participating institutions lends itself to a quasi-experimental framework where prospective or new clients are used as a comparison or control group. In doing so, the outcomes for the mature clients are compared with this control group to ascertain impact. The first section details the sample selection process. The second section motivates questionnaire design based on the conceptualization of women's empowerment outlined in the conceptual framework. And, the final section details the actual implementation of the survey in the urban slums of the Lahore district of Pakistan.

5.1. Sampling Framework

Primary data was collected from 275 households in the Northeastern regions of Lahore District. The target population comprises all women registered with the Kashf Foundation and CSC in the areas of Shalimar Town and Aziz Bhatti Town, where the microfinance activity is concentrated within the Lahore District.

A stratified random sample of 275 households was drawn from the population of 10,000 female clients, with approximately 8000 Kashf clients and 2000 CSC clients. The sample of 275 clients with 225 clients of Kashf Foundation and 50 clients of CSC represented the proportion of Kashf and CSC clients in the population. The sample has further been divided into treatment and control groups. The people belonging to the treatment group are the mature clients. The minimum criteria for being counted as mature client are that the respondent must have completed at least one loan cycle with the MFI. The people belonging to the control group are prospective clients registered with the MFI in the month of November or December. For about every three people in the treatment group we have one person in the control group. We have a total of 196 people in the treatment group and 75 people in the control group. The reason why control group comprises the potential clients of the microfinance institution instead of non-clients is because there may be initial differences between the client and non-client samples. These initial differences may be due to tangible and intangible factors, such as household income, entrepreneurial ability, and degree of female empowerment. The potential for these initial differences arises in research situations, where the individuals studied are free to choose to participate in the program assessed. Also having a comparison of potential clients versus mature clients, allows us to assess the different degrees of exposure to the program and how it has impacted the clients' lives. A drawback with requiring the control group to comprise potential MFI clients is that this necessitates that both the new and mature clients come from similar types of communities and have similar characteristics. As not all the clients within the control group will remain with the program, and there will be drop outs over time, it is important to include both the active and inactive clients of MFIs, to make both groups representative of the underlying target population. Moreover, by including inactive clients of MFIs who were either in default or have completely dropped out from the program, this study tries to address the concerns for attrition bias-where the 'failures of the program' are systematically dropped out from the research design as pointed out by Karlan and Alexander-Tedeschi (2008).

While doing the research we realized that the microcredit market in Lahore is more saturated than expected and multiple borrowing from different institutions is a norm rather than an exception. This necessitated forming the control group based on all the new clients registered with these MFIs, and, these clients were further screened during the interview process, to sift and identify the clients that had taken a loan for the first

time from any MFI operating in the area. A number of clients deemed 'new' by the CSC staff were later dropped because they had borrowed from other MFIs earlier, defeating the purpose of having prospective or pipeline clients as a control group. Though we were unable to find a reasonable number of new clients of CSC, we were lucky to come across a new branch of Kashf starting operations in the same area as CSC, and through this newly established branch we were able to find a reasonable number of first time borrowers adjacent to the area of CSC clients. This was part of the effort on the part of the researcher in order to make the control group resemble the treatment group as closely as possible in terms of pre-treatment characteristics, for only then could an unbiased treatment effect of the intervention be teased out.

5.2. Questionnaire Design

The most important aspect of any empirical research is the questionnaire design that draws out informative data to answer the underlying research question: what is the impact of microcredit on indicators of women empowerment? The questionnaire design for this study was primarily guided by this research question and followed the international guidelines as laid out in the three volume series by Grosh and Glewwe (2000) on the Living Standards and Measurement Survey (LSMS).

In terms of the survey design, the author benefited enormously from his earlier research experience in designing and implementing questionnaires for research projects in Pakistan[15]. The earlier survey on the microfinance sector conducted by the author forms the baseline for this study, and the data collected for this questionnaire implemented in August 2006, provided necessary insights from the field to design a better instrument for this study. This initial groundwork helped us in establishing parameters that define empowerment, within the particular context of Pakistan. We tried to motivate the module on 'women's empowerment' through the exploratory evidence obtained from the focus group sessions and subsequent interviews carried out for the baseline survey.

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[15] This included a comprehensive survey on Devolution Reforms in Pakistan, implemented in Faisalabad, Lodhran and Sargodha districts of Pakistan; secondly, the author also worked on a questionnaire for mapping voting networks and role of informal institutions in rural areas of Sargodha district and thirdly, the author was also involved in conducting a household survey to provide insights into the dynamics of microfinance sector in Pakistan, with some questions on women empowerment, to motivate this study

Figure 5.2: Perceived Benefits of Microcredit
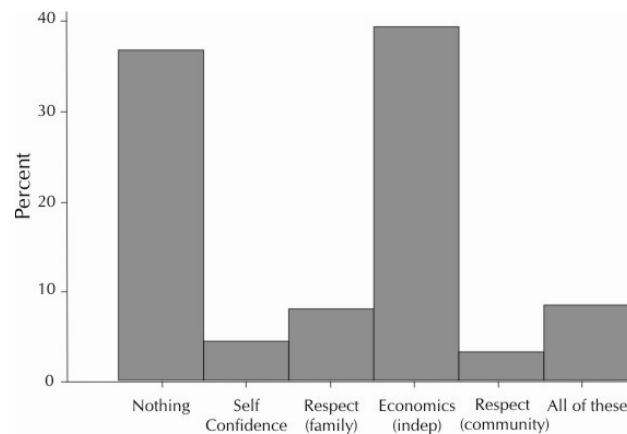
As shown in the Figure above, in response to the question as to what they have gained from participating in the program, 39% of the program participants reported that they had become more economically independent. This motivated the development of the 'empowerment module' of our new instrument to focus more on quantifying measures that captures this enhanced role in economic decision making brought about by access to credit. The questionnaire tries to capture the outcomes associated with this economic independence. The 'independence' in decisions relating to household purchases capture this dimension of economic empowerment. Moreover, theoretically economic independence should transpire into greater 'say' in the decision making process. This dimension is aptly captured through a series of questions relating to decisions concerning children, social mobility and economic empowerment. We have also included some qualitative questions in the survey to reinforce the validity of our findings.

The final questionnaire drew upon the Ethiopian Integrated Rural Household Survey and other carefully designed surveys used in previous studies [see Khandker et al. (2003) and Montgomery (2005)] The earlier questionnaire designed for analyzing the dynamics of the microfinance sector was also consulted and results of focus group interviews and qualitative responses of Kashf clients were also incorporated.

The questionnaire was translated into Urdu by a professional translator. The draft instrument was pretested in the field and repetitive questions were dropped to optimize on the length of the questionnaire. The section

on the credit profile of the household was revised and an additional loan product: home-improvement loan was included in the latter version. Substantive revisions were made in the design and flow of questions based on pretesting exercise. Minor changes were also made in the translations to ensure that the questions reflected the intended meanings.

The final questionnaire could be reasonably delivered in three quarters of an hour. The sequence of the questions followed the best practices for household surveys where less sensitive sections were administered first. The more sensitive sections on finances and empowerment were administered last. The survey instrument entailed comprehensive information on household economic and demographic characteristics, credit and expenditure profile of the household and finally a section on empowerment[16].

5.3. Implementation

The final instrument was implemented in a three week period between 15th December 2007 and 5th January 2008. The period selected was not the most appropriate as it coincided with the local holidays for the festival of Eid[17]. Given the holiday season compounded by the political turmoil and the tragic death of Benazir Bhutto, former Prime Minister of Pakistan, this was not an ideal time to carry out field work. Nevertheless, there were some blessings in disguise, i) the holiday season helped us in hunting down most of the working women in our sample who otherwise would have been missed; ii) the field staff of participating institutions were more cooperative in helping us in locating clients on non-working days.

The same instrument was implemented both for the control and treatment groups[18]. In order to ensure that only 'new' incoming clients were part of the control group a second screening check was made at the time of implementation. Any client who claimed to have borrowed from any 'other' MFI before joining this particular one was dropped out from the sampled group of control clients. This ensures that the control group comprises only 'incoming' clients and not the ones switching from some other MFI to be a part of this credit program.

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[16] Questionnaire is available on request from the author.
[17] Eid is the local festival to celebrate the end of pilgrimage to Makah
[18] A few questions asking for what have the participants gained from the program were obviously left out from the instrument used for the control group.

The survey was carried out by a team of four female surveyors and was headed by the author for supervision during the implementation phase. Surveyors were located from local areas and given a two days class room training to familiarize them with the questionnaire. These surveyors had prior experience of field work and their experience of the location helped us in hunting down the sampled clients including the drop out ones. The author back-checked 10% of the surveys everyday for consistency and mistakes. If any problems were identified the questionnaire was dropped and feedback given to the surveyor to ensure quality control. Once the data collection was completed, the data was processed into the computer by a team of five individuals including the author himself. Data consistency was checked at all three stages including collection, processing and analysis[19].

5.4. Descriptive Statistics

Table 5.1 summarizes the observable pre-treatment characteristics both for the control and the treatment groups. The table also provides a t-statistic value for differences in mean between the two groups. Table 5.2 summarizes all the exogenous and endogenous variables used in the probit, oprobit, biprobit and bioprobit regressions. Table 5.3 provides summary statistics for the preference related indicators of empowerment. Table 5.4 summarizes the autonomy based indicators of empowerment.

Table 5.1: Control and Treatment Group: Pre-existing Client Characteristics

Characteristics	Control	Treatment	t-stat for difference in means
Mean number of HH members before loan	6.04 (2.97)	6.1429 (2.207)	0.3103
Mean number of earning men in HH	1.9066 (1.327)	2.0102 (1.121)	0.5987
Proportion of women working before taking loan in the sample	.3866 (.490)	.3724 (.485)	-0.2154
Proportion of Married respondents in the sample	.8933 (.311)	.9031 (.297)	0.2384
Mean age before loan	32.9867 (9.763)	34.903 (8.477)	1.5949
Mean years of schooling of respondent	2.8667 (4.212)	2.8051 (3.643)	0.1189
Mean number of children before taking loan	3.56 (2.434)	3.7806 (2.025)	0.7574

*Standard deviation in parenthesis

[19] The author is willing to share the data collected for this study for any future research.

Most of the variables are self-explanatory; a few need further explanation. In Table 5.1 we report adjusted values of pre-treatment characteristics to show the comparability of the comparison and treatment group. For instance, mean number of household members for the treated units is adjusted for the number of children born before taking a loan. All the variables reported in the table are adjusted in the same way for treated units except for the years of schooling of the respondents and the number of earning men in the household. The first is going to be time invariant with respondents having passed their school going age while there is not enough information in the data to adjust the number of earning men. Nevertheless, even for non-adjusted number of earning men in the household, the mean difference is statistically insignificant. The t-statistics for the equality of differences in mean is reported in the last column, none of which is significant at the 10% level.

In Table 5.2 the two exogenous variables: 'working female before loan' and 'first utilization by respondent' need some clarification. Working female before loan is a dummy variable constructed by assigning '1' to all the women who had a working status before taking the loan and '0' otherwise. Since a woman's working status before taking the loan will not be influenced by the intervention, this variable serves as an exogenous proxy for initial endowment of empowerment. The choice of this proxy is motivated from economic theory, where one of the routes identified for empowerment earlier is through access to independent income. As a natural extension, we argue that by controlling for the women who were working prior to taking a loan we can capture most of the unobserved heterogeneity between the incoming borrowers and those who took the loan at an earlier date. Likewise, the dummy for the 'first loan utilized by the respondents' captures whether they were already empowered at the time of taking the loan and acts as another exogenous proxy for controlling the levels of initial empowerment. Moreover, the variable measuring the 'proportion of adult males ever school' expresses adult males who have received formal education as a proportion of all adult males in the household. It basically captures the effect of having more educated males in the household on indicators of women's empowerment.

In Table 5.3 you notice a significant drop in the number of observations available for each indicator both in the treatment and control group. Observations on widowed women who are household heads are dropped from the analysis. Also, some of the questions were inapplicable to those

Table 5.2: Summary Statistics: Explanatory Variables from Household Data Set

Variable Name	Definition	Treatment Group Mean and SD	Control Group Mean and SD
Endogenous Variable			
Treatment Dummy	0-1 Dummy Variable, = 1 if the client registered with MFI before Nov. 2006; = 0 for clients registering in/after Nov. 2007	1.00 (0)	0.00 (0)
Exogenous Variables			
Age of Respondent	Reported age of respondent confirmed by observation	37.821 (8.712)	32.987 (9.763)
Schooling of Respondent	Number of years of schooling completed by the respondent	2.7908 (3.64)	2.8667 (4.212)
Children	Number of total children of the respondents	4.372 (2.112)	3.773 (2.334)
Proportion of Adult Males Ever School (PALM)	Number of adult males in the HH that went to school divided by total number of male members in the HH.	.6141 (.426)	.6521 (.412)
Age difference	Difference between the reported ages of husband and wife	5.596 (4.527)	6.137 (6.17)
Joint Family	0-1 Dummy Variable, = 1 if the respondent lives with the parents of her husband; = 0 otherwise.	0.143 (3.51)	0.16 (.369)
Years After Marriage	Number of years that have elapsed after marriage (for married respondents only)	20.727 (9.078)	15.171 (10.743)
First Utilization by Respondent	0-1 Dummy Variable, = 1 if the first loan taken from MFI was utilized by the respondent herself; = 0 otherwise.	3.622 (.482)	0.32 (.4696)
Working Female before loan	0-1 Dummy Variable, = 1 if the respondent was a working women prior to taking loan; = 0 otherwise.	.3724 (.484)	.3867 (.4902)
Instrument Distance from branch	Clients' approximate walking distance in kilometers from the MFI branch from where she has taken the loan.	1.874 (.7406)	2.374 (1.249)
Interaction Term Institution* Treatment	0-1 Dummy Variable, = 1 if the respondent is a member of KASH Foundation; = 0 if the respondent is a member of CSC [MFIs]	1.874 (.7406)	0.00 (0.00)
Number of Observations		196	75

*Standard Deviation in parenthesis

Table 5.3: Summary Statistics: Outcome Indicators—'Say' in HH Decision Making: Ordinal Development Variable (1-5)

Dependent Variable	Treatment Group Mean and SD	N	Control Group Mean and SD	N	t-stat for difference in means
Boy's Schooling Decisions	3.53 (.7029)	149	3.56 (.743)	41	-0.30
Girl's Schooling Decisions	3.51 (.725)	147	3.54 (0.777)	41	-0.27
Children Marriage Decisions	3.159 (0.784)	126	3.235 (.8314)	17	-0.34
Children Medical Care	3.452 (.7877)	168	3.635 (.747)	63	-1.05
Family Planning Decision	3.336 (.7884)	122	3.625 (.7614)	48	-1.61
Respondent's Medical Care	3.475 (.8205)	162	3.597 (.7397)	67	-2.22
Social Visits to women's family	3.167 (.8631)	173	3.358 (.7920)	67	-1.60
Social Visits to husband's family	3.2814 (.813)	167	3.353 (.8556)	65	-0.20
Social Visits in Neighborhood	3.8103 (.7081)	174	3.821 (.7770)	67	-0.53
Work for earned income	3.060 (.9322)	166	2.746 (1.197)	67	2.16
Borrow Money from MFI	3.218 (.8856)	174	2.985 (1.148)	67	1.74
Purchase of HH assets	2.543 (.8245)	173	1.88 (.9296)	67	4.75
Decision about House Repair	2.289 (.9072)	173	1.552 (.9092)	67	4.98
Sale/Purchase of House	2.097 (.9536)	174	1.537 (.9427)	67	3.82

* All the dependent ordinal variables measure the extent to which women's preferences are reflected in decision making process; 1 indicating that her preferences are never reflected in these decision while '5' indicates that her preferences are always taken into account.
** Standard deviations are reported in the parenthesis.

women who have not yet been required to make such a decision. For instance, women having young children did not know whether their preferences would be reflected in their children marriages. In such cases the observation is dropped. Nevertheless, for most of the variables there are enough observations in the control and treatment groups to allow for a meaningful estimation, except for the decision regarding children marriages. Hence, the question regarding children marriage is dropped from the final analysis[20].

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[20] There were only 17 observations in the control group.

Table 5.4: Summary Statistics: Outcome Indicators—'Autonomy' in HH Decision Making: Dichotomous Dependent Variable (0/1)

Dependent Variable	Treatment Group Mean and SD	N	Control Group Mean and SD	N	t-stat for difference in mean
Purchasing Ice-cream for Children	0.9005 (-0.3)	171	0.758 (-0.4317)	62	1.06
Purchasing Grocery/Fruits	0.786 (-0.411)	173	0.6567 (-0.478)	67	-1.42
Purchasing Medicine for herself	0.6781 (-0.468)	174	0.6418 (-0.483)	67	0.73
Purchasing Clothes/Cosmetics for herself	0.7529 (-0.4326)	174	0.6865 (-0.4673)	67	0.79
Purchasing Books/Uniform for Children	0.4267 (-0.4961)	157	0.46 (-0.5034)	50	-1.55
Purchasing Furniture	0.052 (-0.2227)	173	0.0299 (-0.1714)	67	0.89
Purchasing Refrigerator/TV	0.0289 (-0.168)	173	0.0298 (-0.1714)	67	0.19
Sale/Purchase of Personal Assets (Jewellery)	0.0289 (-0.168)	173	0.0298 (-0.1715)	67	-0.04
Taking a Child to a doctor	0.6316 (-0.4838)	171	0.625 (-0.488)	64	-0.50

* All the dependent binary variables measure whether the women requires someone's permission in making the decision. '1' indicates that she does require someone's permission in the HH while '0' indicates complete autonomy in decision making.
** Standard deviations are reported in the parenthesis.

Moreover, it is important to note that the difference in unconditional mean across control and treatment group comes out to be negative for child related, health and social mobility decisions as reported in Table 5.3. The differences however are positive and significant for economic and financial decisions. The pattern in the data gives some credence to our earlier conjecture that power distribution differs across various domains of household decisions. A purview of the basic statistics suggests that microcredit might be important in increasing the level of power coefficient in the economic and financial aspects of household decision making.

6.   Estimation Results

This section uses fourteen ordinal dependent variables as summarized in Table 5.3 to test whether participation in the microcredit program has increased the extent to which women's preferences are taken into

consideration in various domains of household decisions. The ordinal dependent variables take a value on an increasing scale of 1-5 where the lower end of scale indicates virtually no 'say' in decision making while higher end values indicate full recognition of her 'voice' in household decisions. The dependent variables span five broad categories of household decisions regarding children, economic, health, social mobility and resource allocation *sufficient* to indicate the extent of women's centrality within the household.

6.1. Hypotheses 1: Preference Based Indicators of Empowerment

The results from ordered probit regression are reported in Table 6.1 A-D. The results are grouped on five different dimensions of empowerment: children related decisions; health decisions; economic decisions[21] social mobility decisions; and major household purchases.

The preferred regression is regression (1) in Table 6.1A-D. It uses proxies including a dummy for 'working women before taking a loan' and a dummy for self utilization of *first* loan by the respondent. Both these proxies seek to control for potential endogeneity stemming from initial differences between the control and treatment groups. Regression (2) adds an interaction term of institution dummy interacted with treatment dummy to control for any unobserved institutional differences between the treated units of participating institutions[22].

The estimated effect of treatment on indicators of empowerment varies across these five dimensions. For decisions relating to children, health, economic and social mobility, the treatment effect is statistically insignificant at 95% confidence level for both the specifications[23]. However, we find strongly positive and significant effects of treatment when it comes to major household decisions like purchases of household assets, household repairs and sale or purchase of house. All the effects are significant and positive at 99% confidence level for both the specifications. The signs and significance of all other controls are

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[21] Household and economic decisions are both reported in Table 6.1B for spatial convenience.

[22] Note that the experiment design constrained by dynamics on the ground as outlined in the sampling framework precludes the use of simple institutional dummy to capture fixed effects.

[23] Except for the marginal significance of treatment in explaining empowerment in family planning decision for one of the specifications reported in Table 6.1 B, the impact on all other decisions is insignificant across both specifications even at 90% confidence level.

consistent across the two specifications. They are also broadly in line with the determinants of women's empowerment outlined in the literature.

Consider then, the second test for robustness of our coefficients for treatment in the above regressions: the Propensity Score Matching (PSM) as outlined in Section III. We can still not dispense with the assumption of conditional mean independence so the covariates used for computing propensity score must credibly satisfy this assumption. Only those variables that are unaffected by participation are included in the model. In doing so most of the variables are either time invariant or measured at the time of taking the loan. The choice of variables used for participation regression is motivated by the potential determinants influencing the timing of participation. Table 6.2 reports the results for the first stage probit regression. It shows the importance of the education of the respondent in influencing the timing of participation in the program. The model has been over parameterized, using squared terms of observable covariates to improve the predicted probability of assignment to the treated group (Godtland et al., 2004) and (Rubin and Thomas, 1996).

This predicted probability of participation in the program or propensity score is then used to match treated units with observationally similar control units. A number of different matching algorithms can be used to match the treated and control units based on propensity scores. All these methods including nearest neighbor, kernel, radius and stratification matching yield similar results asymptotically. However, Smith and Todd (2001) demonstrate that estimates are highly sensitive to matching methods in case of small samples. In order to address this concern in our sample we derive impact estimates using two different methods for matching treated units with control comparators. Caliendo and Kopeinigz (2005) notes that if different approaches give the same results the choice of matching algorithm is unimportant[24]. In addition, the distribution of propensity scores both for the treated and control units [see Fig 6.1] show that the overlap condition is satisfied. Further, to improve the robustness of our estimator we drop observations that lie outside the common support condition.

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[24] We obtained the same results using nearest neighbour matching with replacement.

Figure 6.1: Histogram of Propensity Score Distribution for Treated and Control Groups
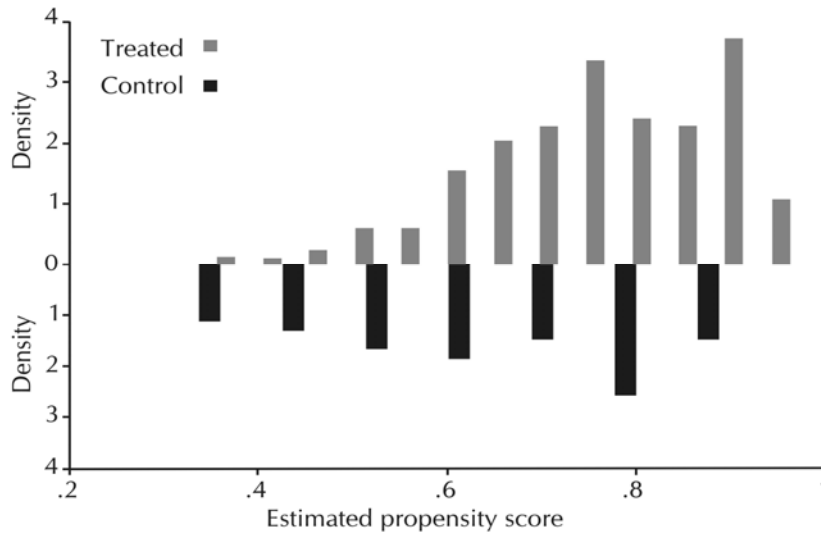
Table 6.3A reports the results for the balancing test on the difference in means of observable covariates. The balancing property is satisfied in all the three blocks with statistically insignificant difference in means between all the observed covariates in three strata of propensity scores. Note that there are no treated and control units in the first two blocks. The results with bootstrapped standard errors for all five categories of dependent variables are reported in Table 6.3B. For both the specifications we find results similar to the ones reported for ordered probit regressions. The decisions relating to major household purchases are still highly significant and positive. The treatment effect is statically insignificant for all other dimensions except a marginally significant coefficient on children medical care decisions. However the magnitude of ATT in that case is economically insignificant to merit any further discussion.

The validity of our estimates both for the simple ordered probit regressions and propensity score matching relies critically on the conditional mean independence assumption or selection on observables. Even after controlling for initial levels of empowerment through proxies and making the control units resemble treated units as closely as possible, through propensity score matching, we still fear endogeneity or selection on unobservables. As a final robustness check we use instrumented

regression to address concerns for selection bias. We use distance of the client from MFI branch as an instrument for treatment. The lack of funds did not allow us to get global positioning coordinates for the sampled households; hence the estimate of the distance of a client from the MFI branch is approximately calculated by using satellite maps of Lahore district[25]. Limited software routines are available to implement instrumented regression with binary treatment and ordinal outcome variables; thanks to the World Bank Poverty Research group that they have developed a bivariate ordered probit routine that allowed us to estimate the full-information maximum likelihood model with endogeneity. The results reported in Table 6.4A-D are based on bioprobit routine in Stata with endogeneity option specified (Sajaia, 2006).

The first step regression of participation equation shows that *distance* from MFI branch, in most cases, is strongly significant in explaining assignment to the treated units. The sign on the distance coefficient confirms our earlier conjecture that those who are at a further distance from the branch are likely to become part of the program at a later date as compared to the ones closer to the branch. The results suggest we have a *strong* instrument. The results for instrumented regression show *statistically insignificant* treatment effects across all categories of decisions. Once we instrument for distance the effect of treatment dummy on major household purchases is also wiped away. It is interesting to note, that the value of the parameter measuring the correlation between the errors of two equations is significantly high for the regressions on household repairs and sale or purchase of house. The Wald test for independence of the two equations as reported in Table 6.4D is also strongly rejected for these two regressions. This implies that there must be some selection on 'unobservables' going on for these empowerment indicators. In the wake of this evidence our earlier results are biased and inconsistent. The unconfoundedness assumption seems not to hold for indicators relating to the major household purchases.

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[25] This is made possible by a software developed by Naqsha.net. By mapping Lahore district on Google Earth it allowed us to calculate approximately the distance of the localities of the client from their respective MFI branches. These distance estimates were verified by the local branch staff and any discrepancies were removed.

6.2. Hypotheses 2: Autonomy Based Indicators of Empowerment

In this section we use another set of empowerment indicators where we ask the respondents whether they require someone's permission to make decisions related to household purchases and children. We generate a dummy variable capturing their independence in decision making whenever they do not require someone's permission to make such decisions.

We divide our indicators into three broad categories: 'small' purchases like grocery, medicine and clothes/cosmetics for herself; second category includes children related decisions like buying sweets/ice cream, books/uniform and taking a child to doctor; third category includes purchasing household assets, sale/purchase of personal assets like jewelery and purchase/sale of house. As shown in Table 5.4 there is limited variation in the last category of decisions and failures/successes are perfectly predicted in these cases. We also tried pooling the last three questions and clustering the standard errors, but the limited variation still makes it impossible to estimate the model.

The unconditional means for the last set of questions show that these decisions fall within the prescribed 'male' spheres of decision making and as expected only a handful of respondents could make those decisions independently. For the other two categories we use a simple probit model with the same covariates and proxies as for the ordered probit case discussed earlier. The results are presented in Table 6.5A-B for the same specifications used to test the earlier hypothesis. The estimated coefficient of treatment dummy is statistically insignificant across both categories except for the question regarding purchasing medicine for herself. Once, institutional level fixed effects are introduced in the second specification, the significance of the coefficient on this particular question is also wiped away. One possible explanation for the strong fixed effects in this question might be the accessibility of medical stores. It seems likely that institutional level fixed effect is picking up the differential level of access to medical stores available in these areas. If the medical store is at a fair distance from the respondent's home then it is unlikely that women will purchase medicine without someone's permission. Nevertheless, most of the

findings presented in this section are consistent with our earlier results. It is obvious that if there are no first order changes in the extent to which women's preferences are reflected in decision making process, then it is unlikely that there will be higher order changes in the 'autonomy' or independence in decision making for the female partner.

In Table 6.6 we present results for the propensity score matching. The covariates used for propensity score matching are identical to the ones used earlier. The results are similar to those for the probit estimation except for marginal significance of 'buying medicine for herself. The effect is so marginally significant that the significance is wiped away for the radius matching algorithm. The table reports result for both kernel and radius matching.

As a final robustness check for our estimates we present results for bivariate probit estimation of the model allowing for correlation between the errors of both participation and structural equations. The results for our bivariate probit specification with distance from MFI branch as an instrument are reported in Table 6.7A-B. Our instrument is again significant and negative for five of the six regressions reported in the tables. The treatment dummy is statistically insignificant for all, except for decisions relating to buying ice cream for children and buying personal clothes/cosmetics. In both cases the treatment seems to have a significantly negative effect on women's autonomy. However, these results must be taken with a pinch of salt as both participation equation and structural equations of the model seem to be perfectly correlated. And, a slight correlation of our instrument with the error term in structural equation will bias the estimates reported for these questions in Table 6.7A-B. Further, the analyses of 12 impact evaluation studies by Glazerman et al. (2003) in comparing experimental and non-experimental estimates of treatment effects showed that the standard econometric techniques such as instrumentation used to address concerns for unobservable heterogeneity tended to *increase* the divergence between experimental and non-experimental estimates of the impact (cf. Ravallion, 2006). As pointed out by the authors there is no guarantee that more ambitious and sophisticated econometric methods will perform better in reducing total bias.

6.3. Empirical Conclusions

The empirical results presented in the last section produce the following conclusions for our testable hypotheses:

Hypothesis 1: Impact of participation in microcredit program on preference based indicators of women's empowerment:

1.1: Upon 'say' in child related decisions: **Do not reject the null hypothesis: microcredit has *no* effect on power coefficient 'θ_1'**

1.2: Upon 'say' in health related decisions: **Do not reject the null hypothesis: microcredit has *no* effect on power coefficient 'θ_2'**

1.3: Upon 'say' in economic decisions: **Do not reject the null hypothesis: microcredit has *no* effect on power coefficient 'θ_3'**

1.4: Upon 'say' in social mobility decisions: **Do not reject the null hypothesis: microcredit has *no* effect on power coefficient 'θ_4'**

1.5: Upon 'say' in sale or purchase of major household items: **Do not reject the null hypothesis: microcredit has *no* effect on power coefficient 'θ_5'**

Hypothesis 2: Impact of participation in microcredit Program on 'autonomy' based indicators of women's empowerment:

2.1: Upon 'independence' in small personal purchases: **Do not reject the null hypothesis: microcredit has *no* effect on power coefficient 'θ_6'**

2.2: Upon 'independence' in children related decisions: **Do not reject the null hypothesis: microcredit has *no* effect on power coefficient 'θ_7'**

6.4. Discussion of Results

Our empirical findings suggest that microcredit intervention in the urban slums of the Lahore district of Pakistan has *no* impact on child related, health, economics and social mobility decisions. The triangulation of results using proxy controls, matching methods and instrumentation

clearly demonstrate the robustness of our findings for these four dimensions of empowerment. However, we are unable to find convincing evidence for the last set of indicators relating to major household purchases. We need to pause a second to contemplate on the results for major household purchase decisions. The estimates as reported in Table 6.1D and Table 6.3 are robust across matching method and proxy controlled regressions under the 'ignorability of treatment' assumption. However, the results fail to withstand the IV estimation using bivariate ordered probit specification. The results suggest that unobserved latent heterogeneity *might* be driving the impact of treatment on those set of indicators. Given the high coefficient for correlation between the errors of the two equation captured by the 'rho' coefficient reported in Table 6.4D, the appropriate specification of the model is bivariate probit. It must however be noted that the conditions embedded in bivariate models are quite restrictive: we need to specify both the functional form for conditional mean of Y, and, a joint distribution of unobserved factors affecting treatment and outcome indicators Y. It is well known in the literature that the results for full information maximum likelihood estimate of recursive bivariate models, are highly sensitive to these assumptions and model misspecification. Though the model is identifiable in the presence of 'one' exogenous instrument but none of these assumptions used for identifying the model are testable. Further, as mentioned earlier the estimates rely heavily on the exclusion restriction for the instrument, for which no formal tests can be provided either. With these methodological limitations of the proposed robustness check we argue that there is no reason to believe that the results of our earlier univariate specification under 'ignorability of treatment' are less plausible.

We need more informative data to draw conclusive evidence for the last set of preference indicators. More robust estimators can be obtained using semi-parametric methods to estimate treatment effects in binary treatment and response models as proposed by Abadie (2003). However, to implement this method we need at least two rounds of observations. In the absence of more informative data our preferred results are matching estimators computed under the 'selection on observables assumption'. Both the ordered probit and matching estimators make less demanding assumptions for identification and hence are likely to be more precise and unbiased for such a small sample framework.

The results under the maintained assumption of 'selection on observables' are well aligned with the dynamics on the ground. The results suggest that women are gaining more 'say' when it comes to higher order decisions regarding purchase/sale of household assets, while both the control and treated enjoy the same level of participation in decisions relating to children, health, social mobility and paid work. Looking at these results in the backdrop of the urban slums we feel that the theory of the 'perpetual dependency' of women in the household seems more relevant. The unconditional means reported in Table 5.3 for decisions relating to children, social mobility and economic decisions take a value of about 3.5 on a scale of 5 suggesting that for an average woman in the sample, both in the control and the treatment group, her preferences are reflected *most of the time*[26] in these decisions. Within the framework of patriarchy we believe this is the best that women can score on these outcomes and hence an intervention like microcredit will have marginal effects if any for these indicators. In the framework of the 'perpetual dependency' hypothesis this means that women have reached a level of maturity in their relationship with their spouses. They have been given the bargaining power in these decisions commensurate with the prescribed gender norms of the society.

However when it comes to decisions relating to major household decisions like purchase of assets, house repairs and sale/purchase of house, women do not have much 'say'. The unconditional mean values for those decisions have a value of about 2 on a scale of 5 suggesting that female preferences are rarely or never considered for these household decisions. It seems reasonable that once women have gotten more involved in economic and financial matters by borrowing money from an MFI they enjoy greater confidence of their husbands, and hence we see their participation increasing in decisions which initially fell under the prescribed domain of 'male only' decisions. The results from our ordered probit and matching estimators reinforce this finding that microcredit intervention empowers women by giving her more 'say' in decisions where she had limited or no participation before this intervention.

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[26] Corresponds to scale 4 in the questionnaire.

Our results are consistent with autonomy based indicators reported in Tables 6.4 A-D. These results also serve as an informal check for the patriarchal framework proposed in this paper. Critics might have argued that if microcredit is not increasing women's 'say' in the decision making process beyond a societal threshold level, it is still possible that they may have greater independence in making decisions. Our results suggest that on average women in the treatment group are no more independent or autonomous than the control units, even when it comes to decisions relating to small household purchases. This further reinforces our earlier findings that women have reached a certain threshold level of independence within the structural norms of the society and microcredit has no marginal impact on all such indicators.

7. Conclusions

Empirically, the study has sought to contribute the first econometric impact assessment of microcredit on women's empowerment in Pakistan. As showed in Section VI, a number of interesting insights emerge from our analysis. First, our results suggest that microcredit intervention has no effect on the bargaining power of women within the household for a broad range of decisions including child related, health, and economic and social mobility decisions. Second, our results suggest that microcredit intervention is empowering women in the domains of household decisions where they are the least empowered to begin with. These include decisions relating to purchases of TV/Refrigerator, house repair and sale/purchase of house. These decisions generally fall within the domain of 'male-only' decisions in patriarchal societies.

Methodologically, the study has sought to contribute to a cost-effective and resource savvy strategy for analyzing program impacts for various interventions. Firstly, we have shown that within the constraints of cross sectional data, we can draw robust estimators for most of the testable hypotheses. Our empirical results provide strong evidence for six of the seven hypotheses analyzed in the paper. Our results for all these estimators are robust across the three econometric methods used in the study, and other robustness checks implemented in Section VI provide further credence to our results. Secondly, we have shown that most of the concerns raised in the literature regarding biased estimates of impact can be controlled through careful design and a better understanding of

ground dynamics. Finally, to the author's knowledge this is the first impact assessment that corrects for selection bias in evaluating the impact of microcredit institutions without exploiting any irregularity in the intervention design. Hence, the methodology proposed in this research can readily be applied to other settings.

7.1. Implications

The findings of this research have implications for policy makers and future research. First, we must remind the reader that we did not find any significant effect of microcredit on different aspects of intra-household decisions that was robust across all econometric specifications. Nonetheless, even the weaker results suggest that microcredit has limited empowerment potential in the urban context of Pakistan. This is a very important finding as more than 50% of the total microcredit clients in the Punjab Province of Pakistan are concentrated in the urban suburbs of Lahore district. The donor agencies and policy makers can get more value for subsidizing microcredit activity in Pakistan if future subsidies are targeted at non-urban households with less empowered women. It must however be emphasized, that the link between microcredit and empowerment in non-urban context needs to be further investigated before drawing lessons for future policy. In addition, the results of this study provide another piece of evidence, albeit modest, that the gender-specific approach for microcredit service is *over rated* in the urban context of Pakistan. Future research with richer data sets could compare the results from this quasi-experimental framework with randomized intervention. Research in this direction will provide a good comparative analysis for the robustness of the estimates reported in this study.

Tables

TABLE 6.1A: Ordered Probit Estimates of Women's Empowerment: 'Say' in Child Related Decision

Women's opinion taken into consideration in household decisions on:

Independent Variables	Boy's Schooling Oprobit Coefficients		Girl's Schooling Oprobit Coefficients		Child's Medical Care Oprobit Coefficients	
	(1)	(2)	(1)	(2)	(1)	(2)
Treatment Dummy	0.205	0.556	0.228	0.396	-0.150	-0.084
	(0.236)	(0.351)	(0.257)	(0.357)	(0.203)	(0.280)
Age Difference with Husband	0.050**	0.047*	0.034	0.032	0.029	0.028
	(0.024)	(0.024)	(0.026)	(0.026)	(0.020)	(0.020)
Age of Respondent	-0.028	-0.026	-0.013	-0.012	0.020	0.020
	(0.023)	(0.024)	(0.024)	(0.024)	(0.018)	(0.018)
Education of Respondent	0.014	0.014	0.022	0.021	0.064***	0.064***
	(0.025)	(0.026)	(0.026)	(0.026)	(0.023)	(0.023)
Number of Children	-0.069**	-0.075**	-0.054*	-0.055*	-0.019	-0.018
	(0.033)	(0.034)	(0.031)	(0.031)	(0.025)	(0.025)
Proportion Adult Males Ever School	0.528**	0.526**	0.555**	0.562**	0.509***	0.507***
	(0.227)	(0.228)	(0.232)	(0.231)	(0.188)	(0.188)
Joint Family	-0.077	-0.082	-0.054	-0.063	0.132	0.127
	(0.351)	(0.342)	(0.294)	(0.287)	(0.343)	(0.339)
Years After Marriage	0.018	0.018	0.014	0.014	-0.000	-0.000
	(0.021)	(0.021)	(0.022)	(0.022)	(0.018)	(0.018)
Working Female Before Loan	0.594***	0.538**	0.792***	0.758***	0.356*	0.343*
	(0.221)	(0.226)	(0.232)	(0.236)	(0.182)	(0.190)
First Utilization	0.333	0.315	0.259	0.250	0.209	0.206
	(0.203)	(0.204)	(0.217)	(0.217)	(0.164)	(0.164)
Institution*Treatment		-0.415		-0.204		-0.087
		(0.318)		(0.305)		(0.258)
Number of Observations	165	165	161	161	196	196
Pseudo R-Squared	0.110	0.115	0.110	0.111	0.085	0.086
Chi2 statistics	28.918	34.923	30.701	31.220	42.211	43.475
Log Likelihood	-144.630	-143.841	-146.224	-145.997	-193.281	-193.214

Standard deviation are reported in the parenthesis
*p<0.1, **p<0.05, ***p<0.01

Table 6.1B: Ordered Probit Estimates of Women's Empowerment: 'Say' in Health and Economic Decisions

Women's opinion taken into consideration in household decisions on

Independent Variables	Medical Check Up		Family Planning		Working for Earned Income		Borrowing from MFI	
	Oprobit Coefficients		Oprobit Coefficients		Oprobit Coefficients		Oprobit Coefficients	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Treatment Dummy	-0.133	-0.140	-0.095	0.522*	0.247	-0.001	-0.051	0.011
	(0.200)	(0.266)	(0.228)	(0.296)	(0.196)	(0.281)	(0.207)	(0.269)
Age Difference with Husband	0.023	0.023	0.059**	0.057**	0.041 **	0.043**	0.034*	0.034*
	(0.023)	(0.023)	(0.025)	(0.025)	(0.020)	(0.020)	(0.020)	(0.020)
Age of Respondent	-0.025	-0.025	-0.021	-0.010	0.001	-0.000	-0.009	-0.009
	(0.019)	(0.019)	(0.025)	(0.027)	(0.019)	(0.019)	(0.017)	(0.017)
Education of Respondent	0.049**	0.049**	0.013	0.017	0.003	0.002	0.025	0.026
	(0.023)	(0.023)	(0.029)	(0.030)	(0.022)	(0.023)	(0.024)	(0.024)
Number of Children	-0.043*	-0.043*	-0.071 **	-0.071 **	0.019	0.017	-0.022	-0.022
	(0.026)	(0.026)	(0.032)	(0.032)	(0.026)	(0.026)	(0.025)	(0.025)
Proportion of Adult Males Ever School	0.355**	0.355**	0.341	0.309	0.239	0.238	0.131	0.128
	(0.181)	(0.180)	(0.233)	(0.241)	(0.190)	(0.192)	(0.185)	(0.185)
Joint Family	0.311	0.312	0.311	0.297	-0.030	-0.023	0.116	0.112
	(0.332)	(0.329)	(0.381)	(0.377)	(0.282)	(0.286)	(0.296)	(0.295)
Years After Marriage	0.031 *	0.031 *	0.013	0.011	0.015	0.015	0.011	0.011
	(0.018)	(0.018)	(0.026)	(0.027)	(0.019)	(0.019)	(0.016)	(0.017)
Working Female Before Loan	0.135	0.137	0.491 **	0.436*	0.707***	0.762***	0.622***	0.610***
	(0.182)	(0.186)	(0.224)	(0.225)	(0.173)	(0.176)	(0.176)	(0.181)
First Utilization	0.386**	0.386**	0.185	0.091	0.408**	0.426**	0.313*	0.309*
	(0.188)	(0.189)	(0.214)	(0.218)	(0.185)	(0.185)	(0.167)	(0.168)
Institution*Treatment		0.010		-0.790***		0.316		-0.081
		(0.247)		(0.282)		(0.253)		(0.243)
Number of Observations	188	188	138	138	193	193	200	200
Pseudo R-Squared	0.065	0.065	0.092	0.110	0.098	0.101	0.061	0.062
Chi2 statistics	29.816	29.811	27.863	39.122	45.380	49.068	32.422	32.404
Log Likelihood	-195.987	-195.987	-137.655	-134.882	-226.015	-225.175	-224.942	-224.884

Standard deviation are reported in the parenthesis
*p<0.1, **p<0.05, ***p<0.01

Table 6.1C: Ordered Probit Estimates of Women's Empowerment: 'Say' in her Social Mobility Decisions

Women's opinion taken into consideration in decisions regarding:

Independent Variables	Social Visits to Woman's Family Oprobit Coefficients		Social Visits to Man's Family Oprobit Coefficients		Social Visits in Neighbourhood Oprobit Coefficients	
	(1)	(2)	(1)	(2)	(1)	(2)
Treatment Dummy	-0.182	-0.084	-0.176	-0.091	-0.094	0.230
	(0.195)	(0.252)	(0.191)	(0.263)	(0.173)	(0.221)
Age Difference with Husband	0.035*	0.034*	0.031	0.030	0.045***	0.043**
	(0.020)	(0.020)	(0.019)	(0.019)	(0.017)	(0.018)
Age of Respondent	-0.005	-0.005	0.010	0.010	0.032*	0.034*
	(0.019)	(0.019)	(0.020)	(0.020)	(0.018)	(0.018)
Education of Respondent	0.021	0.022	0.035	0.035	0.008	0.010
	(0.025)	(0.025)	(0.024)	(0.024)	(0.025)	(0.025)
Number of Children	-0.044*	-0.044*	-0.004	-0.004	0.019	0.020
	(0.024)	(0.024)	(0.025)	(0.025)	(0.027)	(0.028)
Proportion Adult Males Ever School	0.375**	0.372**	0.281*	0.278	-0.128	-0.136
	(0.187)	(0.187)	(0.170)	(0.171)	(0.205)	(0.206)
Joint Family	-0.148	-0.155	0.049	0.047	0.593	0.579
	(0.324)	(0.323)	(0.306)	(0.305)	(0.368)	(0.361)
Years After Marriage	0.008	0.009	0.001	0.001	-0.005	-0.004
	(0.018)	(0.018)	(0.018)	(0.018)	(0.017)	(0.018)
Working Female Before Loan	0.574***	0.555***	0.195	0.182	0.019	-0.036
	(0.172)	(0.178)	(0.184)	(0.189)	(0.170)	(0.170)
First Utilization	0.283*	0.277*	0.141	0.137	0.132	0.115
	(0.167)	(0.165)	(0.170)	(0.170)	(0.174)	(0.175)
Institution*Treatment		-0.128		-0.110		-0.414 *
		(0.229)		(0.250)		(0.232)
Number of Observations	200	200	195	195	200	200
Pseudo R-Squared	0.077	0.077	0.037	0.037	0.051	0.058
Chi2 statistics	36.824	37.862	16.314	17.713	18.829	20.865
Log Likelihood	-217.117	-216.967	-223.635	-223.524	-173.452	-172.174

Standard deviation are reported in the parenthesis
*p<0.1, **p<0.05, ***p<0.01

Table 6.1D: Ordered Probit Estimates of Women's Empowerment: 'Say' in Sale/Purchase of HH Assets

Women's opinion taken into consideration in decisions regarding:

Independent Variables	Purchase of HH Assets (TV, Fridge) Oprobit Coefficients		House Repair Oprobit Coefficients		Sale/Purchase of House Oprobit Coefficients	
	(1)	(2)	(1)	(2)	(1)	(2)
Treatment Dummy	0.711***	1.061***	0.996***	1.081 ***	0.758***	0.993***
	(0.225)	(0.282)	(0.236)	(0.296)	(0.243)	(0.287)
Age Difference with Husband	0.073***	0.070***	0.076***	0.075***	0.067***	0.065***
	(0.019)	(0.020)	(0.017)	(0.017)	(0.020)	(0.021)
Age of Respondent	0.016	0.017	0.048***	0.048***	0.061***	0.062***
	(0.019)	(0.019)	(0.017)	(0.017)	(0.020)	(0.020)
Education of Respondent	0.018	0.021	0.016	0.016	-0.004	-0.002
	(0.022)	(0.022)	(0.024)	(0.025)	(0.023)	(0.023)
Number of Children	-0.013	-0.012	0.018	0.018	0.023	0.024
	(0.022)	(0.022)	(0.022)	(0.022)	(0.024)	(0.024)
Proportion Adult Males Ever School	0.020	0.009	-0.130	-0.133	-0.139	-0.150
	(0.190)	(0.190)	(0.186)	(0.187)	(0.190)	(0.192)
Joint Family	-0.043	-0.066	0.109	0.102	-0.029	-0.047
	(0.278)	(0.272)	(0.266)	(0.263)	(0.271)	(0.266)
Years After Marriage	0.004	0.006	-0.018	-0.017	-0.039**	-0.039**
	(0.018)	(0.018)	(0.015)	(0.015)	(0.018)	(0.018)
Working Female Before Loan	0.062	-0.005	0.096	0.080	-0.189	-0.239
	(0.160)	(0.163)	(0.173)	(0.175)	(0.171)	(0.176)
First Utilization	0.335**	0.317**	0.181	0.175	0.177	0.164
	(0.161)	(0.159)	(0.179)	(0.179)	(0.171)	(0.171)
Institution*Treatment		-0.454**		-0.110		-0.304
		(0.214)		(0.225)		(0.212)
Number of Observations	199	199	200	200	200	200
Pseudo R-Squared	0.097	0.104	0.121	0.122	0.081	0.085
Chi2 statistics	44.328	47.620	63.183	63.363	35.647	37.540
Log Likelihood	-229.569	-227.610	-228.997	-228.879	-236.599	-235.706

Standard deviation are reported in the parenthesis
*p<0.1, **p<0.05, ***p<0.01

Table 6.2: Participation in Microcredit Program Probit Regression
Dependent Variable Treatment (0/1)

Independent Variables	Probit Coefficient	p-value
Age of respondent (at the time of taking loan-TTL)	0.155	0.141
Age of Respondent-TTL -Squared	-0.002*	0.088
Education of Respondent	0.191 **	0.028
Education of Respondent - Squared	-0.018**	0.037
Age Difference with Husband	-0.023	0.290
Number of Children -TTL	-0.337	0.188
Number of HH Members-TTL	0.354	0.143
Proportion Adult Males Ever School	-0.244	0.337
Working Women-TTL	-0.207	0.364
First Loan Utilized by Respondent	0.195	0.401
Joint Family	-0.137	0.776
Years After Marriage-TTL	0.033	0.165
Number of Observations	198	
Pseudo R-Squared	0.094	
Chi2 statistics	20.984	
Log Likelihood	-101.396	

Table 6.3A: Balancing Test
t-stat for equality of means for observed variables in the treated and control groups

Independent Variables	Strata 1	Strata 2	Strata 3
Age of respondent (at the time of taking loan-TTL)	-0.7537	-0.4574	0.5956
Education of Respondent	-1.1436	-1.0959	-0.8703
Number of Children -TTL	-0.5905	-1.1039	1.3443
Number of HH Members-TTL	-0.5905	-1.1039	0.6598
Proportion Adult Males Ever School	0.2477	-0.894	1.1177
First Loan Utilized by Respondent	-0.0674	0.4257	-0.9708
Working Women-TTL	-0.0674	0.4035	
Age Difference with Husband	-0.5362	0.5897	0.1391
Joint Family	-	0.2472	-0.1499
Years After Marriage-TTL	-0.0137	-1.303	1.2407
N	21	111	66
Controls	11	30	9
Treated	10	81	57

**Table 6.3B: Treatment Effect Estimates of Women's Empowerment
Propensity Score Matching Method**

Women's opinion taken into consideration in decision regarding:

Independent Variables	ATT (Kernel) (1)	ATT (Radius) (2)	NT	NC
Boy's Schooling Decisions	0.043	0.038	130	33
	(0.154)	(0.148)		
Girl's Schooling Decisions	0.045	0.020	127	32
	(0.179)	(0.160)		
Children Medical Care	-0.093	-0.069	145	49
	(0.131)	(0.134)		
Family Planning Decision	-0.171	-0.267 *	101	35
	(0.193)	(0.136)		
Respondent's Medical Care	-0.078	-0.068	136	50
	(0.153)	(0.138)		
Social Visits to women's family	-0.127	-0.127	148	50
	(0.179)	(0.184)		
Social Visits to husband's family	-0.097	-0.099	144	49
	(0.136)	(0.156)		
Social Visits in Neighborhood	-0.040	-0.027	148	50
	(0.094)	(0.103)		
Work for earned income	0.023	0.111	141	50
	(0.165)	(0.170)		
Borrow Money from MFI	-0.162	-0.129	148	50
	(0.151)	(0.176)		
Purchase of HH assets	0.404	0.518	147	50
	(0.167)**	(0.153)***		
Decision about House Repair	0.703	0.730	148	50
	(0.151)***	(0.159)***		
Sale/Purchase of House	0.516	0.611	148	50
	(0.186)*	(0.199)***		

Standard deviations are reported in the parenthesis.
*p<0.1, ** p< 0.05, *** P <0.01

Table 6.4A: Instrumented Ordered Probit Estimates of Women's Empowerment:
'Say' in Child Related Decision

Women's opinion taken into consideration in household decisions on:

Independent Variables	Boy's Schooling Coefficients FIML	Girl's Schooling Coefficients FIML	Child's Medical Care Coefficients FIML
Treatment Dummy	0.132	0.127	0.029
	(0.422)	(0.463)	(0.323)
Age Difference with Husband	0.051 *	0.034	0.035
	(0.027)	(0.029)	(0.023)
Age of Respondent	-0.027	-0.013	0.020
	(0.023)	(0.024)	(0.019)
Education of Respondent	0.012	0.019	0.061 **
	(0.027)	(0.028)	(0.026)
Number of Children	-0.069**	-0.054*	-0.022
	(0.034)	(0.031)	(0.030)
Proportion Adult Male Ever School	0.557**	0.577**	0.548**
	(0.275)	(0.270)	(0.221)
Joint Family	-0.122	-0.110	0.178
	(0.393)	(0.358)	(0.383)
Years after Marriage	0.016	0.012	-0.004
	(0.023)	(0.025)	(0.023)
Working Female Before Loan	0.618**	0.818***	0.349*
	(0.247)	(0.261)	(0.193)
First Utilization	0.316	0.238	0.228
	(0.222)	(0.223)	(0.171)
First Stage: Treatment			
Age Difference with Husband	-0.024	-0.023	-0.032
	(0.022)	(0.023)	(0.023)
Age of Respondent	-0.000	-0.004	0.003
	(0.032)	(0.032)	(0.031)
Education of Respondent	0.022	0.031	0.034
	(0.034)	(0.035)	(0.029)
Number of Children	-0.009	-0.002	0.052
	(0.037)	(0.036)	(0.035)
Proportion Adult Male Ever School	-0.430	-0.396	-0.382*
	(0.263)	(0.266)	(0.229)
Joint Family	0.386	0.424	0.601
	(0.433)	(0.427)	(0.371)
Years after Marriage	0.018	0.020	0.035
	(0.031)	(0.032)	(0.031)
Working Female Before Loa n	-0.357	-0.382	-0.244
	(0.255)	(0.257)	(0.232)
First Utilization	0.171	0.167	0.111
	(0.255)	(0.256)	(0.225)
Distance	-0.253*	-0.244	-0.303 **
	(0.144)	(0.151)	(0.132)
Observations	164	160	195
Wald Chi Squared	10.509	9.956	25.558
p> Chi2	0.397	0.444	0.004
Wald test of pho = 0			
Chi2(1)	0.002	0.000	0.112
p> Chi2(1)	0.964	0.983	0.738

Standard deviations are reported in the parenthesis.
*p<0.1, ** p< 0.05, *** P <0.01

Table 6.4B: Instrumented Ordered Probit Estimates of Women's Empowerment:
'Say' in Health and Economic Decisions

Women's opinion taken into consideration in household decisions on:

Independent Variables	Medical Check Up Coefficients FIML	Family Planning Coefficients FIML	Working for Earned Income Coefficients FIML	Borrowing From MFI Coefficients FIML
Treatment Dummy	-0.075	0.152	0.400	0.308
	(0.351)	(0.150)	(0.299)	(0.329)
Age Difference with Husband	0.026	0.063**	0.058**	0.049**
	(0.025)	(0.026)	(0.023)	(0.023)
Age of Respondent	-0.025	-0.025	-0.002	-0.010
	(0.019)	(0.026)	(0.022)	(0.018)
Education of Respondent	0.052*	0.007	-0.014	0.011
	(0.028)	(0.029)	(0.028)	(0.029)
Number of Children	-0.039	-0.083 **	-0.001	-0.039
	(0.031)	(0.034)	(0.034)	(0.033)
Proportion Adult Male Ever School	0.358	0.333	*00405**	0.252
	(0.242)	(0.229)	(0.232)	(0.211)
Joint Family	0.442	0.197	-0.116	0.039
	(0.342)	(0.370)	(0.376)	(0.384)
Years after Marriage	0.033	-0.001	-0.000	-0.005
	(0.023)	(0.027)	(0.024)	(0.024)
Working Female Before Loan	0.105	0.587**	0.704***	0.629***
	(0.192)	(0.246)	(0.197)	(0.188)
First Utilization	*00427***	0.089	0.382*	0.290
	(0.190)	(0.243)	(0.221)	(0.201)
First Stage: Treatment				
Age Difference with Husband	-0.034	-0.025	-0.035	-0.033
	(0.024)	(0.030)	(0.022)	(0.023)
Age of Respondent	-0.001	0.045	0.010	0.001
	(0.032)	(0.040)	(0.032)	(0.031)
Education of Respondent	0.037	0.026	0.046	0.038
	(0.029)	(0.037)	(0.030)	(0.030)
Number of Children	0.050	0.066	0.063*	0.059*
	(0.036)	(0.060)	(0.035)	(0.034)
Proportion Adult Male Ever School	-0.355	-0.178	-0.472**	-0.347
	(0.232)	(0.329)	(0.229)	(0.228)
Joint Family	0.601	0.691	0.621 *	0.591
	(0.374)	(0.487)	(0.363)	(0.370)
Years after Marriage	0.041	0.047	0.034	0.041
	(0.031)	(0.035)	(0.031)	(0.030)
Working Female Before Loan	-0.208	-0.908***	-0.187	-0.247
	(0.236)	(0.297)	(0.234)	(0.231)
First Utilization	0.087	0.553**	0.158	0.133
	(0.227)	(0.271)	(0.228)	(0.223)
Distance	-0.266**	-0.784***	-0.303**	-0.264**
	(0.129)	(0.222)	(0.136)	(0.129)
Rho	0.020	-0.369	-0.285	-0.345
	(0.368)	(0.249)	(0.351)	(0.373)
Observations	187	137	192	199
Wald Chi Squared	24.925	57.334	30.530	27.018
p> Chi2	0.005	0.000	0.001	0.003
Wald test of pho = 0				
Chi2(1)	0.003	2.195	0.662	0.855
p> Chi2(1)	0.957	0.138	0.416	0.355

Table 6.4C
Instrumented Ordered Probit Estimates of Women's Empowerment:
'Say' in Social Mobility Decisions

Women's opinion taken into consideration in household decisions on:

Independent Variables	Social Visits to Woman's Family Coefficients FIML	Social Visits to Man's Family Coefficients FIML	Social Visits in Neighborhood Coefficients FIML
Treatment Dummy	0.169	-0.063	-0.222
	(0.367)	(0.323)	(0.369)
Age Difference with Husband	0.047**	0.036	0.044**
	(0.022)	(0.022)	(0.022)
Age of Respondent	-0.006	0.009	0.031 *
	(0.019)	(0.020)	(0.018)
Education of Respondent	0.012	0.037	0.017
	(0.031)	(0.029)	(0.030)
Number of Children	-0.055*	-0.002	0.033
	(0.030)	(0.031)	(0.036)
Proportion Adult Male Ever School	0.456**	0.295	-0.186
	(0.216)	(0.205)	(0.237)
Joint Family	-0.157	0.188	0.840**
	(0.393)	(0.315)	(0.389)
Years after Marriage	-0.002	0.001	0.004
	(0.024)	(0.025)	(0.024)
Working Female Before Loan	0.577***	0.168	-0.051
	(0.177)	(0.183)	(0.186)
First Utilization	0.279	0.185	0.204
	(0.186)	(0.177)	(0.186)
First Stage: Treatment			
Age Difference with Husband	-0.033	-0.032	-0.033
	(0.023)	(0.023)	(0.023)
Age of Respondent	0.001	-0.000	0.000
	(0.031)	(0.030)	(0.031)
Education of Respondent	0.038	0.037	0.038
	(0.030)	(0.030)	(0.030)
Number of Children	0.059*	0.062*	0.059*
	(0.034)	(0.035)	(0.034)
Proportion Adult Male Ever School	-0.343	-0.317	-0.350
	(0.227)	(0.229)	(0.228)
Joint Family	0.596	0.495	0.587
	(0.374)	(0.368)	(0.366)
Years after Marriage	0.040	0.038	0.041
	(0.030)	(0.030)	(0.030)
Working Female Before Loan	-0.245	-0.287	-0.247
	(0.230)	(0.233)	(0.232)
First Utilization	0.132	0.127	0.129
	(0.222)	(0.225)	(0.224)
Distance	-0.260**	-0.279**	-0.267**
	(0.128)	(0.130)	(0.129)
Rho	-0.261	-0.005	0.181
	(0.395)	(0.354)	(0.396)
Observations	199	194	199
Wald Chi Squared	27.246	25.606	26.983
p> Chi2	0.002	0.004	0.003
Wald test of pho =0	0.435	0.000	0.210
Chi2(1)	0.509	0.989	0.647
p> Chi2(1)	0.964	0.983	0.738

Table 6.4D: Instrumented Ordered Probit Estimates of Women's Empowerment:
'Say' in Sale/Purchase of HH Assets

Women's opinion taken into consideration in household decisions on:

Independent Variables	Purchase of HH Assets (TV, Fridge) Coefficients FIML	House Repair Coefficients FIML	Sale/Purchase of House Coefficients FIML
Treatment Dummy	0.387	-0.342	-0.352
	(0.400)	(0.300)	(0.296)
Age Difference with Husband	0.087***	0.037	0.036
	(0.024)	(0.031)	(0.032)
Age of Respondent	0.016	0.036	0.049*
	(0.021)	(0.022)	(0.026)
Education of Respondent	0.012	0.035	0.019
	(0.029)	(0.023)	(0.025)
Number of Children	-0.019	0.046*	0.048*
	(0.033)	(0.026)	(0.025)
Proportion Adult Male Ever School	0.091	-0.272	-0.285
	(0.252)	(0.187)	(0.183)
Joint Family	-0.065	0.412	0.267
	(0.378)	(0.310)	(0.297)
Years after Marriage	-0.004	0.013	-0.007
	(0.027)	(0.022)	(0.028)
Working Female Before Loan	0.061	-0.021	-0.240
	(0.177)	(0.184)	(0.179)
First Utilization	0.366**	0.225	0.211
	(0.172)	(0.182)	(0.182)
First Stage: Treatment			
Age Difference with Husband	-0.034	-0.037*	-0.032
	(0.022)	(0.021)	(0.021)
Age of Respondent	0.004	-0.001	0.001
	(0.031)	(0.030)	(0.030)
Education of Respondent	0.038	0.036	0.038
	(0.029)	(0.029)	(0.029)
Number of Children	0.053	0.054*	0.053*
	(0.033)	(0.032)	(0.032)
Proportion Adult Male Ever School	-0.344	-0.307	-0.326
	(0.226)	(0.225)	(0.225)
Joint Family	0.583	0.616	0.574
	(0.369)	(0.381)	(0.386)
Years after Marriage	0.041	0.042	0.041
	(0.030)	(0.029)	(0.029)
Working Female Before Loan	-0.222	-0.200	-0.227
	(0.231)	(0.232)	(0.228)
First Utilization	0.107	0.134	0.106
	(0.222)	(0.225)	(0.222)
Distance	-0.262**	-0.229*	-0.245**
	(0.122)	(0.117)	(0.121)
Rho	0.000	0.936**	0.791 *
	(0.412)	(0.418)	(0.416)
Observations	198	199	199
Wald Chi Squared	27.379	25.910	25.982
p> Chi2	0.002	0.004	0.004
Wald test of pho =0	0.000	5.004	3.616
Chi2(1)	1.000	0.025	0.057
p> Chi2(1)	0.000	0.936**	0.791 *

Table 6.5A: Probit Estimates of 'Autonomy' in HH Decisions

Women do not require someone's permission to purchase:

Independent Variables	Grocery/Fruits		Medicine for herself		Personal Clothes/Cosmetics	
	Probit Coefficients		Probit Coefficients		Probit Coefficients	
	(1)	(2)	(1)	(2)	(1)	(2)
Treatment Dummy	0.012	0.358	-0.532**	-0.012	-0.365	0.185
	(0.245)	(0.379)	(0.249)	(0.359)	(0.262)	(0.371)
Age Difference with Husband	0.003	0.000	0.D28	0.024	0.001	-0.003
	(0.020)	(0.020)	(0.021)	(0.021)	(0.021)	(0.021)
Age of Respondent	0.020	0.023	0.026	0.029	0.046*	0.051 **
	(0.026)	(0.027)	(0.025)	(0.025)	(0.025)	(0.026)
Education of Respondent	0.043	0.047	0.067**	0.072**	0.038	0.042
	(0.031)	(0.032)	(0.030)	(0.032)	(0.028)	(0.029)
Number of Children	-0.037	-0.036	-0.012	-0.011	0.038	0.039
	(0.034)	(0.034)	(0.031)	(0.031)	(0.034)	(0.035)
Proportion Adult Male Ever School	0.155	0.137	-0.138	-0.166	-0.118	-0.138
	(0.259)	(0.265)	(0.248)	(0.255)	(0.244)	(0.251)
Joint Family	-0.509	-0.525	0.043	0.012	0.022	-0.002
	(0.325)	(0.325)	(0.312)	(0.320)	(0.329)	(0.326)
Years After Marriage	0.029	0.030	0.034	0.035	0.011	0.012
	(0.024)	(0.024)	(0.023)	(0.023)	(0.023)	(0.023)
Working Female Before Loan	0.180	0.117	0.269	0.183	0.161	0.070
	(0.237)	(0.243)	(0.224)	(0.228)	(0.229)	(0.232)
First Utilization	0.090	0.071	0.384*	0.355	0.286	0.239
	(0.231)	(0.231)	(0.226)	(0.225)	(0.218)	(0.219)
Institution*Treatment		-0.442		-0.657**		-0.693**
		(0.353)		(0.327)		(0.330)
Number of Observations	200	200	200	200	200	200
Pseudo R-Squared	0.114	0.122	0.141	0.158	0.113	0.131
Chi2 statistics	20.676	22.745	34.778	37.790	30.803	33.979
Log Likelihood	-92.278	-91.435	-103.453	-101.411	-99.807	-97.726

Standard deviations are reported in the parenthesis.
$*p<0.1$, $**p<0.05$, $***p<0.01$

Table 6.5B: Probit Estimates of 'Autonomy' in HH Decisions

Women do not require someone's permission to purchase:

Independent Variables	Buy Ice Cream For Children		Buy Books/Uniform for Children		Take a Child to the doctor	
	Probit Coefficients		Probit Coefficients		Probit Coefficients	
	(1)	(2)	(1)	(2)	(1)	(2)
Treatment Dummy	0.303	0.335	-0.227	0.122	-0.387	0.232
	(0.291)	(0.400)	(0.272)	(0.364)	(0.250)	(0.387)
Age Difference with Husband	-0.018	-0.019	0.027	0.023	0.006	0.002
	(0.024)	(0.024)	(0.023)	(0.023)	(0.020)	(0.021)
Age of Respondent	0.073**	0.073**	0.034	0.035	0.026	0.031
	(0.031)	(0.031)	(0.027)	(0.027)	(0.026)	(0.026)
Education of Respondent	0.039	0.040	0.060*	0.061 *	0.108***	0.113***
	(0.038)	(0.038)	(0.032)	(0.032)	(0.031)	(0.034)
Number of Children	-0.022	-0.022	-0.002	-0.003	-0.033	-0.033
	(0.042)	(0.042)	(0.033)	(0.033)	(0.031)	(0.031)
Proportion Adult Male Ever School	-0.234	-0.235	0.216	0.229	-0.290	-0.309
	(0.316)	(0.317)	(0.251)	(0.253)	(0.252)	(0.262)
Joint Family	-0.044	-0.046	0.052	0.042	0.338	0.344
	(0.404)	(0.406)	(0.320)	(0.327)	(0.338)	(0.343)
Years After Marriage	0.003	0.003	0.001	0.002	0.023	0.024
	(0.026)	(0.026)	(0.026)	(0.027)	(0.024)	(0.025)
Working Female Before Loan	-0.022	-0.029	0.539**	0.481 **	0.616***	0.533**
	(0.304)	(0.304)	(0.220)	(0.223)	(0.225)	(0.231)
First Utilization	0.509*	0.505*	0.516**	0.510**	0.284	0.234
	(0.290)	(0.291)	(0.228)	(0.227)	(0.214)	(0.216)
Institution*Treatment		-0.041		-0.432		-0.779**
		(0.392)		(0.304)		(0.356)
Number of Observations	196	196	174	174	197	197
Pseudo R-Squared	0.186	0.186	0.133	0.141	0.152	0.175
Chi2 statistics	30.153	30.088	27.312	31.709	35.889	38.657
Log Likelihood	-57.667	-57.662	-103.958	-103.025	-107.087	-104.227

Standard deviations are reported in the parenthesis.
$*p<0.1$, $**p<0.05$, $***p<0.01$

**Table 6.6: Treatment Effect Estimates of Women's Empowerment
Propensity Score Matching Method**

Women do not require someone's permission to:

Independent Variables	ATT (Kernel) (1)	ATT (Radius) (2)	NT	NC
Purchase Grocery/Fruits	-0.007	-0.015	148	50
	(-0.073)	(0.069)		
Purchase Medicine for self	-0.104 *	-0.100	148	50
	(-0.061)	(0.072)		
Purchase Cosmetics/Clothes	-0.027	-0.026	148	50
	(-0.066)	(0.074)		
Purchase Ice Cream for Children	0.081	0.067	147	47
	(-0.064)	(0.059)		
Purchase Books/Uniform for Children	-0.056	-0.045	136	36
	(-0.104)	(0.096)		
Take a Child to Doctor	-0.068	-0.084	147	48
	(-0.087)	(0.075)		

Standard deviations are reported in the parenthesis.
*$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6.7A: Instrumented Probit Estimates of 'Autonomy' in HH Decisions

Women do not require someone's permission to purchase:

Independent Variables	Grocery/Fruits	Medicine for herself	Personal Clothes/Cosmetic
	Coefficients FIML	Coefficients FIML	Coefficients FIML
Treatment Dummy	1.109	-1.069	-1.605***
	(1.722)	(1.638)	(0.171)
Age Difference with Husband	0.017	0.019	-0.015
	(0.021)	(0.033)	(0.019)
Age of Respondent	0.018	0.026	0.030
	(0.028)	(0.024)	(0.023)
Education of Respondent	0.028	0.072**	0.048**
	(0.042)	(0.031)	(0.024)
Number of Children	-0.054	-0.001	0.061 **
	(0.042)	(0.052)	(0.030)
Proportion Adult Male Ever School	0.262	-0.197	-0.251
	(0.273)	(0.289)	(0.201)
Joint Family	-0.516	0.080	0.341
	(0.346)	(0.410)	(0.316)
Years After Marriage	0.011	0.041	0.033
	(0.034)	(0.029)	(0.022)
Working Female Before Loan	0.182	0.239	-0.006
	(0.233)	(0.275)	(0.194)
First Utilization	0.092	0.381 *	0.291
	(0.229)	(0.225)	(0.198)
First Stage: Treatment			
Age Difference with Husband	-0.032	-0.034	-0.032
	(0.022)	(0.023)	(0.021)
Age of Respondent	0.003	0.000	-0.001
	(0.030)	(0.031)	(0.031)
Education of Respondent	0.043	0.037	0.048
	(0.034)	(0.030)	(0.029)
Number of Children	0.068*	0.055	0.066**
	(0.038)	(0.035)	(0.033)
Proportion Adult Male Ever School	-0.329	-0.364	-0.418*
	(0.234)	(0.233)	(0.228)
Joint Family	0.607	0.557	0.474
	(0.419)	(0.385)	(0.412)
Years after Marriage	0.040	0.041	0.040
	(0.028)	(0.031)	(0.032)
Working Female Before Loan	-0.240	-0.236	-0.195
	(0.239)	(0.239)	(0.225)
First Utilization	0.168	0.115	0.124
	(0.223)	(0.233)	(0.239)
Distance	-0.214	-0.270**	-0.252***
	(0.239)	(0.126)	(0.085)
Observations	199	199	199
Wald Chi Squared	75.691	67.391	132.554
p > Chi2	0.000	0.000	0.000
rho	-0.657	0.356	1.000
Wald test of pho = 0			
Chi2(1)	0.228	0.088	64.642
p > Chi2(1)	0.6327	0.7670	0.0000

Table 6.7B: Instrumented Probit Estimates of 'Autonomy' in HH Decisions

Women do not require someone's permission to:

Independent Variables	Buy Ice-cream/ Sweets for Children Coefficients FIML	Buy Books/Uniform for Children Coefficients FIML	Take a Child to the doctor Coefficients FIML
Treatment Dummy	-1.228***	-0.992	-0.188
	(0.165)	(0.844)	(1.173)
Age Difference with Husband	-0.038*	0.015	0.005
	(0.021)	(0.027)	(0.022)
Age of Respondent	0.038	0.029	0.026
	(0.027)	(0.027)	(0.027)
Education of Respondent	0.038	0.063**	0.104***
	(0.026)	(0.031)	(0.036)
Number of Children	0.019	-0.001	-0.036
	(0.032)	(0.033)	(0.037)
Proportion Adult Male Ever School	-0.390	0.083	-0.270
	(0.240)	(0.262)	(0.284)
Joint Family	0.274	0.082	0.265
	(0.362)	(0.353)	(0.369)
Years After Marriage	0.033	0.014	0.021
	(0.027)	(0.028)	(0.029)
Working Female Before Loan	-0.142	0.523 **	0.633***
	(0.222)	(0.225)	(0.230)
First Utilization	0.326	0.503 **	0.257
	(0.214)	(0.233)	(0.221)
First Stage: Treatment			
Age Difference with Husband	-0.035	-0.037	-0.025
	(0.022)	(0.027)	(0.023)
Age of Respondent	0.003	-0.034	0.005
	(0.029)	(0.028)	(0.032)
Education of Respondent	0.045	0.027	0.036
	(0.028)	(0.035)	(0.030)
Number of Children	0.047	-0.003	0.047
	(0.037)	(0.037)	(0.035)
Proportion Adult Male Ever School	-0.353	-0.519**	-0.417*
	(0.236)	(0.262)	(0.233)
Joint Family	0.528	0.667	0.570
	(0.386)	(0.460)	(0.380)
Years after Marriage	0.037	0.060**	0.038
	(0.029)	(0.027)	(0.031)
Working Female Before Loan	-0.216	-0.072	-0.286
	(0.222)	(0.262)	(0.236)
First Utilization	0.097	0.205	0.112
	(0.222)	(0.287)	(0.224)
Distance	-0.255***	-0.271 **	-0.295**
	(0.099)	(0.132)	(0.134)
Observations	195	173	196
Wald Chi Squared		54.917	63.047
p > Chi2		0.000	0.000
rho	1.000	0.459	-0.126
Wald test of pho = 0			
Chi2(1)	0.474	0.677	0.036
p > Chi2(1)	0.491	0.8504	0.4108

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